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Opponent review on the master's thesis
by
Bc. Andrej Zubal′

Application of band spectrum regression in economic probelms

Supervisor PhDr. Jozef Baruník

Thesis are devoted to the problem which is widely studied for several decades, namely modeling the structure of stochastic process and estimating the parameters of the corresponding models. The tools for this purpose are various, the author selected nowadays very popular (and a bit modish) wavelet analysis. The thesis are divided into four parts and concluded by a list of references. After a (rather) brief (but sufficient) introduction the second part focuses on a sketch of (or more precisely- a random sampling from) the theory which backs up the continuous and discrete wavelet transform. The third part consist of two case studies: *Implied versus realized volatility relation in the oil market* and *Relationship between labor productivity and unemployment*. The fourth part - Conclusions - summarizes the previous text.

First of all, it is to be said that the author had to learn a lot of above the framework of master studies on the Institute (and could spend nearly the rest of life to come to understanding to all theoretical tools, he used, up to very roots; I don't know whether there is in the Czech republic - after a untimely demise of Josef Štěpán - at least one mathematician who understands the stochastic calculus really in its complexity). Nevertheless, the amount of work and endeavor of author should be appreciated. Also the form and level of both case studies are to be praised.

On the other hand, the structure of the second chapter is a bit messy and in fact it is written in a "folklore language" (or even a "slang") unfortunately nowadays frequently encountered in the papers about this topic (and regrettably about more and more topics not only in economics) which already led to some evident controversial conclusions. Reading such a text always reminds me the famous paper by Paul Halmos (1981), one of the greatest US mathematicians of all ages. It may seem that its title speaks very clearly but Paul Halmos himself delivered a series of nice applications. Halmos explained in the paper that whenever we abandon the principles which were started by Pythagoras of Samos, Thales of Miletus, Archimedes of Syracuse or Euclides of Alexandria and continued by Rodrigo de Arriaga, Issac Newton, Galileo Galilei, Bernard Bolzano, Augustin Cauchy, Adrien Legendre, Carl Gauss and many others, we are on a way to hell. In the Introduction the author wrote: *The emphasis should be put on basic ideas and intuition rather than full rigor.* To rely on

intuition is a bad thing. It is easy to find nearly endless collection of examples demonstrating that (human) intuition is frequently misleading. Personally, I am in favor to explain ideas which are "behind" the theory but this explanation is to follow exact definitions which - as it is evident even from text of the second chapter - could save a lot of space and would give the notions in unambiguous form. Moreover, when we write down the definitions, we realize what inconsistencies we have committed in text. Let me give an example. On the last row of page 12 is written: ... *(with Jchosen beforehand, indicating the number of scales)*

$$\hat{W}_1, \hat{W}_2, ..., \hat{W}_J, \hat{V}_J.$$

If an exact definition of $\hat{W}_i$'s is given it would contain someting like:

$$\forall \quad (J = J_0, J_0 + 1, ...) \quad \text{consider} \quad \hat{W}_1, \hat{W}_2, ..., \hat{W}_J, \hat{V}_J$$

etc. and the reader would not hesitate on the page 14, relation (2.17), how we can sum up to ∞, leaving aside that the relations (2.12) and (2.13) on page 13 say nothing if $\hat{W}_1, \hat{W}_2, ..., \hat{W}_J, \hat{V}_J$ are not given by some definition(s).

Similarly on page 14 on the first line we can read:
In case of the MODWT, this essentially boils down to assign to the j-th lowest scale (denoted τ_j) a finite series of numbers $\{h_{j,t}\}_{t=0}^{L_j}$ called j-th level MODWT filter. With the use of such filter, ... But this sentence says nothing if the reader doesn't know how $\{h_{j,t}\}_{t=0}^{L_j}$ were selected.

