Analysing what cannot be modelled: a defence of Marshallian equilibrium analysis[1]

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I conceive no more calamitous notion than that abstract, or general, or 'theoretical' economics was economics 'proper'.

1 This paper is based on a comment on a paper by Michel de Vroey prepared for a workshop at the University of Keele October 18th, 2005. I am grateful to Michel de Vroey for many discussions on this topic from which I have learned much.

Alfred Marshall (Whitaker 1996 II:393)

The cuttle fish called Marshall (long-period) is far too wily to be caught.

Joan Robinson (1973:249)

1. Introduction

Alfred Marshall's theory of value is puzzling. He was a trained mathematician who derived some of his crucial theoretical observations through translating existing work into mathematics. However, not only did he come to disparage the use of mathematics in economics, and even economic theory, but he created an engine of analysis that was denounced by his British followers as imprecise and in need of replacement by a more rigorous box of tools (Robinson 1933:1). After outlining the problem, in Section 2, the paper then draws, in Section 3, on recent work to argue that the problems in his theory were a direct result of his concern with heterogeneity and evolution. This provides the basis for the argument that making sense of Marshall's argument requires a new approach to the logic of partial equilibrium analysis. Section 4, therefore, offers a rational reconstruction of Marshall's method, according to which he was offering not a single general model of which partial models are special cases, but a series of models, the simpler models establishing mechanisms, or tendencies, which are then combined to create more complex models. This reconstruction is then tested against some of his methodolical statements in Section 5. Finally conclusions are drawn that read the theory of value developed in the *Principles,* in the light of *Industry and Trade*, as an exercise in applied economics.

2. On criticising Marshall's value theory

Alfred Marshall's equilibrium analysis has long been a puzzle to economists. The list of those finding fault with it is very long: Piero Sraffa (1926), Lionel Robbins (1928), Joan Robinson (1933), Paul Samuelson (1967) and virtually any exponent of Walrasian general equilibrium theory. Given the dominance of the Walrasian paradigm in the post-Second World War era, Marshall's star waned, except as the creator of partial equilibrium analysis that was useful to applied economists and suitable for introductory courses in microeconomics. Ironically, it was typically expounded using geometric analysis that was developed by Joan Robinson, whose method was very different. Though there have been recent defenders of Marshall, the move towards game theory rather than general equilibrium as the dominant foundation for economic theorizing, and interest in formal models of imperfect competition may have led to partial equilibrium analysis being taken more seriously but it has hardly caused a change of attitude towards Marshall's method.

The main exception to this generalization has been Milton Friedman who, certainly since his "Methodology of positive economics" (1953) has been a consistent supporter of Marshallian methods. However, though Friedman could justifiably call his approach Marshallian, it was Marshallian economics with a distinctive twist, placing far more emphasis on individual maximizing behaviour and perfectly competitive markets than Marshall would have countenanced. There has also emerged a recognition that the way John Maynard Keynes approached economics was fundamentally Marshallian (see, for example, Leijonhufvud 2006) but this has focused on issues relevant to macroeconomics.

The revival of interest in Marshall has come from different sources. Much more of

Marshall's unpublished writings have now been analysed, from his early work in economics (Whitaker 1975) and psychology (Raffaelli 2003), through his notes on philosophy (Cook 200*) to his correspondence (Whitaker 1996). Marshall's analysis of industrial districts, especially in *Industry and Trade* (1919), stimulated a revival of interest in his work, centred on Italy which culminated in the systematic review of Marshall's work found in *The Elgar Companion to Alfred Marshall* (2006). The result of this work has been that Marshall's theory of value and its behavioural foundations have been re-examined.

