**Response Letter**

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| **Subject:** | Your Submission EJOR-D-18-00185 |
| Ref.:  Ms. No. EJOR-D-18-00185 Forecasting Retailer Product Sales in The Presence of Structural Breaks European Journal of Operational Research      Reviewer #1: This is a nice paper that investigates the influence of structural change (see one point I make below) on retail data in the IRI data set. It proposes to augment established Autoregressive Distributive Lag (ADL) models, by either fitting multiple models to shorter and shorter terminal segments of the series, then averaging the forecast (so data points after the conjectured structural break are weighted more heavily), or by attempting to estimate a bias from the terminal in-sample fits and correcting for this estimated bias. The forecast accuracy is improved in either way. I have few important points to make and mostly recommend toning down some overly enthusiastic claims.    Medium points:  - Highlights: "Retailer product sales from a wide range of product categories" is not very useful.  We now remove this point from the highlights.  - "Structural breaks" suggests a sudden and abrupt change in a parameter, like a step change in the overall level of the time series. I find this choice of words somewhat unhappy. On the one hand, in a retail environment, I would typically not expect a sudden abrupt change (except in exceptional cases, like a new store opening next door), but rather a gradual one, which would also be more consistent with drivers of such change that the authors discuss, e.g., shifts in lifestyle. On the other hand, there is a vast literature on detecting structural changes in time series (e.g., look at the documentation of the strucchange R package on CRAN), but the authors nowhere apply such a test (which would not be overly useful in my opinion, per above). Thus, maybe "structural change" instead of "structural break" would be a more useful word for the concept.  We thank the reviewer for this helpful advice. We change the term from structural break to structural change and add the following footnote:  “The term ‘structural change’ is also used interchangeably with the term of ‘structural break’ in the literature. In this study, we use the term “structural change” as in the retail context we may expect the effect of the marketing activities to change gradually rather than in a sudden and abrupt way. We thank one of the anonymous reviewers to point this out.”  Previously we described the sequential Chow test we used to detect the presence of structural change in the Appendix of the supplementary material. We now describe the test explicitly in section 6.  - The EWC is very similar to estimating a single model with weighted observations (or residuals), with the weights decreasing as we go further into the past. I wonder whether a direct approach like this would be feasible. It would have the advantage of returning a single parameter vector that could be examined and analyzed, whereas in EWC, one has to look at multiple such vectors, one from each of the models we average.  What the reviewer has mentioned is a possibility. We have evaluated the forecasting performance of ADL models with time-varying parameters (TVP) which has the same rationale described here. However, the preliminary results are mixed. One possible explanation is that the model may have some sophisticated structures which are not robust to the retailer data at SKU level. We add the following to the last section:  “A method alternative to the ADL-intra-EWC method and the ADL-intra-IC method is to directly model the changing process of the effect of the marketing activities. For example, the time-varying parameter model. However, a disadvantage of this method is that we need to make strong assumptions of how the effect of the marketing activities change overtime. e.g., Foekens et al. (1999) modelled the effect of the marketing activities as a linear function of previous promotional activities. The model has a sophisticated structure and was not developed for forecasting. Therefore, we leave the exploration of the potential of this type of model to future research.  ”  - None of the accuracy measures used is minimized in expectation by an unbiased forecast (see the introduction of Kolassa, 2016, IJF). I am somewhat concerned that the increase in accuracy may have come at the cost of bias. I would recommend using either a measure that is minimized in expectation by an unbiased forecast, e.g., a scaled RMSE, or assessing bias in some way.  