# Chapter 7 Economists' Classic General Market Models

## § 1. Economists' Models

Every natural science is an undertaking aimed at studying, understanding, and applying knowledge of specific kinds of empirical phenomena experientially encountered in the world. The "social sciences" do not differ in aim from this although each "social science" has its critics who point out its shortcomings and sometimes even challenge whether this or that "social science" deserves to be called a "science" at all. This applies to economics as much as any of the others despite the distinction economics has of being the only "social science" for which Nobel Prizes are awarded. The numerous notable shortcomings of the "social sciences" do not arise from lack of effort by their practitioners to study and understand the empirical world. They arise from inadequate focus on human beings as the "social atoms" of all human-natural phenomena. This misfocus prevents the present day "social sciences" from being social-*natural* sciences.

Economics is as culpable for this misfocus as all the others. Economists do employ particular assumptions about human behavior but this "economists' psychology" is extremely limited, not based on rigorous psychology research and has even been called a childish caricature of human nature<sup>1</sup>. Neither is it especially accurate to say economists employ only one generally agreed to psychology model. Present day economics distances itself so fast from its social atoms that psychological assumptions used by economists tend to show up only in assumptions that go into defining mathematical idealizations employed as models of economic circumstances.

A *model* is a representation that mirrors, duplicates, imitates, or in some way illustrates a pattern of relationships observed in data or nature. Practitioners of every kind of science make and use models because the world is a very complicated place. If a scientist is to be capable of drawing any findings or conclusions about it at all, he must winnow out an enormous number of factors that appear to make no *significant* contribution to the phenomena he observes – even if the ignored factors are known to be present in the actual situations – and give his attention to those factors that provably *do* make a *causative* difference in the outcomes. William James wrote,

There can *be* no difference anywhere that doesn't *make* a difference elsewhere – no difference in abstract truth that doesn't express itself in a difference in concrete fact, imposed on somebody, somehow, somewhere, and somewhen. The whole function of philosophy ought to be to find out what definite difference it will make to you and me, at definite instants of our life, if this world-formula or that world-formula be the true one. [James (1907), pg. 25]

Models are pragmatic attempts to take into account factors that make a difference in the way James expressed and to leave out of the account those factors which do not. Determining what the factors are that should be taken into account is called *qualitative* modeling; putting numbers to these factors to represent their relative real causal efficacies is called *quantitative* modeling. It is a fact, skipped over or downplayed in most undergraduate college courses in economics, that all the major modeling ideas used in economics – supply and demand curves are examples – are in fact qualitative rather than quantitative models. When an economist must have numbers for how the demand for a specific commodity varies with the price of that commodity, he must go and make a measurement of some sort to estimate it because his models are not quantitative and so can not predict actual empirical demand curves.

<sup>&</sup>lt;sup>1</sup> Perhaps a different adjective should be used in this description because to call it a "childish" caricature is unfair and a little insulting to a typical ten-year-old. A ten-year-old doesn't know much about supply-and-demand or marginal utility but typically is more keenly aware of the nuances of social interactions than "economists' psychology" models can deal with [Piaget (1932), pp. 42-50, 65-84, 295-325].

This facet of the classic economic models – that they are qualitative rather than both qualitative and quantitative – is the reason why economics theorems are deduced by what mathematicians call *dialectic* proofs. A dialectic proof is an existence proof; it proves that an answer or a solution exists but it does not tell us what that answer or solution is. This is in contrast to what mathematicians call an *algorithmic* proof. Renowned Swiss applied mathematician Peter Henrici said,

*Dialectic mathematics* is a rigorously logical science, where statements are either true or false, and where objects with specified properties either do or do not exist. *Algorithmic mathematics* is a tool for solving problems. Here we are concerned not only with the existence of a mathematical object, but also with the credentials of its existence. *Dialectic* mathematics is an intellectual game played according to rules about which there is a high degree of consensus. The rules of the game of *algorithmic* mathematics may vary according to the urgency of the problem at hand. We never could have put a man on the moon if we insisted that trajectories should be computed with dialectic rigor. . . . *Dialectic* mathematics generates insight. *Algorithmic* mathematics generates results. [quoted in Davis & Hersh (1981), pg. 183]

There is certainly nothing wrong with dialectic proof or dialectic solution (provided the proof or the solution does not contain logic errors). Indeed, many very important questions that sciences seek to answer can only be answered dialectically because we do not know enough to be able to say more than "there is a solution" or "that is not a solution." Dialectic answers tend to be qualitative answers, not quantitative ones. They are also the only kinds of answers that can be obtained from qualitative models. Without dialectic mathematics, economics as a science really could tell us nothing that is particularly fecund. A scientist is well advised to establish a dialectic existence proof *before* he goes to the time and expense of seeking an algorithmic solution to a problem. But one thing that *must* be clearly understood about economics and economic theory is that dialectic mathematics cannot give anyone a *specific* quantitative result. It cannot tell Bill the Baker how many loaves of white bread he should bake on any given day. It cannot tell Congress that this or that tax policy will reduce unemployment by so many million people.

Dialectic mathematics *can* put qualitative economic models to work to inform us about trends or tendencies. Indeed, in the vast majority of cases where a businessman must make a decision a trend is the only thing he practically *can* know because the data that is needed to get any more precise an answer is data he simply cannot obtain. He must make his decision *with risk* and then modify his plans and change his tactics as more data becomes available. For example, about twenty-five years ago I chaired a committee, called "the tactical planning committee," made up of people from Research & Development, Marketing, Production, Purchasing, Finance, and some other functional areas of our Company. There were about a dozen people on this committee who represented a few hundred people working on our project in divers ways. We met every Tuesday morning at 10:00 a.m. for the express purpose of reviewing what had happened in the world since our last meeting and, when necessary, changing our tactical plans. It was a very unusual Tuesday if we did *not* make alterations to our tactical plan. Some of these changes affected the expenditure of millions of dollars or the employments of hundreds of people.

Economists do recognize and try to account for many factors that significantly affect the commercial environment of business entities. The name given to the particular sub-branch of economics that studies and analyzes these factors is "microeconomics" – also known as "the theory of the firm." Some of these significant factors are

- the number and size distribution of sellers of a particular commodity or service;
- the extent to which these sellers recognize that the actions of one of them will cause others to change their behavior;

- the nature of the similarities and differences in the products of different sellers;
- the nature, number, and size of purchases;
- the extent of knowledge buyers and sellers have about prices of other transactions;
- the ability of individual sellers to change market demand by advertising, by quality improvements in the product or services, etc.; and
- the ease with which new sellers may enter or existing sellers may exit the particular market environment. [Lipsey & Steiner (1969), pg. 273]

There are no fewer than ten potentially causative factors in this list and this list is not even a complete list of possible economic factors. Some of the factors in this list could, in principle, be quantified with numbers, although week-to-week or day-to-day variability of the factor can make the accuracy of a model very dubious<sup>2</sup>. Other factors in this list, such as "the extent of know-ledge" buyers and sellers have cannot be quantified with real objective validity.

What this means is that we cannot find fault with economists when they resort to having to use the sorts of qualitative models that presently underlie the most important findings of economics theory. We *can* find fault with teachers of economics who fail to convey this qualitative nature of the models to their students. Their students will later quite often either misapply the theories (by thinking they have a quantitative model when they do not) or, upon discovering later that they cannot in practice put numbers to the model, utterly dismiss economics theory altogether in the practice of business. When I was young, I was first surprised and then appalled at how ignorant most corporate general managers, vice presidents, and presidents are of the most basic lessons of economics. I have seen high-priced executives make mistakes a sophomore economics major would not make. I have seen some of their mistaken decisions put their divisions or companies out of business. Ignorance of economics is an important contributor to the general level of incompetence with which most large American corporations are managed.

A ten-factor model is a very complicated model. It can be usefully employed in many real world empirical situations, but to do so successfully generally requires that the model maker/user be someone who has deep technical expertise in systems theory plus broader knowledge of the field to which the model is being applied. Such people are in chronic short supply. Further, a model with ten or more factors generally requires the use of a computer in order to get any useful answers from it. This adds the rather obvious additional challenge of "how does one put some kind of number in the computer that is really descriptive of a *qualitative* factor?" As it happens, there is a branch of applied mathematics by which this can be done. It is called 'set membership theory' (SMT) [Wells (2011)]. Unfortunately, there probably aren't more than a few dozen set membership theorists in the entire United States at present. It isn't something schools teach.

This means economists have to find more practical approaches if economic theory is to have any significant applicability to the real world of business and commerce. They know this and the approach they have undertaken has been to develop a relatively small set of approximate 'limiting case' market models that have been useful in actual practice. Two of these models have names familiar to many people: perfect competition and monopoly. The others are less familiar to most.

<sup>&</sup>lt;sup>2</sup> As you may already know, economics makes extensive use of statistical models. Quantitative variability in these factors produces what statistical signal processing theorists call 'non-stationary statistical models.' The mathematics that must be used for models of this class present some of the most formidable challenges in applied algorithmic mathematics. I doubt if there are as many as a hundred people in the United States who have the mathematical training necessary to understand and competently use models of this class. Non-stationary statistics are the reason why attempts that have been made to use signal processing models to predict what the stock market is going to do have failed spectacularly. Indeed, people who have tried this have used *stationary statistics* models, which is enough to demonstrate they lack adequate understanding of the theory, methods, and practices of statistical signal processing.

Seller-side Models	Buyer-side Models
perfect competition	perfect competition
monopolistic competition	monopsonistic competition
oligopoly	oligopsony
monopoly	monopsony

Table 1 Economists' Classic Market Models

Table 1 presents the typical fourfold arrangement of the seven major market models that are most often used by economists. All of these models are idealizations rarely, if ever, encountered in the real world. Their fecundity comes from the fact that, taken together, it is possible to make use of their divers trends and characteristics to get approximations for real commercial situations, all of which tend to lie somewhere in between the extremes these classic models represent and can empirically be regarded as mixtures or combinations of them. Each model is discussed in the sections that follow.

## § 2. 'Efficiency' in Economics

Economists have long held an idea of something they call 'efficiency.' Most of us tend to regard efficiency as a good thing most of the time (all else being equal) and so the economists' label for their idea tends to create a favorable impression on people. However, one should be aware that in the sciences everyday words are put to use with specialized meanings that often bear little overt resemblance to how most people typically use these same words. In physics 'work' is such a technical term. What physicists call 'work' is something no coal miner would recognize as "work." When science and engineering students in college encounter this word in their first physics course during their freshman or sophomore years, most of them covertly raise an eyebrow at the peculiar way their professors are using the word 'work'. After all, pushing a pencil (or, these days, pressing a computer key) is the only thing in the "work" a bookkeeper does that recognizably matches up at first glance with the physics definition of 'work'. College students are clever people and soon figure out that the most prudent thing to do is shrug and start using 'work' the same way the professor does. The alternative is to flunk the physics course.

