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Review of the Doctoral Thesis

The impact of climate change on the Polish economy

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by Roberto Roson

General Comments

This is a thesis which is written as a single, long essay. It is about assessing the consequences of (some) climate change impacts on the Polish economy. The overall approach is empirical, multidisciplinary and model based.

On the positive side, I have to say that I have been impressed by the comprehensiveness of literature review and by the candidate eclecticism in combining many different modeling techniques. The whole field lays on the intersection between natural and social sciences, and the candidate proved to master methodologies and concepts that does not quite belong to economics, while at the same time connecting them to it. When it comes to empirical modeling, I can see that the candidate employs a wide array of different techniques, from computable general equilibrium to overlapping generations, etc. To the extent that a doctoral thesis is meant to show the capability of a candidate to engage in scientific research, I believe that Mr.Gaska demonstrate his skills in a very convincing way.

On the negative side, I have to say that what is gained in terms of coverage is lost in terms of coherence, solidity and reliability of results. Yes, there are many citations, but some of them misplaced or misinterpreted. The different models are often combined in a disconnected and inconsistent way, leaving me the feeling of facing something like a Frankenstein monster. I shall be more detailed on this issue in the following section.

There is also another big problem with the adopted approach. I got the feeling that the whole exercise is driven by "a priori" expectations and it is "ideologically" oriented. In other words, not scientifically neutral. This is especially evident from the initial choice of "naturally" associating climate change to extreme events. It looks like, for the candidate, climate change needs to have some "catastrophic flavor". At some time he even regrets of not being able to consider the existence of tipping point in climate, after what the collapse would be inevitable.

Quite the contrary, climate change is a slowly unfolding process, displaying its effect mainly in the long run. Most of its impacts are likely to be determined by changing averages, more than enlarging variances. As far as I know, there is no physical model, or theory, satisfactorily demonstrating a direct causal link between climate change and extreme events. There is, actually, some anecdotal evidence, suggesting that more energy could accelerate the water cycle, but I do not believe that more typhoons or different monsoon patterns could significantly affect Poland.

On a different note, let me comment about the language style of the manuscript, which is generally rather good in my opinion (but I am not English mother tongue, though). However, I noticed some oddities, like that many times an article is missing in the text between the verb and the object. I wonder if this is something one can find in the Polish language.

Specific Comments

In this section I shall point out some critical points by going through the thesis, from the start to the end.

Page 5. Nordhaus did not receive a Nobel prize (only) for IAMs.

Page 7. "As assuming some adaptation and shifts in the agricultural production structure, this impact can be positive, I decided not to include this in the model." First, the impact itself should be taken ceteris paribus, adaptation is a consequence of the impact, not part of it. Second, you exclude it because it could be positive? That's silly. Indeed, some studies suggest that more CO2 could increase soil fertilization.

Page 8. "The probability of such extreme outcomes lead to the conclusion, that we should do our best to reduce greenhouse gas emission regardless of the cost-benefit analysis". So, what is your economic assessment for?

Page 12. Mitigation policies. The whole section hardly fits into the thesis. Most of the studies cited there do not explicitly address climate change mitigation. They would rather consider some sort of "green infrastructure" investment, possibly motivated by other reasons, and also use methodologies and approaches which are difficult to contrast.

Page 18. Channels of impact. Why should impact on agriculture be the "first" one?

Same page. "heat waves and resultant heat stress". Serious mistake waves and stress are NOT the same thing!

Page 19. "Although there will be some reductions in the demand for heating, especially in cooler climates, it will be outbalanced by the increase for air conditioning (Davis and Gertler, 2015)". This statement is unwarranted. I did some studies on the subject and I found the opposite.

Same page. Trade patterns are NOT the same as international transport costs.

Again. "Changes in trade patterNs will be required by more intensive migration". This sentence means nothing.

Again. "In general, the number of channels through which climate change will affect economy is vast and it is difficult to enumerate all of them. However, more elaboration on the societal and economic impact of climate change can be found in Carleton and Hsiang (2016)." This is useless.

Again. "On the other hand, general equilibrium analysis can be used". Why on the other hand?

Page 20. "Another example is the impact of restricted water", which has nothing to do (directly) with climate change. Afterwards, "the" CGE model. There is no single CGE model. Also, repeatedly in the following.

Page 21. "climate change is the redistribution from rich to the poor". Not "is the" but "causes a". Redistribution of what? Who are the rich and the poor?

Same page. "Dennig et al. (2015) argue that if poor are to be affected by climate more than rich, thEn mitigation effects should be intensified". Meaningless.