I suppose that the goal of qualification thesis is to show that the author is able to write a consistent text and to achieve some new results (for me, preferably *theoretical*). The second part of the task was surely fulfilled by the case studies. The first part was (probably) assumed to be given by the second chapter of thesis (I assume so because otherwise I don't see any reasons for including it - it has common with the case studies in the third chapter - if I exaggerate it a bit - only the word "wavelet" but the "gap" between the "theory" presented in the second chapter and the theory used in the case studied is large - containing the whole stochastic calculus but also some not very simple topics from probability theory, as martingales etc.). Moreover, nowadays when even the social sciences use some mathematical tools, the text should be also up to some level rigorous. Except of others, such a text indicates that the author understands what the theory which is behind the models, is about. If students (but the same applies for the researchers on the start of their carrier) "overjump" the period when they can really "inprint' in their minds that the things should be done exactly with high care of details[1], they proceed further with less and less care about correctness of results. In the second chapter however the author (more or less) gave a signal that he doesn't keep in mind some basic things. E.g. when the regression model is recalled(by the way, it is puzzle for me why the *simple regression model* was recalled, although the notation for the *multiple model* is simpler), it is assumed that (see page 16[14]) ... *$\epsilon = (\epsilon_1, \epsilon_2, ..., \epsilon_n)$ is a multivariate normal random vector with zero mean and $n \times n$ identity covariance matrix, i.e. $N(0, I_{n\times n})$ (again, by the way, why the explanatory variables are assumed to be deterministically given is a puzzle; the theory which doesn't assume that*

[1] Although at the end of research task, the paper presenting its results, can be written a bit non-exact way because of limited space in journals or proceedings of conferences.

the explanatory variables are (p-dimensional) random variables is very complicated, see some papers by Igor Vajda, because when we try to establish some asymptotic results we have to assume rather intricate scheme of asymptotic arrays). Nevertheless, on 16_7, one can read that *..The traditional way of estimating β is using the OLS ... which turns out to be the best linear unbiased estimator according to standard theory.* But standard theory says that $\hat{\beta}^{OLS}$ is *best unbiased estimator* which is significant difference because restriction on the family of linear estimators is drastic. Such signal as this can wake up a suspicion that the author is not able - if he would be ask to do it - to covert the text into fully mathematically correct text.

All after, in modern days one of most significant statistician, leading person of ETH in Zurych, wrote: *The most practical thing in world is well-written and clearly in formalism given mathematical theory.* If not anything else, the text given in the form of definitions and theorems can be read in the only way, avoiding so any misunderstanding leaving aside that it is easier to read such text than a narrative one.

Summa summarum: I have to say that the author convinced me that is able to work hard, to create (I believe) some useful results by fine case studies but did not convinced me that is able to write exact, mathematically correct text which then indicates that the author of such a text understands really the mathematical background. Fischer Black, Myron Scholes, Robert Merton (but also many Nobel prize winner as Clive Granger or Robert Engle) and finally last but not least (especially for us living in Bohemia) Oldrich Alfons Vasicek gave their results because they wrote in fact mathematical qualification thesis (adding that the success of Oldrich Vasicek - as he stressed during his lecture in the Czech Academy of Sciences several years ago - was anchor by the fact that he never resigned on full rigor). So, I propose to classify the thesis as fulfilling more or less their goal, they can be appreciated by mark corresponding to level of other thesis on IES (I have to admit frankly that I have no idea about this level) but I am afraid that I can't to recommend the thesis to be assumed as exceptional.

P.S. (a proposal for author) In the second chapter, when the DWT is given, an idea offers immediately that it could be explained using the notions of *eigenvalues* and *eigenvectors*. Taking into account that the factor analysis and the analysis of main components were much simplier explained by them then earlier (when in 19. century these "intuitively" created theories were built up by physicians and psychologists), one can expect the same "story".

SUMMARY OF POINTS AWARDED		
CATEGORY		POINTS
Literature	(max. 20 points)	20
Methods	(max. 30 points)	30
Contribution	(max. 30 points)	25
Manuscript Form	(max. 20 points)	10
TOTAL POINTS	(max. 100 points)	85
GRADE	(1 2 3 4)	1

References

Halmos, P. R. (1981): Applied mathematics is a bad mathematics. *Mathematics tomorrow*, Steen, L. (ed.), Springer Verlag, New York, 9 - 20.

Prague, June 3, 2015 .....

(signature)