Economics and Philosophy, **18** (2002) 29–44 Copyright © Cambridge University Press

SYMPOSIUM ON *MARSHALL'S TENDENCIES*: 4 COMMENTS ON *MARSHALL'S TENDENCIES*

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Professor Sutton opens his lively monograph on the nature of economic theory with the following question: is it possible to find economic models that work? He uses the question to guide us on a methodological tour with Marshall's characterization of economic theory as the point of departure. I must say I enjoyed the trip. Along the way, the animating issue of what works in economics could hardly have been addressed without dealing with issues in verification, and the author's arguments include an appraisal of what he considers as standard econometric methods. In these comments, I will revisit at some length one of the key sites on John Sutton's tour bringing along a view of modern econometrics which is somewhat different from his and which affords a different perspective on the Marshallian paradigm.

In my opinion, many econometricians today are not asking the question posed above but rather the more modest version 'is it possible to find economic models that work better in some dimensions'? The additional terms are crucial because they suggest that the assessment is only a relative one: the only goal of the econometrician is to find models that work better than others while keeping in mind that the improvement is not uniform but only with respect to some particular classes of

Acknowledgements: I am grateful to Marc Fleurbaey and Philippe Mongin for very helpful comments and suggestions. Special thanks are due to Bryan Campbell for his invaluable advice. He suggested many improvements on a first draft of this text. Of course, I assume full responsibility for any remaining errors or debatable positions.

https://doi.org/10.1017/S0266267102001050 Published online by Cambridge University Press

applications. In other words, I entertain a view of empirical economics which is, in some respects, even more pessimistic than Sutton's. For instance, I will argue that one of the models that is presented as an example of 'models that work' does not work in reality. Even so, I am also going to claim that one often needs, for practical applications, much more precise statements than the 'looser framework' termed the 'bounds approach' which is put forward as a modification of sorts of Marshall's paradigm in the latter part of the monograph. Moreover, my pessimism regarding the empirical success of economic models is not at odds with the insistence that these allegedly wrong models should generate empirically precise statements. What is important to my mind is encapsulated in the famous conclusion of Keynes's General Theory: 'the ideas of economists and political philosophers . . . are more powerful than is commonly understood. In fact, the world is ruled by little else'. In other words, the issue is not a psychological one like the human taste for explanations but is rather very pragmatic in nature: people do things with economic theories and these theories cannot be valuably appraised without reference to their use.

My comments will ultimately follow the organization of Sutton's book. But first, by way of a framework for subsequent discussion, I am going to offer more detailed comments on the Black–Scholes model and its extensions than is provided in the book. This review will serve initially to support the author's conclusion in Chapter 1 of *Marshall's tendencies* (MT hereafter) that 'the early concerns voiced by . . . Keynes and Hayek . . . were not misplaced'(MT p. 32). However, it will enable me to take strong issue with the claims in Chapter 2 that there are 'rare and happy circumstances in which the problem of model selection virtually disappears' because we would 'know – to a good order of approximation, at least – what the true model is' (MT p. 33). Here the focus is very much misdirected. In my opinion, the relevant concern is not the search for the 'true model', but is much more related to the idea of 'common models' put forward by the rational expectations literature. Moreover, I will try to indicate that this literature may provide some paradigms which, while also arriving at the 'class-of-models approach', are much more productive from a decision-theoretic point of view than the bound approach solution proposed in Chapter 3.

1. BLACK–SCHOLES AND BEYOND

As stressed in Chapter 2, one can trace the use of continuous time models in finance back to Bachelier (1900).[1] However, I must take issue

[1] Bachelier's Ph.D. thesis (1900) cited in MT (p. 35), was translated into English and published in Cootner (1964). Reference to the latter work appears in Sutton's bibliography under Mandelbrot (1964).

https://doi.org/10.1017/S0266267102001050 Published online by Cambridge University Press

with the book's assertion that the widespread adoption of continuous time models in asset pricing proves that 'we are dealing with a situation in which the true model – the underlying model of stock price movements – is known to a degree of precision adequate for the purpose at hand' because 'we have reasonably closed agreement of an underlying true model that is known both to the agents in the market and to the economist whose concern lies in testing the theory' (MT pp. 46–7).

Actually, these continuous time asset pricing models have opened up many new problems for econometricians. In an invited lecture presented at the sixth World Congress of the Econometric Society (Barcelona, 1990), A. Melino stressed that, as far as estimating these continuous time models is concerned, not only is there not a 'closed agreement of an underlying true model', but one could 'argue that they have been widely adopted not because of their empirical properties but in spite of them' (Melino, 1994, p. 313). In my own invited lecture on option pricing given five years later (Tokyo, 1995), I added that 'the contradiction between perfect Black–Scholes pricing and statistical inference is even clearer for testing' than for estimation (Renault, 1997, p. 230).

