Report on the paper
"Mechanics of good trade execution in the framework of linear temporary market impact"

by C. Bellani and D. Brigo

The paper under review considers optimal liquidation problems by developing a pathwise approach allowing for solutions which are robust under model misspecification. The results obtained therein, especially those in Section 3 are new, interesting and rigorously justified. I recommend major revision though, mainly for the following reasons:

A) I think the paper is too long and very wordy, especially Section 2, which would require a pretty drastic reorganization and rewriting as many concepts introduced and discussed therein are rather obscure and not always rigorous;

B) Indeed, apart from many specific issues with several assumptions and definitions that are listed below, in the text important statements and remarks or discussion on the literature are all mixed up, while it would be helpful for the reader to distinguish clearly the main statements and auxiliary remarks, which can be skipped at first reading. Examples follow in this list.

C) Subsection 2.2 is pretty confused, it contains a discussion of the literature (from line 30 onwards) which is way too long and should be isolated from the rest. Moreover, it is not clear what the authors are trying to say there. Are they saying that many cases in the literature fits their setting?

D) Another subsection needs rewriting, it's subsection 2.3: indeed, it seems the authors try to introduce a classification of the many models available in the literature, unfortunately the discussion is definitely not very reader friendly due to, among other things, the too many acronyms the reader has to keep track of (LTIF, mart-LTIF, semimart-LTIP, LTI, det-LTIP and so on).

E) The beginning of Section 3 could be also written in a better way: for instance, at the beginning (lines 5-) the authors should refer to the definition (that'd better be numbered) given before instead of repeating it; state the optimization problem right away and put the comments after it, instead of the other way around as it's done in the current version; finally, the text from line 58 (pg 18) to line 12 (pg 19) could definitely be turned into a remark (or two remarks) while the part after those lines should definitely be highlighted as it is important for what follows.

My conclusion is that the paper contains interesting new results, but the material could definitely be explained in a better way in particular by shorten the paper while making it more effective. I have also concerns related to some of the concepts and some steps in one of the proofs (listed below). Therefore I recommend major revision.

List of more technical remarks:

1

- Pg 5, line 26: is $\mathcal{F} = \mathcal{F}_T$? If not, the completion refers to the null sets of which σ-algebra?

- Pg 5, line 32: the definition of special semimartingale seems to be different from the standard one (check Protter's book), where usually $S = S_0 + M + A$, where A is a predictable finite variation process, M is a local martingale (not necessarily with zero mean) and $A_0 = M_0 = 0$. Please check.

- Pg 5, line 42: give the definition of 2-variation; the elements of the set in eq (2.2) should be processes $(q_t)_{t \in [0,T]}$ and not random variables q_t (same remark applies to the other sets in page 6).

- Pg 6, line 19: when you talk about random variables, say "$\mathcal{F}_0$-measurable" instead of $\in \mathcal{F}_0$.

- Pg 6, line 32 onwards: is this discussion heuristic? If it is, you should make this clear as some of the concepts introduced therein are not very clearly described. For instance: what is the function q in (2.3) is just a (deterministic function) or it may contains some extra-randomness. Remember that the filtration has been completed with null sets in $\mathcal{F}$ which in principle could be bigger than $\mathcal{F}_T$ which is generated by S. Probably those equalities ((2.3) and (2.4)) have to be understood in an almost sure sense.

- Pg 7: Definition 2.1 is not very rigorous, please try to restate it in a clearer way; the assumption in Proposition 2.2 is not clear, I guess that you mean "assume that $q_t = q(\ldots)$ for some (deterministic) function q etc". In this case, where is the function q defined?

- Pg 7, line 58: reframe the sentence starting with "We say that ..." in a proper definition.

- Pg 8, line 4: don't need to say that g is continuous since it is in $C(\mathbb{R}^2)$; lines 28-45: this part can be shorten.

- Pg 9, line 18: it is a local martingale, so to justify the computation just after that you should localize with a sequence of stopping times; line 25: using $\mathfrak{q}_t$ is not a good idea as it's not that different from q_t; line 40: you should justify carefully how do you pass from "inf over pathwise" to an "inf over static" as this is the core of the proof.

- Pg 10, line 29: global Lipschitz implies linear growth, so no need to impose the latter.

- Pg 15, Definition 2.11: are the two price processes S and $\tilde{S}$ necessarily defined on the same probability space?

- Pg 21, lines 19-24: could you justify that part of the remark better? I don't think is that obvious that by replacing S_u with its expectation you get a minimiser in the optimization problem over static strategies.