Page 22. "Food crises will affect the child malnutrition in poorer regions of the world (see Arndt et al. (2012) for the analysis of Mozambique and Campbell et al. (2009) for Bangladesh) and migrations (De Brauw, 2011). Political disruptions and riots in low-income countries resulting from decrease in the availability of food will exert pressure on migrations to richer countries, threatening the security there (Berazneva and Lee, 2013)". No ground for these statements. Cheap talk.

Page 23. Damage functions do not necessarily affect GDP (directly).

Same page. "the exponential abatement cost curve was implemented". Ugly.

Same page. "Also, it does not take into account the potential of catastrophic damages and extreme events that are caused by climate change. As the impact of climate change on the economy is subject to huge uncertainty, this limitations can be considered important.". Catastrophic impacts and uncertainty are NOT synonymous.

Page 25. Climate change in Poland. "Climate change will contribute to the changes in frequency of extreme weather events". Not demonstrated. You cannot start with an act of faith. "lead to losses in production capital, leading to shift in production structure from consumer goods towards investment goods" this is totally unsubstantiated.

Page 26. "Climate models suggest that both the distribution of temperature and precipitation will flatten (e.g. Scherrer et al. (2005)), what means that the probability of extreme observations (both minima and maxima) will increase". I do not see the link.

Page 31. "There are few candidates for climate indices to be linked with losses from extreme events." "These numbers show no correlation with floods" "So I decided to turn to climate indices". You are in the darkness because no data support the hypotheses you want to impose.

Page 32. "I will use maximum monthly amount of rainfall as an estimate of probability of flood". This is a KEY assumption but totally ARBITRARY.

Page 35. "Paprotny and Terefenko (2017) show the potential impact of sea level rise on polish coast. They argue that frequency of coastal floods will increase and 50cm of sea level rise will double the impact of 100-year storm surge event". Potentially dangerous confusion between sea level rise (slow and steady) and floods related to storms.

Page 38. "agriculture conditions in the origin country are important determinant of migrations". Once more, this is just ASSUMED TO BE TRUE, without any critical appraisal. On the contrary, I am not aware of any study demonstrating that climate change (not local droughts, etc.) is a relevant determinant of migrations (so far, at least). This is actually admitted at page 54.

Page 40. "I will describe, how the change in the frequency of floods will impact losses. This is by far, the most important consequence of climate change for the economy in temperate climate zones and for country like Poland". Why should be this one the most important consequence? Under which metric? Who demonstrated this?

Page 42. "According to the estimates by Alfieri et al. (2015b), in no SSP pathway, the damages caused by floods in Poland barely changes between 2020 and 2080." Oops! So what? I think this refer to physical impacts. Yet, it is astonishing to see how you are bending model and data to impose your a priori.

Page 43. "the amount of research on the future influence of storms and flash floods is surprisingly narrow". "it is not possible to build physical model of storms and price inundation depth". So the whole section is based on hot air. I will not comment it any further.

Page 47. "the overall impact of climate change on weather-related mortality is still disputed" "mortality is driven by seasonal factors rather than by the ambient temperature as such". And you are confounding heat waves with changing average temperature. Again, the empirical basis to justify a negative impact is thin, to say the least.

Page 48. Migrations. You starts from "Adam and Eve"...

Page 50. Hurricane Katrina has no clear link with climate change. Furthermore, you are mixing short term displacements with permanent migrations.

Page 51. "positive and statistically significant relationship between temperature anomaly and migration flows". Temperature anomalies are NOT climate change.

Page 53. "The risk of conflict over water will increase as a result of climate change". Unwarranted and unsubstantiated. Water wars are often preached but never empirically detected.

Page 55. "My aim is to use three scenarios". It is not your aim, it is your strategy.

Page 58. "in my view SSP2 is closest to *business-as-usual*". I do not share this view. Furthermore, SSP scenarios do not correspond to RCP, so it is not clear how the two sets are combined here.

Page 59. "losses as a percentage of GDP based on EM-DAT disaster database". It is known that the EM-DAT database is not a reliable source. It is also outdated.

Page 60. "assessment of the direct relationship between annual precipitation maximum in given year and economic losses is not possible". No comment.

Same page. "Changes in hazard are shown in the section 4.2.2, while exposure is the number of people and physical capital accumulated in given area.". Incredibly confused. How do you measure capital stock? How can you add apples and oranges?

Same page. "exposure is the value of capital accumulated in given area, which is proxied by the level of GDP produced in given county, which in turn is estimated using the county share in the revenues from income taxes". Unbearable: capital is a stock, GDP is a flow, shares sum up to unity and therefore cannot represent levels...

Page 86. "The tourism traffic potentially lost was also calculated." In some mysterious way.

Page 89. "Gravity models are very popular tool to assess the scale of migration between countries". I do not think so. They are quite popular to model international trade flows, not migration.

