Report on **Master Thesis**

Institute of Economic Studies, Faculty of Social Sciences, Charles University

Student:	**Mgr. Alžběta Křížková**
Advisor:	**Prof. Tomáš Havránek**
Title of the thesis:	**Marginal Abatement Costs of Greenhouse Gas Emissions: A Meta-Analysis**

OVERALL ASSESSMENT

Short summary

Estimates on marginal abatement costs of GHG emissions from 59 studies are reviewed, using a meta-analysis approach. The study follows and extends Kiuik et al. (2009) meta-analysis, extending the period till 2021. Overall, 242 estimates were collected and 135 and 107 were included in the analysis of MAC for 2030, and 2050, respectively. The thesis primarily aims at the publication bias, following Stanley (2005), relying on approximation of SE, following Havranek et al. (2015). Then, the study is aiming at heterogeneity of the MAC estimates with respect to study characteristics, exploring the association between MAC estimates and these characteristics.

Contribution

The objective of the study is very important and the study outcome is greatly policy-relevant. I like the topic and the study very much. Alhough, there have been published several meta-analyses on MAC of carbon emissions, most of them are quite outdated or specific to IAM modelling framework. Therefore this thesis might greatly contribute to the literature and fill this gap. The study also contributes to this subject by exploring the most recent method refinements how the meta-analysis shall be performed.

However, I have some doubts about the used approach that makes its contribution likely problematic, see below (Methods).

Minor comment: I do not share authour‘s claim that the main objective of this study is „estimating the true effect of the MAC.” First, I doubt whether the presented study may in principle say anything about the „true“ effect, and, secondly, this study is not analysing the effect, rather it examines the association between the MAC estimates and the study characteristics. Still, analysing this association is very useful.

Methods

The core of the method presents the state-of-the-art meta-analysis, following very recent method updates, developed by the team around student’s supervisor, prof. Tomas Havranek. This is great, the MA method is appropriate, well-performed, and I do have nothing to add at this point. In the case there are no concerns, as described just below, I would grade the method by full points.

However, I have serious concerns about author’s understanding the key problem, or in other words, what is the object of the meta-analysis, and, in particular, how the impact assessment modelling actually works, what are the assumption of various models, and how MAC are determined. Here are my concerns, written point-by-point:

- The cost of emission abatement can be in principle quantified using either a bottom-up simulation model (e.g., LEAP, GAINS) or (cost) optimisation model (like TIMES, MARKAL), or on

Report on **Master Thesis**

Institute of Economic Studies, Faculty of Social Sciences, Charles University

Student:	**Mgr. Alžběta Křížková**
Advisor:	**Prof. Tomáš Havránek**
Title of the thesis:	**Marginal Abatement Costs of Greenhouse Gas Emissions: A Meta-Analysis**

the other hand by a top-down macro-structural economic model, including a CGE, a macro-econometric, or a dynamic IO model. Typically, present value of total costs (investment, O&M, fuel, and regulation costs) and/or annualised total present value of the costs (analyst's choice on the discount rate matters also here) are derived by the former type of the models (i.e. the bottom-up models), whilst the latter-type top-down models typically derive the costs through the endogenosuly determined impact on GDP (or welfare). Following the static efficiency of market-based instruments, the MAC may be also derived as implicit carbon tax (or price of emission allowances) that is endogenosuly determined by the economic model, like, for instance, in Scasny et al (2015). Now it should be more clear why MAC's coming from the bottom-up models are higher; it is just matter of the fact that the cost-optimisation outperforms a simulation wrt the costs and a model built within a general-equilibrium framework usually outperforms the bottom-up simulation, optimisation, or partial-equilibrium models. One need not carry out a meta-analysis to get what is an inherent feature of the modelling, unless one acknowledges this fact and aims specifically at this modelling feature (what is not done in the presented thesis). The thesis also ignores the economics behind the impact assessment modelling and seems to run the MA mechanistically, without thinking about the problem (i.e. the impact assessment) and the model features, for instance, whether MAC comes from a cost optimisation, neoclassical GE framework, or a post-Keynesian model that allows imperfect situations and more flexible adjustments to exogenous shocks and/or regulation and hence implies (inherently) lower regulation costs. In sum, the outcome from the IA model is not only conditional on the model assumptions and model structures, but also what is actually endogenous in the model, i.e. whether it is the total costs or general-equilibrium inputs and outputs (hence GDP); let me here then raise a question whether is it reasonable to put to one MA model both present value of the costs (investment, O&M, fuel costs, etc.) that is a typical outcome from a bottom-up model, the differences in GDP levels that is a typical outcome of a top-down model, or the implicit carbon tax that may be derived from both. Moreover, impact assessment may assume the cost-effectiveness (i.e. the least cost solution for pre-defined emission target), while the other IA studies represent rather the benefit-cost analysis that is carried out within the optimality set-up. The MAC from these two different set-ups will be very likely different (what is hideen in the presented meta-analysis). In sum, understanding the problem (what is the subject of MA) is at least as much important as properly performed MA (and it is true not only when one needs to interpret the results from of MA).

