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SYMPOSIUM ON *MARSHALL'S TENDENCIES*: 6 *MARSHALL'S TENDENCIES*: A REPLY[1]

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In her opening contribution to this symposium, Mary Morgan has provided a critical evaluation of Marshall's Tendencies in which she reviews a series of methodological issues. She characterizes my views quite accurately, while pinpointing the gaps in my account (most notably in relation to pinning down what is meant by a 'mechanism'). I am therefore going to leave this aspect of things on one side, and turn to other matters.[2] In order to focus attention on those issues which are relevant to my main theme, I would like to begin by pulling out the book's main argument in a series of assertions:

> THE POINT OF DEPARTURE. In many situations that economists set out to analyse, a number of factors that exert large and systematic influences on outcomes may be difficult to identify, let alone to measure, proxy or control

[1] I would like to take the opportunity to draw attention to two misprints in Marshall's Tendencies (which have been corrected in the current reprinting). On page 80, final paragraph, line 3: for 'profit' read 'gross profit'. On page 97, line 18, for 'is offset by' read 'lies in'.

[2] There is, however, one point of terminology on which I would like to remark. Professor Renault dislikes the use of the term 'true model'; as a matter of general usage, I share his dislike, and favour the econometrician's language of a 'preferred specification'. My reason for using the phrase 'true model' in Marshall's Tendencies is that my focus of interest lies in the standard paradigm, as interpreted by reference to the analogy of the tides. Here, the idea is that the data we observe is driven by some deterministic model which incorporates all large and systematic influences on outcomes, together with a small random noise term – and that we can uncover this underlying model from the data. It is relative to a discussion of this story and the inadequacies of this story, that I find it appropriate to speak of attempts to identify the 'true model'.

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> for. This may happen because some factors are intrinsically hard to observe (the beliefs of agents, say), others are lost in the mists of history (the various patterns of entry to different industries within a dataset, say), while yet others may come into play only in a sporadic fashion (as, for example, in the case of shifts in coalition structure within the OPEC cartel).
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> The presence of such unobservables can make the problem of 'model selection' a hazardous one; and since most tests on theories are carried out jointly with some 'model selection exercise', this may pose a major problem in regard to testing economic theories.[3]

This statement, as it stands, is pretty uncontroversial. The question is, how serious is this problem? Here, views differ widely. At one end of the spectrum, there are those who think this is a minor issue. On this view, if our exploration of the dataset leads us to the realization that some unanticipated factor is playing a role, then we can fix the problem. If the role of this new factor is sporadic, then it is labelled as being 'outside the model', and its influence is allowed for *ex post* by sticking in a dummy variable. If the new factor is one which exerts a continuing influence on outcomes, but which cannot be measured, we can infer its role by applying standard techniques. To someone of this view, the answer lies in extending the standard regression analysis setup by allowing for the presence of 'latent variables'. This is the viewpoint advocated by Professor Renault in his comment. To Professor Fisher, on the other hand, the problem is relatively manageable in some areas but may be deeply problematic in others. At the opposite end of the spectrum, we find the extreme pessimists, represented by Keynes and Hayek. Here, the view is that the problem posed by unobservables is not

[3] In making this point, I note that most economists, when asked to cite empirically successful theories or models, tend to come up with two examples: option pricing and auctions. I point out in Chapter 2 that in these two areas, the model selection problem is much less severe than in most of the areas we analyse. I also emphasize that the model selection problem still remains in these areas, and limits what we can do (see pages 45–6 and page 57; all page references relate to Sutton (2000)). Professor Renault argues at length that the model selection problem is very serious in these two areas. The difference between our positions on this point lies in a difference of emphasis; indeed, his comment on auctions is closely similar to my own statement on page 57. As to options, I have drawn attention in Chapter 2 to the ongoing disagreements as to how we should model the underlying movement of stock prices; what Professor Renault emphasizes is that these differences of view bring us beyond a framework in which there is some fixed 'volatility' parameter. More controversially, he argues that 'the success of the Black–Scholes theory is not based on its correspondence with truth but rather on its ability to support economic decisions . . . No bank would sell an option without software to hedge it'. This raises the question of whether the popularity of the Black–Scholes model may nowadays affect the way in which options are priced, and it is for this reason that I chose to focus in Chapter 2 on illustrating the empirical success of Bachelier's model of 1900.

