CHARLES UNIVERSITY, FACULTY OF SOCIAL SCIENCES
INSTITUTE OF ECONOMIC STUDIES,
DEPARTMENT OF MACROECONOMICS AND ECONOMETRICS
Mailing address : **Opletalova 26, 110 01 Prague 1, CZECH REPUBLIC**
Phone : **+420 222 112 318** *Mobil* : **+420 732 344 585** *e-mail* : **visek@fsv.cuni.cz**

Opponent review on the master's thesis
by
Bc. Tomáš Jelínek

Innovative Financial Instruments: An alternative to traditional grants

Supervisor doc. PhDr. Petr Teplý, Ph.D.

Thesis are devoted to the discussion how to improve *efficiency* of spending the financial support for the improvement of *efficiency* of spending the energy. The new instruments should attract different agents to enter this region and to spend their financial means in order to help to improve energy-spending-*efficiency*. The idea was inspired (or we can say inevitably invoked) by the bad *efficiency* of spending the grants given to agents by state or some public institution. The author collected really respectable amount of material, at least according to the list of references. From what I have written up to now, it is clear that the key word of the thesis is *efficiency*. I have not count how many times it appears in the thesis but surely goes to hundreds. But the *efficiency* of delivering the information to reader is low.

First of all, it is to be said that the author took into account all aspects which are usually taken into account in similar reports[1], e. g. by European Commission, Energy Regulatory Office, Expert Group on Energy Efficiency, International Energy Agency, etc.. Unfortunately, he overtook also the language of these documents, so that the thesis resemble a program of some political party or just documents of EU (redundancy of which are presumably even higher than of Spanish language). There is a phrase (usually attributed to Thales from Miletus, 5BC) saying: *"It is possible by words to prove anything, especially the opposite."* and a lot of nice examples that the intuition (which can be assued to be supported by an approprite chain of words) can lead to completely wrong conclusions (especially when the intuition is driven by some "evidently" good vision), are well known. One of them is e. g. the Simpson paradox.

When one opens the thesis and looks into the Contents he/she can expect nice explanation of inspirations with some examples of possible improvements of efficiency of energy spending. E. g. in Chapter 5 the appropriate segment for making the simulations are discussed. The paragraph 5.1.4 considers the residential buildings and gives reasons why it is suitable for

[1] Maybe that it is a proper place to say following: I don't like *"to strut in borrowed plumes"* and hence I should say that the previuos statement about covering all important segments of discussed topic, is not mine. I have discussed the character of thesis with several colleagues so that sometimes I claim something which is out of my horizon but of course the resposibility for the opinion expressed in oponent report is mine.

the intended research. This paragraph was a nice opportunity to give an example how some measures (as additional insulation of facades of houses usually by polystyrene boards) can decrease the demand for heating and that it is one (among many other reasons) to select this segment for proposals of desirable financial instruments. I should say that when reading Chapter 5 I was already suspicious what I would find there because when passing Chapter 3 (which is too long introduction into the topic, moreover written by the language of political reports not as the scientific description and would be much more transparent and convincing if reduced to the half) I expected in Chapter 4 (which is called "Methodology") finally some explanation how the data (which I expected to be given later) will be processed but there are again rather vague statements. To understand me properly, I do not claim that there should be (but of course, can be) a formalized description of methodology, e. g. a formalized explanation what is e. g. the probit or logit model (P&LM) - just opposite. It can be described by words and reference where it was firstly published but then there should be some explanation how it works - in the case of L&PM that main idea of both is to model the probability that response attains this or that value. If one assumes that for further simulations, prediction and sensitivity studies some model (e. g.) of regression type (contingency tables, regression trees, etc.) is the best possibility, then I would expect in Chapter 4 some argument how to verify (on some training data from the past) that such and such model is suitable and that usually it is treated (estimated) by this or that method. Usually (in scientific text) the author gives some references which models and methods were employed by predecessors, what is his/her contribution in improvment of model and/or method, etc. It is a rare case that somebody starts something on a "green field" (even John Tukey who opened may be 10 new areas of science, referred to some previous ideas which can be related to the new topic). Moreover, to assume that the suitability of model can be justified only by words is a foolish idea - there are a lot of examples from history of modern science demonstrating how treacherous our intuition can be. Even a reliance upon results of some predecessors, although commonly accepted, can be misleading - let's return to it at Summary. In many (if not all) branches of science one can find a folklore, claims things which are questionable or it employs method which are wrong - but it was sometimes recognized only due to exact formalized analysis. In economics a nice example is the principle of "Ceteris paribus". The relations among the economic factors are so involved that when we consider any model and we would try to learn the sensitivity of the "response" on a change of one "explanatory" variable, we cannot usu "Ceteris paribus", simply we cannot assume that in real life we can change one of "explanatory" variable and the others could be kept fixed - they are changed inevitably, too.