The crucial issue underlying our concerns is well summarized by Bossaerts and Hillion (1993): 'It suffices to back out parameter values of a limited set of option prices, and to find an additional option price which does not exactly match the corresponding theoretical price at these parameter values' to get 'empirically untenable results'. In other words, it is amazingly myopic to 'use the observed price of one option to infer a value of σ' (MT p. 43) (the only unknown parameter in the Black-Scholes option pricing formula) and then to 'use this to predict the values of the other options' (MT p.45) without even noticing that some other options prices data on the same underlying asset (say, a stock) have already been observed and correspond to different values of σ. Moreover, one cannot refer to a scientific view of falsifiable scientific theories to propose another 'procedure . . . to combine information on all options to obtain a 'best estimate' of σ'(MT, p. 45, fn. 7) while, if the Black–Scholes model were correct, it would suffice to back the parameter value of one option price to get not only the best estimate, but the true value of the volatility parameter σ.

I am saying that not only does standard statistical theory not allow one to use the term 'best estimate' in this context, but that a sentence like 'the only parameter that needs to be estimated empirically is the volatility parameter' (MT p. 46) does not make sense as long as one does not provide a definition of this parameter different than the one given by the Black–Scholes model. Of course, we know that practitioners usually compute the so-called 'implied volatility' parameter σ_{imp} by looking for the value of σ which matches the observed option price with its theoretical Black–Scholes value. This is the best known example of the aforementioned practice to 'back out parameter values of a limited set of option prices'. But the volatility measurement (the so-called implied volatility σ_{imp}) obtained in this way has nothing to do with the parameter σ of the Black–Scholes pricing model since any 'limited set of option prices' (actually more than one written on the same stock) shows that the latter does not exist (as a given parameter which is supposed to characterize the volatility of the underlying asset return and, therefore, should be the same for all options written on the same underlying asset).

However, one can use a given one-to-one function of the option price, like the implied volatility, as a unit of measure, without postulating any particular option pricing model. In the same way, a set of future cash-flows which are expected at different dates is often assessed through its 'internal rate of return' without any empirically untenable assumption like a deterministic and flat term structure of interest rates.

Perhaps the usual terminology 'implied volatility' is misleading, since it leads one to believe that it is logically implied by the Black–Scholes model. Like D. Bates one may regret that 'implicit volatility' is 'also commonly if ungrammatically called the implied volatility'(Bates, 1996, p. 572). Whatever the terminology, it first remains to *define* a notion of volatility which could be measured or estimated by this implicit quantity, and, second, to *explain* why 'debates are directed not at questioning the correctness of the theory, but on the fine-tuning of the underlying description of the true model' (MT p. 47).

If it is true that 'studies that use such indirect methods to infer a value of σ generally achieve somewhat better predictions of option prices' (MT p. 45), it only means that the implied volatility parameter has an informational content about the value of some future 'state variables'. These are the variables that are truly relevant to the description of the conditioning information used by agents in the market to determine their offer or demand of option contracts. In other words, the option market equilibrium reveals some information that was not available on the spot market. Accordingly, the relevant state of the economy should be described not only by the current value of the stock price, but also by some other latent stochastic processes, the first of which is a volatility process. Even though I am going to argue that the volatility process is not the only relevant state-variable process, it is often considered as the primary one precisely because the option traders see the option market as a market where volatility is traded. It is for this reason that their unit of measure for option pricing is not the price process itself but the implied volatility process.

The immediate consequence of this approach is that volatility is now seen as a stochastic process that has nothing to do with the fixed number σ defined in the Black–Scholes world. However, the definition of this process is not new but was initially inspired by the random walk paradigm put forward by Bachelier (1900). Bachelier's idea was to introduce a process with independent increments to capture the idea that 'the expected price tomorrow must coincide with today's price' (MT p. 36), a position which we now realize neglects not only the interest rate but also the risk premium.

In other words:

$$E[S_{t+1} / I(t)] = S_t, \tag{C}$$

where I(t) denotes the relevant conditioning information at time t to forecast the value S_{t+1} of the stock price at time $(t+1)$. But the condition (C) is consistent with several different models of the stock price process (for a given information set I(t)).

