Referee report on "A Parsimonious Model for Intraday European Option Pricing" by E. Scalas and M.Politi

This paper derives a general formula for the martingale price of an intra-day European call option. In short, this is done by defining an equivalent martingale measure of the log-price at each trading epoch, thus incorporating tick-by-tick price fluctuations.

The paper contributes to an interesting strain within the high-frequency trading literature, as well as to a move away from Merton jump diffusion models, which, as the authors point out, have some unrealistic features. However, my first comment has to do with unrealistic features. The authors assume that the trading epochs are a renewal process as in Cox (1970) such that the intraday trade durations are *mutually dependent* and *i.i.d* positive random variables. As the authors correctly point out in Section 3, these assumptions are clearly not supported by empirical research and they thus continue with a brief discussion on alternative modeling techniques that could be used in order to deal with (for instance) heteroscedasticity and dependence. They state that a suitable mixture model as in Scalas (2007) could be used but refrain from discussing how such a modification would affect *their* model. I realize that some of the elegance may be lost in such a discussion but, since such features may significantly alternate intraday hedging strategies, I feel an urge to stress its importance.

This noted, as far as I can see, the analysis is correct, if somewhat fragmented. For instance, I would like to see the discussion regarding the convolution in Section 1 to be carried out in direct relation to the derivation of the model. A more general discussion regarding the techniques could then be moved to an appendix or, if preferred, simply mixed into the derivation of the model. Within such an outlay, the independence and i.i.d. assumptions could be discussed briefly in the running text. I would also like to see at least one example of how the model is "flexible" as the authors state at the end of the Introduction when the model's advantages are discussed.

In addition, I have some comments regarding the quality of the running text. Firstly, I recommend the authors to make suitable corrections regarding the running text in, primarily, Section 2. For example, I find the discussion regarding the assumption of a zero risk-free rate to be rather unstructured and its length disproportionate. There is no

such lengthily discussion regarding the more serious assumptions (as discussed above) and I recommend the authors to shorten and restructure it.

I also have some minor suggestions regarding the text, even though these comments by no means aim to be exhaustive:

1. Page 4, line 10... a procedure that allows for probabilities to be written as expectations.

2. Page 4, line 21. Remove "can".

3. Page 5, line 17 needs to be reformulated.

4. Page 6, line 1. This is the case if...

5. Page 6, line 1 & 2. However, one can always find an equivalent martingale measure (e.m.m.) by replacing $Y_i$ in equation (11) with $Y_i - a$, defining the following log-price process

6. Page 6, line 5. Remove "Now".

7. Page 7, line 4. Remove "that".

8. Page 7, line 15. Remove "can".

9. Page 7, line 16. Begin with "where $\tilde{S}_n$ is the..."

10. Page 7, line 18 & 19. put "the Mellin transformation is discussed..." in parenthesis.

These comments aside, I believe that this paper significantly contributes to the high-frequency trading literature as well as to the option pricing literature. Thus, I recommend this paper to be published in the journal, after the above comments have been noted.