'Efficiency' is a concept on which economists place great emphasis, and I have no doubt that economists regard 'economic efficiency' as a "good thing." But their term doesn't mean the same thing most people mean by "efficiency" and *sociologically* 'economic efficiency' is *not* a "good thing." The concept is one that moved historian and essayist Thomas Carlyle to label economics "the Dismal Science" in 1849. The concept of 'efficiency' is wedged deeply into all the market models in table 1 and so we must understand what 'efficiency' means in economic theory.

Economists define *economic efficiency* as

**economic efficiency:** The state of an economy in which no one can be made better off without someone being made worse off. For this to be true three types of efficiency must hold: (a) *productive efficiency*, in which the output of the economy is being produced at the lowest cost; (b) *allocative efficiency*, in which resources are being allocated to the production of goods and services the society most values; and (c) *distributional efficiency*, in which output is distributed in such a way that consumers would not wish, given their disposable income and market prices, to spend these incomes in any different way. [Bannock *et al.* (2003)]

There are some fundamental flaws in this definition. An economy is said to be efficient if no one can be made better off without someone else being made worse off. What does it mean to be "better off" or "worse off" and who decides this? Economists do assume that being "better off" or "worse off" is something that is judged by individuals and is judged in terms of how they assess the condition of their own welfare. So far, so good; but economic theory does not leave matters in this subjective and ill-posed state of definition. After all, if it did then an economist would find it prohibitively difficult to calculate anything and would be faced with a qualitative model of enormous factor complexity. If he is to be able to make any kind of general finding about economic phenomena, he must impose special restrictions defining what *he* will regard as "being better off" and "being worse off." Furthermore, these restrictions must be made to define something or some things that can be measured in some way such that these measurements are indicative of "being better off" or "being worse off." The definitions and measures that have been adopted by economic theorists turn out to be definitions by mathematical fiat for which empirical objective validity becomes a fundamental issue and raises doubts about whether the theory explains anything human beings encounter in the real world.

I want to dwell on this point for a moment because the issue is a broad one not confined to the theory of economics. It shows up also in engineering, in physics, and in other sciences that make extensive use of mathematics. The issue is this. Mathematics is both an analytical *tool* and a *language* for saying things precisely and in such a way that consequences can be deduced from what has been said. However, because the great majority of people, including scientists, are not professional mathematicians there is a pronounced tendency in all the sciences for practitioners to use the mathematics with which they are most familiar and have been trained in school to use.

But actual phenomena – to anthropomorphize them a bit – are utterly indifferent as to whether or not they can be accurately described by the tools of the "everyday math" people use: algebra, calculus, etc. Mathematics is wonderfully protean and mathematical structures can usually be invented and designed to describe just about any sort of actual phenomenon. In my professional engineering and science practices I encountered the need for inventing non-ordinary mathematical structures rather frequently. I used to describe this to my students as "rolling your own math." This idea can sound intimidating when you first hear about it, and it does take a little getting used to, but I found it to be one of the things that made the practice of science fun to do. It does, after all, really amount to just making it clear to yourself how you are going to describe and explain a particular phenomenon in nature *with precision*. Philosophers have often wondered how it is that mathematics seems to describe the real world. The answer is perhaps disappointingly simple. Math describes nature because scientists *make it* describe nature. If one description does not work, we try another and another until we find one that *does*.

Unfortunately, only professional mathematicians are usually provided with the training needed to invent and design new mathematical structures. Most people, including most bachelor's degree mathematics majors, aren't even aware that *all* of mathematics involves invented or designed mathematical structures. The relatively few people who do learn about this usually learn about it in graduate school and, even there, often by accident. What I call 'the engineering of mathematics' is not taught in any course offered by any university anywhere in the world so far as I know.

This has a consequence. If you do not know "it's okay" to design mathematics to specifically "fit" a particular phenomenon, what will you do instead? The answer in an enormous number of cases is: you will try to *make the problem fit the math* instead of *making the math fit the problem*. But to do this is, as the old saying goes, to put the cart in front of the horse. Mathematics is the product of human creativity and *all* of its objects – without exception – are *noumena* (objects that can never be actually experienced in sensible nature). Mathematical objects are the way they are *because we define them to be that way*. If you make the problem fit the math, what you have is an *imaginary* problem, not a real one. Set membership theory, which I mentioned above, is based on understanding that we, as scientists, have to make our math fit the problem and not *vice versa*.

Now let's get back to 'economic efficiency' and what economists are trying to describe by this mathematical concept. It perhaps might be best to start with what they are *not* trying to describe or suggest. The concept has nothing whatsoever to do with economic egalitarianism. If there *is* such a thing as a state of economic efficiency *in empirical nature* – and, mind you, there is no proof or ground whatsoever for saying that there is or even that such a state could be possible<sup>3</sup> – this state does not imply that everyone has an equivalent stock of economic wealth-assets such that for one person to increase his stock he could only do so by taking something from another person's stock of wealth-assets. There are some people – mostly people who are not economists but a few who are – who interpret the first sentence of the definition above as implicating this. This is a false interpretation of what the concept of 'economic efficiency' means.

A clearer idea can be obtained from the three 'efficiencies' said to be necessary for economic efficiency to be possible. The first, 'productive efficiency' is said to occur when 'the output of the economy' is 'being produced at the lowest cost.' The notion behind this idea is a simple enough one. If something – *specifically* some *thing* – can be produced at a lower cost than it is currently and if the behavioral dynamics of commercial enterprises adapt in such a way that it does come to be produced at that lower cost, then in some sense production can be said to be made more cost efficient. If you produce something for \$10 per unit and I can start profitably producing (and selling) the same thing for \$8 per unit then I am a more cost-efficient producer of it than you.

The problem emerges when theorists try to generalize this idea to that of "the output of an economy." What exactly *is* "the output" of "an economy"? Is it the sum total of *every* economic good and service provided by everyone engaged in commercial activities? It would seem it must be precisely this; but in that case, do we count the labor services provided by factory workers, truck drivers, bookkeepers, etc., etc.? If not, why not? What is the 'economic output' of a public school teacher? a school superintendant? a police officer? a janitor? a railroad conductor? a bank teller? an airline flight attendant? In fact, economists do not measure and account for *these* kinds of economic activities in their statistics because they are *practically* difficult to *estimate* at all, much less *precisely measure*. How do you put a cost assessment on an intangible? What *is* the 'output of an economy'? No one really knows. Any dollar figure put on it measures nothing but an arbitrary definition by mathematical fiat. Here is an example of making the problem fit the math.

'Allocative efficiency' is said to be attained when "resources are being allocated to the production of goods and services the Society most values." Implicit in this pseudo-explanation is the notion that "resources" are strictly finite and their allocation is therefore a zero-sum game. If one puts more "resources" into producing Good A, then there must be fewer "resources" available for producing Good B. What resource limit, though, applies to that economic good/service we call "information"? People pay to "get information" every day. News companies and survey

<sup>&</sup>lt;sup>3</sup> Economic efficiency is an Object of pure mathematics. It is not an object of the world of actual experience but, rather, one of the "mathematical world" – what David Slepian called "facet B" of scientific theory [Slepian (1974)]. The concept of economic efficiency has no ontological significance whatsoever; it has only epistemological significance.

takers depend on this fact for their livelihoods.

But a more serious flaw in the idea of allocative efficiency is the idea that a Society "values" anything at all, much less values one thing more than another. The objectively valid explanation of a **Society** is the Object understood as a higher concept of divers individual concepts of society retaining what is contained in common among these divers concepts and manifested by a mathematical field construct. A society is the mathematical object of a mathematical concept formed by an individual and: (1) suitable for one or more of his purposes; (2) having its principal quantities represent appearances of individuals; (3) having no ontological significance whatsoever; and (4) in logical essence, the concept is a concept of relationships and associations. A Society is an Object of mathematics, as is a society. Mathematical Objects "value" nothing. Individual people "value" things. What economists more or less mean when they say Society "values" this or that is that a particular aggregate of people regard these and those things as wealth-assets and are willing to trade something for them. But what does it mean to say some thing is "valued the most"? Because there are always at least two parties in any exchange, why aren't the things traded for the "valued" commodity just as "valued"? Shall we make the definition of "valued" depend on the quantity of things traded? the number of people who trade them? the rate at which they are consumed? Any answer proposed is going to be another mathematical fiat.

Finally, 'distributional efficiency' is the situation said to be attained when "consumers would not wish to spend their incomes in any other way" (given what these incomes are and what the price of commodities and services are). Which consumers, though? All of them? Most of them? Some of them? Not long ago in Detroit there was a political uproar because potable water from the city was being denied to a not-small number of Detroit residents who did not pay their water bills. No one ever said these people had no money at all; therefore the disposable incomes they did have was being spent on things *other* than water, *e.g.* groceries and shelter. Did these people *not*-value water as a commodity? If so, why did they object to not getting it? Were city and public service officials in Detroit being 'distributionally efficient' by turning off the supply of city water to these people? If so, why was the matter controversial even among people who did pay their water bills and even among people who did not live in Detroit?

Economists do not say or imply that there is any one person or group of persons who make determinations of what sorts of possible allocations would be 'efficient'. Rather, what is proposed is that commercial transactions among many people over a long enough period of time will evolve towards circumstances where everyone is satisfied with the accidental allocations of goods and services he receives and with those he provides. This is as much as to propose that natural dynamics of commerce are such that the members of a Society make choices and actions leading to a state of domestic tranquility insofar as this involves commercial transactions. The metaphor used by Adam Smith to describe this proposition was "an invisible hand":

The produce of an industry is what it adds to the subject or materials upon which it is employed. In proportion as the value of this produce is great or small, so likewise will be the profits of the employer. But it is only for the sake of profit that any man employs a capital in the support of industry; and he will always, therefore, endeavor to employ it in the support of that industry of which the produce is likely to be of the greatest value, or to exchange for the greatest quantity either of money or of other goods.