However, a key methodological issue surrounding Marshall's theory of value has still to be addressed. Marshall's partial equilibrium analysis, with its analysis of time in terms of "periods" has widely been taken to be logically inconsistent. Most notoriously, Samuelson (1967:109,111) argued that Marshall was insisted "on having his cake and eating it too": that he "was so afraid of being unrealistic that he merely ends up being fuxzzy and confusing – and confused" it too that he ended up being confusing and confused". If his partial equilibrium analysis is viewed as, essentially, Walrasian general equilibrium in which certain variables are held constant in the short run, it is hard to avoid this conclusion. Among recent commentators, De Vroey (200*) has gone further than most in delving into the differences between Marshallian and Walrasian equilibrium. However, whilst he has made perceptive observations about issues such as the informational requirements of a Walrasian auction market and a decentralized Marshallian market, and the treatment of time in the two types of theory, he has reinforced the perception that Marshall was inconsistent and lacking in logical rigour.

The idea explored in this paper is that Marshall might have been arguing in a way that, though it fails according to conventional methodological canons, nonetheless

follows a defensible, logical method that, far from being confused, makes good sense given his objectives. Keynes once wrote of Friedrich Hayek's *Prices and* Production (1930) that, 'It is an extraordinary example of how, starting from a mistake, a remorseless logician can end up in Bedlam' (Keynes, 1931: 394). The suggestion here is that, had Marshall not been congenitally averse to public controversy, he might, if confronted with Samuelson or Walrasian general equilibrium theorists, have replied using the same words. The mistake lies in not seeing that Marshall's goal was an applied economics relevant to a world characterised by heterogeneity and evolutionary processes.

2. Analysing evolutionary change in a heterogeneous world

Critics of Marshall's theory of value almost invariably criticise it from the standpoint of pure theory. Pure theory based on the logic of choice applied to situations that are sufficiently simple for it to be possible to optimization subject to constraints that can be deduced from the behaviour of other optimizing agents given very general assumptions about those individuals. Outcomes can then be analysed using the mathematics of differential calculus and simultaneous equations, or properties of convex sets. This was achieved in the 1930s by abandoning Marshall's concern with heterogeneity. This was almost the hallmark of the new value theory.

From this perspective, Marshall's assumptions did not make sense. Though he did not use the phrase, he assumed imperfect competition (that firms had their own special markets) but he spoke of supply price and analysed normal value in terms of supply and demand. As Marshall would have learned from Cournot (1838), equilibrium is characterized by supply and demand curves only when the number of competitors becomes infinite. He would also have understood the problems that the irreversibility of

increasing returns caused for analysing price in terms of static equilibrium analysis. Once firms had achieved economies of scale through expanding their output, these economies would not all be lost when their output fell, producing a ratchet-effect on the supply curve. As Robinson (1973:259) unkindly put it, Marshall worked out the analysis for forward movements with great clarity, but when it came to backward movements, he filled the book with tear gas, in the hope that no one would notice what was going on. As for the representative firm, from the perspective of equilibrium analysis, Robbins (1928) was right to argue that the concept of the representative firm, a key Marshallian concept, is redundant and therefore misleading.

Part of this was recognised, yet the defence of Marshall by the Marshallians was weak. Dennis Robertson sought to defend Marshall's notion of the representative firm against its numerous critics, arguing that it was a device for reconciling competition with increasing returns. Yet he failed to offer a methodological defence of Marshall, arguing about the relationship between the firm and the industry did not convince the critics. The clearest methodological remark was to say, "I am quite aware that the position cannot be cleared up mathematically: so was Marshall, and issued stringent injunctions against making the attempt" (Robertson 1930:89). However, he did not explain why this might be. A more elaborate defence, though not mentioning Marshall by name, came from Gerald Shove (1933:121).

> My questions have been framed to bring out what I conceive to be the fundamental dilemma presented in all attempts to treat these matters precisely. So long as we are content with a rough and ready indication of the forces at work, we can keep fairly near to the facts : but any attempt to make our treatment exact is apt to lead either to a degree of abstraction which renders the apparatus

> inapplicable to the actual phenomena we set out to explain or to a degree of complication which makes it cumbrous to use.

