Marshallian General Equilibrium Analysis

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In an assessment of Alfred Marshall, Paul Samuelson (1967) writes that "The ambiguities of Alfred Marshall paralyzed the best brains in the Anglo-Saxon branch of our profession for three decades." In making this assessment he carried on a tradition of Marshall-bashing that has a long history in economics, dating back to Stanley Jevons and F. Y. Edgeworth, who accused Marshallian economists of being seduced by "zig zag windings of the flowery path of literature." (Edgeworth, 1925)

These harsh assessments of Marshall and his approach to economics have had their influence on the modern profession and, other than historians of economic thought, few young economists know much about him. Fewer still would see themselves as Marshallians.[1]

Today, Marshall is best remembered for his contribution to partial equilibrium supply and demand analysis.[2] For the true economic theorists of the 1990s, however, this contribution is de minimus; the partial equilibrium approach is for novice economists with no stomach for real economic theory—general equilibrium. The profession's collective view of Marshall in the 1990s

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[1] Until recently Chicago economists, especially Milton Friedman, saw themselves as working in a Marshallian tradition. More recently, however, younger Chicago economists know little of Marshall, and work in the same Walrasian general equilibrium framework as does the majority of the profession.

[2] Even here, Marshall's contribution is questioned. As Humpries and(to come) (1994) argue, Marshall was neither first, nor clearest, in his presentation of partial equilibrium supply and demand.

is that Marshall is passé--at most a pedagogical stepping stone for undergraduate students, but otherwise quite irrelevant to modern economics. The motto of recent 20th century economics has been:

> Marshall is for kids and liberal arts professors; real economists (professors at universities) do Walras.

Since Marshall's name is synonymous with partial equilibrium analysis, the title of this paper will seem strange to many. (One well known economist, upon hearing it, labeledthe title an oxymoron.) Most economists think of general equilibrium analysis as synonymous with Walrasian general equilibrium analysis. In this paper I argue that this is not true. *Marshall was centrally concerned with general equilibrium analysis*; he was, after all, a classical economist, and he drew on, and saw his work as extending, the work of Adam Smith, David Ricardo, and John Stuart Mill, all of whom were concerned with general equilibrium, not partial equilibrium, issues.

I shall also argue that the profession's negative assessment of Marshall is wrong. Specifically, I shall argue that, conceptually, Marshallian general equilibrium analysis is at a much higher level than Walrasian general equilibrium analysis, and, becasue it is, is far more compatible with modern developments in economics than is Walrasian general equilibrium. Thus, Marshall's work is not a stepping stone *to* Walras, but is instead a stepping stone *beyond* Walras. It is consistent with a fundamentally different conception of general equilibrium, one which recognizes that the mathematical formulation of a meaningful general equilibrium model is much more intractable than those with which Walras and later Walrasians dealt.

Marshall's Interest in General Equilibrium Analysis

Marshall's interest in general equilibrium is more than simply a conjecture of mine. In Note 20 (Note 21 of 2-9th edition) of *Principles of Economics,* Alfred Marshall discusses the issues of general equilibrium in his "bird's eye view of joint demand, composite demand, joint and composite supply when all arise together." In discussing this note in a letter to J.B. Clarke, Marshall (1908) comments that "my whole life has been and will be given to presenting in realistic form as much as I can of my Note 21. If I live to complete my scheme fairly well, people will, I think, realize that it has unity and individuality."

Consistent with this view we can find discussions of interrelationships among markets in his *Principles*. (See, for example, p. 711.) But what those discussions present are observations of realities, not analytics. As I will argue below, Marshall used the real world observations as a guide to the interrelationship among markets because he believed that an analytic understanding of these interrelationships was beyond the mathematical specifications of the time. Given that belief, it is not surprising that Marshall's discussions were not about abstract mathematical interrelationships, but were about observed interdependencies that acknowledged institutional realities. In a sense Marshall used the actual economy as an analog for the analytic model. If, in the short run, observed prices were relatively fixed and quantities were variable, then one solution to the complicated general equilibrium model underlying the economy must be relatively fixed nominal prices and fluctuating quantities. Observations

served as the basis for his discussions of the interrelationships among markets.