We add the scaled MSE as one of the error measures and our proposed methods have the best forecasting performance in general. The increase in the accuracy comes from the reduction of the bias but at the cost of increased forecasting error variance. The IC method adds the estimated bias back to the forecasts, which directly reduces the bias but increase the forecast error variance. The EWC method combines the forecasts from different estimation windows. Compared to the original full estimation window, other estimation windows are all smaller but with more recent information (e.g., closer to the forecast origin). Forecasts based on these ‘smaller’ estimation windows are less biased (e.g., these estimation windows contain fewer pre-structural change data) but potentially with higher forecast error variance (e.g., fewer estimation information). Thus, the final forecasts will be less biased and potentially with higher forecast error variance. Whether or not the final forecasts by the EWC method can be more accurate depends on the trade-off between the reduced forecast bias and the potentially increased forecast error variance.    - I see no discussion of how the estimation was actually carried out. Was some third-party software used, e.g., an R package? Please cite software and packages used (with version numbers). Software authors are entitled to recognition.  We estimate the model parameters using the OLS estimator, and we implement the estimation using the MODEL procedure with macros in SAS. 9.4. We add this in section 6 and section 7.  - p. 11: what is "four-week seasonality"? Does this refer to cutting the year into 13 four-week periods (13\*4=52), then expending 12 dummies? If so, this corresponds to fitting a periodic \*step\* function, which stays constant for four weeks and then may change sharply when we enter the next four-week bin. This is discretizing a continuous variable (namely, time). Don't do this. The disadvantages of discretization have been well documented over the years - apart from the almost certainly ecologically invalid step fit I discuss, it also expends far too many degrees of freedom. Instead, use a number of periodic spline transforms of time. Three or four parameters expended here would be much better invested than twelve in discretization. Further reading here:  https://stats.stackexchange.com/questions/230750/when-should-we-discretize-bin-continuous-independent-variables-features-and-when  https://stats.stackexchange.com/questions/41227/justification-for-low-high-or-tertiary-splits-in-anova (see the links in Glen\_b's answer)  In this study, we use the deterministic four-week dummy variables (e.g., 12 four-week dummy variables for the 52 weeks) to capture the seasonality which cannot be captured by the holiday event dummy variables (e.g., Christmas, New Year’s Day etc.). This approach has the limitation that the effect is assumed to stay constant within the four-week bins and has a cost of degree of freedom. However, in the retailer context, the models are usually estimated with a comparably large sample (e.g., 160 weeks in our study), where the loss of the degrees of freedom is not an issue.  We think the performance of alternative methods for the seasonality depends on the characteristics of the data. In this study, we focus on weekly retailer data at SKU level which have unique characteristics. e.g., product sales have high variations, the effect of marketing activities change overtime, and less ‘seasonal’ compared to data from other industries (e.g., the electricity demand data). Also, we propose multi-stage sophisticated methods. The methods initially include a vast number of independent variables and then attempt to recursively simplify its specification. Thus, the performance of any other methods to capture the seasonality for retailer product sales becomes an empirical question, especially when gauged with sophisticated model simplification strategies.  We thank the reviewer for the suggestion on how to improve the model’s performance using the alternative methods for seasonality such as periodic spline functions or seasonal exponential smoothing etc., which are useful in modelling seasonality in other fields such as electricity demand. We also thank the reviewer for the reference where the natural splines method is compared with the traditional deterministic method based on the simulation data. We address the limitation of our current approach in the last section as an avenue for further research.  - p. 19: To be honest, I do not find the introduction and discussion of the ADL-EWC-IC model convincing, since it was created after analyzing the performance of the separate models on subsamples. It is always easy to build a new model ex post that appears to perform well, but this is little better than data snooping. Can the ADL-EWC-IC model be compared to the other models on previously unseen data? If not, please label this discussion explicitly as exploratory. Similarly, please revisit the discussion in the first paragraph on p. 25.  We thank the reviewer for this useful comment and suggestion. We now evaluate the forecasting performance of the ADL-EWC-IC model based on previous unseen data. That is, based on 1605 SKU’s from the same product categories but from a different set of 28 stores. The results are consistent (we have included an additional Table for the results). We accept the reviewer’s suggestion and describe the ADL-EWC-IC model as “exploratory”.  - p. 23: In discussing a statistically significant positive coefficient for "Randomness and growth" in ADL-intra-EWC, the authors write that "This suggests that our proposed models tend to be more advantageous for the SKUs which are difficult to forecast and exhibit a trend in sales". I do not understand this. The dependent value modeled is MASE, so a positive coefficient of 0.4 should mean that a unit increase in "Randomness and growth" should be associated with a 0.4 unit (up to multiplication by 100, per the footnote to Table 7) \*increase\* in MASE. That is, ADL-intra-EWC should perform \*worse\* than the benchmark for high-"Randomness and growth" series, not \*better\*. Please clarify. Same for the rest of the discussions on the same page, and the third paragraph on p. 25.  The dependent variable is not the MASE but the percentage reduction of the MASE by the ADL-intra-EWC model or the ADL-intra-IC model compared to the ADL-intra model. e.g.,    Thus, a positive coefficient of 0.4 indicates that one unit increase in "Randomness and growth" would cause a 0.4% reduction in the MASE. We now provide a clearer description for the interpretation (please note that now in the revised version we use the figure of two decimals, e.g., we report 0.38 rather than 0.4, and we change "Randomness and growth" to "Randomness and trend" as we find the term “trend” to be more appropriate).  We have revised this section accordingly and make the description more explicit.  - p. 23, "All the results here indicate that we may pre-test these features for each SKU and then determine the optimal sales forecasting method specifically for that SKU." This is a very exploratory finding, not guided by prior hypotheses. Unless this can be verified on previously unseen data, please emphasize the tentative nature of this recommendation.  We thank the reviewer to point out tentative nature of this indication. We now rewrite the sentence and only address the indications based on the current findings “Overall, the exploratory results here highlight the situations where our proposed methods gain most benefits.”  - p. 24, " The improved forecasting accuracy for product sales substantially contributes to retailers' profit". This is a very strong claim, and not backed by anything the authors did in their paper. Whether forecast accuracy improvements actually translate into better profits is not this clear-cut, since forecasts must still be translated into operational plans, which are constrained by logistics. In addition, supply chain operations rely far more on quantile forecasts than on point forecasts, since the total order includes safety amounts, and whether these quantile forecasts can be improved by the authors' proposals is not obvious. Please tone this claim down.  We thank the reviewer for this comment and we tone down the claim of our contribution- we remove this sentence and add “Therefore, our study provides retailers more effective forecasting methods.”.  Minor points:  - p. 6, 1st equation: there is no intercept in the formula. If u\_t is assumed to have mean 0, this implies that a price of x=0 is associated with sales of y=0. I assume the authors meant to include an intercept parameter.  We no longer consider the variables to be price or price reductions (as pointed out by the reviewer, the sales will not be zero even there is no price reduction). We only use this example to analytically demonstrate the forecast bias caused by the structural change. We highlight that more sophisticated scenarios (e.g., with an intercept and/or with endogenous variables) can be proved using simulation and we include the simulation example in the supplementary material where the model has an intercept.  - Table 3: why do different models serve as benchmarks?  All the candidate models significantly outperform the basic benchmark model, i.e., the Base-lift method, which is widely used in practice. We compare the ADL-intra-EWC model and the ADL-intra-IC model with the ADL-intra model (which has similar specifications but overlook the problem of structural change), so that we know how much improvement is contributed by taking into account the problem of structural change. We now highlight this in the manuscript.  - Figure 3: please provide more information in the figure caption instead of in the text or even in footnotes - the reader should not need to hunt through the text to understand the figure. Are the diamonds joined by lines group means? Are box widths \*proportional\* to numbers of SKUs in each category? "Are determined by" can be a log transform, a square root or anything else. Please ensure that the horizontal axes have the same extension so the plots are comparable, and that the whiskers are not cut off by the figure bounding box.    We now describe the details explicitly. The diamonds joined by lines represent the group means and the widths of the box are proportionate to the numbers of the SKUs for each product category. We now show the boxplots with the same extension and without outliers being clipped.  - Table 7: please indicate in the table caption what "their counterparts" are. The entire table is unclear to me; how do the top and the bottom half differ? What does "Model with 5 factors and category dummy variables" as a caption to the bottom half (or is it?) refer to, in contrast to the top half? The text says on p. 23 that "the horizon is one to eight-week ahead", but the table says "Horizon = 8" - please clarify whether the horizon is eight weeks or \*up to\* eight weeks.  We now rewrite the section by focusing on the results of the ADL-intra-EWC method and the ADL-intra-IC method. We also define the dependent variables explicitly using equations. We reduce the previous Table 7 by removing the bottom half of results (because they were only used to suggest that the findings were consistent even if we include category dummies in the regression model). Instead, we put the following footnote:  “For robustness, we develop an alternative regression model which also include dummy variables to capture potentially unobserved category effects, and we find the parameter estimate to be consistent with those shown in Table 7.”  we also clarify the description for the horizons. e.g., it is one to eight weeks forecast horizon.  - References: please provide full details for Loeb (2015) revised  Typos:  - p. 3, 2nd para, l. 3: "The model which is subject to structural break" - either add "a" or change to "breaks"  Revised  - p. 3, 2nd para, l. 7: remove "in" after "including"  Revised  - p. 4, l. 2: "mention" -> "mentioned"  Revised  - p. 4, l. 3: "values" -> "value"  Revised  - p. 4, 2nd para, l. 2: "the change of" -> "changes in" Revised  - p. 5, l. 5: "forecast" -> "forecasts" Revised  - p. 6, 2nd para, l. 3: "structure" -> "structural" Revised  - p. 6, third equation block: italic and upright versions of beta, X and Q are mixed, which is painful to me (also on p. 7)). Please ensure proper mathematical typesetting. Lowercase x\_{T+h} and uppercase X\_{T+h} is used inconsistently here.  We thank the reviewer for this correction. We now revise those inconsistent versions of letters and symbols. We now discard the symbol “Q” and use the lowercase consistently.  - p. 7, equation for the bias correction: lowercase omega is used both as the starting index of the summation (with the ending index indicated by W - why mix Greek and Latin?) and as the summation index itself.  Revised: we now use alternative letters to make it easier to distinguish.  - p. 7: "The estimated bias are" should be "is" Revised - p. 10, equation: can be slightly simplified by removing "=\eta, \eta"  We thank the reviewer- we think possibly it could more be readable if we keep it  - p. 19/20: Figure 3(c) is not "in the bottom-right corner", but in the bottom left.  Revised. Also, we no longer show the boxplot for the ADL-EWC-IC model using Figure 3(c) because we focus on the ADL-intra-EWC method and the ADL-intra-IC method, and only consider the ADL-EWC-IC model as an exploratory model.  Reviewer #2: This is an interesting paper that is trying to investigate the forecasting performance of several ADL models over retailer product sales. The study is particularly focusing on the effects of structural breaks originating from marketing activities over the products.  Main Comments: 1.     I found the structure of this paper very confusing. For example, the introduction and literature review section are very poorly written with many overlaps and repetitions that are not at all informative for the reader. The contribution of the paper, as outlined, is very weak.  We now revised the structure of the paper and more importantly we taken out some the arguments that may sound a bit repetitive.  2.     In the introduction the authors should clearly indicate what is the model they are introducing and why. On top of that, it should be made clear to the reader why the specific models seem appropriate for the retail forecasting exercise. It is quite astonishing that the reader does not get a gist of what he/she is going to see unless he/she reaches page 10.  We now introduce what is new in our research earlier in the paper than we did in the previous version. We have also highlighted the value of the work in terms of how it might impact on inventory management practices similar to other related work in the field of forecasting.  3. The literature review is very short and several references are packed all together without any meaningful commentary (for example in page 5). I would really be interested to see what are the findings of studies assuming constant marketing activities, as this would highlight/clarify/validate potentially comparisons with the models at hand.    We now revise the literature review. In section 2.1., we summarize the findings of previous studies which forecast retailer product sales at SKU level. e.g., their proposed methods and the rationales in more detail. In section 2.2., we summarize the effect of the marketing activities.    [what does the reviewer here mean by he/she would like to see the findings??? We need to say here that the argument here is that we are not testing our model with their data but we are just saying that these models do not account for structural change…I don’t think he means findings in terms of whether increase in price leads to fall in sales as what seems to be written in the response, he actually is asking what do these people find without structural change, well they have not done it, we are doing it??? Or I may be wrong!)    [Didier, I agree, I think you are saying that he is asking us to compare our methods with those forecasting methods with constant parameters. This is not fair, because we cannot replicate the forecasting models proposed by those previous studies. However, probably, he only wants us to describe more details about those studies which forecast retailer product sales? – I have rewritten the literature section so that we do have more details (e.g., the rationale of the model, for which product categories did the models have good performance etc.). Robert, do you think this is the ‘findings’ he wants?]  4.     The text is characterized by some generalizations that make the reader confused on what the authors are claiming. For example, in page 3 '… The data in retailer product sales… macroeconomics).' I would suggest a thorough read-through to the authors in order to make the text more to the point.  We have now streamlined the manuscript in line with this comment and taking into consideration other comments/suggestions by the other reviewer too.  5.     Section 3 and 4 seem a bit redundant the way they are presented. I would expect to see a methodology section, where these two sections could motivate/inform the selection of models in section 6. The ideal approach would be that section 3 and 4 are reduced substantially and included in the commentary of section 6 or if needed in an appendix.  In our revisions, we now merge sections 3 and section 4.  6.     In section 5, it should be explained what the display and feature percentage is along with the motivation of the selection of these inputs. In my view, the data section should have been after the intro/literature review.  We now add the description below Table 4.  7.     In terms of the analysis during structural breaks, it is interesting that the authors do not make explicitly clear how they detect, test or analyse their results for structural breaks. It would be expected that in that type of paper where forecasting performance is evaluated within the presence of structural breaks that vast emphasis would be given on that issue. The general information provided in section 4 are not enough in my opinion. What tests have been done and what was the result? Except from the Chow test, have the authors investigated the Andrews approach (2003 , Econometrica) or Fixed Regressor Boostrap? The vague analysis on that aspect is a main shortcoming of this paper.  We have previously described how we conduct the sequential Chow test in the Appendix in the supplementary material - we now include this in the section 6 of the paper. We conduct the sequential Chow test for up to 95% of the weeks in the estimation period. For example, imagine we have an estimation period of 160 weeks, we conduct the Chow test for each of the 160\*0.95= 152 weeks. We initially conduct the Chow test assuming a structural change occurring at week 5 and we obtain the p-value. We then conduct the Chow test for week 6, 7, and so forth until week 156 and each time we obtain the p-value accordingly. We reserve at least 5% of the weeks for the estimation of the test. Thus, we may obtain up to 152 p-values in total. The null hypothesis of no structural change will be rejected only when none of these p-value is below the threshold. To mitigate the multiple comparison problem, we adopt a very small threshold, i.e., 0.001.  We also address the fact that previous studies have proposed alternative tests (e.g., Donald W K Andrews, 1993; Donald W. K. Andrews & Ploberger, 1994; Bai & Perron, 1998, 2003; Brown, Durbin, & Evans, 1975). However, these tests have different focuses (e.g., the size and the location of the structural change) and more stringent assumptions (e.g., a known number of multiple changes as a priori knowledge). The estimation of the locations and the sizes using these tests were not satisfactory (Pesaran and Timmerman, 2005). In our study, the purpose is neither to detect the locations nor the number of structural changes, but focusing on investigating the presence of any structural change, so that we can estimate and then offset the bias (if using the IC method) or to take a trade-off between the forecast bias and the forecast error variance (if using the EWC method). Therefore, we conduct a sequential Chow test which serves for this purpose and has the benefit of simple implementation. The empirical results suggest that our models generate the more accurate forecasts.  