Model 1 (Bachelier, 1900) Arithmetic Random Walk.

$$S_{t+1} = S_t + \sigma\, u_{t+1} \tag{C1}$$

where (u_t) is Gaussian white noise with zero mean and unit variance. Therefore, the stock price process has independent stationary increments $(S_{t+h} - S_t)$, the normality of which is simply a consequence of the central limit theorem, insofar as the two following assumptions are maintained: the above price increments have a zero mean, a finite variance and the model is valid for any time interval h. Bachelier was right to conclude in this setting that price increments could be described by a normal distribution whose standard deviation increased proportionally with the square root of the time interval h. In continuous time, the approach yields arithmetic Brownian Motion: $dS_t = \sigma dW_t$, where W_t is a standard Wiener process.

But Bachelier probably did not realize the considerable variety of other models that are consistent with (C).

Model 2 (Black–Scholes,1973)[2] Geometric Random Walk.

$$S_{t+1} = S_t + \sigma\, S_t\, u_{t+1} \tag{C2}$$

where (u_t) is Gaussian white noise with zero mean and unit variance. While (C1) and (C2) are both inspired by and consistent with the condition (C), (C1) maintains a stationarity assumption about the absolute variation $(S_{t+1} - S_t)$ of prices while (C2) corresponds to the more realistic assumption of stationary returns $(S_{t+1}) / S_t)$. In this case, the parameter σ is called the volatility of the return. If one wants to justify the normality assumption by a central limit theorem applied with arbitrarily small time intervals of length h, it is more convenient to

[2] See citation in MT.

https://doi.org/10.1017/S0266267102001050 Published online by Cambridge University Press

consider continuously compounded rate of returns Log $[S_{t+h} / S_t]$, which leads to a slight modification of the model (C2):

$$\text{Log}\,[S_{t+1} / S_t] = \sigma\, u_{t+1}. \tag{C2'}$$

Of course, (C2) and (C2') are almost equivalent for small rates of returns and provide the intuitive foundations of continuous-time geometric Brownian Motion:

$$\text{dLog}\,[S_t] = \sigma\, dW_t, \text{ where } W_t \text{ is a standard Wiener process.}$$

Model 3 (Mandelbrot, 1964)[3] Pareto–Levy distribution.

$$\text{Log}\,[S_{t+1} / S_t] = \sigma\, u_{t+1} \tag{C3}$$

Here u_t is viewed as a white noise with possibly an infinite variance and the scale parameter σ loses its interpretation in terms of standard error. Actually, it is true that 'others, following Mandelbrot, argue that the selection of the particular solution to Bachelier's equation is justified only by arbitrarily imposing the *ad hoc* restriction of finite mean and variance. If we set aside this restriction, we admit a broader set of solutions, and the normal distribution is replaced by the family of stable distributions, and in particular by the Pareto–Levy distribution, which has fatter tails than the normal' (MT p. 46). In the modern setting of (C3), the lognormal probability distribution of returns will then be replaced by a log-stable one which is the only consistent one involving the addition of log-returns on any frequency. However, whatever the 'recent developments' of this model that can be found in Mandelbrot (1997),[4] I am afraid that this model is not often considered by option traders as an 'economic model that works', for the simple reason that such a relaxation of the Black–Scholes model throws out the baby with the bath water. And the baby in this instance is particularly cute! So cute that 'debates are directed not at questioning the correctness of the theory, but on the fine-tuning of the underlying description' (MT p. 47) of the world *à la* Black–Scholes.

In my opinion, this response to Black–Scholes can only be understood through the realization 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; in particular, in suggesting to banks how to hedge options contracts written for its customers. No bank would sell an option without software to hedge it. The main argument in favor of the Black–Scholes model rests upon the existence of a hedging strategy that is theoretically perfect in continuous time, the so-called delta-hedging strategy. In this respect, the important contribution of the Black–Scholes

[3] See citation in MT.

[4] See citation in MT.

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(1973) paper was not the pricing formula itself but the suggestive logic of its proof. It is shown that the option price can be duplicated perfectly by a portfolio of bond and stock, the composition of which is determined by the value of the delta coefficient, that is, the partial derivative of the option price with respect to the underlying stock price. In short, perfect duplication allows perfect hedging.

As McCulloch explained so well, the crucial problem is that this 'logic of the Black–Scholes model cannot be adapted to the log-stable case, because of the discontinuities in the time path of an α-stable Levy process' (McCulloch, 1996, p. 405). Actually, unlike the Brownian Motion, which is almost surely (a.s.) everywhere continuous, an α-stable Levy Motion is a.s. dense with discontinuities, and each jump of this dense set leaves a delta-hedged position imperfectly hedged.