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amenable to any such easy fix, and that the standard econometric research programme is fatally damaged by such difficulties.

Now what I have been setting out is an intermediate view: I think this is a serious problem, but I do not believe that this implies that we should give up on our conventional strategies. I do however believe that it is worth being open-minded and eclectic in using approaches that go beyond the standard paradigm when trying to handle such issues. I can sum up my view in the form of two implications:

> The first implication. If my concerns about unobservables are justified, then it is particularly important to begin any analysis by considering an appropriately wide set of 'candidate models', and placing heavy emphasis on the idea that both any estimated parameters, and any tests of theories (carried out in the usual setting of a model selection exercise) should be taken seriously only if the results are robust, that is, the conclusions do not rest upon some arbitrary choice of one model specification as against another, where neither specification can be rejected by reference to the data.

Again, this statement, as it stands, will be seen by most economists as uncontroversial. There will, however, be a big difference in emphasis, depending on where one stands in relation to the spectrum of views described earlier. Those at the first end of the spectrum will respond that all is well; we are accustomed to insist on such robustness. Those at the other extreme will claim that the standard I have just suggested is desirable, but unattainable.[4]

> The second implication. So if unobservables pose a serious problem, how can we find some constructive way forward? My claim is this: rather than work with a fully specified model of the classical kind, it may in some (rather special) circumstances be more fruitful to begin with a 'class of models' approach. Such an approach involves a search for such empirically observable implications as follow for all models which share some common features; in other words, we aim to handle the unobservables by designing the theory in such a way as to allow us to work round them. One particular way of implementing a 'class of models' approach is represented by the 'bounds approach' set out in Chapter 3; here, the idea is to characterize the space of (observable) market outcomes, within which any equilibrium outcome of any of the models in our specified class must lie.

[4] My position may to some degree reflect the fact that my field is industrial organization, where it is common practice to begin by proposing some quite particular oligopoly model, continue by estimating parameters within this model, and then draw policy conclusions from these estimated parameters. My point is that such estimates are only as good as the set of (unstated) assumptions implicit in the original choice of model specification. Unless alternative models of equal prior plausibility are estimated, we are not in a position to draw any confident conclusions as to the way in which a change in some exogenous ('policy') variable will affect outcomes.

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Let me begin by stating at once that, *pace* Professor Renault, I do not claim that this is a good approach in general; rather I emphasize the very special character of the 'market structure' problem which makes it possible to implement this line of attack by specifying a bound in the space of observable outcomes, (see my discussion on page 85). In the same spirit, I do not believe, *pace* Professor Renault, that this particular way of relaxing the standard paradigm is the only way, or even the best way, to do the job. I am quite open to arguments in favour of any constructive approach, including the one which Professor Renault himself advocates in his contribution. What I am insisting upon is that it is important to be open to any constructive way forward that can help us deal with the problems posed by unobservables: in different contexts, different methods will be appropriate. It is exactly at this point that we arrive at the first of my three targets.

> THE FIRST TARGET. What views am I arguing against? My first target is the one I have heard expressed most frequently when proposing the bounds approach to market structure; it runs as follows: 'the only ''proper'' kind of model is a fully specified model of the classical kind'.