But this leaves open the possibility that if one can cope with a cumbrous theory, it may be possible to provide a formal analysis. There is no mention of the problems of time or evolution that undermine any way forward, at least without tools radically different from those available at the time. The evolutionary dimension of Marshall's thought had been pointed out, (e.g. Parsons 1931), but depth and pervasiveness of Marshall's commitment to evolutionary thinking was not revealed till much later. It was not until very recently that his early work psychology was used to illuminate his economics (Raffaelli 2003).

A satisfactory account of Marshall's theory of value should therefore have several features. It should be a theory that takes account of what Marshall considered central to his work – evolution. It should also take account, not simply of those elements of his theory that make sense to modern economists, but also of those that do not (such as his reliance on normal value, determined by supply and demand, at the same time as he made assumptions that were apparently inconsistent with this theory).

One starting point is his motto, 'Natural non facit saltum', to which he clearly attached great importance. In the context of evolution, this means that change is slow and, presumably, happening all the time, perhaps without our realising it. Evolution produces enormous changes, from the amoeba to the elephant, but without (so Marshall believed) discontinuous jumps. If nature does not make jumps, it equally cannot stand still. This immediately implies something about equilibrium: that even in the shortest period, things cannot be constant. They can be held constant only as an analytical device. The theorist who holds something constant is making an assumption that should

not be taken literally but merely as an approximation that is good enough for the purpose in hand. Evolution is also irreversible, which is not to say that it is progressive. Darwinian evolution is has no goal and is all the time leading up blind alleys. Without this, there would be no extinctions. So in the long run, not only is technology changing – not exogenously, as manna from heaven, but as a result of human economic activities – but so too is human character. That is no doubt why Robinson felt that the Marshallian long-run cuttlefish was too wily to get caught.

But how do we analyse such a system? Three methods come to mind. The first, not available to Marshall and only just coming available to us, is simulation. We accept that the world is a complex system, continually in disequilibrium, in all the senses of the term, and we simulate it. In such models there is no need, in principle, to make any assumption about equilibrium. Decisions can be made sequentially, in continuous time, with offers, counter offers, bargains being struck, conventions being established, and so forth. Agents need not be the optimizers of Walras or modern economics, though they might be the agents described by behavioural economics. However, apart from the practical problems, and the non-availability of suitable information technology, this route was not open to Marshall. In any case, to construct such a system we would have to know too much about the economy: perhaps not everything, but a lot. It is highly likely that, even today, we do not know enough about such a system to be able to explain what Marshall was interested in – emergent properties that might bear no relation to the properties of the system's component parts. Although Marshall did not talk in these terms (it is perhaps anachronistic to think that he could have done) it provides a useful reference point, for it is surely in such a system that Darwinian evolution applies. The question then, is how could Marshall analyse such a system, out

of equilibrium and continually evolving, using the means at his disposal?

A method Marshall could have used, but did not, is the Walrasian method of simplification whilst remaining at the level of the economy as a whole: general equilibrium analysis. There is no reason to believe that Marshall did not fully understand general equilibrium. He was a trained mathematician and knew more than enough to understand Leon Walras's somewhat primitive mathematics. He stated the equations of general equilibrium very succinctly in his mathematical appendix. It is far more likely that he realised that it was absurd as a way to analyse evolutionary change. How could there be a complete equilibrium when change is continually taking place and future production conditions and even future preferences are not known? The idea that general equilibrium theory could provide a representation of a complex, evolutionary system was absurd.

The method Marshall chose, and which in modern partial equilibrium analysis has been sanitized and separated from the evolutionary features that motivated it, is to analyse the economy one piece at a time and then fit the pieces together. This required him to adopt a different style of reasoning, going beyond 'pure theory' to a form of argument where the logical connections between parts of the theory are looser. His apparently *ad hoc*, pragmatic approach to defining markets and firms is a necessity. It is perhaps helpful to jump forward to modern genetics. Natural selection is generally argued to focus on genes, not organisms. But what is a gene? It is something that is sufficiently long lasting to serve as the subject of natural selection. Organisms are ephemeral and cannot be the subject of natural selection. Humans are distinguished from one another by something like 25,000 genes, enough for every human being to be genetically unique. Furthermore, one generation is far too short for natural selection to

operate. Genes are not immutable, but they last through enough generations for natural selection to be able to operate. There is no hard and fast rule – merely a pragmatic judgement, just like the judgements Marshall made. Marshall had no choice but to look at the system piece by piece. A corollary of this is that, because of the complexity of the system, these pieces cannot be fitted together so that the connections are logically rigorous.