Why Marshall Shied Away from Developing a Formal General Equilibrium Model

Why did Marshal focus his analysis on partial equilibrium and not formally develop his conception of general equilibrium? One possible explanation is that he was not the mathematician or conceptualizer that Walras was, and that he knew he was incapable of formally specifying a general equilibrium system. I think it is correct that he felt incapable of specifying a meaningful formal general equilibrium system, but not because he was unable to formulate a system such as Walras's. One reason I believe this is that Marshall was a trained mathematician, and by most accounts, a good one. He understood simultaneous equations and had the ability to solve systems of simultaneous equations. His Note 21 summarizes the essence of a broad conception of general equilibrium better than any other one page written on the subject.

I believe the reason Marshall didn't formally analyze general equilibrium issues is that he demanded intuitive correspondence between math and his understanding of the economy. When that correspondence was

not present, the math was irrelevant; such irrelevant math should be discarded.[3]

Marshall's recognition of the analytic intractability of the general equilibrium problem, given the math available to him, and his desire for concreteness in his economics, led him to shy away from abstract specifications of general equilibrium. Leon Walras, meanwhile, had less aversion to abstraction devoid of intuitive correspondence with reality, and trod where others would not go. Unfortunately, it was a path that others followed, and Walras's version of the general equilibrium system has become the foundation of modern 20th century economics, while Marshallian general equilibrium economics never developed.[4] Thus, when Paul Samuelson

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[3] In a well known letter to A. L. Bowley, Marshall wrote: "I had a growing feeling in the later years of my work at the subject that a good mathematical theorem dealing with economic hypotheses was very unlikely to be good economics; and I went more and more on the rules (1) Use mathematics as a shorthand language, rather than as an engine of inquiry. (2) Keep to them until you have done. (3) Translate into English. (4) Then illustrate by examples that are important in reallife. (5) Burn the mathematics. (6) If you can't succeed in (4), burn (3). This last I did often." (Pigou, *Memorials* pg. 427.)

[4] I want to be careful to avoid the type of unfair criticisms of Walras that I believe earlier economists have made of Marshall. I am not an expert on Walras, and what I am criticizing as Walrasian is what has been passed down as Walrasian, not necessarily what a fair interpretation of Walras would include. I an sure that there are many subtleties in Walras, which, if given a sympathetic reading, can lead one to conclude that Walras would have opposed what came to be known as Walrasian general equilibrium--that is analysis of the aggregate economy that assumes a unique equilibrium system in which an auctioneer sets price and no trading is done at disequilibrium prices even though the system is always in disequilibrium.

developed the mathmatical foundations of modern economics, (Samuelson, 1), he developed them around Walrasian economics. Similarly when the microfoundations to macro were developed, they were developed along Walrasian general equilibrium lines.

To Marshall, once one mastered the intuition of the general equilibrium reasoning, going through formal specification in the way Walras did was laborious but trivial. Such an exercise was worth one page in an appendix in the *Principles*. Anyone with reasonable training in math could work out a system of general interrelated equations. Marshall did not do so because it would not add much to our understanding, and would violate the law of significant digits, since such a specification would be incomplete. The problem was the interrelation between dynamic and static issues; such interrelationships clearly existed and, in Marshall's mind, invalidated any static analytic conclusion at which one could arrive. Marshall followed the maxim: better to be ambiguous and relevant than precise and irrelevant.

For example, Donald Walker (1994) argues that while this view of Walras follows from the fourth edition of the *Elements,* the version most English speaking economists are familiar with (since that was the version translated), in earlier versions there was a different, and what Walker believes is a more meaningful system--one that is closer to the system I am attributing to Marshall, and that elsewhere (Colander 1995) I have called Post-Walrasian. Walker calls this earlier version the "mature Walras" and attributes the later version to Walras's intellectual decline that began in the mid-1890s. Others Walrasian scholars I have talked to argue that the Walrasian system does not even follow from the fourth edition.

