September 29th, 2020

Dr. Val Snow

Editor in Chief

Agricultural Systems

Dear Dr. Snow,

We are writing you with respect to the paper entitled “**THE EFFECT OF CLIMATE VARIABILITY ON COLOMBIAN COFFEE YIELD: A DYNAMIC PANEL MODEL APPROACH**" (Manuscript ID: AGSY\_2020\_203). We are very thankful to you and the three referees for the feedback you provided on the previous versions. In our efforts to address all the points that were raised we ran again all our models in the STATA statistical package and discovered that some lines of coding deserved to be updated. We report below the old and the new versions of these codes. The new codes calculate GMM estimates that are more robust by defining instruments for all, not only some, covariates including the location specific fixed effects. Recent developments in STATA’s xtdpdgmm package recommend this process (See slide 9, Kripfganz, 2019). The updated estimates appear in table 2 of the manuscript. Their sign is consistent with the estimates reported in previous versions of this manuscript but their magnitude has changed, hence forcing us to update the results interpretation and forecast in the current version. We wanted to make sure you and the referees are aware of the origin of the changes you will observe.

We have addressed in the current version all the points that were raised so we provide you below with the details of our revisions. We thank you in advance for taking the time to consider our work. Note that we make the data and the codes available in the following github repository (<https://github.com/f-ceballos-s/CChange>) for transparency and replicability purposes.

Furthermore, we have expanded the projections to include other Representative Concetration Pathway Scenarios, providing estimates for RCPs 2.6, 4.5, and 6.0. As a consequence we have updated our abstract, highlights, manuscript and appendix. We hope this comprehensive analysis would improve the quality of our paper.

Kripfganz, S. (2019). Generalized method of moments estimation of linear dynamic panel data models. In *London Stata Conference 2019* (No. 17). Stata Users Group.

Old code:

xtabond2 v\_cafe L.v\_cafe tmean\_mar tmean\_mar2 tmean\_aug tmean\_aug2 prec\_mar prec\_mar2 prec\_aug prec\_aug2 c.tmean\_mar#c.prec\_mar c.tmean\_aug#c.prec\_aug alt\_mn ,gmm(L.v\_cafe) level robust

New code:

xtdpdgmm v\_cafe L.v\_cafe tmean\_mar tmean\_mar2 tmean\_aug tmean\_aug2 prec\_mar prec\_mar2 prec\_aug prec\_aug2 c.tmean\_mar#c.prec\_mar c.tmean\_aug#c.prec\_aug alt\_mn, gmm(L.v\_cafe, lag(1 6) m(l)) iv(tmean\_mar tmean\_mar2 tmean\_aug tmean\_aug2 prec\_mar prec\_mar2 prec\_aug prec\_aug2 c.tmean\_mar#c.prec\_mar c.tmean\_aug#c.prec\_aug, m(d)) iv(alt\_mn, m(l)) vce(robust)

***Reviewer #1: I am globally satisfied with the way the comments and suggestions were taken into accounts by the authors. Still, there remains some minor concerns that were not addressed.***  
  
1- The publication date for the Peres et al. reference appears in the list of references but not in the full text.

The publication has been removed from the text as it was not needed.

2- Table 1 still contains too many significant numbers. Precipitations are measured in mm, not in 1/100th of mm. At best, temperatures are measured in 0.1C, not in 0.01C. Elevations are measured in m, not in 0.01m.

The decimals have been adjusted accordingly.

3- Figure 1 remains very hard to read. I believe that the editors will point this out further in the publication process. I suggested to introduce a small gap between data points to ease the reading (as it is the case in Figure 4), rather than deleting the confidence intervals altogether. I was also suggesting to put yields on a separate graph rather than overlap the data, as it is presently tedious to check which data point represent climatic data and which data point refer to yield.

Figure 1 has been modified accordingly.  
  
Additional comments  
4- L.183: change "CRL" by "CLR"

Done

5- the other reviewer seems to be more knowledgeable about modelling. His point on cross-validation is particularly important. However, the authors have not provided the recommended scatterplot. The R2 value for the maximum likelihood model seems low. I don't have much expertise on this but, if it is similar to a previous work, that should be ok.

True. The current version reports both the root mean square error (RMSE) and the adjusted R squared or R\* values at the bottom of table 2.

6- L. 339-341: I think that the authors have mistaken March and August, and temperatures and precipitations. In Table 2, 8.5E-4 refers to "Mean temperature in March x altitude" and not to "Mean precipitation in August x altitude". Similarly, -7.4E-4 refers to "Mean temperature in August x altitude" and not to "Mean precipitation in August x altitude".

Correct. We modified the text accordingly.

7- Authors should avoid bottom page notes. Either is it important and it should be part of the text/legend (in the case of a table or figure), or it is not important and it should be deleted.  
  
All the footnotes are now part of the text body.  
  
