Response to the November, 8 invited comment on
"Testing for Breaks in Cointegrated Panels -
with an Application to the Feldstein-Horioka Puzzle"
by Francesca Di Iorio and Stefano Fachin

As pointed out by this reader, since the comments by Joakim Westerlund and an anonymous referee already cover much of the paper this comment is devoted to a few additional points. We will now try to answer to these remarks referring to the previous discussion when convenient. Obviously, we are grateful for the chance we are given to improve the presentation of our results.

1. The first objection is on the very idea of a breaking cointegrating relationship. According to the reader, this concept is acceptable only when "fundamental changes in economic conditions", such as wars, take place. Since these are obvious no testing is required. We fully agree that a "cointegrating relationship" whose coefficients are allowed to change in periods of no obvious economic significance is not reflecting the original idea of a long-run equilibrium. However, a question naturally arises: what is a fundamental change? How do we decide if an historical event is such? Further, an historically fundamental event may affect different economic relationships in different degrees. In our empirical application we have to deal with the widespread move towards financial liberalisation that took place in the late 1980's. Although this is undoubtedly a break in historical terms (financial regulations changed), the questions is: was the saving-investment relationship affected? Following the stance taken in a large body of literature, we believe that to give an answer we need a statistical test.

2. The second objection is on the use of the median of the individual estimated breaks in the model used to obtain the residuals to be resampled. Here the point is very simple. The procedure could easily allow for heterogenous breaks (in fact, it would *simpler* to do so). However, as explained in the paper, this produces severe size distortion. On the opposite, using the median delivers good results under both the null and the alternative hypothesis, with much better size properties and comparable power. In other words, imposing this restriction we obtain a robust test, globally superior to that computed under heterogenous breaks. We did not report results on this comparison in the paper for the sake of brevity, but they can be provided on request.

3. The final remark is based on the key statement that "whenever a constant is included in the regression, the bootstrap residuals will have a zero mean (since they are drawn from a population with mean zero".

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As discussed in the reply to Westerlund, this does not hold when breaks affect the Data Generating Process (DGP). More precisely, if the DGP has a break in the constant the residuals estimated under the null of no break are easily seen to have a broken, non zero mean; even worse, if the break is in the slope they will be non-stationary. Since the residuals to be resampled must be stationary with a zero mean under *either* the null and the alternative hypothesis, they *must* be estimated under the latter.