But the annual revenue of every society is always precisely equal to the exchangeable value of the whole annual produce of its industry, or rather is precisely the same thing with that exchangeable value. As every individual, therefore, endeavors as much as he can both to employ his capital in the support of domestic industry, and so to direct that industry that its produce may be of the greatest value, every individual necessarily labors to render the annual revenue of the society as great as he can. He generally, indeed, neither intends to promote the public interest, nor knows how much he is promoting it. By preferring the

support of domestic to that of foreign industry, he intends only his own security; and by directing that industry in such a manner as its produce may be of greatest value, he intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. . . . By pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it. [Smith (1776), Bk IV, chap. II, pp. 398-399]

This idea of Smith's is probably used more than any other to argue for *laissez faire* free enterprise. But is the proposition true? If you read earlier in *Wealth of Nations*, in the paragraphs leading up to this most famous of Smith's ideas, what you will find is that the proposition above is a speculation that was based on some particular hypotheses concerning human psychology and human behavior. In other words, the proposition is not an observation of economic social-nature but, rather, is a consequence derived from psychological presuppositions. These presuppositions have, over the course of many years, come to be known as the "rational-economic man model." I have more to say about this model below, but just at this point I will say that this psychological model of human nature is not an accurate description of human nature. Therefore, Smith's entire "invisible hand" argument is a speculation erected on very unsolid grounds.

Some observable features of commercial phenomena are not-incongruent with Smith's proposition. Some observable features are incongruent with it. Such instances are fairly common in the financial capitalism of stock market investing. Furthermore, if individuals act to locally optimize, or attempt to optimize, their own welfare circumstances, this by itself is neither a necessary nor a sufficient condition for optimum results to occur all throughout a Society. Oftentimes what makes one person's aims easier to achieve also makes others' aims harder to achieve. One sees examples of this very frequently in large corporations or large public Institutes. The finance department, in order to do its task more efficiently, promulgates paperwork and record keeping requirements on all other departments, which takes away from people in those other departments some amount of time and requires some efforts on their parts that do not immediately serve to help them achieve *their* local aims. What is at issue here is the issue of *interactions* within what I have called the Enterprise-protein model of commerce. Figuratively speaking, sometimes interaction phenomena are like mixing amphetamines with tranquilizers: the effect is something quite different from the effect of either dosage by itself.

All this is to say: no connection is established between distributional efficiency and domestic tranquility that is *known* to be objectively valid. Distributional efficiency is an hypothesis that is by no means established as an empirical fact. There are ample grounds to doubt the generality of the hypothesis. Thus it lacks what a general principle is required to have.

The simple fact is 'economic efficiency' and all three of its necessary components are ideas of mathematics that economists define by fiat and then treat as occult qualities. From the time of the Renaissance to the present day, philosophers and scientists have held that to explain a natural phenomenon by using an occult quality is the same as not explaining it at all. The hard part, sometimes, lies in recognizing that some idea *is* the idea of an occult quality, i.e., the idea of a *noumenon* beyond the horizon of possible human experience. So-called 'economic efficiency' is such a *noumenon*. Its idea has no real objective validity and its use is an *asylum ignorantiæ*. It is an idea that a natural science cannot use in its practices and speculations with objective validity; it is, in other words, an idea without actual *scientific meaning*. Occult qualities produce vague theories. Physicist and Nobel Laureate Richard Feynman said,

In general we look for a new [scientific] law by the following process. First we guess it. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation to see if it works.... You can see, of course, that with this method we can attempt to disprove any definite theory. If we have a definite theory, a real guess, from which we can conveniently compute consequences which can be compared with experiment, then in principle we can get rid of any theory. There is always the possibility of proving any definite theory wrong; but notice that we can never prove it right. . . In the future you could compute a wider range of consequences, there could be a wider range of experiments, and you might then discover that the thing is wrong. . . .

Another thing I must point out is that you cannot prove a vague theory wrong. If the guess you make is poorly expressed and rather vague, and the method you use for figuring out the consequences is a little vague – you are not sure, and you say, "I think everything's right because it's all due to so and so, and such and such do this and that more or less, and I can sort of explain how this works . . .", then you see that this theory is good because it cannot be proved wrong! Also if the process of computing the consequences is indefinite, then, with a little skill, any experimental result can be made to look like the expected consequences. . . . By having a vague theory it is possible to get either result. . . . Yes, but then you cannot claim to know anything about it. [Feynman (1965), pp. 156-159]

'Economic efficiency' is such a vague thing with an indefinite process of computing its consequences. So are its three constituent ideas of 'efficiency'. This is an understanding that must be kept in mind at all times as we look at the classic market models.

But if 'economic efficiency' is such a vague object, how can economists use it at all? Here is another place where pseudo-psychological modeling in economics meets up with speculation. Economists presume that consumers and producers are "rational" in the choices they make and rationally act "in their own best interests<sup>4</sup>." If they would always do so, then more specific particular behaviors having trends that are statistically characterizable can be postulated, leading to qualitative trends suitable for dialectic mathematics argumentation. And thus a 'finding' is born.

This 'psychology' peculiar to economics theory is known as the "rational-economic man" model. The definition given to this term by psychologists is:

**rational-economic man:** a general model of behavior predicated on the assumption that the basic, fundamental principle underlying human nature is the rational striving for self-interest, particularly economic and material gain. [Reber & Reber (2001)]

The problem with the rational-economic man model is that it isn't true. Human nature is not fundamentally based on the rational striving for self-interest. Human beings are satisficing decision makers and problem solvers. They do not seek for the rationally best answer nor do they even take the time to evaluate what is or is not in their own self-interest. They seek for an answer that satisfies. So-called "utility" in economics is another occult quality introduced to "explain" why individuals do not behave according to the way the rational-economic man model says they should. This, too, must be kept in mind in looking at the classic market models.

#### § 3. Objectives and Difficulties of Market Models

The criticism of the assumption of 'efficiency' just concluded is one of many criticisms that are leveled against economics as a science from time to time, and most such criticisms come from economists themselves. Economists are aware at a more than peripheral level that what they do is theorize about abstractions. But the same is true of physicists, chemists, and biologists. It is not the abstract nature of economic Objects *per se* that is at issue in this Critique. Nor is it at issue

<sup>&</sup>lt;sup>4</sup> Another economically vague concept vaguely expressed by economists. It is tied in with another occult quality, namely, the idea economists call 'utility'.

whether economics is a science or not. It is a science but not a *social-natural* science. The issue and question is: What social-natural phenomena are described by economics – and with what degree of accuracy – and what social-natural phenomena are not? Put another way, when and to what level of detail are economic predictions to be trusted? There are limits to what any science is capable of describing, and it is important to know what these limits are in order to not misuse the theory and thereby learn something that isn't true.

The classic market models of table 1 attempt to state where commercial behavioral trends tend to converge given economic circumstances used to define the specific models. The approach long taken in the practice of economics has been to assume there is a kind of "average" behavior that is exhibited by a "firm" (a producer and/or seller of a particular economic good) and that by understanding "the theory of the firm" (microeconomics) gross behavioral trends in the real world can be understood. It holds little to no interest to you or me if Joe's Donut Shoppe succeeds or not. It might perhaps hold some interest for you or me whether or not donut shops are viable as commercial entities. This *would* be an interest if either of us is considering making a donut shop our means of personal enterprise. The same can be said of *any* peculiar type of business enterprise.

Although economists do not share a single general technical definition of the term 'firm', they do tend to use the term in more or less the same way. This way is to use it to mean an identifiable business entity regarded as an artificial person engaged in producing and/or selling a single and identifiable commodity. We will call this usage an **economic firm**. The restriction of a single and identifiable commodity makes this usage an approximation to many real commercial entities because many real commercial entities deal in more than one commodity. Economists often handle this approximation by regarding particular *sets* of commodities as if they constituted a single *abstract* commodity. For example, the "produce section" of a supermarket might be treated as dealing in a single abstract commodity, namely, "produce."

I think it likely that from this you can appreciate the sorts of abstractions economics treats. All such abstractions are models of a real object or objects obtained by retaining in the concept of the model those aspects of real objects that are the same and discarding those aspects in which they differ. The crucial test of any such model is whether predictions based on it are robust in the face of variations in those factors or whether small changes in the model lead to significant changes in the predictions of theories based on the model. Economists have been known to argue heatedly over precisely this issue. Such disagreements still abound in economics today. Lipsey & Steiner wrote,

From certain assumptions about the firm's motives and its opportunities we have derived a number of predictions about market behavior that are capable of being tested by reference to the real world. We have abstracted from many of the complications of the real world by assuming that productive decisions are made by "firms" that are assumed to be efficient single-purpose organizations, operating with full information in well-defined markets. We have suppressed the notion of the firm as an organic collection of individuals. So also have we suppressed any conflicts in motivation among the owners of a firm, or between the owners and the managers. We have instead assumed that the firm's decision makers seek simply to maximize the firm's profits.

Obviously, such abstractions do violence to descriptive reality. But that is not our current concern; our concern is whether such abstractions are convenient simplifications on the one hand or fundamentally misleading on the other. Do the predictions of our theories account for observed behavior, or are they refuted by it?

In many cases, if a particular implication is refuted, only a minor change in the basic theory is needed to make it once again consistent with the facts.... In other cases, however, economists have claimed to have made empirical observations that strike at the very core of the theory of the firm. If these refutations were substantiated, we would either have to make very drastic amendments to our theory or we would have to abandon it completely. [Lipsey & Steiner (1969), pp. 363-364]

Economists' modeling abstractions are often presented in the form of hypotheses. One way to think about the divers market models is to regard them as representing divers sets of hypotheses. The "market" (economic circumstances) a firm is said to operate in is then defined by the special set of hypotheses used to model the firms said to be "in" that "market." This understanding of what it is economists are doing is very important for understanding why some businessmen choose to dismiss and disregard economic doctrine on the grounds that businessmen do not use or think about the mathematical terms and abstractions used by economists. They mistake the theory of the firm for a theory of business decision-making. Lipsey & Steiner respond to this by saying:

The observation that businessmen do not use the terms marginal cost and marginal revenue would refute the theory that businessmen make decisions by calculating marginal values and consciously equating them. But it does not refute the theory that businessmen make decisions in such a way as to maximize profits. The economic theorist uses the mathematical concepts of marginal cost and marginal revenue to discover what will happen as long as, by one means or another – by guess, hunch, clairvoyance, luck, or good judgment – the businessman does approximately succeed in maximizing his profits. The constructs of the theory of the firm are, in other words, merely tools employed by the economist to discover the consequences of certain behavior patterns. They are not meant to describe *how* the businessman reaches his decisions. If the businessman wants to maximize his profits, then the theory of profit maximization allows us to predict how the businessman will react to certain changes – e.g., the introduction of a tax. This prediction is independent of the thought process by which the businessman actually reaches his decision. [*ibid.*, pp. 366-367]

Economists are, or try to be, empirical pragmatists. My criticisms of economics in regard to its lack of adequate connection with grounds in human nature probably sound as though I dismiss all of economic theory; this is not so. My aim is to help *improve* the theory at causative foundations, not to throw it out wholesale. The predictions of economics are statistical and in any statistical theory it is not enough to be able to get an estimate of average (statistical mean) behaviors. It is also important *to understand statistical variance* around this mean. A theory is not robust if the range of its variance is as great as its mean. Human nature is a necessary consideration in understanding economic variances. At the same time, the point raised above about the irrelevance of *how* a businessman reaches a decision for the consequences *of* that decision is a point well-taken.