The Andrews’ approach published in *Econometrica* in 2003 (e.g., the end‐of‐sample instability test) is more generally applicable as it can be used when there are very limited data (e.g., even one observation) before or after structural change within the estimation sample. In comparison, the sequential Chow test needs to reserve more observations before and after the structural change (e.g., it is an F-test which compares the fit of the model before and after the structural change). However, as pointed out by the other reviewer, the effects of the marketing activities tend to change in a gradual way rather than an abrupt way. Thus, it is unlikely that there is a sudden and abrupt structural change occurring exclusively for a very few observations (and it matters only if there are so few that a Chow test cannot be conducted) which are close to the forecast origin or the beginning of the estimation window. Empirically, we conduct the sequential Chow test for different percentages of weeks. For example, we previous conduct the sequential Chow test up to 70% of the weeks in the estimation period, and now we conduct the sequential Chow test for up to 95% of the weeks in the estimation period. There is little difference in the results for the final models’ performance. Therefore, we may not expect benefits from this feature of the Andrews’ instability test. Also, the Andrews’ instability test assumes that explanatory variables must be strictly stationary, otherwise the test will be associated with a distorted inference and mix the instability of the explanatory variables with the instability of the regression model. In the retailer context, the assumption of stationarity of the explanatory variables may not always be true as the in general prices increase gradually during the two-to-three-year timeframe due to inflation, though intermittently associated with price reductions.    We thank the reviewer for pointing out the possibility of further improvement if a set of alternative tests can be evaluated (as there is always an opportunity to improve by trying alternative methods) but we leave that to future research.  8.     In the modelling part, I would expect to see a small description at least of the benchmark model, the base-lift model. Also, it is not well explained why Lasso is used two times. What motivates the authors to double apply Lasso shrinkage? Is there any similar study suggesting that? What are the expected benefits of this approach? Given that this paper comes down to a horse-racing application between ADL models constrained by Lasso operators, it is important to clarify why the ADL-raw models is combined with the ADL-own model? Wouldn't be a logical question from the reader as to why the Lasso operator is not used in a third stage, for example? A lot of discussion is provided before-hand (in cases unnecessary), but at section 6 that the reader wants to understand the reasoning and process of modelling, he/she struggles to do so because of the lack of explanations. Another important shortcoming for this work.  We thank the reviewer for pointing this out. We now describe the Base-lift benchmark model better.  We also explain explicitly the details of the modelling process for our proposed ADL-intra-EWC method and ADL-intra-IC method. The LASSO procedure was initially used as a variable selection method to identify the important marketing variables (e.g., Huang et al, 2014). It was then used as a model simplification strategy following Ma et al. (2016). The general ADL model becomes the ADL-raw model after being simplified by the LASSO procedure, as shown in Figure 2.  We choose the LASSO procedure as a model simplification strategy because it proves to be effective, and it is also automatic. However, it runs the risk of missing important variables (e.g., potentially the price and promotions of the focal product). If we miss important variables, the final forecasts will be biased and the forecast error variance will also rise (and this bias is not the bias incurred by the structural change but from the bias of the parameter estimate). Missing important variables is far more serious compared to the cost of efficiency (Davidson and MacKinnon, 2004).  Thus, we try to prevent this as possible as we can. For example, if the price and promotion variables of the focal product are not included in the ADL-raw model, we try to bring them back, but only if they are retained in the ADL-own model by the LASSO procedure (because this suggests that they are useful/important). The supplementary parallel ADL model (i.e., model 6, in the revised manuscript), by definition, has fewer explanatory variables (and no competitive promotional variables) compared to the general ADL model, thus we suffer less from multicollinearity when implementing the LASSO procedure to simplify the supplementary parallel ADL compared to when we simplify the general ADL model. Therefore, it is unlikely for any truly important variable to be missed by the LASSO procedure twice. In the end, the ADL-intra model will be unlikely to miss the price and promotion variables of the focal product if these variables are truly important, and we only bear a cost of reduced efficiency. However, if we further implement the LASSO procedure, we will bear the risk of missing important variables.    