Model 4 (Clark, 1973) Mixture of normal distributions.

$$\text{Log}\,[S_{t+1} / S_t] = \sigma_t\, u_{t+1}, \tag{C4}$$

where as in model 2, (u_t) is a Gaussian white noise with zero mean and unit variance, but (σ_t) is another square integrable white noise belonging to the information set I(t) available to the agents on the market at time t. While (σ_t) is observed by the agents (and therefore the Bachelier condition (C) is fulfilled), it is not necessarily the case for the econometrician who is led to conclude that the distribution of returns 'has fatter tails than the normal' only because he does not observe the mixture component. In other words, the well documented fat tail phenomenon can be captured with a much more user-friendly model than the α-stable Levy model.

The success story of the mixture model in the modern asset pricing literature, jointly with the relative failure of the α-stable Levy model, is in my opinion of great interest to the proposed agenda in Sutton's lecture. It shows why it is that certain models work: not because they are true, but because 'the world is ruled by little else' until new more powerful ideas emerge. The problem at hand was to plug the Black–Scholes model in a more general class in order to address the issue of some empirically untenable results. While the class of Levy models was underpinned by purely statistical theories (stability without normality implies infinite variance), the class-of-mixtures models focus on the quantity of interest for the option trader, namely the volatility parameter whose existence is denied within the α-stable Levy class. In other words, the empirical evidence of interest is not one documented by the statistician in his ivory tower, but one faced by the option traders on a day-to-day basis: the need to change the value of the volatility parameter which is then used to compute delta-hedge ratios.

I am now going to argue that this idea of parameter-driven models, stemming from a useful conceptual division of models proposed by Cox (1981) (see also, Shephard, 1996), is a modern way to deal with Edgeworth's objection to Marshall's view of economic theory. As presented in Chapter 1, we should: '. . . imagine that there exists some supermodel that embodies all the particular models . . . it contains additional explanatory variables – which index the different constituent models that it encompasses – but these additional variables are not ones we can measure, proxy or control for in practice (the 'unobservability' view)' (MT p. 8). My claim is not that the 'latent state variables' approach is the only way to relax the standard paradigm. But it turns out that, in the field of option pricing, various attempts to propose a bounds approach via various concepts of stochastic dominance or transaction costs have not been very successful until now. On the contrary, the benchmark option pricing model is nowadays a stochastic volatility model where the volatility parameter is indexed by some probability distribution and is even endowed with the dynamic properties of a given stochastic process.

Model 5 (Taylor, 1986 and Hull–White, 1987) Stochastic Volatility Model.

$$\mathrm{Log}\,[S_{t+1}\,/\,S_t] = \sigma_t\, u_{t+1}, \tag{C5}$$

where as in model 4, (u_t) is a Gaussian white noise with zero mean and unit variance, but (σ_t) is no longer assumed to be a white noise but more generally a Markov process endowed with some autoregressive dynamics, as for instance lognormal ones:

$$\mathrm{Log}\,[\sigma_{t+1}] = \omega + \gamma\,\mathrm{Log}[\sigma_t] + \eta_{t+1}, \tag{C6}$$

where (η_t) is a Gaussian white noise with zero mean. This model maintains the nice features of the mixture model but, by introducing some volatility dynamics, it is more realistic, with respect to both the evidence of volatility clustering on stock returns (captured by a positive auto-regression coefficient γ in (C6)) and to the practice of forecasting volatility. As far as option pricing is concerned, there is still room for arbitrage pricing in the framework of this model (an impossibility with the Levy-stable model), even though the markets are now viewed as incomplete and perfect hedging of an option contract with a portfolio of the bond and the underlying asset is no longer possible. Moreover, the practice of forecasting volatility from Black–Scholes implied volatilities is consistent with this option pricing model, termed the Hull–White option pricing model, while it was logically inconsistent to base it on the Black–Scholes pricing formula.

To see this point, it is useful to think about the conditional dynamics of the stock price process given a path of the volatility process. The option traders are then right to consider that the implied volatility is related to the market expectation (or forecast) of the time-averaged value of the volatility process on the lifetime of the option, and definitely not to the estimation of a fixed volatility parameter σ which would be perfectly known if it existed.

Model 6 (Renault, 1997) State Variable Model of Option Pricing Errors.

The previous Stochastic Volatility model is one example of a parameter-driven model which allows one to solve the paradox pointed out above: if one backs out the volatility parameter value of one option price observed at each date, there is no longer a patent contradiction between the option pricing model and the observed option prices. One state variable process, namely the stochastic volatility process, allows one, precisely as a consequence of its unobservability, to reconcile exact arbitrage pricing with a time series of option prices. But the contradiction will reappear as soon as one observes, at a given date, a cross section of option contracts written on the same stock. If, for instance, these contracts all correspond to the same maturity date but with different exercise prices, Renault and Touzi (1996) have shown that the above stochastic volatility model (without leverage) will produce a symmetric 'volatility smile', that is, a U-shaped curve of implied volatilities as a function of the logarithm of the exercise price.

