Referee report for the paper "Empirical Linkage between Oil Price and Stock Market Returns and Volatility: Evidence from International Developed Markets" by Abderrazak Dhaoui and Naceur Khraief

General comments

The research question seems interesting. However, the paper is not only poorly written but also I'm not convinced about the paper contributions and empirical identification strategy.

Specific comments:

- Abstract could be improved. The contribution and significance of the paper is not highlighted. The same can be said about the introduction and the lengthy literature review sections.
- My guess is that main value-added of the paper is slightly different methodology of the paper that might make it slightly distinct from a vast literature in the field. However, it is not clear in the paper. If the authors would like to claim the methodological contribution of asymmetric effects on stock returns they need not only to highlight it in the abstract and other sections of the paper but also to employ the "common" methodologies in the topic for their specific data to show that their approach adds anything to this literature.
- There is no theoretical framework. The functional form of the empirical model is *ad hoc* and incomplete.
- Given a long time span of the data (1991-2013) I am not sure why at least some deterministic time variables are not used to account for technological change and other factors.
- Basic descriptive statistics tables are missing.
- No explanations are given why the authors consider the specific countries in their analysis. What makes them interesting? Why other countries are not considered?
- The assumption of weakly exogenous regressors is crucial for correct interpretation of the results. The obvious potential endogeneity issues are not discussed.
- The paper is poorly written. For instance, the authors report statistical information again and again in the discussion and estimates sections which are already summarised in the tables. In the discussion section and the estimates sections the authors limit themselves to providing what they find significant or not but it is missing wider economic discussions and implications of their findings.
- Some tables are poorly made with the obvious mistakes (see table 4).

To sum up, I do not recommend the paper for the publication for the following reasons:

- the paper contribution is very unclear or adds little to the existing literature;
- the empirical identification strategy is not adequate enough to identify the effects;
- the paper is poorly written.