

From: KENNETH JUDD [mailto:kennethjudd@mac.com]
Sent: Sunday, November 30, 2014 10:58 PM
To: James Heckman
Cc: Lars Peter Hansen; Yongyang Cai; Thomas Lontzek
Subject: JPE Co-Editor Rejects Cai-Judd-Lontzek Submission

Jim,

JPE rejected the Cai-Judd-Lontzek paper, a paper that you seemed to like.

The primary reason was that we did not obey the JPE requirement that we choose one set of parameters and present the results for that case. JPE told us that calibration is the only acceptable view of how to do research, and criticizes us for considering the uncertainty about model parameters. should be acknowledged.

“Both reviewers ... recommend rejection. Both believe that the paper does not have a clear economic message that the reader can take away. Reviewer 1 says that by covering all possible constellation of parameters, the paper manages to obtain “any possible carbon tax value.” Reviewer 2 writes that the paper stands as an “impressive black box.”

This summary is factually false. We did not try “all possible” parameters, nor did we get any possible carbon tax for the initial period, ~2010. It is true that the future path of the optimal carbon tax does cover an enormous range of values, but that is true for any parameter vector. This arises because the optimal carbon tax is strongly related to the marginal utility of consumption, an economic value which has enormous uncertainty simply because of the long-run risk specification of productivity, chosen to roughly match the empirical literature’s view on consumption volatility.

“When you submitted [the first revision] to the JPE, I did not send the draft back to reviewers but gave you my own long list of comments on the draft instead. I was hoping that we could settle down on a specification of the model and a set of parameter values that provide a reasonable description of dynamics. Moreover, I was hoping that the improved exposition would make it easier for

reviewers to appreciate the exercise.

The reviewers do acknowledge that the exposition has improved a lot. Still, the process has not lead to a draft that convinced them.”

Why are they not convinced by the revision? At one point, we included a procedure that provided error bounds on the key results, in particular the optimal carbon tax. We were told that any numerical innovation would be ignored by JPE in its decision regarding this paper. We were not allowed to provide all the information we had that would help convince a reader of the accuracy of our results. Using sound computational techniques and error bounds is not valued by JPE; instead, the way to convincing people is to come up with some “intuition”.

More generally, JPE’s treatment of this paper has been horrendous. This was a conference issue, but we had to wait for 11 1/2 months to get even the first comments. Here is an example of JPE’s first comments:

"Why do you use a production economy? The utility function is over labor, but DSGE models have standard problems with labor markets. Why is this it important to include it?"

The purpose of this literature, going back to Nordhaus’ initial papers over 20 years ago, is to quantify the impact of climate change on economic output, including the impact to capital accumulation. However, we were criticized for using a production economy. Apparently, JPE demands that any model with production and productivity shocks must account for labor market frictions.

Here are some gems from JPE’s July 2 comments on the first revision, which we sent on July 1.

We used many computational innovations in this paper, and wanted to describe them to give the reader confidence in our results. Instead, we were told:

"If the JPE publishes this paper, it will be because of the economic lesson, not the new model or the computational techniques. Both are very much appreciated, but they are not what will get this paper into the JPE."

We wanted to describe how our work fits into the larger literature on Integrated

Assessment Models (IAM), how it is related to Nordhaus' DICE, and to brand our model as "DSICE". JPE declared:

"There should be no acronyms in the paper. I have complained about acronyms before, and I am puzzled by the lack of reaction. If you want to write a paper about DICE, DSICE, IAM, SCC, IPCC, you will need to send the paper to a different journal. The JPE reader wants to learn about content -- what these models do, whether they are neoclassic growth models with climate externalities or something else -- not labels."

The demand was so emphatic that we dared not refer to Gross World Product as GWP. We have seen the accepted versions of some of the other papers presented at this conference. They were allowed to use acronyms. Many things tell us that JPE was against publishing that paper, but making a silly demand of us but not of others clearly and decisively shows that JPE never had any intention of dealing with our paper in a fair and unbiased manner.

We calibrated a production process so that the resulting consumption process would be similar to recent empirical analyses of consumption processes. JPE disliked the fact that we had a deterministic trend:

"I would like to see a version of the model without deterministic trend. Will that version produce much higher social costs? Intuitively, I would think that uncertainty about long run growth matters. By having a deterministic positive trend, a bit of the uncertainty is lower -- we already know we are growing. What happens if we eliminate it?"

The fact is that the consumption process that we were trying to match has a deterministic trend. We asked Lars to explain this comment. He also did not understand the point.

We were also surprised to learn that a JPE paper must be accessible to second-year PhD students in any field:

"You really need to work to make this paper readable for the general audience of the JPE. That audience includes IO economists, theorists, labor economists etc. I warmly recommend asking a second year PhD student (or, in fact, several students) for comments, otherwise authors + editor will be exhausted by the end of the process."

We did have PhD students look at this, and they had no problem. Of course, they are attending the University of Zurich.

According to this process, it is clear that JPE demands that people follow a calibration approach to quantitative analyses such as ours. Such a strict policy must be something that the Editorial Board has agreed on. I would like to know why people like you have agreed to a hardcore calibration ideology.