In this context, we can move towards the construction of 'supermodels' in the following manner: when one has to explain a cross section of prices of, say, K options contracts written on the same underlying asset, one has to introduce K unobserved state variables to get a no-arbitrage setting consistent with the 'pricing errors' which would be evident if one kept to a more parsimonious (that is, with fewer state variables) option pricing model. These state variables are often suggested by some evidence of stochastic variation, or jumps in some parameters, that were seen as constant in a more parsimonious model. For instance, volatility is fixed in the Black–Scholes model, stochastic in the Hull–White (1987) model and may even feature, in some modern extensions, some jumps of stochastic amplitude occurring according to a given point process. There is no limit in the industry of specifying such parameter-driven supermodels to eliminate a given set of 'option pricing errors'. Each new latent stochastic process introduced to replace a constant parameter is a state variable considered by the econometrician as implicit in the observed option price because the agents in the market are supposed to know its current value in equilibrium.

2. THE STANDARD PARADIGM

Edgeworth was an early critic of Marshall's views that the world is 'approximated by a well-behaved model with a unique equilibrium . . . the outcomes we observe are no more than the 'true equilibrium' outcome plus some random noise' (MT p. 9). Sutton neatly explains that Edgeworth's objections can be accounted for within two equivalent paradigms: one is the 'class of models' approach, while the other one leads to the adoption of a 'more complete' supermodel that should incorporate additional latent variables.

Actually, as explained in the previous discussion of option pricing, I am ready to go further than Sutton and argue that even for equilibrium models that are presented as 'working' in Chapter 2 (option pricing and auction models), there is a need for a supermodel with additional latent variables. This approach does not invalidate the standard paradigm. On the contrary, when one imagines that agents on the market observe some state variables which are not ones the econometrician can measure directly but whose values are precisely incorporated in the observed option prices, the addition of latent variables is a neat way of reconciling the equilibrium paradigm and the statistical data on market prices. In other words, what matters in holding to the paradigm that the world is correctly 'approximated by a well-behaved model with a unique equilibrium . . . the outcomes we observe are no more than the "true equilibrium" outcome plus some random noise' is not really the fact that these 'random noises' are small, but that they can be rationalized within the equilibrium paradigm. Otherwise, even very small discrepancies between the equilibrium values of the prices and the realized ones would open the door for huge arbitrage opportunities.

In my opinion, Marshall's tendencies can be understood as follows. By modeling the 'astronomical factors' (in economics the 'tendency' for prices to be consistent with an equilibrium) we can 'still arrive at a theory that affords us an adequate prediction' (MT p. 5) even though there are additional factors (like meteorological factors for tides or transaction costs for financial markets) which may be sources of errors. Moreover, the predictions are considered as 'adequate' not by their absolute value but in relative terms: as long as the microstructure theory of financial markets is unable to provide more accurate predictions, the arbitrage pricing of derivative securities is the best one we have at our disposal. The state-variable approach described above appears to be much more productive than the strategy of downplaying Marshall's tendencies with the view that observed 'pricing errors' are simply mispricing that is 'indeterminate within a certain region' (MT p. 7).

I want to emphasize that the critical issue is not so much to determine whether out of equilibrium factors are truly of 'secondary importance', but to decide whether we have better advice to give to the market investor than the predictions produced by the no-arbitrage argument. This approach shares Robbins's commitment to the search for a 'deep understanding of a stable underlying mechanism' (MT p. 12), in this case, as the best way at our disposal to predict option prices. 'Robbins's bête noire was the prototypical business-cycle analysts of the 1920s' with their view that 'statistical regularities existed in macroeconomic data, and these regularities had a considerable degree of stability over time'(MT p. 12). My '*bête noire*' here is a sloppy theory of fads or sunspots to justify either the use of chartist figures or the use of an obviously misspecified model such as Black–Scholes, calibrated in some unsupported way or other, in an attempt to capture some 'statistical regularities'.