I think a personal anecdote is useful to relate in this regard. My father owned and operated a bakery in a small Iowa town. Part of what he had to do as a proprietor was decide each day how many bakery goods of which kinds he was going to bake that day. His method for reaching this decision each day was one of the simplest imaginable. Every day he recorded how many of each kind of bakery good he baked that day and how many were left unsold at the end of each day. He kept this data in little record books. Each day he would consult his record book to look up how many of each kind of bakery good he had sold on that day of the week *one year earlier*. Those were the numbers he would bake that day. In a way, this showed a considerable degree of insight into customer psychology inasmuch as it recognized that tastes do change over time and that other factors – for example, whether the Thanksgiving or Christmas holidays were coming up – affect people's decisions about what bakery goods they are interested in. Occasionally he would underestimate the day's demand and would have to bake an additional batch of bread, cookies, or whatever. Occasionally he'd overestimate demand and the leftovers would go into the "Day Old" case the next day. This data was also faithfully recorded in his record books.

I never once heard the phrase "profit maximization" pass his lips. As a young man - and not

unlike many twenty-one-year-old sons – I had my doubts if Dad was as sharp a businessman as I regarded myself as being at that age. My father never went to college and never studied one word of economics; I, on the other hand, was a college student at the time and had studied economics quite extensively. I was therefore quite confident that I could put my knowledge of economic theory to use and show Dad how he ought to be running his business. I therefore undertook a very detailed project to analyze how one ought to operate that business in that town. I had the advantage of having access to all his detailed records plus the advantage of being intimately familiar with the town itself. After considerable labor – using 'marginal costs' and all the other tools of economic theory – I at last completed the project and knew exactly how *I'd* operate Dad's business. To my considerable surprise, when I compared how it "ought" to be done with how Dad actually did it, I discovered that my plan *made not one single change in how to operate it*. I have to admit that I gained a considerable amount of respect for Dad's business acumen after that. The next time I saw him, I rather excitedly told him about the project. "It turns out," I told him, "that you're doing it just exactly the way you should." Dad frowned at me and replied, "I knew that." I suspect he was entertaining doubts about whether my college tuition had been money well spent.

Economists do encounter variances in actual commerce that depart significantly from averages projected from their models. These observations give rise to a number of modifications to the theory and the hypothesis sets used in the practice of economics. Lipsey & Steiner noted,

We have in this chapter examined a number of competing theories. These theories might be complementary rather than competing. It may well be that no one motive is dominant. The firm may have several strong motives – profits, security, growth, etc. – so that any theory that concentrates on only one of them will, sooner or later, be refuted. If this is so, it will be necessary to develop a much more complex theory of the firm than any of those outlined here.

We may have conveyed in this chapter the image of conventional theory embattled, with its proponents beating off attackers right and left. In a sense, this image is correct; in a broader sense it is misleading. Each of the criticisms we have examined arises from the frustration that comes from trying to apply the concepts of the economist to the world of businessmen. When one cannot easily bring the theory into contact with the data generated by firms, it is hard both to apply the theory and to test it. The most damaging charge against the theory of profit maximization may be that it is *nonoperational*, which means it cannot be applied to the real world.

Whether this charge is or is not generally valid, it has had a profound effect on contemporary work in economics. Models of firm behavior in production, in pricing, and in other forms of market interaction are increasingly being restated in more specific terms, and in terms that make predictions on the basis of available data... The main result has been an attempt to state narrower, more specific, and more readily testable propositions about firm behavior. [*ibid.*, pg. 374]

This, too, is important to keep in mind when looking at the classic market models.

All the classic market models attempt to model the effects of competition on the price, availability, and cost to produce of some reasonably well-defined single economic good. The idea of competition is a foundational notion in economics, which makes it surprising that economists have no generally agreed upon *technical* definition of 'competition'. Competition is used as a primitive term in economics and different market models are identified by an adjective modifier, e.g., 'perfect competition' and 'monopolistic competition'. What most economists appear to mean by 'competition' is more or less a 16th century legal definition: "the struggle for commercial advantage; the effort or action of two or more commercial interests to obtain the same business from third parties" [Garner (2011)]. This usage implicates the presence of two classes of people in market modeling. The terms economists use for these classes are 'consumers' and 'producers'.



Figure 1: Economic activities model of a complete transaction.

Each of these classes has its own way of defining what a 'market' is. Lipsey & Steiner explain,

From the point of view of a consumer, the market consists of those firms from which the consumer can buy a well-defined product; from the point of view of the producer, the market consists of those buyers to whom he can sell a single well-defined product (if the conditions of sale are sufficiently favorable). [Lipsey & Steiner (1969), pg. 272]

These two viewpoints are consistent with the Critical explanation of a market that was set out in chapter 1, i.e., a market is the population of people who regard a particular economic good as a wealth-asset and are willing to exchange some part of their own stock-of-goods for it. From the viewpoint of the 'consumer' the economic good is the "single well-defined product" referred to above. From the viewpoint of the 'producer' the economic good is the wealth-asset obtained from the 'consumer' in exchange for this "single well-defined product." Usually the 'consumer' is taken to be the *buyer* in a commercial transaction and the 'producer' is taken to be the *seller*. However, this customary terminology does reveal a prejudicial point of view on the part of the theorist in as much as it seems to give favor to extractors and manufacturers as "producers" and to retailers and private final-users as "consumers" in terms of economic activities (figure 1). To a practitioner of general systems theory, this nominal convention is a caution flag because it runs contrary to a key principle of interdisciplinary systems theory; namely, the *principle of indifference* which states

Laws should not depend on a particular choice of notations. [Weinberg (1975), pg. 72]

'Producer' and 'consumer' are notations in economic theory and the customary distinctions in these terms that have been used by economists ever since Adam Smith have the consequence that table 1 is dichotomous, i.e., has a "seller side" and a "buyer side" layout with different market models in each. The fact that I am using the model distinctions presented in that table is not an endorsement of the practice; it merely reflects a practical pedagogical advantage in expressing economic ideas in the same terms the social science of economics uses in its common practices.

To put this in somewhat more concrete terms, suppose you go into a shop in your local mall and buy a shirt for which you pay cash on the spot. Traditional nomenclature labels you as the buyer and the shopkeeper as the seller. In economic market modeling terminology, you are the consumer and therefore the shopkeeper must then stand in the role of producer. But why should the transaction not be regarded as one in which the shopkeeper buys a wealth-asset (money) from you and you sell this wealth-asset for a shirt? There is no fundamental reason why the transaction must be regarded in the first way of looking at it and not the second. To see this more clearly, suppose in this transaction you trade the shirt which the shopkeeper has for, say, a twenty pound bag of apples that you have. Here is a bartering transaction in which both of you present a "welldefined product" for exchange. The only difference between these two examples is that in the first one you present a wealth-asset (money) that is not well-defined in terms of what a shopkeeper will spend it on at some later time when he is in the role of a final-user of some other wealthasset. By violating the principle of indifference, economists can and occasionally do run into paradoxes of theory they might have avoided by a more discerning understanding of the real world meaning-implications of economic terminologies. Imagine the fix economic theorists could find themselves in if dollar bills grew on trees; either apples or dollar bills could serve as specie.



Figure 2: Competition modeling schema used by economists.

The market models presented in table 1 represent economists' efforts to model economic phenomena in terms of 'competition'. If one calls the theory presented in undergraduate college textbooks the "mainstream thinking" of economic theory, there is an interesting pattern or schema in the way in which economists have approached their modeling endeavors. I do not say this is an 'official' or 'recognized' modeling schema in economics. Indeed, I am suspicious that economists are not conscious of it at all *as a schema*. But it is an empirical pattern which, from the viewpoint of modeling science (the science of making models), emerges from examination of details of the classic economists' models and what economists say about them. Figure 2 illustrates this empirically observed economists' modeling schema.

The first important characteristic to be noted about this schema is that it is stratified into two quite different modelers' viewpoints: a seller-side competition viewpoint and a buyer-side competition viewpoint. By a lopsided margin, most mainstream thinking in the practice of economics has gone into the seller-side viewpoint. A presupposition that goes into all the seller-side models is that the number of buyers ('consumers') of a well-defined economic good is so large that buyer behavior needs no special treatment or characterization beyond what many economists usually call the "industry demand curve." (By 'industry' an economist typically means "the group of producers of the specific well-defined economic good"; the term 'industry' itself is taken to be a 'primitive' term needing no technical definition – a vagueness not conducive to advancement of a rigorous natural science; a *business industry* is a classification of similar businesses). One rarely sees any vigorous attempt by seller-side modelers to try to explain or understand this industry demand curve. It is, rather, usually treated as an hypothesis going into a specific model.

Corresponding to this lopsided emphasis on seller-side models is a lopsided underemphasis on buyer-side models. Labor unions, their behaviors, and their economics have been one of the chief motivators of buyer-side modeling, especially regarding monopsonistic competition for labor vs. the monopsony buyer of labor – phenomena involving economic situations in which there is only one employer of labor in a particular town. That employer is "a buyer of labor services" and so a 'monopsony' is said to exist in that town. It is a view again favoring interests of conglomerates over those of people. However, as Lipsey & Steiner pointed out, models such as these apply to any "factor of production" and are not confined to the context of labor unions. Given that some writers strenuously object to "labor" being regarded as a commodity - Polanyi is notable in this regard [Polanyi (1944), pg. 75] – it does seem curious to some (me, for instance) why buyer-side modeling has not received more attention. That there is "competition" among buyers is not disputed. If you have ever been to an auction, you have seen buyer-side competition in action. If you have had the misfortune to be a young person living in a community with skyrocketing housing price inflation – or an elderly retired person living in such a community and feeling its effect via rising property tax assessments – you have also seen buyer-side competition in action. Many Americans experienced some of its effects in the Great Recession of 2008, which was partially caused by a debt-fueled house-buying competition that drove up prices and debt.