- The core of the main part in the MA relies on the approximation of SE, based on Havranek et al. 2015. I have again a concern about this approach and in my opinion what has been presented in this thesis is not appropriate. Let me explain why. First, the approximation of SE, as done in Havranek et al. 2015, was appplied for Social Cost of Carbon (SCC) estimates drived from the Integrated Assessment Models like DICE/RICE, FUND, or PAGE. In fact, relying on IAM, the SCC may be derived from deterministic approach or stochastic modelling when the latter would require runing the IAM model for several times (typicaly several hundreds times) with varying the key model assumptions. Then the statistical inference of the SCC estimate can be indeed derived. However, modelling the impact of emission abatement by energy system optimisation or by macro-structural models is very different – first, typically, stochastic modelling is very rare here, and a major part of the model outcomes is just deterministic (yileding a fixed number), deriving the value from the endogenously determined outcome variable. True, several outcomes for the costs or GDP loss are often reported in the same IA study, however, this set of model outcomes (I am not saying the „estimates") can be hardly considered as an input for the

statistical inference. These outcomes are just model results when the model was ran for several times assuming different assumptions like on fuel costs, availability of certain technologies (such as nuclear, CCS, CCU, etc.) and/or fuels. It implies the outcome is conditional on the chosen assumptions and model runs. In fact, this set of model outcomes is matter of interpretation in order to discuss pathways and trajectories under different 'worlds', but these outcomes, in my opinion, can't be put to one basket and then use this basket for manipulating the statistical inference (since there is often even no mean). Maybe, I did understand the used approach incorrectly (since a description of the manipulation is not provided in detail even in the referenced original paper on SCC), and if it is so, I would appreciate deep discussion about the issue during the defence.

- It is a well-known fact that the costs of regulation are primarily dependent on stringency of the regulation – the more stringent regulation, the more abatament and the higher cost of abatement is. It is also well-known fact that the MAC curves follow typically an upwarding slope with respect to the level of abatement, since at the beginning relatively cheap abatement options are utilised. And it is also true that assuming more expensive tech's like CCS would imply higher MAC. It would be therefore useful to investigate the slope of the MAC surve (or fiting the relationship between MAC and the level of emission abatement) rather then to control for these well-known facts using a dummy in the MA.

- The study use the stabilisation targets as one of the controls. As done I consider this approach problematic and marginally useful for the following reasons.
  First, For not only these reasons, I found problematic, say me this straight, I think it is wrong how the stabilisation targets for the reviewed studies were computed from Table 2.1. As noted by the author „All the collected targets from studies were converted to ppm/CO2-eq based on the conversion table 2.1" (page 10). I think, IPCC (2014) does not provide a table for „a conversion". In fact, GHG emissions will increase carbon concentration (that is subject of non-economic modelling), and this consequently causes global warming effect, reflected very likely in excess of energy in the atmosphere and likely increases in global mean temperature, represented more recently in *Representative Concetration Pathways*, all by the end of this century, i.e. by 2100. What IPCC reports is just the targets by the end of the century, by 2100, while the studies that were included in the meta-analysis, derived the MAC by 2030 and 2050, not around the year 2100 .
  Second, most of the impact assessment studies, especially the ones that relied on the bottom-up model and some that aimed at the impacts at local level (for a sector, single country) are quantifing typically the costs for carbon emission reductions, while the studies carried out regionally or globally may analyse the costs for Representative Concentration Pathways (i.e. one possible representation of stabilisation targets), with endogenously determined CO2 emission levels (and hence emission abatement), see, for instance, Scasny, Massetti, Melichar, Carrara (2015). Since both bottom-up and top-down models usually report carbon emission pathways, and even for practical (policy) purposes, it would be better to express the MAC per carbon emission abated and work with GHG emission target (not with the stabilisation targets).
  Last, carbon concentrations in atmosphere and hence the stabilisation target might be impossible to determine for a local policy, like climate change mitigation in Central Europe.
  In sum, to make the IA studies consistent and comparable I suggest defining the target by GHG emission reduction.

Student:	**Mgr. Alžběta Křížková**
Advisor:	**Prof. Tomáš Havránek**
Title of the thesis:	**Marginal Abatement Costs of Greenhouse Gas Emissions: A Meta-Analysis**

- Controlling for the baseline might be tricky as well since the baseline, as defined in the study, actually represents a BAU scenario and as such it comes from the model as well (actually from the same model and model assumptions as the outcome for a counter-factual scenario is coming from as well). Is certain treatment of this issue needed?

- It seems the MA reviewed nation-wide studies and studies on agriculture only. If it is so it shall be more clear highlighted. It is known that abatement potential and hence MAC vary a lot across industry (power, heat, transportation, energy-intensive industry, and so on), across consumption domains in household sector (building, transportation, diets) and over time (with delayed or immediate action). I was not able to find what is the objective of this review, in other words whether the meta-analysis on MAC aimed ate specific industry or sector or whether it targetted rather nation-wide policies. This shall be clarified and discussed, especially when summarising and concluding the results. Ideally, the sector(s), industry, timing the action shall be all controlled in the MA.