As with Haavelmo, the aim is to have 'theories that, without involving us in direct logical contradictions, state that the observations will *as a rule* cluster in a limited subset of the set of all conceivable observations' (MT p. 17). While the use of Black and Scholes's option pricing formula will surely lead to a logical contradiction (different values of a volatility parameter are used, while its constancy is a substantive assumption), the introduction of a convenient number of unobserved state variables will avoid this logical contradiction. Moreover, this approach maintains the no-arbitrage principle needed to obtain 'structural equations' on option prices, as well as quite useful 'constraints on the space of measurable outcomes' (MT p. 17). Of course, it should not be forgotten that these latent state variables are 'not hidden truth to be discovered', but our way to 'assume some structure for the disturbance terms'. The number K of state variables introduced by an option pricing model is not dictated by some hidden reality but only by the number of option prices which are observed in a cross section. If the data set is changed, the model is changed as well.

This possibility of changing the model as data sets accumulate is a modern way to do econometrics which can go as far as considering parameters which are functions of the number of observations. Perhaps the bounds approach put forward by Professor Sutton should also be more explicitly embedded in a recursive procedure of inference where the bounds spread becomes narrower when new data arrive. The recent literature about 'near unit roots', 'weak instruments', 'sequences of local alternatives', and so on, provides a large number of examples of this type. These modern developments render somewhat obsolete the criticism of the standard paradigm based on the argument that 'as data sets accumulate, we might reasonably expect to converge bit by bit to a closer approximation to the true model, as all the most important xs reveal their influence' (MT p. 21). The fact that, in practice, many of the xs may be difficult to measure is not an issue in this respect. The only relevant question is the statistical identification of their influence on the relationships of interest.

Actually, while I agree with the emphasis on determining 'some desirable list of nice properties' (MT p. 22) for the error terms, I do not believe that 'to use the quest for such nice properties as a basis for model selection may now lead us badly astray' (MT p. 23). From a decision-theoretic point of view, the 'nice' features of the errors terms have to be assessed in context. For instance, a calibration of a Black–Scholes implied volatility parameter on some explanatory variables for option prices can provide a convenient *ex post* description of a set of option prices. However, the same analysis can be completely wrong for other applications. The implied value of the volatility parameter which makes observed option prices consistent with the Black–Scholes formula does not justify the use of the same formula for hedging purposes (Renault and Touzi, 1996). Moreover, *ex post* explanatory models of the volatility parameter can have very poor forecasting performance out of sample (Dumas, Fleming and Whaley, 1998) because such descriptive models without any structural foundations have a very low degree of stability over time. Structural models based on the absence of arbitrage (through the introduction of a convenient number of state variables by way of error terms) will typically be more stable. In general, both the model and the inference procedure have to be assessed in the context of the objective of the analysis.

Our models and estimations are 'all our own artificial invention in search of an understanding of real life' (MT p. 16) or, more generally, they reflect a search for some guidelines for decision. 'They are not hidden truths to be discovered' (MT p. 16) because, if they were, there would exist an optimal inference procedure which would not depend on any particular objective. The variety of procedures used for addressing the same economic reality offers further evidence for a decision-theoretic perspective. In the same way that 'if Marshall's analogy were valid, we would have seen spectacular progress in economics over the past fifty years' (MT p. 5), I claim that if option pricing models worked as well as is alleged in Chapter 2, any investor should be interested to discover the 'hidden truths' and one would not observe the huge variety of theories and empirical strategies currently at play.

The terminology 'true model' is not appropriate. There is a reality and there are models that may be useful for some purposes. For several reasons, economic models are more often than not misspecified, in the sense that they are obviously erroneous descriptions of the reality. But, irrespective of misspecification, the very notion of 'true model' does not make more sense than the notion of map of scale one. For instance, I consider that the search for 'the absence of serial correlation' may 'now lead us badly astray' in the context of genuinely dynamic economic phenomena. For instance, while according to Bachelier's equation (C) in its modern form (C5), there is no serial correlation in asset returns, the literature on ARCH-type processes over the last twenty years has stressed the importance of serial correlation in squared returns as a signal of the so-called volatility-clustering phenomenon which may exist while returns themselves are not serially correlated. Now, when one asks how good are the available volatility models (Andersen and Bollerslev, 1998; Engle and Patton, 2000; Alami and Renault, 2000), one realizes the importance of higher-order dynamics (hetero-skewness, hetero-kurtosis) which are captured neither by the serial correlation of returns, nor by the serial correlation of their squared values, but by the serial correlation of returns to the power of three or four. These higher-order dynamics are not of secondary importance when one is interested, as portfolio managers are, in assessing the accuracy of some volatility forecasts, that is, in getting a measurement of the volatility of volatility. Once more, the search for a 'true model' does not make sense in this respect.