I leave it for historians of thought to determine whether Walras and Marshall are closer in their view of general equilibrium than my argument suggests, and whether the entire development of modern general equilibrium is based on a wrong interpretation of Walras, or on the translation of the wrong edition of his book.

Marshall was not the only economist of the time who did not make the jump to Walrasian-style general equilibrium. Auguste Cournot and F. W. Edgeworth were also superb mathematicians, and they too shied away from developing a formal general equilibrium system. Only Walras made the jump to a formal specification of the general equilibrium system. One possible explanation for why Walras trod where others would not is that Walras simply wasn't a top flight mathematician, as evidenced by his failure to gain admittance to the Ecole Polytechnique. Moreover, as Landreth and I argue in (Landreth and Colander 1994), Walras relied on others to clear up mathematical problems. For example, his development of marginal productivity followed Wicksteed's superior treatment. Similarly, his knowledge of multivariate calculus was limited, and his early editions demonstrated confusion about interdependent derivatives where cross partials were required. Thus my conclusion on this question of mathematical ability is that Marshall was lost in the "zig zag windings of the flowery path of literature" by choice, not by relative lack of understanding or mathematical ability compared to Walras.

What I am arguing is that Marshall understood the intricacies of general equilibrium far better than did Walras, and knew that a formal mathematical specification of those intricacies that was necessary to meet his demand for correspondence between the math and the intuition was beyond him. Consider his description of the stability of a supply demand equilibrium. He writes:

> When demand and supply are in stable equilibrium, if any accident should move the scale of production from its equilibrium position, there will be instantly brought into play forces tending to push it

> back to that position; just as, if a stone hanging by a string is displaced from its equilibrium position, the force of gravity will at once tend to bring it back to its equilibrium position....
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> But in real life such oscillations are seldom as rhythmical as those of a stone hanging freely from a string; the comparison would be more exact if the string were supposed to hang in the troubled waters of a mill-race, whose stream was at one time allowed to flow freely, and at another partially cut off. Nor are these complexities sufficient to illustrate all the disturbances with which the economists and the merchants alike are forced to concern themselves. If the person holding the string swings his hand with movements partly rhythmical and partly arbitrary, the illustration will not outrun the difficulties of some very real and practical problems of value. For indeed the demand and supply schedules do not in practice remain unchanged for a long time together, but are constantly being changed; and every change in them alters the equilibrium amount and the equilibrium price, and thus gives new positions to the centres about which the amount and the price tend to oscillate. (Marshall, *Principles* pp. 346-347.)

As Barkley Rosser (1991) points out, the metaphor in this passage is a system that exhibits chaotic, or at least partially chaotic, dynamics. To meaningfully analyze such a system requires an interdependent system of equations involving, at a minimum, complex second and third order differential equations. The solutions to such systems are anything but simple; they exhibit path dependency, and sensitive dependence on initial conditions.

Marshall recognized this complexity, and did not try to fly before the airplane had been invented. He knew he could not deal with the issues formally, so he did the best he could to deal with them informally. In Walras, observed reality was forced to be consistent with available mathematical techniques. In Marshall what is, is what we observe, and if what we observe doesn't fit the available math, then we will simply have to write about the ambiguities in words, and wait for the mathematical techniques to develop.

Marshall introduced his period analysis with a market period, short period, and long period. As Axel Leijonhufvud points out, this period approach to studying the adjustment of potentially complex non-linear systems was the type of approach physicists were using in studying problems involving non-linear dynamics. It was known as *adiabatic transformations* in the older thermodynamics literature. (Leijonhufvud 1995.)