Another peculiarity observed in classic modeling is the extent of the disconnect from sellerside models and buyer-side models. This disconnect is not total; all the models necessarily must make hypotheses regarding at least the qualitative character of supply curves and demand curves. But it is nonetheless rather pronounced inasmuch as the analysis of trends implicated by these models is thereby restricted by and large to dialectic mathematics. It is uncontestable that the most successful dialectic theorems produced by economists involve either the extreme of perfect competition (unlimited competition) or the extreme of monopoly/monopsony (no competition). The intermediate models are noticeably less fecund. In the case of oligopoly economists have produced next to no causative findings at all. Furthermore, where one model leaves off and another begins is not and really cannot be crisply defined other than by fiat. For example, in monopolistic competition there are "many" suppliers whereas in oligopoly there are "few" suppliers; but what is the lower limit of "many" or the upper limit of "few" and what do we have in between these limits if they do not coincide? Issues like this in other technical fields were prime motivators for developing the mathematics of so-called "fuzzy logic" [Nelson (2003)] as well as for the development of set membership theory.

What one might call "issues of fuzziness" are frequently major hindrances to economics research and underlie more than a few rather passionate arguments among economists. What I will call "issues of model*ing* fuzziness" in regard to the schema of figure 2 underlie quite a few more. Most professional economists are aware that economics as a science faces many more questions of pertinence to Societies than its practices can currently answer. Lipsey & Steiner adroitly admitted this without explicitly admitting it in the earlier quote on page 203. For a long time it was the practice of economic theorizing to confine itself to the extreme cases of perfect competition and monopoly, where the questions are somewhat clearer and more results that are fecund can be obtained using dialectic mathematics. Controversies that arose in the 1930s during the Great Depression fueled the development of the monopolistic competition model and, perhaps more importantly, reduced the amount of dogmatic thinking in the practice of economics. Lipsey & Steiner were very upfront about this:

Monopolistic competition, as a theory, is more than thirty-five years old [in 1969]. Its survival over so long a period cannot be explained by the number of tested and confirmed predictions, for there are few of them so far.

Whatever the outcome of the debate of the predictive value of monopolistic competition may be, there can be little doubt that it has contributed two important things to the development of the subject. At the time that it was first developed, perfect competition was under severe attack because of the lack of realism in its assumptions. The theory of monopolistic competition recognized the facts of product differentiation, the ability of firms to influence prices, and advertising. By incorporating all of these new assumptions into a new theory, economists were encouraged to consider the question of their effect on the operation of price systems. Also, by showing that such dramatic changes in the assumptions might not affect the predictions of the theory, economists have become a little more skeptical of attacks based solely on the assertions that the assumptions in their theories are unrealistic.<sup>5</sup>

The second major contribution of the theory is that many economists have been profoundly influenced by it. It has served to call attention to the potential importance of the extent to which the sellers recognize that they are interdependent, the extent to which entry [into the market] is free, and the nature of the product. These things were freed, by the theory of monopolistic competition, from the polar models of monopoly and competition.

<sup>&</sup>lt;sup>5</sup> I will remark that a little skepticism is usually a good thing in science; it tends to defend theories against being dismissed too quickly on too few grounds. But there is a thin line between skepticism and ignórance in science, and the latter is usually a terribly destructive thing for a science. Discovery requires an attitude of open-mindedness and ignórance (the act of ignoring) is closed-mindedness in action.

This has led to the development of other important models [Lipsey & Steiner (1969), pg. 314].

I think the pace of model development and the degree of predictive successes achieved have been rather slower since 1969 than Lipsey & Steiner were perhaps expecting to see. With the exception of monopsony – modeling of which is conceptually only a small step away from the modeling of monopoly – there have been few additional models developed. Indeed, the remaining two models of figure 2 (monopsonistic competition and oligopsony) do not even have entries in Bannock *et al.* (2003). The terminology I am using here was suggested by Mansfield [Mansfield (1970), pp. 367-368]. I will propose that economists really should take modeling *as a science* more seriously. In economics this has a long way to go yet. As of the date of this writing, there is no Critical applied metaphysic for economics – and a metaphysic is "the way one looks at the world" and therefore is concerned with "viewpoints" – and no *ontology*-centered metaphysic is capable of providing objectively valid grounds for a social-natural science of economics. Only the *epistemology*-centered Critical metaphysics-proper is capable of doing that [Wells (2006)].

# § 4. The Perfect Competition Model

The adjective "perfect" in "perfect competition" only means no other market exhibits a greater degree of competition. It is not an aesthetic judgment that it is "the best kind of competition," although many economists speak as if perfect competition is "better" than other markets. Perhaps this is because the perfect competition model results in the highest degree of "efficiency" found in any of the mathematics of market models (in the context of "efficiency" discussed in §2).

The perfect competition model is deduced on the basis of two assumptions:

- 1. the economic firm is a price-taker; and
- 2. the business industry displays freedom of (lack of barriers to) entry into or exit from participating in the market.

To be a "price-taker" means the economic firm cannot through its own actions alter the market price of the economic good it offers to sell. If it raises its price above that established by the industry demand curve, it will sell no units; if it lowers its price below that established by the industry demand curve it might sell more units but cannot achieve the amount of profit it would achieve by selling at the market price. Perfect competition 'homogenizes' firms and commodities.

Every market model is defined by the assumptions/hypotheses used to characterize it. The two just stated for perfect competition represent a seller-side viewpoint. What most textbooks do not mention is that the perfect competition model is really a joint model of seller-side *and* buyer-side viewpoints. What I mean by this is that buyers also have freedom to enter into or decline to enter into commerce for the sellers' economic good *and* that consumers *are also price-takers* inasmuch as: (*i*) if they demand a buying price lower than that of the industry demand curve no seller will sell to them; and (*ii*) if they offer a buying price higher than that of industry any seller will sell to them *but* they will spend more of their wealth-assets on the purchase than they needed to spend to obtain that economic good. The model assumes price-knowledgeable consumers.

This set of assumptions is descended from an older set of assumptions that used to be used by all economists and still are by some [Mansfield (1970), pp. 222-252]. These are:

- 1. there are a very large number of sellers, no one of whom commands a large share of the market;
- 2. the products of different sellers are identical and buyers have no preferences among the sellers;

- 3. there are so many buyers that sellers and buyers do not establish personal relationships with each other; and
- 4. buyers are perfectly informed about prices of sellers.

The older assumption set makes it much more clear that perfect competition is a seller-side plus buyer-side viewpoints model. Over time economists moved from this once-traditional set of assumptions to the newer set because (i) they found it is price-taking that is required to produce the model's results and (ii) there are conditions other than the four listed in the once-traditional assumption set that can also result in price-taking (i.e., produce a horizontal demand curve of price as a function of quantity of economic goods sold by a firm).

There is an important criticism that can be leveled against the current assumption set. It is this: By not pursuing deeper characterization of the socio-economic conditions that result in pricetaking behavior by sellers and buyers, the theory cuts off deeper *causative* explanations and thereby opens the theory up to the same sorts of criticisms leveled against behavioral psychology, a school of thought that dominated the American psychology profession prior to 1960. One of the more important of these was the criticism that behaviorism led to no prescriptions for how undesirable behavior could be *modified* – a key objective in many cases of psychological therapy. In an analogous way, the assumption set used in perfect competition theory makes that model a model of economic behaviorism that forces the theory to remain silent when a Society or its government attempts to either promote Progress in its state of general welfare or improve its state of domestic tranquility, thereby tending to render economic theory impractical for Society.

It can be strongly argued that this economic behaviorism leads to circular arguments in economics, i.e., "*if* economic phenomena match the results predicted by perfect competition *then* there must be price-taking behavior occurring"; this, of course, is role reversal in cause vs. effect and thereby the theory *explains* nothing. Economists, of course, deny that they engage in role reversal of this sort. However, "price-taking" has no technical definition or real-explanation in current economic doctrine. One can therefore ask, "How do you know you don't do role reversal? How do you know when you aren't merely changing what you call price-taking to make the theory 'work' ?" To put it in terms of Feynman's words quoted above, the assumption set is vague and produces a vague theory. Vague theories have no scientific authority.

One wouldn't be likely to call this state of affairs a stunning endorsement of what is in many ways the crown jewel of economists' achievements. It is an example of how a science can drift away from being a natural science because of excessive abstraction that tends to take place over time when that science divorces itself from its natural "atoms" – in this case, its *social*-natural atoms (real human beings) – and becomes too much a merely mathematical exercise.

Like elementary thermodynamics in physics, the principal achievement of the model of perfect competition subsists in what the theory concludes in regard to economic *equilibrium*. Strictly speaking, this achievement is a theorem asserting that a state of long term economic equilibrium does exist under perfect competition *assuming* there is not enough exodus by either producers or consumers to alter price-taking<sup>6</sup>. This state subsists in three results deduced from the theory:

- 1. Every economic firm produces where price equals marginal cost;
- 2. Every economic firm produces where price equals short-run average cost; and
- 3. Every economic firm produces where price equals long-run average total cost.

<sup>&</sup>lt;sup>6</sup> Going one level deeper, the theory of perfect competition does not *prove* such an exodus will *not* happen. If farming – the poster child of perfect competition – is an indicator, such an exodus is not only possible but might even be likely. How many stones can you take from a heap of stones before it is no longer a heap? In mathematics, infinity minus one equals infinity; in business, large minus one equals less large.

I will spare you from a recital here of the proofs of the above and the mathematical definitions of the terms used in them. These can be found in any of the most popular college textbooks used for a first course in economics. Like my father, most businessmen and most consumers do not think about things like 'marginal cost', etc., so why should anyone who is not an economist care about these findings? The answer is: these three findings, taken together along with the assumption of free entry and exit, actually do have real-world implications people *do* care about. One of them is: in long-term equilibrium under perfect competition, *no producer makes an economic profit*. His profit is just enough so he does not abandon the industry and that is all. It does not allow him to "grow" his business by supplying him with enough income revenue to produce gains in his capital. Economists call this "efficiency." I call it "stagnation." It *might* conduce to Order in a Society, but it does *not* conduce to Progress. Perfect competition arrests capitalism.