3. DO AUCTION MODELS WORK WELL?

A year before these Eyskens Lectures on 'Marshall's Tendencies', J. J. Laffont gave the Marshall lecture at the 1995 meeting of the European Economic Association in Prague. The lecture was devoted to 'game theory and empirical economics: the case of auction data'. However, J. J. Laffont's views, as expressed in this lecture, appear to be somewhat less optimistic than the presentation of Chapter 2 as 'rare and happy circumstances in which the problem of model selection virtually disappears' (MT p. 33). Actually, he was more cautious than that in first noticing that in an auction, 'the rules of associates games are usually very well defined' (instead of 'the rules of the game are specified explicitly'), but he above all avoided adding 'we are very close to knowing the true model of the situation' (MT p. 47). Following Laffont (1997),[5] I can see at least three reasons to be more cautious:

(i) 'These games are analyzed by assuming that the characteristics of the players are drawn by Nature from probability distributions which are common knowledge to all players. . . . When using field data these distributions are unknown to the analyst. This fact complicates considerably the empirical analysis of auction data' since, as for option pricing, the key-point of the structural model is a number of latent state variables (private values, reservation prices . . .) which prevents the economist from 'writing down a representation of the true model that is known both to the agents in the market and to [him] .' (MT p. 47). Typically, while an agent can infer the private value of a competitor from the observed bid and his knowledge (common to all players) of the probability distribution of

[5] See citation in MT.

https://doi.org/10.1017/S0266267102001050 Published online by Cambridge University Press

the private values, the economist cannot do this. It is quite similar to an option market where agents can infer the current value of the underlying stochastic volatility process from a given option price and the common knowledge of the probability distribution of the volatility process, while the economist cannot do so. It is in this respect somewhat ironic to report that six years before reading Chapter 2 of Professor Sutton's Lectures, I was already attempting, in my 1995 Tokyo Lecture, to draw an analogy between empirical issues of option markets and auctions. At this time: 'in the case of option pricing with stochastic volatility, a BS implicit volatility . . . is often fairly well correlated to the stochastic volatility. In the case of auction theory with the private-value paradigm, the bid formed by an individual should be close to his private value. This remark leads one to an *indirect* strategy'. By using italics, I referred to 'indirect inference' *à la* Gourieroux, Monfort and Renault (1993) which was used in this context of option pricing by Pastorello, Renault and Touzi (2000). But it is tightly related to Sutton's observation that 'many of the practical difficulties with obtaining testable predictions from auction theory derive from the fact that we are forced to uncover this information *indirectly*, by looking at the pattern of bids' (MT p. 47).

(ii) While the rational expectation equilibrium hypothesis can perhaps be maintained in financial markets to assume that agents have learned in equilibrium the value of the state variables, it seems to be more often the case in auction markets (see, Laffont, 1997, Appendix A) that 'we are led to a model of adaptive behavior' which opens difficult questions in attempting 'to give some empirical substance to the common knowledge assumption'. Actually, J. J. Laffont expressed very pessimistic views in this respect: 'the identification from available past data of such adaptive behaviors assuming that they are stable enough in environments which are never completely stationary is clearly not promising'.

(iii) One reason why it is not so clear that 'the rules of associates games are very well defined' is the endogeneity of the number of bidders. J. J. Laffont also reached a pessimistic conclusion in this respect: 'It is not obvious that the modeling of entry will be easy and that much will be gained in general from this extension which makes us leave the comfortable world of well defined game forms'.

With this last remark, one will realize that J. J. Laffont has precisely demarcated the debate that is the focus of interest in Chapter 3. The unpalatable modeling issue of entry also leads Professor Sutton to conclude that 'the simple two-stage game that we introduced earlier is too primitive to allow for the kinds of complex interactions that may

https://doi.org/10.1017/S0266267102001050 Published online by Cambridge University Press

occur as successive firms enter the market and introduce new production plants or production designs. But what would a realistic representation in terms of some multistage game look like? It is in grappling with this issue that we arrive at the point where the search for a true model becomes futile' (MT p. 70).

But, while both J. J. Laffont and I would probably share this judgement of futility, I do not agree with the conclusion that one should 'leave the comfortable world of well-defined game forms' or well-defined arbitrage-based option pricing models. As far as I can extend the proposed 'bounds approach' to option pricing, I am afraid it looks very much like an attempt to bound option prices from stochastic dominance criteria. These criteria were never very productive, in particular, with regard to characterizing efficient hedging strategies.