This, from a scientific viewpoint, is a rather curious view. The fact that it is widely held among economists is a rather striking illustration of just how tight a grip the standard paradigm has obtained over the past fifty years. Of course, it should be said at once that not all economists feel like this. Rather, there is a broad range of views, running from this very narrow orthodoxy at one end, to the extreme openness exemplified by Professor Christ at the other. To Professor Christ, the 'class of models' approach does not even lie outside the standard paradigm. He likes to think of the class of models as being a kind of supermodel within which we embed all the constituent models. Now this is almost true; it would be exactly true if we could reduce all the differences between models within the set to some unobserved parameters, so that we could move across the models by simply shifting the values of these unobserved parameters. Now this is indeed sometimes possible, though in the market structure examples I consider in Chapter 3, for instance, it is not practicable to do this. The two unobservables that matter in these problems are the form of price competition and the nature of the entry process. For the former, it is indeed possible to find a suitable parameterization which brings us from Bertrand competition at one end, to joint profit maximization at the other (Symeonides, 2001), but there is no way of classifying, ordering or ranking the huge variety of entry models that we might consider, so this strategy just does not work here. The only way to handle things is by formulating the theory in terms of a set of constraints that must be satisfied by any equilibrium of any model of our class. All in all, notwithstanding these difficulties, I am very

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sympathetic to Professor Christ's interpretation, and if all economists shared his view, I would have little to argue about.

My problem lies with those economists who occupy the opposite end of this spectrum. Here the usual line of criticism, which is stated by Professor Renault, goes like this: the partial constraints placed on the space of outcomes by reference to a class of models are not tight enough to allow us to answer many of the questions we may wish to ask about the possible impact of exogenous variables on outcomes. This view appears to be held by many economists, and I believe it is seriously misplaced for two reasons: First, it misses the central point about the bounds approach, which is that this approach is not intended as a rival alternative to the programme of looking for a complete model of the classical kind. Rather, it is complementary to such an approach. The best way to see the complementarity between the two approaches is to think in terms of a hierarchy of assumptions. We begin with a couple of assumptions which we can expect to hold good across the general run of industries. Using these assumptions alone, we arrive at some limited restrictions on the space of outcomes. These restrictions can be tested directly by reference to a broad cross-section of industries. This is the 'bounds approach'. We now move to the next set of assumptions. Here, we are dealing with assumptions which would be valid for some industries, or sets of industries, but not others. By adding these assumptions to our earlier set, we may hope to narrow the set of outcomes further, but in the process we also have to narrow the domain of application of our model. In principle, we could keep adding assumptions until we had a fully specified model of the classical kind which was appropriate to some specific industry which we wish to analyse. This brings us to the 'single industry approach' which became popular in the industrial organization area from the early 1980s onwards, and which is nowadays often described by the label 'structural estimation'. Now my point of view on this, which is spelt out on page 85, is that it is sometimes possible to go beyond the bounds approach in the direction I have indicated, but the trade-off between getting tighter predictions while narrowing the domain of application may be a pretty unattractive one. By the same token, it is possible to begin at the other end of the spectrum by synthesizing the body of single industry studies as they accumulate, with a view to exploring whether some further assumptions might be valid for some interestingly wide set of industries. In this way, we might hope to widen the domain of applications, at the cost of some loss in the precision of predictions.[5]

Apart from this issue of complementarity, there is a second reason

[5] This is the route that Professor Renault indicates as a way of finding a bridge between the bounds approach and the latent variable approach – but see footnote 6 below.

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why I feel that this kind of criticism is misplaced. The central point that gets overlooked in abstract discussions of this point is that the characterization offered by the bounds approach to market structure is sufficient to generate testable predictions which provide clear evidence that certain basic competitive mechanisms are in operation across the general run of industries. These mechanisms delimit the set of market outcomes in a particular way, and are robust enough to override the influence of the many omitted factors that vary from one industry to another: it is precisely because they show this robustness that these mechanisms are of practical interest from a policy standpoint. For it is only very robust mechanisms of this kind that we can count on to come into play when we are trying to forecast the likely impact of exogenous changes to the general run of markets as a result of some future changes in the external environment. If, for example, we want to ask about the impact of globalization on industrial development it is crucial to try to isolate those few strong competitive mechanisms that are going to play a strong and systematic role across the general run of industries (for a discussion of this point, see for example Sutton, 2001).