Less clear is whether or not consumers benefit from such a long-term equilibrium. Often I hear people *say* consumers do, but is there proof of this or is it just an opinion? It is true that these conditions are conditions of economic efficiency for consumers as well as producers. But, again, is this "efficiency" a "good thing" for a Society? Or is it a stagnation of people's standards of living and their living conditions, perhaps conducive to Order but not to Progress? Economists' description of economic "efficiency" includes *distributional* efficiency, and this clearly sounds like a state of stagnation consistent with what Toynbee called an "arrested" Society. It has been the historical trend that most arrested Societies do not remain in the equilibrium of their arrested condition; rather, most of them go on to become *fallen* civilizations that vanish from the face of the earth. En route to their fall, they are characterized by development of rigid social castes [Toynbee (1946), pp. 164-186], standardization [*ibid.*, pp. 367-368, 555-558], authoritarian rulers and the formation and secession of Toynbee proletariats [*ibid.*, pp. 360-428]. Very few arrested Societies have avoided this outcome. (BaMbuti Society is, for now, the most successful example of an arrested Society that has not fallen *yet*). I offer to suggest that the fall of their civilization is a disbenefit to consumers and so perfect competition is a "bad thing" for a Society.

The mathematical dialectics that lead to the theoretical conclusions are about as clear and concise as any theoretical deduction ever gets. They also offer valuable insights into the limited psychological assumptions economists do employ for predicting economic behaviors. Mansfield provided a particularly good presentation of these arguments along with excellent illustrations of some additional empirical details and applications [Mansfield (1970), pp. 222-252]. If one accepts or, as is more typical, does not examine the formally stated and informally presupposed assumptions that these deductions employ for drawing conclusions, the dialectic arguments of perfect competition appear to be indisputable. This, however, only means that given the premises the logic of the argument is correct. But an empirical science can *never* ignore or avoid careful examination of the premises of its theoretical arguments, and this is where the human nature of its social "atoms" is of primary importance.

## § 5. The Monopoly and Monopsony Models

The word "monopoly" is derived from the Greek language and means "one seller." Similarly, the word "monopsony" means "one buyer." Monopoly models the case of no competition among sellers (because there is only one of them); monopsony models the case of no competition among buyers (because there is only one of them). Like perfect competition, both are approximations of conditions in actual industries. As the polar opposite of perfect competition, the monopoly model and the monopsony model present simple economic circumstances and admit to a broader sphere of specific conclusions.

The classic monopoly model, like perfect competition, is actually a model combining a sellerside viewpoint and a buyer-side viewpoint. The latter is rarely stated explicitly in economics textbooks, but examination of the dialectics shows that the classic monopoly model – the one from which definite theorems are produced – assumes perfect competition among buyers. As buyer-side market conditions move from left to right in figure 2, general results become harder to obtain. In the limit where on the buyer-side the condition is one of monopsony (a single buyer), no determinate *general* results at all are obtained. This case is usually called a "bilateral monopoly" in textbooks. Thus, the classic "monopoly" model is a seller-side monopoly *plus* buyer-side perfect competition. On the buyer-side, it presumes free entry into and exit from the market by consumers.

On the seller-side, it presumes that significant – actually, insurmountable – hindrances exist to free entry into the market by other sellers. Enormous fixed capital investment cost accompanied by production capacity adequate to supply the entire market demand for a commodity is a typical barrier to entry. Government regulation or law is sometimes another. Patent rights are an example of a legal barrier-to-entry. If a firm owns or controls the entire supply of some commodity that is essential for producing a product, this is another factor that hinders the entry of competitors. A textbook case of this was the control over every source of bauxite in the United States (an essential ingredient for aluminum) enjoyed by Alcoa prior to World War II.

A monopoly supplier is not a price-taker; it is a price-setter (assuming buyer-side conditions that approximate perfect competition or monopsonistic competition). A supplier is said to have "monopoly power" when it is to some extent insulated from loss of customers to other sellers. This includes loss of customers who, because of the price of the commodity, choose to spend their consumption wealth-assets on other commodities (do without the monopolist's commodity). This loss is a factor, however, only if it shifts the demand curve for the monopolist's commodity in a way that affects the quantity of the commodity he chooses to produce in comparison with what he would produce if the buyer-side condition was one of perfect competition.

The commodity price is generally higher under classic monopoly than it would be for classic perfect competition. It is in principle possible for a monopolist to lose money (negative profit), but doing so tends to require a significant degree of managerial incompetence in operating the business or a significant departure from buyer-side perfect competition conditions. More typically a monopolist supplier is able to make an economic profit because barrier to entry prevents other suppliers from entering the market. *This means monopoly is capable of capital income revenue*.

Of course, if a monopolist attempts to make too much profit this can provide other potential suppliers with enough incentive to overcome whatever barriers to entry there might be – in which case the industry is no longer a monopoly industry. If the commodity is essential to Order in the public Welfare and the monopolist tries to maximize his profits by creating a shortage of the commodity, this can provoke the government into intervening in order to promote the general Welfare. For example, electric power as a commodity is central to modern life in industrialized nations and in most geographic locations the supply of electric power is provided by a single supplier. For this reason, electric utility companies are generally regulated by some kind of Public Utilities Commission charged with a Duty to strike a balance between a utility being able to make a "fair" profit and consumers being able to afford to buy electric power<sup>7</sup>.

<sup>&</sup>lt;sup>7</sup> In the United States there is a political faction who professes an ideology that no business, including electric utility companies, should ever be regulated. This is the ideology of extreme *laissez faire* free enterprise. The typical argument one hears is that by "freeing competition" consumers always benefit because the price of the commodity is automatically lower. The argument presumes that it is only government regulation that prevents conditions of perfect competition from occurring. Either the propagandists for this position are utterly ignorant of economics or else they are being disingenuous – i.e., they are lying. People who subscribe to this ideological position out of honest belief the ideology is true also tend to believe "competition" is a universal good-in-itself. Empirical facts demonstrate this is simply not true.

Because economic profit is possible under classic monopoly, any competent entrepreneur will prefer being a monopolist to being one of the faceless many suppliers under conditions of classic perfect competition. For him the interest is immediate, whereas any possible public disbenefit of established monopoly is, for him, a remote interest unless he is atypically well educated in civics and particularly well informed of any *actual* disbenefits detrimental to the general Welfare or to domestic tranquility that might be occurring. I stress the word "actual" here. A merely theoretical possibility of disbenefit is not a justifiable ground for legislation or executive regulation limiting civil liberty in a Republic. For one thing, most regulative legislation of this sort is *necessarily* predicated on the basis of stereotyped Platonic social idealizations - i.e., predicated on ideas of *noumena* that lie beyond the horizon of possible human experience – and therefore lack grounds for real objective validity. The necessitation arises from the impossibility of adequately analyzing extremely complicated circumstances such as those characteristic of real economies and real commerce. Platonic idealizations are introduced in an attempt to make the understanding of these circumstances practicable. The practical and pragmatic protection of civil liberty necessitates awaiting detection of actual disbenefits beginning to accrue so that actual circumstances can be studied and properly limited countermeasures can be devised. It is as Montesquieu said:

Should there happen to be a country whose inhabitants were of a social temper, openhearted, cheerful, endowed with taste and a facility in communicating their thoughts; who were sprightly and agreeable; sometimes imprudent, often indiscreet; and besides had courage, generosity, frankness, and a certain notion of honor, no one ought to endeavor to restrain their manners by law unless he would lay a constraint on their virtues. If in general the character be good, the little foibles that may be found in it are of small importance....

It is the business of the legislature to follow the spirit of the nation when it is not contrary to the principles of government; for we do nothing so well as when we act with freedom and follow the bent of our natural genius. [Montesquieu (1748), Bk. I, pg. 294]

It does fall to government as a Duty to ensure adequate public education in civic Duties of its citizens and to maintain a watchful monitoring of the state of the general Welfare and indicators of lack of domestic tranquility. It is contrary to the Duty of government to constrain and restrict civil liberty when no actual injustices are present. It is contrary to the Duty of government to fail to take corrective actions when actual injustices are present. *Laissez faire* policies promoting uncivic free enterprise are derelictions of the latter Duty of government. The concept of *civic* monopoly is not contrary to human nature; monopoly *per se* is not a universal evil-in-itself.

*Monopsony* is the dual of monopoly. However, circumstances favoring situations where there is only one buyer tend to be relatively rare compared to circumstances favoring monopoly. I do not mean by this that monopsonists are uncommon; that is not true. The classical monopsony case is the "company town" in which there is a single employer who is the sole purchaser of labor services. Rather more common are situations in which a large factory firm is the sole purchaser of specialized tooling, parts, or assemblies that are produced by a number of other and generally smaller local firms. Another example would be a situation that occurs when a city has a single large and dominating construction firm that subcontracts pieces of large construction projects to smaller local firms. A university in a small "university town" is another example of monopsony because there are many specialized jobs in a university that cannot be had with any other employer in that town and are relatively scarce in others (a factor that reduces the mobility of entrepreneurs who provide those types of labor services). Mansfield has noted that major league baseball has many close similarities to monopsony [Mansfield (1970), pp. 374-375].

An important consequence of special circumstances such as these is that the seller-side usually does not approach the limiting case of perfect competition among sellers. Rather, the seller-side circumstances are more typically those of monopolistic competition or oligopoly or, on occasion,

bilateral monopoly. For this reason there are fewer general theorems deducible for monopsony and monopsony studies tend to be on a more case-by-case basis. One of the general theorems for monopsony is that the price for the economic good purchased by a monopsonist is generally lower than would be expected under conditions of perfect competition. Furthermore, the monopsonist is usually a price-*setter* and not a price-*taker*. This is because the monopsonist's demand for the commodity *is* the total market demand for that commodity.

One consequence of the typically *laissez faire* government policies in the U.S. in regard to corporate mergers and acquisitions is that these policies tend to favor the establishment of monosony employment in many smaller cities for particular types of jobs. One immediate cause of this is the merger-formed large retail company. These companies tend to have an advantage in terms of prices they pay for the retail goods they re-sell because they are able to buy commodities in very large lots. When a large "box store," such as a Wal-Mart store, is established in a small city, it tends to be able to offer goods for sale at lower prices than local merchants are able to offer. Over time, smaller retail stores are forced out of business and the resulting smaller number of retail jobs to be had are offered by the surviving large retail store. This employer, now enjoying a monopsony position as the purchaser of retail labor services, can take advantage of the situation to lower wages for its employees, thus reducing the average level of income revenue available in the small city. Because capital revenue is extracted from income revenue, the situation is often followed by what I call *capital efflux* from the small city because a larger percentage of the retail income revenue flows back to the corporate parent in another city. Because this income revenue does not remain in the small city and re-circulate locally, a smaller fraction of locally-held wealth-assets is available to be employed as capital. All else being equal, this commercial phenomenon tends to reduce prosperity and the general Welfare of the small cities because lack of local capital hinders new business development to replace that which was lost en route to the monopsony situation.