My point is not that Marshall's treatment of such issues was satisfactory; it had serious problems, and Marshall knew it. For example, he wrote that his treatment of time and the various runs was the weakest element of his analysis. (Marshall, 1908) My point is that Marshall recognized that these issues were of fundamental importance, and that the then available mathematics was insufficient even to begin to handle those problems. Since such complicated issues were central to understanding the workings of the aggregate economy, why formulate formal models that deviated so much from observations? Only now, in the 1990s, are economists becoming sufficiently familiar with the math relevant to such situations—non-linear dynamics, chaotics and complexity—to start to apply them in their models.

A second reason Marshall did not formally specify his general equilibrium system was that he was a cautious man; for example, although he had worked out the central elements of partial equilibrium supply and demand analysis, and his foundations of neoclassical economics, in the 1870s when Menger and Jevons were espousing their claims, he did not publish them until the 1890s—twenty years later. Keynes, reflecting on Marshall's cautious nature writes: "Jevons saw the kettle boil and cried out with the delighted voice of a child; Marshall too had seen the kettle boil and sat down silently to build an engine." (Keynes 1956.) Marshall recognized that the jump to general equilibrium was, by contrast to the jump to partial equilibrium, a gigantic leap worthy of at least a 100-year wait, if partial equilibrium took a 20-year wait.

The Marshallian General Equilibrium System

I admire Marshall, but I do not share his cautiousness. I have more the personality, and the mathematical ability, of Walras. Moreover, mathematics has developed enormously since the late 1800s; work in complexity theory, non-linear dynamics, chaos theory, and the developments in computers has given us tools needed to gain more understanding of complex systems—tools Marshall did not have. In short, our formal tools have begun to catch up with Marshall's intuition. Thus, there are many similarities between this new work and Marshall's approach to general equilibrium.

These developments in math, combined with an inherent incautiousness, place me in an ideal position to do what Marshall would not do—to spell out a possible vision of his conception of general equilibrium, and

to show how it contrasts with Walras's. [5] It will be a broad vision, one that will likely raise as many questions as it answers. But, I believe, that while I will not adequately specify a Marshallian general equilibrium system, I will make clear why it is what we should be working on, rather than adding yet another detail to almost vacuous Walrasian vision.

Introducing Stability Through Institutions

The central organizing theme of the Marshallian general equilibrium system that I am proposing is the following observation: Our economy may be messy and sometimes chaotic, but it is nowhere near as chaotic as one would expect of the solution to a general equilibrium system of simultaneous equations. Realistic assumptions about interactions would cause a system of simultaneous equations of a Walrasian type to exhibit dynamic path dependencies, non-linearities, and strategic interdependencies, which will making it far more chaotic than the observed reality. *This means that our economy cannot be described by such a system of simultaneous equations.* It follows that it is senseless to accept what might be called the Walrasian fudge—the assumption of a Walrasian auctioneer who eliminated all dynamic disequilibrium adjustment problems and brought about equilibrium.

[5] I should make it clear that in this endeavor while, I believe, the conception I put forward in a Marshallian tradition, I make no claims of it being the only general equilibrium conception consistent with Marshall. It is what I elsewhere call a Post-Walrasian conception. What Marshall would have put forward, or what can be teased out of Marshall, is infinitely debatable. I do not want to be part of that debate; my interest in the past is in its ability to generate ideas about the present and future, not in the past itself.

Mathematically, this fudge created the possibility of a solution to the system, but it was a conceptually uninteresting solution to anyone who agrees with the Marshallian observation. [6]

For Marshall the question was what to do in specifying an alternative system, and given his cautiousness, he simply did nothing. What I am proposing is that he should have posited a the existence of a set of equations combined with restrictions on the aggregate combinations of individual actions, and hence on individual actions themselves, that eliminated these instabilities. These restrictions are what might be the Marshallian fudge.