Page 90. Equation 5.1. From where this equation was derived? It looks to me conceptually wrong. Why population in the receiving country should constitute a pull factor (rather than, say, income per capita)? And population in the origin a push factor?

Page 93. "The impact of damages from droughts, storms and floods on migrant stock is in line with theory, but not statistically significant" "I did not found the statistically significant interdependence between mean precipitation or SPEI in origin and migrant stock, what is perhaps the most surprising outcome of this analysis." Not surprising at all to me!

Same page. "other results are relatively clear - extreme mean temperatures in the hottest month (over 30∘C) increases outward migrant stock and decreases inward migration. This can be used as a proxy of inconveniences related to the high temperature or as a proxy of hydrological drought". Wrong. Extreme mean temperature are correlated with economic development. Poorer countries, origin of migrations, are in tropical and arid zones. Nothing to do with climate (directly). Income differentials drive migrations, something completely and surprisingly overlooked here.

Page 94. "Neither losses from extreme events nor population affected exert the pressure on migration strong enough to be detected by the statistical models presented above." No comment.

Page 104. "Some shocks induced by the climate change are deliberately excluded from the analysis due to the huge level uncertainty." Why some shocks are included and others excluded remains a little bit of a mystery. The statement suggests that uncertainty is low in those considered, which is untrue. I already commented that some of those included are not directly related to climate change, whereas others do not have an empirical basis.

Same page. Introducing the DSGE model. First, the author is confounding CGE and DSGE models, which are very distinct classes. The proposed model cannot be even classified as a typical DSGE model (for a cursory description, see: https://en.wikipedia.org/wiki/Dynamic_stochastic_general_equilibrium. You can easily notice that the description does not fit here). More importantly, I claim that the model does NOT describe a general equilibrium. You can talk of a GE if all markets are considered, and the circular flow of income is fully represented, without loopholes. I cannot see the identification of a price

numeraire, which is indispensable, given that only relative prices can be determined in equilibrium. Corollary: the Walras law, which is an essential property of GE, is clearly violated here.

Page 105. "it is calibrated, not estimated in the way that is commonly used in CGE-style modelling." CGE models ARE calibrated.

Page 110. "hiring costs are increasing function of the *labour market tightness*". I take this as an example of a general issue. There is no concept of "cost" in a general equilibrium model, in the sense that if you pay for something, then some other agent in the system must receive that money. This confirms once more that the model proposed here is not describing a general equilibrium.

Page 111. "That is the standard setting used in the CGE modelling, that was adapted for the DSGE modelling". Absolutely not. And where are the primary factors employed for the realization of the final demand goods?

Page 115. "if the decision on the size of *reconstruction investment RIi,t* would be result of standard optimization process, all the investment should be used for reconstructive purposes, infinitely. Therefore, they must be constrained endogenously." I confess that I read this sentence many times and it was just impossible for me to understand, or simply interpret it. What seems rather clear, though, is that the author decided to apply an ad-hoc patch, by imposing yet another arbitrary assumption.

Page 116. "Investment demand is equal to the supply of investment good". I doubt this condition can be applied to an open economy.

Page 117. "That was the source of the steady-state values of different macroeconomic variables". Steady state was not defined nor demonstrated to exist.

Page 119. "Each year, some part of capital is destroyed due to the storms and local flooding and some tiny fraction of land is taken due to the sea level rise.". This is astonishing. Extreme events occur only in some special years and in some specific areas. In my understanding, here it is assumed that an extreme event, with impact multiplied by its probability of occurrence, happens every year.

Same page. "In the other words, instead of shocking just the capital level in given year, they rather decrease total factor productivity. This is in line with empirical evidence on the recovery, following disasters, when destroyed capital stock leads to fall in employment rather than to replacing capital stock but labor, which would be suggested by traditional Cobb-Douglas approach." Capital is complementary to labor. Labor productivity falls (therefore employment) if less capital is available. Labor demand also depend on activity levels, which are of course lower after a negative shock. No need to consider tfp, and "Cobb-Douglas approach", whatever this means, is not suggesting it.

Model results. Given all the deficiencies described above, I do not think it is worthwhile commenting them, as I just do not trust them.

Final Comments

Is this thesis good enough to showcase the skills of the candidate, bringing me to believe that he could successfully undertake a career in research (academic or not)? Yes.

Is this thesis conducive to some advancement in knowledge, fresh insights or results, which could possibly inform actual policies? No. Too many arbitrary assumptions, too many unwarranted claims.

Nowadays, many universities are requiring that doctoral theses should be built as collections of peer-reviewed, published or potentially publishable papers. If this thesis would have been structured this way, perhaps the candidate could have received the comments I made much in advance, by other scholars. Admittedly, he made a tremendous volume of work, but without receiving a constant feedback about its quality, the big effort has turned out to be quite wasteful and unproductive.