One rarely hears of this, but it is an effect that has been occurring in the U.S. since the consumer products boom of the 1950s. The mere fact people don't hear about a phenomenon does not mean the phenomenon does not exist. It tends to make local commercial mini-Societies into arrested mini-Societies. This is an effect I claim to have observed in almost every small city and town I have ever examined. I speculate it is a partial cause of so many American small cities resembling arrested mini-Societies. As a citizen, I make it my personal Duty to trade locally and to avoid trading with extra-regional retailer conglomerates when possible. Capital efflux arrests economic Progress in those delimited geographic regions where the phenomenon occurs.

# § 6. The Other Models

The theories of the remaining four classic market models are much less thoroughly developed than are the theories of perfect competition and monopoly. The monopolistic competition model has received a sufficient amount of attention from economists that it is usually presented in introductory college-level textbooks, but the situation is otherwise for monopsonistic competition.

The monopolistic competition model was developed during the Great Depression in reaction to mounting criticisms that the perfect competition model was too unrealistic. As Lipsey & Steiner noted, this model did have the effect of provoking economists out of the rut that had been fallen into of trying to treat everything as either perfect competition or else as monopoly. The theories of the remaining models are often called "imperfect competition" models.

By almost any disinterested assessment, these models have been something less than showcase successes so far. Of course, this is not unusual in any nascent scientific field as practitioners are groping for fecund models. As Feynman said, new scientific laws are discovered by guessing. What sets scientists apart from other people is their training and experience in effective guessing. In this vein, I will be so bold as to offer a suggestion or two to economists on this topic.

One of the things people might find frustrating about the remaining four models is that they seem to be capable of producing diametrically opposite findings depending on the details and assumptions used by divers economists. I offer to suggest that part of this stems from the rather vague descriptions – one cannot really call them definitions – of what these four model labels are supposed to mean. There are some divers usages of the labels – which perhaps accounts for the fact that these labels have no entries in Bannock *et al.* (2003) – and so the point about vagueness can probably be as well illustrated by citing one usage as by another. Lipsey & Steiner described *monopolistic competition* as the situation in which:

- there are a large number of producers with free entry and exit from the market;
- each producer sells a product that is somewhat differentiated from those sold by his competitors; and
- the firm is not a passive price-taker (i.e., it can exercise a limited amount of influence over its price) and can engage in non-price competition. [Lipsey & Steiner (1969), pp. 309-315]

Most of the classical analyses of monopolistic competition models tend to assume a buyerside perfect competition model to approximate market demand. With this additional assumption, the analysis results in the following findings: (*i*) producers cannot make an economic profit in the long run; (*ii*) the equilibrium output of the firm is less than the output level at which average total cost-to-produce is a minimum (this is called the 'excess capacity theorem'); (*iii*) all else being equal, equilibrium prices will be higher than under perfect competition; (*iv*) a greater variety of similar products will be offered to consumers (brands, styles, and possibly qualities) than in the case of perfect competition; and (*v*) producers will have many incentives to engage in non-price competition of a kind it would not pay to use under perfect competition [*ibid.*, pp. 312-313].

These and other findings are sensitive to the buyer-side assumption. For example, Lovell has reported that:

- monopolistic competition can generate two kinds of market failures depending on various factors: excess product diversity and insufficient product diversity;
- an increase in minimum wage is likely to lead to an increase in employment at some individual firms but this effect is counteracted by a decrease in the number of firms so that overall industry employment is reduced [Lovell (2000)].

Any model capable of producing such diametrically opposite results is a model that is omitting key factors. I offer to suggest to economists that buyer-side market models really cannot be separated from supplier-side market models; tagging a particular model with a label tends to encourage this sort of separation. The schema of figure 2 suggests it might be more fecund to consider at least 16 distinct kinds of *total* market models because 4 types of supplier-side models times 4 types of buyer-side models gives 16 types of total market models.

One of the issues that badgers the classic models is the use of vague terms used in describing the idea of the model. For monopolistic competition this shows up when it is said that the number of producers is "large." By what criterion does one say a number of producers is "large"? If I am standing amid a group of elementary school children, I am a "large" person; if I am standing amid a group of NFL football players, I am a "small" person. Terms like "many" and "few" lack real meaning without something to put them in a comparative context because these are adjectives of comparison. I offer to suggest that one comparative context necessary for understanding classic market models is seller-side to buyer-side comparison.

This same issue shows up again in the description of the *oligopoly* model. Lipsey & Steiner describe the term 'oligopoly' as "competition among a few sellers." Again, how many is a "few" and how many sellers does it take before they become more than "a few"? Lipsey & Steiner do somewhat better when they say that the basic characteristic of oligopoly is that the firms recognize to some "substantial" extent that they are interdependent and that anything they do will probably lead to a reaction by rival sellers. But, again, how much recognition does it take in order for the extent of it to be "substantial"?

The oligopoly model is in many ways the least satisfactory and least fecund of the seller-side market models. It actually makes no important predictions of any kind. This is because it has no general theorems that have been deduced for it. What it has instead is a set of *hypotheses* about "oligopoly behavior." The usual ones are:

- 1. The industry will tend to be closer to the joint profit-maximizing price and profits<sup>8</sup> the greater the degree of mutual recognition of interdependency. Mutual recognition of interdependence will tend to be greater (a) the smaller the number of sellers, (b) the more nearly equal the sellers are in market shares and in methods of production, (c) the more nearly identical the products of the sellers;
- 2. The easier it is for firms in an industry to reach a tacit agreement, the closer the firms will come to the joint profit-maximizing levels of price and profits. The ability to reach and abide by tacit agreements will tend to be greater (a) the greater the degree of mutual recognition of interdependence, (b) in an industry with a dominant firm rather than in an industry without one, (c) in a market in which the price that maximizes joint profits is stable or rising rather than falling, (d) the less the degree of uncertainty attached to the firms' estimates of future demands, costs, and other relevant factors, and (e) among firms with similar expectations of the future than among firms with widely differing expectations;
- Prices will tend to be more inflexible in the face of changes in demand or cost the more uncertain the firm is about what its rival's response will be. This uncertainty in turn will be greater (a) with a substantial number of rivals than with only a few, (b) in periods of slack demand and industry-wide excess capacity;
- 4. Prices will tend to be more inflexible the more effective is tacit agreement;
- 5. Non-price competition will tend to be more vigorous the greater the limitation on price competition;
- 6. The industry will tend to be closer to the joint profit-maximizing price the greater the barriers to entry of new firms;
- 7. Prices will tend to be less flexible upward and more flexible downward if there are small barriers to entry than if there are large ones;
- 8. Non-price competition will tend to be greater the weaker the other barriers to entry of new firms. [Lipsey & Steiner (1969), pp. 316-317]

These eight hypotheses are not predictions of a theory. They are signposts. If you see all of these characteristics describing the behavior of a business industry, then you can say, "Oh, this industry is an oligopoly industry." But notice that this is merely descriptive; the hypotheses have little *predictive* power. What predictive power they do have is that if you see all these things in a business industry then whatever the observed prices are, those prices must be close to what is needed for joint profit-maximization and the industry overall will be more monopoly-like than if these characteristics are absent.

<sup>&</sup>lt;sup>8</sup> Joint profit-maximization basically means the firms adopt policies and behaviors that to an external observer make it appear as if the firms in the business industry were really a single monopoly firm.

To seek out deeper *causative* explanations for whether or not these eight hypotheses will hold for a particular business industry, one must examine the *buyer*-side characteristics of the business environment. It is simply not enough to analyze market competition by *only* looking at your competitors. Companies do not behave as they do for no reason. *Buyer*-side market analysis is always a key determiner of *seller* actions. Every *competently* managed company undertakes to understand its buyers' market in detail. I submit to you that economists need to do the same.

Failure to do so usually has grave consequences for a company. I worked for fifteen years as a manager and engineer in what is called the "mass storage" business. Mass storage basically means the storing of enormous amounts of data in computer systems. The business has two principal divisions: the storage systems (or "box") industry and the OEM (original equipment manufacturer) hard disk drive industry<sup>9</sup>. For most of those years, my company held a position as a monopoly supplier to the part of the "box business" market we served. I call those years "the golden age of the mass storage business" because we made profits hand-over-fist in those days. Our revenues were about \$300 million a year and the ratio of our average price to our factory cost per unit was in excess of three to one. One of my products in the 1980s brought in over a billion dollars in revenue all by itself in just over three years at a price to factory cost ratio of over 3:1.

In the late 1980s our upper management made a decision to move our business into the OEM hard disk drive industry. The thinking went along the lines, "Look at all the computers that are sold every year! If we do this we'll grow our customer base tenfold<sup>10</sup>." Including us, there would be about a dozen producer firms in this industry and, naively, those decision makers who actually knew the word "oligopoly" assumed we were entering an oligopoly competition environment. After all, isn't a dozen "a few"? This was a colossal mistake. I said so at the time.

I strongly opposed the decision because *my* analysis of that customer environment was telling me it was a commodity business in which most of the conditions characteristic of oligopoly would not be met. That meant we were about to sail into an environment of monopolistic competition, and that meant "no economic profit" in our future. We had always been price-setters; now we were going to be price-takers. We had acquired a lot of monopolist's bad habits over the years – bloated overhead costs, little to no experience in dealing with take-no-prisoners competitors, we didn't know how to be a price-taker – and I did not foresee that we would quickly break these bad habits and rapidly acquire new ones we would need.

Unfortunately, the response I got from our upper-level managers to these *economics* concerns was a paternalistic, "Don't worry. We know what we're doing." My whole point was that, from the viewpoint of basic economics, we *didn't* know what we were doing. I couldn't get the decision changed, and by 1992 our business data was confirming my prediction that we were engaged in monopolistic competition. This was one of the reasons I decided to separate myself from the hard disk drive business in 1993. Consequently, I wasn't around to personally experience the awful financial slaughter that took place in the hard disk drive industry in the '90s and first decade of the new century. Since the invention of the hard disk drives. In the early '90s there were about a dozen hard disk drive companies. Today there are three. My old company isn't one of them. I credit managerial ignorance of basic principles of economics as one key reason. It turned out that in this case a dozen companies was not "a few" despite the enormously large number of computers sold each year; it was "many." Recent business prognoses about the hard disk drive business seems to be indicating that three companies *is* "a few" and these survivors have a fair likelihood of being able to establish an oligopoly industry selling to an oligopony market.

<sup>&</sup>lt;sup>9</sup> Hard disk drives are an essential component in computers.

<sup>&</sup>lt;sup>10</sup> Later this became "We're committed," spoken with all the savvy of the British generals at Passchendaele. The principal mistake cascaded into a series of other mistakes that wrecked our "box" business too.