This Marshallian fudge involves a substantive role of existing institutions and non-market coordinating mechanisms in providing the coordination that is assumed in Walras. These institutions provide a framework of coordination, but they also provide systemic constraints on the decision making of individuals. Any analysis of individual decision making must take into account these systemic constraints. An institution-less economy would, in this Marshallian sense, be unstable; it would be

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[6] There are two reasons why I believe Marshall could not accept the Walrasian fudge. The first is that he did not believe that general equilibrium issues could be reasonably dealt with using a set of timeless interrelated simultaneous equations because individuals do not have the capabilities to process the information necessary to deal with such a system. The second is that if people did have the capabilities to deal with general equilibrium analysis, the result would have been chaos since there were too many options and strategic interdependencies.

characterized by anarchy and chaos.[7] Because these restrictions embodied in institutions provide the stability necessary to prevent chaos, such restrictions must be included in the analysis. Institutions provide stability, but they also provide restrictions on individual actions. You cannot assume stability without institutions.

The Marshallian fudge follows from insights we get from the analysis of complex systems. Inevitably those complex systems are not organized with a single system of simultaneous equations; instead they are organized with hierarchical structures that take advantage of the computational abilities of the various levels. A metaphor for this approach is the way a computer is organized. It has an operating system, software, and nested software. Individuals operating at lower levels do not understand the workings of the entire computer; they accept the rationality of their subsystem.

The essence of my proposed Marshallian general equilibrium analysis is that it sees the interaction among sectors as being solved in a sequential manner in which nested institutions of various longevities are accepted by some set of individual decision makers. These institutions limit instability with a corridor around existing situations. In normal times, individual optimization is conducted given the multiple leveled constraints, but every so often, perhaps because of a large autonomous shock, or simply spontaneous

[7]The irony of Marshall's general equilibrium system is that if it is taken seriously, it undermines the one contribution that he is known for--partial equilibrium, because what is now known as partial equilibrium does not take into account the constraints imposed on individual decision makers by general equilibrium institutions.

dissatisfaction, individuals challenge these constraints; aggregate stability is lost, and new institutional structures, and new constraints, emerge.

Marshallian rationality is fundamentally different than Walrasian rationality, and its role in the system is different. Decisions are made sequentially, and certain decisions, once made, become operating data for lower level systems. Marshallian rationality can mean many different things depending on what level one is operating at. For most decisions the institutions, and the constraints they impose on individuals' decisions, are the central feature of fixity in the Marshallian general equilibrium system, and the shorter the run, the more institutions are assumed fixed. Marshallian rationality is defined locally, not globally.[8] In fact, Marshallian systemic stability depends on individuals not exhibiting global rationality. People's limitations make it possible for institutions to develop; their bounded rationality creates a stability that could not exist if everyone pushed economic maximization to the limit.

But this Marshallian systemic stability is fragile; the economy is always bordering on chaos, and when a sufficient number of individuals try to take advantage of the niches in the system left by institutions—i.e., follow economic rather than social restrictions—the institutions fail, stability is lost, and a new set of institutions must be found to provide the necessary stability. In short, the system takes advantage of people's cost of computing and, whenever, possible, chooses an institution that provides stability.

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[8]Herbert Simon, and his bounded rationality, is the logical follower of Marshall.

Notice the difference between the Marshallian and Walrasian conception of economically rational actor. In the Walrasian conception the ultra rational economic actor drives the system to equilibrium and serves a useful purpose. In the Marshallian system such ultra-rational economic actors can destroy the system by destroying the institutions that give it stability.