3. A rational reconstruction of Marshall's method

The simplest way to relate models is to start with a complex model of the entire process being analysed. The long run solution is when everything is allowed to vary. This is the complete general model of which short run, and partial equilibrium models are simplifications. Different parts of the system are modelled by holding other parts constant. Hold all outputs constant except output in the market being considered, and what remains is a partial equilibrium model of a single market. Hold the capital stock and production methods constant, because it takes a long time to adjust, and what remains is the short run when firms can vary production not change the way they produce output. If production is also held constant, the result is the market period, during which firms can merely decide whether to sell or hold on to the stock of goods they have already produced. The process is exactly analogous to partial differentiation of a multi-variable system.

The point about this conception of the relationship between partial ane general equilibrium analysis is that it is, in principle, reversible. If the model is correctly and fully specified, the partial model can be derived from the general or vice versa, for each partial model is a sub-model of the complete model. However, this cannot be what Marshall was doing, for he was concerned with a complex world in which we cannot

grasp the complete model. A different method is required.

An alternative view is that Marshall was as trying to identify mechanisms that are operating in different parts of the system.[2] Models ares constructed to explain and identify a mechanism, such as the relationship between the quantity of a good offered on the market and the market price, making maximal use of *ceteris paribus* conditions. These *mechanisms,* not the models used to derive them, are then the building blocks for broader models. In the broader models, the mechanisms derived from the narrower models may be expressed in simplified terms, ignoring some factors in the narrower model. This is justified on the grounds that there is no reason to believe that the narrow model was correct in all its details. These broader models can, in turn, be used to generate mechanisms that form the basis of still broader models.

The implication of this view is that Marshall's market period, the short run and the long run should then be seen, not as special cases of a single model (that reaches equilibrium in the very long run) but as *distinct* models that cannot be expected to fit together into one logically consistent model. The narrower models justify components of the broader models but are not necessarily part of the broader models. These components fit together even if the models that are used to justify them contain elements that are inconsistent.

This procedure is common, if not universal in contemporary applied econometrics. The economist builds a theoretical model, and then deduces some results that need to be tested against data. However, in order to confront the model with data, simplifications have to be made – an empirical model has to be constructed, which is typically *not* a

2 "Mechanism" and "model" are not Marshallian terms. However, as this section is concerned with a rational reconstruction, such language is acceptable, the aim being to make sense of Marshall's method. Later sections will discuss its historical merits.

formal representation or simplification of the theoretical model. Functional forms have to be simplified; proxies are introduced for unobservable variables; variables thought inessential are omitted. These changes will get rid of details that are believed unimportant, perhaps because they are thought to result from simplifications that were made solely in order to isolate the mechanism of interest and to which the economist is not committed. It may also be because they are believed to be quantitatively unimportant. This process offends those who simply see the transition from theoretical model to the empirical model as lacking rigour but can be justified as a necessary practice when it is impossible to specify the full model (see Cartwright 2002:144-8 and Backhouse 2002:202-5).[3]

Thus what we have is a layered series of models that fit together but which cannot be derived using formal logic one from the other.[4] This may be what Marshall was doing and may account for some of the apparent inconsistencies in his work. Consider Marshall's market-period equilibrium. As De Vroey (200*) has shown, this relies on

3 On the inevitability of informal reasoning, see Backhouse 1998a.

4 See Backhouse 2007 for a more detailed discussion in the context of modern economics.

strong informational assumptions. However, it can be argued that Marshall is presuming that the market opens many times – the corn market and the fish market open day after day – with the result that traders learn. So when modelling the market, period the story could be a complicated dynamic story of trial and error, but when we move to the short run or long run we assume that there is equilibrium, ignoring many of the distributional and other changes that result from the trades that will have taken place out of equilibrium. Of course, as change is taking place, it is strictly inconsistent to assume market-period equilibrium in the short run or long run when real world markets will take time to achieve this (De Vroey 200*). However, there is a clear rationale for doing this: it provides an approximation without which the economist to move on. Maybe the approximation will fail and we need to consider market disequilibrium in the longer run, as macroeconomic theorists from Patinkin (1965) to Benassy (2002) have argued. But this is an empirical judgement, not a methodological principle.