The remaining two models, monopsonistic competition and oligopsony, also carry no single and generally agreed upon definitions and are not technical terms found in Bannock *et al.* (2003). Mansfield described *monopsonistic competition* as a market characterized by many buyers who purchase non-homogeneous but similar products and in which some buyers prefer some sellers' products over others [Mansfield (1970), pp. 367-368]. Econguru.com describes it as "a market structure characterized by a large number of small buyers that purchase similar but not identical products, have relative freedom of entry into and exit out of the industry, and possess extensive knowledge of prices and technology." They also call it "the somewhat obscure and seldom discussed counterpart to a monopolistic competition seller." Ill-defined as the term is, models of monopsonistic competition that have been proposed tend to be focused on labor markets and/or the study of minimum wage laws, e.g., Bhasker & To (1999), Bhasker *et al.* (2002)<sup>11</sup>. The criticism I voiced earlier in regard to other vague descriptions-posing-as-definition applies here as well. It often seems that the label is being used as a catch-all, i.e., a market economists cannot confidently identify as approximating either perfect competition, oligopsony, or monopsony is given the label of monopsonistic competition.

It strikes me as curiously interesting that *oligopsony* seems to be easier to describe than is the case for monopsonistic competition (especially since the opposite seems to be true for oligopoly vs. monopolistic competition). There are, again, divers descriptions of this market but all of them have one thing in common: there are only a "few" buyers who dominate the market in terms of influencing price-setting and other market factors. There may be many small buyers overall in the market, but the feature of having a few *dominantly* influential ones seems to be the key feature of oligopsony. In the context of labor markets, Bhasker *et al.* (2002) uses oligopsony to describe situations in which employer market power in setting wages persists despite competition with other employers for workers in the same labor pool. They also have a rather peculiar 'definition' of monopsonistic competition as "oligopsony with free entry so that employer profits are driven to zero." One can debate the correctness or usefulness of this pseudo-definition, but what is most interesting about it in our present context is that they use oligopsony (presumably with barriers to entry) as the reference point relative to which monopsonistic competition is distinguished.

The current state of these two models appears to be in considerable flux. It is questionable if they can even be used as "stand alone" labels without factoring in seller-side circumstances. I do contend this is true of all eight market labels, but the point seems to be considerably more evident for the monopsonistic competition and oligopsony labels. There seems to be a lot of controversy among economists over specific model cases reported in the literature and different model results tend to be obtained when different buyer-side and seller-side models are paired up in divers studies. My opinion is that fecund results in employing the models in the figure 2 schema are being frustrated by habits of trying to make the problem fit the math rather than making the math fit the problem.

Given the "fuzziness" of the model descriptions and the range of uncertainties in obtaining reliable measurements of pertinent economic data to plug into the models, the entire subject of market modeling seems exceptionally ripe for the application of set-membership theory (SMT) to both economic system identification and parameter estimation [Wells (2011); Combettes (1993); Belforte *et al.* (1990); Milanese & Novara (2011)]. Set membership modeling methods return solution *sets* that portray many models, all of which have the properties of being congruent with all *a priori* knowledge of the system and all measured data from the system. They have the further advantage of not being dependent on presuppositions about what sorts of probability distributions govern the system. SMT models also have the benefit of automatically providing

<sup>&</sup>lt;sup>11</sup> Bhasker *et al.* (2002) provides a reasonably extensive bibliography of labor market studies using the monopsonistic competition and oligopsony models.

variability range information (again without requiring *a priori* presuppositions about probability distributions) and some of them are also capable of automatically detecting when a proposed model does not take into account enough factors to represent the system being studied [McCarthy & Wells (1997)]. It has even been demonstrated that SMT property sets are capable of uncovering and automatically exploiting unexpected common factors in empirical data taken across a range of different circumstances [Carlson (2012)]. Finally, SMT models provide a smooth pathway in going from dialectic mathematics to algorithmic mathematics in modeling work.

The set membership approach has not been previously applied to economics – or, at least, not widely applied. There is much work to be done in terms of developing its specific applications to economics and economists will encounter a learning curve in understanding the design of mathematical structures. It might also require some economists to acquire a deeper understanding of the nature of what mathematics really is [Wells (2014), chap. 14]. In return for this economists' labor investment, SMT offers the possibility of paradigm developments that might turn out to be revolutionary in terms of Progress in the social-natural science of economics.

## § 7. The Theory of the Firm Within an Enterprise-Protein Field

My treatment and criticisms of the theory of the firm (microeconomics) in this chapter might perhaps lead you to think I hold microeconomics theory to be of little worth. This is not true. What I have criticized in this chapter is the presupposition that simply knowing the economic 'laws' that seem to apply to individual firms is enough to adequately describe the overall actions and dynamics of commerce in Society. There is a missing piece in the puzzle, a piece that is not part of any firm individually but, rather, is an interaction factor for all of them collectively. I call this interaction the *Enterprise-protein* model of commercial Society.

This idea is a mathematical one and, I regret to say, a somewhat advanced mathematical idea at that. It is for this reason a little difficult to adequately explain it without resorting to somewhat abstract mathematical constructs. It is a topic best treated in a separate technical paper. What I am going to try to do here is explain its general principle in a way accessible to readers who do not have the benefit of training and experience at the level of mathematics at which the idea is found.

Suppose we regard each firm as a mathematical "point" in an abstract "commerce space." The set F of all such firms is the aggregate of individual firms  $f_1, f_2$ , etc. Decision-makers in each firm determine the actions of their firms *locally* based upon their business acumen, knowledge of their industry, etc. How they make these determinations and what actions they take locally can be said to be approximately described by "local laws" of behavior, and these "local laws" are what micro-economics attempts to describe. Although I do contend there is still a great deal of Progress needed in microeconomic theory, it is somewhat remarkable how well the existing theory often seems to work despite its apparent shortcomings. Earlier I related the anecdote of my father and his management of his bakery business; this is one of countless examples economists could bring up in defense of microeconomics theory. When I was a manager, I relied upon microeconomics theory to quite an extensive degree.

However, all such decisions *immediately* effect only the local actions at f. These actions are not immune to *remote* actions of others in the "commerce space" F. There is always some effect of what others do that sooner or later feeds back to local firms f and provokes a counteraction there. Scientists call this a "field effect." Field effects are non-negligible in any system of interacting entities and a science that aims to explain phenomena in such a system must therefore posit "interaction laws" and how these interactions affect the local "points" f in the "space" F.

It can be debated whether or not specific (non-vague) interaction laws "belong" to the theory of the firm (microeconomic theory) or to a broader discipline within which microeconomics is a

sub-discipline. I tend to favor the latter, but it is a debatable matter of principal concern to the specialists and of no especial concern here. The type of mathematical construct appropriate for the "space" *F* is called an *embedding field network* and there is a specialized mathematical science which is dedicated to the theory of embedding field networks. This science is at its most advanced state of development in what is known as "neural network theory," and I will not try to downplay the fact that a great deal of additional development is required in order to apply it in social-natural contexts like social Molecules [Wells (2012), chap. 9] and the Enterprise-protein model. My point here is simply that a technical foundation exists, has existed for half a century, and that this foundation is a viable starting point for research and development of economic interaction theory among firms.

Local decision-makers at local firms f can properly be said to determine the *differential direction* taken by the actions of the firm. Field effects can be properly said to *propagate* through an Enterprise-protein network and "arrive" at firm f after some finite time delay. Their arrival can and does provoke decision-makers into "changing the direction" of the firm. When we state this in the language of mathematics, it takes the form of what is known as a difference equation.

Difference equations play a role in science that is philosophically very interesting and was first pointed out in 1950 by physicist Henry Margenau in the context of an important principle in physics called Hamilton's principle. The role is this: Human decision-makers and action-takers base the determinations of their behaviors on *teleological* grounds. This is the capacity of human agents that essentially distinguishes *homo noumenal* human nature from the nature of dead matter. It is what *essentially* distinguishes social-natural sciences from physical-natural sciences. Yet, at the same time, all specific actions expressed by a human being are physical actions and those kinds of actions are inseparably bound to laws of physical cause-and-effect. This is a consequence of the nature of a human being as *homo phaenomenon* in the physical world.

How can these two seemingly-contradictory kinds of "nature" exist in *one* natural object (a human being)? Controversies over this among philosophers and theologians have raged for centuries. Then in the 1930s this same question arose in a different context and set a puzzle and a worry for physicists following the discovery of the quantum theory of physics. The metaphysical conundrum for physicists involved what is called Hamilton's principle. This is an important – one could even say vital – law of quantum mechanics. However, when the language of mathematics in which Hamilton's principle is stated is translated into non-mathematical language, *its statement implies teleology is at work in physics* – an implication in utter contradiction to the most fundamental notions of physical causality and unacceptable in physical-natural science.

It was in 1950 that Margenau resolved the paradox by making what I regard as one of the most prescient philosophical observations ever enounced by a physicist. In Margenau's context he was speaking of "differential" equations rather than "difference" equations but this does not make any difference for the point I am arguing here. Margenau wrote,

By means of a trick well known to mathematicians . . . [the physicist] converts the integral relation [Hamilton's principle] into a set of differential equations called Lagrange's equations, and these are of the causal type [of equation]. He has thus – and this may come as a shock to metaphysicians – *transformed a purpose into a cause*. [Margenau (1977), pg. 423]

This is what difference equations do in quantitative models of social-natural phenomena. They convert aims and goals (teleological purposes) into mathematical forms necessary for expressing *homo phaenomenal* actions. They are essential for developing *quantitative* models of economic systems *grounded* in objectively valid laws of human nature.

Whether they are consciously aware of it or not, businessmen are confronted by and deal with

field effects day in and day out. The "tactical planning committee" I talked about earlier in this chapter is a specific example of this. While some members of this committee, including me, did make some efforts to understand and predict likely field effects, in fact our predictions were often unreliable enough that our policy was to take action on the basis of new factual information, i.e., we were empiricists in our behaviors. It would perhaps be convenient if decision-makers were omniscient, but we're not and the general notion of action-reaction of which Isaac Newton wrote is a valid notion for business and economics theories. So it is, I contend, that "field effect theory" is a legitimate but largely missing *quantitative* modeling element of economics theory. I speculate that the sorts of general trends and tendencies employed in the dialectic mathematics of microeconomics will be largely unmodified *qualitatively*, but I would be surprised if economics theory was *quantitatively* unaffected by such a field theory. Thus, the "moral of the story" here is one of *augmentation* rather than *replacement* of the theory of the firm.

## §8. References

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