I suspect that one reason for the disagreement on this issue is that some economists, when they think of policy issues, tend to think in terms of a policy specific to a single market, and they have in mind their wish to know the parameter values of some fully specified model of that market. However, most economic policies are designed to provide an environment within which a whole run of quite disparate industries will operate: when thinking about this latter context the issue of robustness is central, and the focus should be on trying to isolate those few competitive mechanisms that are likely to operate in a systematic and predictable way across the general run of industries.

> TWO MORE TARGETS: THE PESSIMISTS. My two remaining targets are related, in that both have emerged in the wake of disappointments in respect of what was taken for granted as the proper goal of economic research in the 1960s: the development of a set of theoretical models, which would rest on a small number of well-motivated assumptions, and which would place clear and testable restrictions on the space of observable outcomes. Among some (but by no means all) theorists, this pessimism manifests itself in a retreat to the position espoused by Robbins. Among applied economists the same pessimism manifests itself in the view that all we can expect from theory is a catalogue of candidate models which we can use as a framework within which we can conduct some model selection exercises.

The position taken up by Robbins was that we can hope, on the basis of introspection, to come up with a set of assumptions on which we can build a theory which we can hope to apply with confidence, even though it has not been directly tested. Here, my claim is simple: no theory

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should be taken seriously until it has been developed to the point where it yields a set of clear testable implications, and until the empirical success of its predictions has been confirmed by independent researchers. Any slipping back from this standard leaves us with a body of analysis founded on a priori judgements – and so on nothing more than an argument from authority.

As to the 'theory as a framework' view, I would hope that, whether we start with some a priori assumptions, or develop a theory following some toing and froing between modelling and data fitting, we could arrive at a body of theory whose assumptions are well justified, at least as first order approximations, and whose content lies in a set of restrictions on the space of observable outcomes. This is certainly possible in some areas of the subject, and perhaps in many. If theorists can do no more than provide a framework for parameter estimation, then economic theory is a poor kind of thing.[6]

So is my view optimistic or pessimistic? It is clear from Professor Hoover's comments that he wants very much to classify me somewhere on this spectrum (and probably as a rather confused pessimist). In fact, my stance is a cautious and qualified optimism: I believe that the only target worth aiming for is a body of theory that is well-founded empirically; I believe that this is a hard objective to achieve, but that at least in some areas of the subject it is achievable. In aiming at such an objective, we need to be open-minded and eclectic in respect of research methods, and we need to be ready to abandon a priori views as to the putative importance of any mechanism, where the evidence points to its

[6] It is at this point, I suspect, that the real difference between my position and Professor Renault's lies. The bounds approach to market structure, and the latent variable approach favoured by Professor Renault, are – as we both remark – essentially equivalent 'ways out' of Edgeworth's problem of 'indeterminacy'. The key issue is this: by accounting for observed outcomes by reference to latent variables, we might in principle allow ourselves enough leeway to reconcile any set of observations with a preferred underlying theory. This tension is particularly evident in the rational expectations macroeconomics literature, which I discuss at length in Chapter 4. Professor Renault's closing remarks suggest that, in this debate, he is sympathetic to the school of thought which favours imposing rational expectations as a maintained assumption. In Chapter 4, I argue for the importance of having competing schools of thought, so that the R.E. programme *per se* remains open to challenge.

It may be helpful, in the light of this point, to return to the 'latent variable' interpretation of the bounds approach to market structure (footnote 5). The key feature of this approach is that it begins by confining the 'unobservables' incorporated in the theory (and so the candidate 'latent variables') to two variables that are known to be both important and notoriously hard to measure, proxy or control for, viz., the entry process and the form of price competition. We can then find a bound, relative to which the effects of our unobservables all operate in the same direction. This is the key to retaining a testable theory: we can reject the theory if we can show that the restrictions imposed by the properties of these bounds are violated.

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irrelevance. We are all too susceptible, as a profession, to the lure of a beautiful theory; but nothing better characterizes a scientific discipline than the abandonment of a beautiful theory in the face of an ugly fact.

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