***Reviewer #2: I acknowledge the authors efforts in revising their paper however I admit I am confused about their response to the suggestions about the model performance metrics and still find the statistics reported difficult to follow. Below I outline my concerns again and respond directly to the authors responses. I don't find the authors statement that "complete results can be obtained from the authors upon request" satisfactory. I believe the results could easily be summarised and added to Table 2/3. This is the second time that additional results have been requested, shown only partially in the response to the review from the authors, but then not actually put in the paper.***

The current version reports both the root mean square error (RMSE) and adjusted R squared or R\* values for each model. These figures appear at the bottom of table 2.

- "R2 values reported for the Maximum Likelihood models (columns 1 and 2). They are  
0.077 and 0.329 respectively. As indicated on lines 274-275: "The fit of our maximum  
likelihood models is comparable to the one of Sachs (2015) who reports an R2 = 0.383."  
  
Ok, but Sachs (2015) isn't actually a peer reviewed scientific publication as far as I can tell, so I don't think this actually backs up the results that well

True. The lines above have been removed from the text.  
  
- "R2 values do not exist for GMM-based models. As such, we have reported AIC (Akaike  
Information criterion) / Moment Selection Criterion AIC values for all models."  
  
Then why were R2 values reported for GMM-based models in the original version of the paper? See Table 3 in an earlier version of the paper which gives R2 values of 0.321 and 0.333 for the GMM models?

The original version of the paper reported R\* values. Indicating R2 values was a typo. R\* correspond to the degree of correlation between expected and observed values of the dependent variable. R\* is now available at the bottom of table 2 for the GMM models while adjusted R2 is available for the LSDV model. R2 is not reported for GMM because, unlike LSDV, it is not an estimation strategy based on minimizing the residuals (Cameron and Trivedi, 2009).

Cameron, A. C., & Trivedi, P. K. (2009). *Microeconometrics using stata* (Vol. 5, p. 706). College Station, TX: Stata press.

* Further, if an RMSE value can be calculated then I don't know why an R2 value can't be. They are just derived from the predicted and observed values

RMSE are now reported for all models.

* AIC values cannot be compared across different model structures. The way they are presented currently is meaningless. Lowe AIC values within the same model structure (e.g. OLS) with different predictors would indicate a 'best' model.

True. AIC has been removed from the analysis.  
  
- "Finally, we present here another tool to confirm further the suitability of our GMM  
models (the dynamic approach) compared to the standard static models as seen in Gay et  
al. (2006), Schroth et al. (2009) and Sachs (2015). The table below reports the RMSE of  
the out of sample prediction of each year (the model is thus estimated over the remaining  
years only). Recall that a lower RMSE indicates a better fit. Based on these results, it is  
obvious that, for every single year, the dynamic approach leads to a better (more  
accurate) prediction than the static approach would have. This result confirms our belief  
that the static approach would have failed to account for the perennial nature of coffee  
production and we include this in footnote 3 of the manuscript."

These results have been updated.  
  
- Footnote 3 and 4 are confusing also. I don't think that just because your results don't line up with a previous study is any grounds to disregard them. Mosquera Sanchez (2005) is an experimental study, while yours is a larger scale provincial level study. DaMatta et al. (2007) is a review paper, not an empirical study. A temperature of 16.7 seems well within the error margin in presented in Figure 3? I don't understand then why this is justification to favour the dynamic model. This seems an arbitrary decision.. I don't think any papers have actually clearly empirically demonstrated an optimal temperature for arabica productivity - so maybe the figure of 16.7 you find is actually quite close!

You are right. Your setting and sample are quite different from these studies. We have decided to remove the direct comparison with their work. In addition, we explain very clearly above and below table 2 the reasons why the dynamic models should be favored and we include the proof of omitted variable bias in Appendix 2.

- Finally, it is concerning that the parameter estimates between models vary so dramatically in Table 2. Even between the two GMM models (4) and (5) there are great variations in both the sign and magnitude for several of the predictors. Interpreting the significance of these differences is impossible without clear model performance metrics. This raises concerns about the validity of the forecasting done in the other sections of the paper.  
  
This comment was key for us as it pushed us to go back to our codes and allowed us to discover their updated version, as indicated on page 1 of this answer letter. So thank you. The variability in the models’ estimates has reduced considerably in the current version.   
  
***Reviewer #3: I acknowledge the effort of authors in revising this manuscript. My apologies that you might not see my comments in the previous PDF version. Below is some remaning concerns and suggestions. I would consider this manuscript suitable for publication after receiving the updated version with all of these comments being addressed:***  
  
1. Highlights:  
+ Almost all highlights are more than 85 characters per each. Please check with the journal's guidelines.

Thank you. It has been corrected.

+ Highlight #3: Forecast based on future climate data INDICATES that

Corrected.

+ Highlight #4: "Productivity will increase by 24% in high-altitude municipalities…": Is it 67% in high altitude municipalities as in the body text?

A typo. Sorry. It has been corrected.