One aspect of what is going on here is that Leijonhufvud (1968) referred to as the 'ranking' of adjustment speeds.[5] This did not simply mean ordering, but qualitative ranking. Partial equilibrium analysis – cutting the system up through assuming that parts are in equilibrium while other things are changing – is represented by assuming that one variable (market price) adjusts infinitely fast relative to another: market price in relation to output, or output in relation to the organisation of production, or the organisation of production to changes in human nature. Clearly, if we work with the models that theorists could handle, such relations would be difficult to formalise. The use of the term market 'day' should remind us that market-period activities are repeated many times. Not everything is the same from day to day, but there is enough in common for

5 He has since changed his view on the relation of Keynesian economics to Walrasian and Marshallian methods, but this does not affect the point made here.

patterns to occur and learning to occur. We cannot model this learning, except through crude approximations. Time is crucial. Time is also crucial for evolutionary change affecting human nature, given that jumps, Marshall assumes, do not take place.

In this methodology, partial equilibrium is not a restricted version of a more general model, but merely a useful tool – a short-cut that enables us to analyse problems that would otherwise be too difficult. Sometimes it will be a good approximation, sometimes a bad one. Marshall illustrates this brilliantly. Because he needs to cut up the economic system in so many ways, he resorts to many different conceptions of equilibrium.

4. Marshall on Methodology

The methodology outlines in Section 3 is a rational reconstruction of the methodology that might underlie Marshall's theory of value. What evidence is there that this corresponds to the way Marshall approached economics?

In the first edition of the *Principles*, Marshall opened his discussion of "Methods of study" (Book I, Chapter V) by claiming that Auguste Comte had pointed to a great truth, namely that all aspects of social life are closely connected (1890:72). However, though the economist is interested in individuals as members of the social organism, it is necessary to start with the individual "regarded as a member of some particular trade or industrial group" (1890:73). He then praised the Historical School, pointing out that its work involved not just the collection of facts but also economic theory. The reason he gave for this is interesting: "without the aid of careful reasoning, there is nothing to be learnt from economic facts, because no economic event or practical problem was ever exactly like any other" (1890:74). The conclusion Marshall drew was that "induction and deduction go hand in hand" (1890:76).

By the final edition of the *Principles*, Marshall was making the same case with

slightly more precision. being more precise. Induction is used to infer "general statements or laws"; deduction is used to derive "new and broader" generalizations, after which induction is used to "verify" the new laws (Eighth edition, Appendix C, 1949:644). This is the point at which Marshall argues that "there is no room in economics for long trains of deductive reasoning" (ibid.) Mathematical training is needed not so as to be able to solve complex models but because it provides a "terse and exact" language in which these short chains of reasoning can be expressed. These statements about the limitations of deductive reasoning were somewhat stronger than the claim made in the first edition, through a comparison between the production of economic knowledge and the production of goods:

> In the production of goods, when the same kind of thing has to be done over and over again in the same way, it generally pays to make a machine to do it. But when there is so much changing variety of detail that it is either impossible, or at all events unprofitable, to use machines for the goods, they must be made by hand. Similarly in knowledge, when there are any processes of ivestigation or reasoning in which the same kind of work has to be done over and over again in the same kind of way, then it is worth while to reduce the processes to system and to erect the machinery of science in order to deal with them. (1890:88)

However, after outlining the scope for the machinery of science in this way, he immediately emphasised its limitations:

> there is so much variety in economic problems, economic causes are intermingled with others in so many different ways, that exact scientific reasoning will seldom bring us all the way to the conclusion for which we are seeking. It sould be foolish

> on this account to rejects its aid so far as it will reach, but something must be left at the end to be done by practical instinct and trained common sense. (1890:88)

Thus in 1890, Marshall was emphasising the need for deduction, whereas by 1920 this battle was won. In contrast, by 1920 the intellectual climate was different and the need was to emphasise the limitations of "exact" reasoning. The short chains of reasoning he advocated could be seen as referring to theories about small parts of the system, identifying mechanisms or causes, which would then be used as the basis for further deductions (in another short train of reasoning). The need for "practical instinct" and "trained common sense" to link these chains of reasoning means that they cannot be combined together in a longer chain of reasoning. Furthermore, in these passages Marshall makes it clear that the reason why it is necessary to confine attention to short trains of reasoning is that the economic world is continually changing: it is an evolutionary process in which, when things are repeated, they are always repeated in a slightly different way. Judgement is involved at all stages from the initial classifying men into particular trades or industrial groups to the drawing of practical conclusions.

Marshall provides another account of his method in Appendix A of *Industry and Trade.* This is connected, via a footnote attached to its title, to a remark about the limitations of economic knowledge: as well as solving existing problems, economic study raises new problems, increasing the level of "conscious ignorance" (1927:7). Certainty arises only with respect to concrete facts and strict reasoning from axiomatic premises. Economics requires more than the enumeration of facts but is not amenable to purely formal reasoning. Economic problems are complex and inherently different from each other.

> They [sciences of mechanics, astronomy and inorganic matter] discovered at a

> comparatively early stage that complex problems must be broken up into elementary parts; for there is but little chance of finding a class of complex problems which resemble one another so closely that the same sort of systematic reasoning can be applied to all of them, and thus be made to throw light on the others. (1927:676-7)

In contrast,

> the opposite is true of the elementary parts into which complex problems can be broken up. Each such part of any one of them is likely to belong to the same class as a number of elementary parts of other complex problems; and by systematic study, general rules can often be discovered which apply, more or less nearly in the same way, to each member of the class. (1927:677)

The social sciences have learned from the natural sciences that they must select facts, arrange them into groups, and observe tendencies in them. A "general rule appropriate to them is developed; and the rule becomes more definite and also more elastic, as the fundamental laws of Nature which underlie it are more distinctly apprehended" (ibid.). These may be challenged by new observations but if these new observations can be shown to be the result of new conditions, the original tendency is confirmed (1927:678). After this, the next stage is to study the operation of this tendency "working in combination with other tendencies" (ibid.): "conclusions in regard to various elementary parts of complex problems are brought together, and worked up into broader partial truths" (1927: 677). By this method, "the most refractory problems" could be solved.

The first stage in reasoning is thus to establish tendencies. The role of observation in this process is clear in *Industry and Trade*, where "some general relations" are

established in Book I, Chapter II, immediately after the purpose of the book has been outlined. These relations are, to paraphrase: (1) trade of a place is likely to be enhanced by the presence of stronger industries and lessened by an advance of weaker industries; (2) the gain from foreign trade is the difference between the real value of goods obtained and those forgone; (3) countries can benefit from trade if they have different advantages, even if one is stronger than the other; (4) small countries often have much foreign trade; (5) reductions in transport costs increase trade more than proportionately.[6] Aside from the second, the status of which is ambiguous, these all involve causal relationships. Though the relative importance of theory and observation differs, they are all propositions that can be derived in the manner outlined above.