Literature

I put to students attention a review of MAC by Carraro and Favero (2009) that is missing as well a work by ICCGov (International Center for Climate Change) that performed previosuly a comprensive meta-analyses of several impact assessment models.

For some reason „domestic" research (that is conducted in the Czech Republic or by researchers based in the Czech Republic) on this subject is ignored. The costs of GHG emission abatement were quantified for the Czech Republic by a CGE model (Kiulia et al. 2019), by a macro-econometric model (Scasny et al., 2009), and by an energy system model TIMES (Recka and Scasny, various years). Researchers based in CZ quantified the costs of abatement by WITCH model (Scasny et al., 2015) or a hybrid CGE model (Miess et al. 2022, also as IES WP 2018-16). And a review by Carraro and Favero, as mentioned above, was published in CJEF in 2009.

It seems the search querry might be therefore improved (adding, for instance, also „climate change mitigation", „IAM", „GDP impact",…). I am convinced there are much more studies, particularly based on CGE model or energy system optimisation model, that can provide the costs of carbon emission abatement for the meta-analysis (in particualrly I doubt that since the Kuik et al. study, MAC have been derived by a CGE model in only a few papers, as noted at page 21).

Still I found acceptable for the purpose of student's thesis the number of studies that have been found and included in the review. My comment would be only relevant if the student intends to improve her work in order to prepare a manuscript for a journal submission.

For these reasons, I downgrade this criterion by 5 points only.

Manuscript form

The thesis has a standard, logical structure. The text refers to tables and disposes with a complete bibliography. Appropriate language and style are used.

Report on **Master Thesis**

Institute of Economic Studies, Faculty of Social Sciences, Charles University

Student:	**Mgr. Alžběta Křížková**
Advisor:	**Prof. Tomáš Havránek**
Title of the thesis:	**Marginal Abatement Costs of Greenhouse Gas Emissions: A Meta-Analysis**

Minor comment: There are some typos or mistakes, like, for instance, Kiuik et al analysed the studies published before 2006, not as written [they] worked with studies from 2016 (page 15).

Overall evaluation and suggested questions for the discussion during the defense

This thesis is written in very high standard. The meta-analysis is performed very well. However, I have several concerns about the understanding the way how the MAC are actually determined by the impact assessment bottom-up and top-down models, which have consequences on how the meta-analysis might or shall be performed and, in particular, how the results from the meta-analysis shall be interpreted. Despite my quite long list of critical points, I consider this thesis valuable and of very high quality considering how the meta-analysis is techncially performed.

Questions for the discussion during the defense. Please, discuss the following points during the state defense:

1) How the MAC for GHG emission abatement is estimated by bottom-up and top-down models and how this approach (see detailed description above) might be reflected in a meta-analysis?

2) Is the approximation of SE, as done in the thesis, appropriate approach when MAC are derived by cost-optimisation, simulation, or top-down macro-structural economic models? Can the analysis of the publication bias rely on such approximation? Please, discuss.

3) Explain and justify your approach to derive the stabilisation target for the reviewed studies, based on Table 2.1. Is it appropriate and correct?

4) The baseline emission levels are determined for the BAU scenario by the same model as the emission levels for the counterfactual scenario. Discuss whether it is necessery to deal with this issue in the MA.

5) Since MACs vary a lot across sectors and segments (and over time and abatement plans), it is important to highlight for which sector or industry the analysis is relevant. Please, clarify the objective of your study.

In my view, **the thesis fulfills the requirements** for a master thesis at IES, Faculty of Social Sciences, Charles University, **I recommend it for the defense** and suggest **a grade B**.

The results of the Urkund analysis do not indicate significant text similarity with other available sources.

Report on **Master Thesis**

Institute of Economic Studies, Faculty of Social Sciences, Charles University

Student:	**Mgr. Alžběta Křížková**
Advisor:	**Prof. Tomáš Havránek**
Title of the thesis:	**Marginal Abatement Costs of Greenhouse Gas Emissions: A Meta-Analysis**

SUMMARY OF POINTS AWARDED *(for details, see below):*

CATEGORY		**POINTS**
Contribution	*(max. 30 points)*	30
Methods	*(max. 30 points)*	20
Literature	*(max. 20 points)*	15
Manuscript Form	*(max. 20 points)*	20
TOTAL POINTS	*(max. 100 points)*	**85**
GRADE	**(A – B – C – D – E – F)**	**B**

NAME OF THE REFEREE: Milan Ščasný

DATE OF EVALUATION: 8 June 2022

Mgr. Milan Ščasný, PhD.

Digitálně podepsal Mgr. Milan Ščasný, PhD.
Datum: 2022.06.08 15:33:11 +02'00'

_____

Referee Signature