2. Introduction:  
- Line 11-14: I'm not sure why you cited Schroth et al. 2009 here because this study (as provided in the Reference list) is not relevant to your figures (eg. 34% for Mexico and 20% for Brazil). Also, studies of both Gay et al. 2006 and Schroth et al. 2009 are for Mexico, not for Colombia. So you can't say "they forecast an increase between 4 and 24% in yields" for the case of Colombia because I see that Sachs et al. 2015 is the only study that estimated yields for Colombia. I suggest to remove Schroth et al. 2009 if it is not the correct reference and replace Line 14 with something like 'An increase between 4 and 24% in coffee yield in Colombia was forecasted by Sachs et al. 2015'.

Good idea. It has been corrected accordingly.

- Line 14-15: "However, these studies restrict themselves to estimates at the national level" You should re paraphrase this because the study of Gay et al. 2006 is at sub-national level (a state in Mexico).

Corrected.

- Line 36-38: You should specify that "losses in (future) suitability for coffee production".

Done.

3. Abstract:  
- "new climate conditions": suggest to change to "changing climate conditions"

Done.

- "This result implies that Colombian authorities and coffee growers need to develop place-tailored strategies if they want to adapt their production to new climate conditions in the future": suggest to change to "This result implies that place-tailored strategies for coffee production in Colombia are required to adapt to changing climate conditions in the future", for example.

Thank you. Done.

4. Formatting  
- Captions of figures and tables are not consistent.

Corrected.

- Suggest to capitalize Appendix 1, 2, Submodel 1, 2, 3, etc.

It has been done.

- Figure 1 and 3: It is very hard to see the legends as well as information on the Y-axes and data in the graphs.

The legends have been improved for clarity.

- Figure 2: needs to be enlarged

Done.

5. Data and methods:  
- Line 56-60: Were yield data and planted data obtained from the Ministry of Agriculture for 2007-2013 or from Municipal Panel data set including this ministry (Line 91-94)?

The data come from the Ministry of Agriculture. It has been clearly stated in the text now.

- Line 88: "Arabica coffee" (Arabica should be capitalized)

Corrected.

- Line 113: I think you still need to insert citation for the data used in your study (i.e. a reference (Karger et al. 2017?) after CHELSA 1.2)

We added the reference you offered. Thanks.

- Line 119: Suggest to change as your Table caption should be 'stand-alone' (Descriptive statistics of (main variables/ model input used, for example) by altitude group).

It has been changed.

- Line 124: You also need to have references for the data provided in Table 1 (i.e. 2007-2013 data under the Table)

Done.

- Line 183 "CRL": CLR

Done.

- Line 184: "model 4 in the annex": You meant "Appendix 1"?

Correct. Thanks for catching this typo.

- Line 191: "weather IN March and August"

Corrected.

- Line 191, econometric model: Y = f (T, P…): You should still explain the variables following the equations, i.e. T and P are the values of temperature and precipitation in March and August, etc.

It has been done.

- Line 231, 232: or ?

Sorry but we could not understand the meaning of this comment. We assume part of it was deleted when it was communicated to us.

- Line 232: what is ?

Sorry but we could not understand the meaning of this comment. We assume part of it was deleted when it was communicated to us.

- Line 257: Please add "Ordinary least squares" before the abbreviation "OLS"

Added.

- Line 261: "STATA Statistical Package version 15.1" (a reference is needed following this).

The reference has been added.

- You have cited Schroth et al. 2009 quite a lot throughout the manuscript. However, following the reference provided in the Reference list, the version that I found is an abstract in the IOP Conference Series which doesn't have information that was cited in your manuscript (https://iopscience.iop.org/article/10.1088/1755-1307/6/34/342040). Did you use the correct reference? If not, please provide the relevant one.

The reference has been updated as follow:

Schroth, G., Laderach, P., Dempewolf, J., Philpott, S., Haggar, J., Eakin, H., Castillejos, T., Moreno, J.G., Pinto, L.S., Hernandez, R. and Eitzinger, A., 2009. Towards a climate change adaptation strategy for coffee communities and ecosystems in the Sierra Madre de Chiapas, Mexico. *Mitigation and adaptation strategies for global change*, *14*(7), pp.605-625.

- Line 483: I think the reviewer suggested you to mention irrigation and fertilization strategies as other adaptation measures in recommendations in the forecasting section. Your conclusion should not have 'new' information. Thus, you should provide these strategies either in the section prior to your conclusion section, or in both.

We added it on lines 481-494 in the paragraph before the conclusion.

- Line 486-487: I understand that you would like to acknowledge the importance of CLR impact on coffee productivity following the reviewer's comment. However, your recommendation regarding future work on CLR/CBB economic impact is not relevant to your analysis in the body text (i.e. Line 445, development of CLR resistant varieties, Line 182-187, impacts of temperature on CLR, etc.). I suggest to re paraphrase this to reflect your analysis in the previous sections.

Thank you. Following your suggestion we decided to delete this sentence on future work.