Perhaps this is a case of where a Co-Editor is expressing their personal preferences and that JPE gives Co-Editors “autonomy”. If so, then some Co-Editor, like you, could exercise his autonomy, solicit our paper, and handle it in a competent review process. In either case, the Editorial Board of JPE cannot escape responsibility for this. The primary issues are over basic definitions of what is demanded of a JPE paper, and if JPE does not reverse this decision then it has emphatically embraced the calibration religion to the exclusion of methods that incorporate parameter uncertainty into a quantitative study.

I will put together a more detailed list of responses to JPE and criticisms of its “scientific” policies, and send them to the full editorial board. I will also send the material to Sam Kortum, who represented JPE at the conference and never informed us of the JPE rules against acronyms, the JPE demand that authors follow a calibration procedure, and many other criticisms made by JPE.

I will also point to the positive reception to our paper at the conference. It appears that JPE searched for referees who could be counted on to be negative regarding this paper, rather than utilize the large number of people who were at the conference, clearly understood the paper, and were supportive of work on the problem we were studying. You could even examine those reactions at http://www-2.iies.su.se/Nobel2012/video_climate_Lontzek.html

Due to other activities, I will likely do that next Monday unless I hear that you and Lars have convinced JPE to reverse the rejection.

I have been the one who has insisted on including uncertainty quantification (a.k.a., sensitivity analysis) in the paper. However, Yongyang’s and Thomas’ careers are more important than my views on what constitutes intellectually responsible analyses of quantitative policy questions. If JPE insists on a pure calibration approach, then Yongyang and Thomas will be quite happy to have a

conversation with JPE about what that parameter vector should be used in each of the models that are examined.

Sincerely,

Ken

> Begin forwarded message:

>

> From: Thomas Lontzek <lontzek@googlemail.com>

> Subject: Fwd: Your JPE Submission

> Date: November 20, 2014 at 5:09:04 AM PST

> To: "Kenneth L. Judd" <kennethjudd@mac.com>

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> Anfang der weitergeleiteten Nachricht:

>

>> Von: "Journal of Political Economy" <em@editorialmanager.com>

>> Betreff: Your JPE Submission

>> Datum: 19. November 2014 21:08:33 MEZ

>> An: "Thomas Lontzek" <thomas.lontzek@business.uzh.ch>

>> Antwort an: "Journal of Political Economy" <jpe@press.uchicago.edu>

>>

>> Ref.: MS 2012606R2

>> The Impact of Economic and Climate Risks on the Social Cost of Carbon

>> Journal of Political Economy

>> Nov 19, 2014

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>> Dear Ken, Thomas and Yongyang,

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>> Enclosed are two reports on your paper "The Social Cost of Stochastic and Irreversible Climate Change." The two reports are written by Reviewers 3 and 4 in the electronic system (labeled Reviewers 2 and 3 in my previous letter); they also wrote reports in the previous round. I had also sent the paper to Reviewer 1 but have not heard back from him/her. He/she was very negative in the first round, so I believe that it is unlikely that he/she will change his/her mind and therefore believe it is ok for me to go ahead with this decision.

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>> Both reviewers who wrote reports are negative and recommend rejection from the JPE at this stage. Both believe that the paper does not have a clear economic message that the reader can take away. Reviewer 1 says that by covering all possible constellation of parameters, the paper manages to obtain "any possible carbon tax value." Reviewer 2 writes that the paper stands as an "impressive black box." Both conclude that the paper cannot be published in a top econ journal.

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>> In my first decision letter on this submission, I wrote that I was "not optimistic that this process will end well. The initial condition of this paper is just too far away from anything that the JPE would publish. I therefore would understand if you decide to send your paper to a journal with a readership that is more familiar with these models. If you do decide to resubmit, please include responses to the reviewers. A successful revision would have to convince them as well."

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>> Between writing the first letter and this letter, we met to talk about how to rewrite the paper. When you submitted a draft to the JPE, I did not send the draft back to reviewers but gave you my own long list of comments on the draft instead. I was hoping that we could settle down on a specification of the model and a set of parameter values that provide a reasonable description of dynamics. Moreover, I was hoping that the improved exposition would make it easier for reviewers to appreciate the exercise.

>>

>> The reviewers do acknowledge that the exposition has improved a lot. Still, the process has not lead to a draft that convinced them. The three reviewers are all experts on representative agents with Epstein-Zin utility who face some kind of long-run uncertainty. My expectation was that they would appreciate the application to climate change, which is obviously important, and they did. However, they did not see a concrete message or conclusion from this paper. If none of these reviewers (who should be positively inclined toward the paper) can see it, I have to assume that the broad JPE readership will share their views.

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>> Again, I am sorry the JPE has not worked out for this paper. After two rounds of revisions, I know you worked hard to improve this manuscript, and this outcome will be frustrating for you. All I can hope is that the process has improved the paper so that it will soon be published in another good journal.

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>> Thank you for your patience and for working with me.

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>> Best regards,

>> Monika

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>> Reviewer #3: This report was sent as an attachment file. You can view it by logging on to the JPE Editorial Manager system as an author at <http://www.editorialmanager.com/jpolec>

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>> Reviewer #4: This report was sent as an attachment file. You can view it by logging on to the JPE Editorial Manager system as an author at <http://www.editorialmanager.com/jpolec>

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