These methodological remarks do not prove that the reconstruction offered in Section 3 is a correct reconstruction of the way Marshall thought about his method of reasoning. However, they come very close to doing so, especially the remarks in *Industry and Trade.* Moreover, they suggest that, whilst there were changes in emphasis, hardly surprising in remarks written over a thirty year period, his underlying methodology was consistent. It is tempting to see it as an increasingly explicit articulation of a view in which economics involved a series of short chains of deductive reasoning, linked by practical instincts, in which more general models were based on mechanisms justified in more primitive models.[7]

5. Conclusion: Marshall as an applied economist

To equate economics with economic theory was, Marshall argued, a "calamitous

6 These are based on the first five section headings of Book I, Chapter II.

7 Possibly insert a new section exploring the details of Marshall's value theory in the light of these remarks.

mistake": theory on its own could even be a waste of time (Whitaker 1996:393). This suggests that the needs of the general reader may have provided Marshall with a reason for what he would have wished to do anyway: place his pure theory in a Mathematical Appendix with some of the more accessible pieces of theory in footnotes.[8] On this reading it may be better to consider the text, with its discussions about how real-world markets work, as applied economics. The book, after all, was entitled *The Principles of Economics*, not economic theory. If this is correct, it may follow that the *Principles* has more in common with *Industry and Trade* than has sometimes been thought. It would certainly make sense of his description of himself as, methodologically, mid-way between Neville Keynes and Gustav Schmoller.

The difference between pure theory and applied economics is that, though the latter involves deductive reasoning, it has to keep much closer to the world. Otherwise, the economist will erect a structure that will be useless as a guide to action. Keeping closer to the world involves taking account of change, allowing both for the fact that processes take place at different speeds and for the fact that the economic system is continually evolving. The complexity of such a system explains why chains of reasoning had to be kept short: why a pure theory of the entire process could not be developed. In short, the difference between pure theory and applied work is that, in the former, short chains of reasoning can be forged together to obtain a longer chain, whereas in the latter, they are fastened together with less formal reasoning.

Though the learning of routines was clearly very important to Marshall from an early age, even before he became an economist (Raffaelli 2003), it was only in *Industry and Trade* that he integrated it into a formal statement about the role of pure theory.[9] Pure

8 This is not to imply that all the diagrams constituted pure theory.

9 It is conceivable that Appendix A was a part of *Industry and Trade* that was written much earlier, but

theory was applicable only to situations that were repeated over and over again. Had the real world been like this, as Shackle contended had been the case before 1914 (he called an "age of tranquillity")[10], it might have been possible to take pure theory further – to construct longer chains of reasoning – though given that even physical sciences had to break up problems into elementary parts, probably not very much further.

One reason why this Marshall's purpose may have been less clear to contemporaries and to his successors in the early decades of the twentieth century is that, at that time, the distinction between economic theory and applied economics barely existed (Backhouse 1998b). Discussion of the issues involved was not helped by the distinction, fostered by Neville Keynes, between the "science" and the "art" of political economy. This captured part of what was significant about the pure-applied distinction but conflated it with normative and policy issues, thereby blurring its methodological implications. Marshall's *Principles* unquestionably dealt with the science rather than the art of political economy, so if the exercise of judgement was associated with art, that left the *Principles* on the side where rigorous reasoning might be expected. Marshall's point, consistently expressed, was that judgement, and informal reasoning, was an integral part of economic science. Neither did Marshall help himself in described Book V as the only part of the *Principles* that could be called "theory".[11] Though this may have been true, it might have more helpful if he had acknowledged that it contained more than "pure" pure theory, for the many short chains of reasoning it described were

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this seems unlikely. It contained methodological views that would not have been out of place in the *Principles*, and if written 15 years earlier it would have been natural to use it there.

10 Though this was clearly an apt description in comparison with the inter-war period, it is tempting to see this as a statement of what needed to be true if the period's pure theory was to make sense.

11 See Marshall 1961, II:62.

not forged into a single long chain. He was not offering a model in the modern sense.

In the 1920s, for reasons that go far beyond the scope of this paper, economists who favoured a more rigorous, technical approach to economics were in the ascendant. Pure theory became more prominent and Marshall was increasingly judged in terms of his contributions to pure theory, that branch of the subject that he disparaged. Those of his successors, such as Shove, who could see that he had been doing something different, failed to convince their more rigorously inclined colleagues, such as Sraffa and Robinson, that in abandoning Marshal's imprecision, they were losing something important.

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