A Mathematical Specification of Marshall's General Equilibrium System

Mathematically, Marshall's jump to general equilibrium would not be a single jump, but rather a set of jumps; these intermediate jumps complicate the mathematics of general equilibrium enormously. It involves specifying all decisions as a system of multiple nested equations.

$$y=f(g(h(k(l(x)))))$$

One could argue that such a layered problem could be reduced to a Walrasian system by simply reducing this equation into a composite function:

$$y=f'(x)$$

That could be done, but the functional form would have no relationship to our intuition. It would be non-continuous, and there would be no presumption of its having any of the nice properties that we need it to have if we are to analyze it. formally. The reason is that the broader optimization involves complex programming problems that cause strategies to shift substantially as the situation changes slightly. Marshall would argue that we can reasonable

hope to understand the system only when people are operating within the system—when they are accepting the restrictions that are imposed at all but the lowest level. Our intuition doesn't go beyond that. Hence, Marshall's limited focus of analysis.

Marshall sees it as impossible to go from intuition to specification of composite functional form as is done in Walrasian general equilibrium analysis. There is no presumption of correspondence of intuition and functional form. The characteristics of the composite function will likely be significantly different than the characteristics one would intuitively identify.

If you use the composite function rule, the composite function should have built into it all the constraints that would follow from the intuition of combining the various functions. One cannot continue to use one's intuition about functional relationships as if a composite function were not used. But that is precisely what is done in Walrasian general equilibrium. It was because Marshall recognized the limitations of the mathematics of this multiple jump that he chose the zig zags of literary exposition rather than the assured failure of mathematical specification.

The mathematical specification of such a layered equilibrium is extraordinarily difficult, and each layer involves a slight deviation from intuition. Thus, when Robert Solow writes that Alfred Marshall seems to have felt that, at every level of mathematical deduction, a little truth leaked out, Solow was right. But Solow was suggesting that Marshall was wrong in believing that; I am suggesting Marshall was right. If you are trying to relate intuition and observation with formal specification, the tolerances of deviation increase with each functional form one specifies.

How does one deal with such a problem? By relying on what one sees, not on what one deduces. This accounts for the Marshallian dynamics that assumes prices are fixed, and quantities variable in the short run adjustment, as opposed to Walrasian dynamics which sees prices variable and quantities fixed.

Some Implications of the Marshallian Approach

There are many implications of this Marshallian approach to general equilibrium for the way we do economics in the 1990s. For example, consider the justification for the dynamics. In Walrasian economics one must search for a microfoundation for such dynamics. Why don't individuals allow prices to fluctuate, since that would be optimal? In Marshallian general equilibrium there is no such presumption, and thus the search for a contextless micro foundations, a search that characterizes much of modern macro, is meaningless. If institutions exhibit relatively fixed nominal prices, such fixity is a macro systemic constraint that is imposed by institutional requirements.

A second, related, issue concerns optimality of the market system. Since multiple institutions can be chosen, there is no presumption of systemic optimality of a market system. Any conclusion about systemic optimality follows only from a consideration of comparative institutions. There is no assurance that the market system coordinates better than other systems. If it does, this is an observable phenomenon, not a deduced fact. In fact, in the Marshallian system the concept "market" has no meaning without a specification of the institutions that make that market feasible.

In the long run all interactions are possible, but like a computer without an operating system, the long run institutional structure is extremely user unfriendly. Changes in that institutional structure are made with great trouble. There is no omnipotent being choosing the best system, but, instead, there are individuals working within the institutional structure they have. Bill Gates said that God created the world in seven days, but He didn't have an installed user base. It is not simply a single operating system that our economy has, but is instead a multiple layered system of nested software, and that nested system makes change, and any analysis of low level decisions, extraordinarily complicated.

As a final example of where Marshallian general equilibrium theory gives one a fundamentally different view of economic reality than does Walrasian general equilibrium, let me consider a specific aspect of economics, one that has been central to distinguishing different schools of economics: the theory of distribution. In the Walrasian approach income distribution is determined by marginal productivity. Assuming a linear homogeneous production function, one has a complete theory of distribution. This theory of marginal productivity is so built into our way of thinking that it is often not questioned. Marshall, however, had serious reservations about it, and understanding Marshallian general equilibrium explains why. In the Marshallian general equilibrium approach, marginal productivity theory influences distribution, but it is in no way a theory of distribution. You can see Marshall's view where he writes:

> This doctrine (of marginal productivity) has sometimes been put forward as a theory of wages. But there is no valid ground for any such pretension. The doctrine that the earnings of a worker tend to be

> equal to the net product of his work has by itself no real meaning; since in order to estimate net product, we have to take for granted all the expenses of production of the commodity on which he works, other than his own wages.
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> But though this objection is valid against a claim that it contains a theory of wages; it is not valid against a claim that the doctrine throws into clear sight the action of one of the causes that govern wages. (*Principles* p. 519.)