After passing Chapter 4 (and having learnt about methodology only some vague ideas) and then Chapter 5 (where the possible selections of proper segments were supported only by some considerations - not by empirical material) one expects in the next chapter, called "Simulation and model", that a formal model will be established and presented in a such way that the reader can verify that it was done in a sequence of reasonable steps (by the way, I would expect the title of chapter rather "Model and simulations" because I assume that at first a model should be established - on some training data from the past - and then this model can be employed for simulations). In other words, that the model is well identified, reasonably determined, that the estimation of parameters of the model took into account mutual relations of (explanatory) factors (e. g. collinearity, correlation or some other type of mutual dependence), that the character of data was respected (e. g. heteroscedaticity, exogeneity/endogeneity, etc.). Instead of it there are presented some information (results, outputs) without clear explanation how these results were reached. E. g. Figure 6.8 offers forecasts of heat prices development. Leaving aside that any forecast for more than 10 years (except some special situations, as e. g. development of cohorts in demography) is pure prophecy from crystal ball, there should be some comment how the forecast were computed, e. g. by some model of time series with data-generating-process of Box-Jenkins type or their generalizations. Predictions of the type as given in Figure 6.8 have small or even negligible value and they say frequently much more about the author of prediction than about the topic in question (a nice discussion to the topic can be found in Karl Popper *Powerty of historicism*, see [1]). Moreover, I hesitate (on the experience with the explanations in previous chapters) whether in the sensitivity studies (leaving aside that the reader cannot verify whether they were performed in a reliable way) was taken into account mutual dependence of individual factors or if it was done in such a way that all factors were kept fixed and only one was changed (which could and nearly surely is wrong). Of course, the last sentence implicitly says that I assumed that author had some formalized model (naturally, it is only a conjecture but I cannot imagine[2] that somebody makes on PC a simulations - presumably with some random inputs - not having a formula for model). But then it is a puzzle for me why the author concealed it to reader.

The last chapter of dissertations contains some conclusions. I am (at least) a bit skeptical about them. E. g. on one place the author says: *" ... we were able to identify a segment which is underinvested and we recommend an initiative with significant potential to improve the situation."*. That can I say absolutely surely - having read these thesis and being an investor, I don't put my money into project recommended by author.

I assume that the work of this type could be offered to some institution (European Commission, government, etc.) by somebody who has already a high reputation, some Nobel price winner but even then it should (or more likely, has to) be accompanied by a thousands of pages of results of computations (of course, a very first question arises whether Nobel price winner would devote the time for writing such a text but may be that he/she can lead a team of workers etc.). This form of thesis (or some parts of them) can be view as some literature retrieval, some others as a list of some results and we can or need not believe that they were achieved in an accountable and justifiable way. I was said in one discussion that sometimes papers of this type (i. e. in which the reader is assumed to believe that the information was reached in a reasonable way) appear in "peripheral" journals. of course, sometimes the researches refer to some well-known and commonly accepted models (as e. g. TIMES) but even then - by me - it need not be completely reliable because the history many times proved that even facts which are commonly accepted need not be correct. I am not economist and hence I cannot give some nice example from economics (I have only a vague idea that such an example is Phillips curve) but shocking example is from mathematics - David Hilbert ideas about a possible nice "temple" of mathematics - commonly and keenly accepted at the end of nineteen and beginning of twenty century - was destroyed by Kurt Gődel's incompleteness theorems. But the thesis, especially on IES, should have a higher ambitions than to be comparable with papers in "peripheral" journals. What is pity is the

[2] I admitt that it could be due to my insuficient imagination, see again [1].

fact that the author devoted to the thesis presumably extraordinary large amount of energy and that the formal side of thesis is really nice but the result is as I described it above. There are graphs and tables which required a lot of endeavor. And may be that the author performed a lot of computations - but unfortunately we cannot say of what quality and how they were organized, etc.. What is even more pity for me is the thing that the education on our institute allowed the author to consider such a text to be scientific. I meet in the proposals of accreditation with rather strange ideas what is science but it is typically on schools of bad quality - I would not expect it here.

Summa summarum: I assume that the text of this type should not appear in future as a qualification work - but it is a question of instructing the supervisors, if such point of view would be common. I assume that the student after passing IES (where he/she obtain education covering many quantitative methods and after having read the recommended scientific papers) should demonstrate (if he/she treats a topic which requires application of quantitative methods) that he/she is able to use them properly and that the text should have some character given by academic milieu. So I cannot rid of idea that it is also a malfunction of the supervisor - I am sorry that I have to say it. The last sentence together with the appreciation of amount and carefulness of author's work is the reason why I recommend to accept the thesis but the grade (at least by me) should not be 1 (although the extent of work, amount of collected information and presumably level of developed knowledge if submitted in another form of text could lead to an excellent result).

References

[1] Popper, K. R. (1957): *The Poverty of Historicism*, London, Routledge & Kegan Paul, Ltd.

SUMMARY OF POINTS AWARDED		
CATEGORY		POINTS
Literature	(max. 20 points)	20
Methods	(max. 30 points)	10
Contribution	(max. 30 points)	15
Manuscript Form	(max. 20 points)	15
TOTAL POINTS	(max. 100 points)	60
GRADE	(1 2 3 4)	1

Prague, January 30, 2016 ....

(signature)