

-----Original Message-----

From: XXX YYY<xxx@yyy.zzz>

Sent: Wed, Jun 26, 2013 at 4:35 AM

To: Managing Editor

Subject: Re: Request for final decision for manuscript number 2013_BJEMT_3822

I have carefully read the new version of the paper and, although some minor changes have been introduced, I think that its quality is still too low for publication.

As I have already said about the previous version, I think that the main problems are referred to the model specification, estimation and testing.

Regarding the definition of the considered variables, GDP is first defined as “the growth of real GDP per capita” (p. 5).

Since this variable is transformed with logarithms and differences, attention should be paid to the meaning of the variable $d(\ln \text{GDPP})$ as the growth of the initial variable (rate of growth of the rate of growth (or equivalently acceleration) of GDP).

With regard to the stationarity analysis, there is a sentence according to which the non-stationarity results in “superior correlations” (page 5). This question should be revised (could it be a confusion between “superior” and “spurious”?)

Furthermore, once the ADF test are applied, the conclusion leads to a first difference of the series, but nevertheless this transformation is not included in expression (2) and at the end of page 6 the dependent variable is described as “the real growth of GDP per capita” ignoring the transformations (including the logarithms which appear in the equation).

The estimation results summarized in table 3 include the dummy variable related to the ASEAN membership, even in model 2 which provides a negative (and non-significant) coefficient for this variable.

As I have already said in my previous report, the heteroskedasticity analysis is incorrect in two ways. According to the paper the null hypothesis is “that the error term is non-constant” and then concludes that when p values are high then heteroskedasticity exist.

First, the null hypothesis is homoskedasticity, which assumes that the variance of the errors are constant, and second the interpretation of the p-value, as usual, leads to the rejection of this hypothesis for low values of p.

Finally, regarding the English style, although the abstract has been significantly

improved, the paper still has some grammatical mistakes. For instance:

"indicates" in table 2, page 6, should be plural ("indicate")

"hypothesis" on page 8 is not a verb (should be "hypothesize" or "make an hypothesis")

The expressions "level of significant" or "significant levels" should be revised ("significance")

Considering the previously described problems, I am very sorry to inform you that, according to my opinion, this new version of the paper should not be published.

Editor's Details:

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