The problem Marshall had with marginal productivity theory is that institutions have significant effects on distribution, and thus it is simply wrong to talk about marginal productivity independent of institutions' effect on income distribution. In game theoretic terms the argument is that to get an acceptance of institutions, side deals must be made among participants which place constraints on individuals and change the nature of equilibrium.

Let me give an example. Say you have two types of individuals: big heads and big arms. Say also that there are three possible production techniques that are possible. Two of these production techniques require acquiescence among individuals; these two techniques are equally efficient in the sense that when all workers are used, 100 units of output is forthcoming from both techniques. Technique A, however, gives a MP of 3/4 to big arms and 1/4 to big heads, while Technique B gives a MP of 3/4 to big heads and 1/4 to big arms. Techniques A and B require acceptance from both groups; if no agreement is reached, Technique C must be used, which gives a MP of 1/2 for both, but which has a total output of only 40.

Clearly each group will be better off with choosing either Technique A or Technique B, but neither technique dominates the other. How do they decide which technique to use? The obvious answer is to make an inviolable social compact, embodied in an institution, to use one of the two techniques, but to have, say, big arms receive certain side payments, perhaps 25, from big heads. (Of course, big arms would want more since no compact is inviolate, but let me ignore that complication here.)

In Walrasian economics such side payments resulting from prior deals cannot be considered; there is no history and no institutions. In Marshallian economics, to have a theory of distribution one requires both a theory of history and a theory of institutions. In Marshallian economics, to judge any outcome, it is not enough to look at marginal productivities at a point in time; production has a social and historical component, and a particular result can only be interpreted in its historical and social context. Walrasians make the implicit assumptions that all these complications do not matter—that the time inconsistency problem is not dealt with by individuals, and that ,somehow, all institutions are simply plopped down upon individuals. Walrasian marginal productivity distribution theory ignores all that; Marshallian general equilibrium distribution theory could not, and therefore is much more complicated.

Conclusion: A Reversal of Samuelson's Dictum

There is much more to be said about Marshallian general equilibrium, but I believe that I have said enough to make my point. There is some depth in Marshall that belies the many negative assessments of his work. My own

feeling is that there is sufficient depth to warrant a reversal of the negative assessments discussed above. To make my point clear, let me continue the Paul Samuelson attack on Marshall with which I started this talk. Samuelson writes:

> I have come to feel that Marshall's dictum that "it seems doubtful whether any one spends his time well in reading lengthy translations of economic doctrines into mathematics, that has not been done by himself" should be exactly reversed. The laborious literary working over of essentially simple mathematical concepts such as is characteristic of much modern economic theory is not only unrewarding from the standpoint of advancing science, but involves as well mental gymnastics of a peculiarly depraved type. (Samuelson, 1955. pg 6.)

In the 1940s and 1950s, in certain aspects of economics Samuelson was, I have no doubt, right. At that time there were many issues to be cleared up, and his *Foundations* did clear up numerous issues. But the fact that there then existed some poor intuitive literary economic analysis should not condemn all intuitive literary economics, just as the fact that today that there is some poor mathematical economics should not condemn all mathematical economics.