4. CONCLUSION: UNCERTAINTY AND ROBUSTNESS

At the end of this engaging and efficiently conducted tour through the empirical methodology of economics, my general feeling is that this tour was about the role of error terms in economic models. In this respect, I fully agree with Professor Sutton that the Marshall tides analogy is quite old-fashioned. Empirical issues in economics are deeper than simply adding error terms to models produced by economic theory. But, with a view of statistics as a part of decision theory, I still maintain the principle that econometric model should be sufficiently specified to produce guidelines for decision. In this respect, I am afraid that the bounds approach put forward in these lectures does not meet the demand of the decision maker because its predictions are more often than not quite loose.

In some respects, Professor Sutton acknowledges this difficulty 'when we move to the more complicated setting of the macro-economy' because 'it is very difficult to escape the logical force of the argument that brings us to a standard rational expectations (R.E.) model. In such a setting, the imposition of expectations-formations mechanisms that contradict the R.E. model raises serious difficulties: could a smart agent not do better by using an R.E. rule? If agents are not using such a rule, then could the policy maker not exploit this behavior? . . . '(MT p. 102). I have already stressed that the same type of question is raised by a loose view of option pricing, conceived to take into account the micro-reality of the financial markets (transaction costs, noisy traders, fads, unexplained dynamics of volatility or jumps, and so on . . .) but which may contradict the arbitrage model: could a smart agent not do better by exploiting the arbitrage opportunities?

In my opinion, the modern way to introduce error terms in econometric models in a more suggestive way than the tide analogy is explicitly to incorporate within the model specification the fact that random shocks were not invented by the econometrician but are part of the economic reality faced by economic agents. In the 1995 Tokyo lecture, the use of state variables enabled me to introduce error terms in option pricing models without also introducing arbitrage opportunities. More generally, Hansen and Sargent (and co-authors in a series of papers and a forthcoming book) have recently been promoting the concept of robustness in macroeconomics. It is another neat way to view models as approximations without violating the common model requirement imposed by rational expectations.

REFERENCES

Alami A. and E. Renault. 2000. 'Risque de modèle de volatilité'. *Journal de la Société Française de Statistique*, 141:103–36

Andersen T. G. and T. Bollerslev. 1998. 'Answering the skeptics: yes, standard volatility models do provide accurate forecasts'. *International Economic Review*, 39:885–905

Bates D. 1996. 'Testing option pricing models'. In *Handbook of Statistics*, Vol 14:567–605. G. S. Maddala and C. R. Rao (eds.). North-Holland

Bossaerts P. and P. Hillion. 1993. 'A test of general equilibrium stock option pricing models'. *Mathematical Finance*, 3:311–47

Clark P. K. 1973. 'A subordinated stochastic process model with finite variance for speculative prices'. *Econometrica* 41:133–55

Cox D. R. 1981. 'Statistical analysis of time series: some recent developments'. *Scandinavian Journal of Statistics*, 8:93–115

Dumas B., J. Fleming and R. E. Whaley. 1998. 'Implied volatility functions: empirical tests'. *Journal of Finance*, 53:2059–106

Engle R. F. and A. J. Patton. 2000. 'What good is a volatility model?' Working Paper, Stern School of Business

Gourieroux C., A. Monfort and E. Renault. 1993, 'Indirect inference'. *Journal of Applied Econometrics*. 34:5–32

Hansen L. P. and T. J. Sargent. 2000. 'Wanting robustness in macroeconomics'. Working Paper. University of Chicago

Hull J. and A. White. 1987. 'The pricing of options on assets with stochastic volatilities'. *Journal of Finance*, 42:281–300

McCulloch J. H. 1996. 'Financial applications of stable distributions'. In *Handbook of Statistics*, Vol 14:393–421. G. S. Maddala and C. R. Rao (eds.). North-Holland

Melino A. 1994. 'Estimation of continuous time models in finance'. In *Advances in Econometrics*, C. A. Sims (ed.). Cambridge University Press

Pastorello S., E. Renault and N. Touzi. 2000. 'Statistical inference for random variance option pricing'. *Journal of Business and Economic Statistics*,18:358–67

Renault E. 1997. 'Econometric models of option pricing errors'. In *Advances in Econometrics* D. M. Kreps and K. F. Wallis (eds.). Cambridge University Press

Renault E. and N. Touzi. 1996. 'Option hedging and implied volatilities in a stochastic volatility model'. *Mathematical Finance* 6:259–302

Shephard, N. 1996. 'Statistical aspects of ARCH and stochastic volatility'. In *Time Series Models in Econometrics, Finance and Other Fields*, pp. 1–67. D. R. Cox, D. V. Hinkley, and O. E. Barndorff-Nielson (eds.). Chapman & Hall

Taylor S. 1986. *Modeling Financial Time Series*. John Wiley