What I am arguing is that there is a symbiotic relationship between intuitive literary economics and formal mathematical economics. Both are necessary; both can advance our knowledge. Some aspects of good literary economics of a period become the core of good formal economics of a later period. But we will only know which aspects when the formal math catches

up with the intuition. The ideal would be a peaceful coexistence of the two. But peaceful coexistence does not seem to be a stable equilibrium and instead the profession seems to experience these cycles when Marshal's Dictum or Samuelson's Dictum predominates. (Consider, for example, the Ricardo-Mill cycle.) Whether Samuelson's Dictum or Marshall's Dictum is relevant depends on what part of the cycle we are in. The 1930s-50s was a time for formal mathematical economics to export ideas to intuitive economics. In my view, the 1990s is a time for the reverse. More and more top economists are accepting that we have come as far as we can with static Walrasian general equilibrium.

The new reality of the 1990s is an acceptance that the general equilibrium system relevant to our economy is formally complex. Because that is the case, in the 1990s, Samuelson's condemnation of Marshall needs to be reversed. Thus, for the 1990s I suggest that the pendulum has swung and the following reworking of Samuelson's above quotation is relevant. Specifically: "The laborious *mathematical* working over of essentially simple *intuitive* concepts such as is characteristic of much modern economic theory is not only unrewarding from the standpoint of advancing science, but involves mental gymnastics of a peculiarly depraved type." "The intuitive ambiguities of Walras's general equilibrium, and Samuelson's expansion of it, have paralyzed the best brains in economics for the last five decades." It is only now that the profession is returning to the understanding of economic issues that Marshall had at the turn of the century

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Only now, that the problems with Walrasian general equilibrium are beginning to be taken seriously, do we see economists working in that Marshallian tradition even if they don't know it--work of Brian Arthur, Clower and Leijonhufvud in macro, and Nate Rosenberg on technology are the modern inheritors of the Marshallian mantel.

The third point I will argue in this paper is that Marshall's general equilibrium will replace Walrasian general equilibrium in the next decade. Thus, the motto of the 21st century will be:

> Marshall is for real mathematicians and liberal arts professors; Walras is for pseudo mathematicians which includes many of the professors at universities.

Why a Marshallian General Equilibrium Revival is Inevitable

What makes a Marshallian revival almost inevitable is the development of new mathematical and computer techniques to deal with these. Chaos theory, the analysis of complexity, non-linear dynamics ___________ ______ and dynamics vector game theory, and frequency dependent equilibria give a research program in analytics that can begin to get at some of these complications. At the same time computers have developed that make analysis of such problems through simulation possible. The Walrasian fudge need not be made to mathematically analyze these issues. Thus I see a new condition emerging in the 21st century between real mathematicians--who develop the math necessary to deal with the problems in the full complexity in an intuitively satisfying way (as compared to the pseudo mathematical types who force their intuition to fit their available mathematical techniques) and the literature types who can interpret, and

Marshallian general equilibrium policy analysis--the analysis of institutions

Marshallian General Equilibrium

Summary of Paper to be presented at HES meetings

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In his rather harsh assessment of Alfred Marshall Paul Samuelson writes that "the ambiguities of Alfred Marshall paralyzed the best brains in the Anglo-Saxon branch of our profession for three decades." This paper argues that Samuelson is right, but interprets the events differently than did Samuelson. It argues that that paralyzing effect was a good thing—necessary because the mathematics available at the time were not up to the task of considering issues of general equilibrium in the complexity that they needed to be considered. It argues that the misplaced precision of Leon Walras's general equilibrium analysis sent the best brains in the profession on a wild goose chase that reduced, as opposed to increased, our knowledge of economic phenomena. Only now in the 1990s, with new developments in mathematics and computers, are we even beginning to get to a level of mathematics where we might be able to shed some light on general equilibrium issues through analytic means.

The paper argues that Marshall understood the limitations of the current math. It considers his Note 21 of *Principles* where he summarized the essence of Walrasian general equilibrium theory. It explains why he felt that was inadequate, and summarizes what I see, given current mathematical techniques available, to be Marshall's general equilibrium vision and contrasts that Marshallian vision with Walras's.