9.     In the experimental design section, the representation of the competing models is not clear. The authors should have included perhaps a table or a clear text on what is the differences between models in a concise and transparent way. Additionally, I am not sure why the roll-forward period is equal to two weeks, while the forecasts are done for 1, 4 and 8 weeks. The authors should explain why they used sMAPE over MAPE or why MASE is more informative. In terms of statistical accuracy, RMSE and Theil-U statistics are also quite often used. One final point is why are the authors inclined to use the adjustment of Cooper et al. (2009). This was not quite clear to me.  We now revise this section to add a clear text which highlight the difference between the models.  We implement the models with 18 rolling events, and for each time we roll the full estimation window forward for two weeks. Therefore, we will be able to evaluate our models for longer periods of time span (e.g., under this condition, the initial full estimation window is [1:160] and the last estimation window is [35:194], thus the data we used have a time span from week 1 to week 194, compared to shorter time span from week 1 to week 167 if we only roll forward one week each time). This may potential make our results more robust as we evaluate our models for longer time span.  We include traditional error measures in the evaluation. These error measures capture different aspects of the unobserved loss function for the retailer but all of them have limitations. Therefore, they need to be used as a whole to form the broad picture of the model comparison. We now add the scaled MSE as an additional error measure. It conveys the information of the RMSE and the Theil’s U.  We use the adjustment of Cooper et al. (2009) to mitigate the bias due to the logarithm transform (e.g., the expected value of the log transformed variable does not equal to the log transform of the expected value).  10.     The section 8 is quite extensive in terms of results in tables, but the comments are quite limited and badly written. For example, is there consistency in ranking across statistical measures? What exactly is the importance of Table 3? At such a forecasting exercise, a Diebold-Mariano test is needed to assess the significance of the differences in performances. The Wilcoxon Sign Rank is not enough. It is also not quite clear what Table 4 is offering. In Figure 3, is there a particular reason why these six product categories are selected for analysis?  We now highlight that the results are consistent for all the error measures.  We now replace the Wilcoxon SR test with Diebold-Mariano (DM) test in Table 3. We include Table 4 to demonstrate the performance of the various models depending on whether the focal product is being promoted. This is because the product sales variations are much higher for the promoted period compared to the non-promoted period. We choose the six product categories for which the proposed methods have highest advantage over the ADL-intra model and we show the distribution of the improved forecasting using boxplots.  11.     Table 5 should probably be within the analysis of the determinants of the forecasting performance. I also find the explanation of the process confusing. Why is this factor selection followed? Have the authors explored another factor analysis?  We rewrite this section explicitly. Table 7 (the previous Table 5) now consists of the parameter estimates for the regression model with the five factors as independent variables. We have developed an alternative regression model which also have dummy variables for the categories. The parameter estimates are consistent. We now no longer report them in Table 7 but report this in a footnote.  Minor comments: 1.     The writing of the whole paper is not very good. A lot of proof-reading is required. Some examples: \*     'Under such a circumstance…activities' in the abstract.  Revised \*     '…the generated forecasts may potentially…' in the abstract  Revised \*     SKU abbreviation should be defined in the abstact and in text and then explained perhaps on a footnote. \*     '…proposed holistic methods to generate…' page 4, line 57  Revised \*     Footnote 7 should refer to figure 1.  Revised 2.     Sometimes writing lack of academic standard. For example, there is no numbering in equations, there is extensive use of bullet points within text, tables are not explained with footnotes, many equations appear within text, the tables style is not uniform etc.  We have not taken on board your comment on the writing style of the paper  3.     The paper is not well positioned in the OR forecasting literature.  We have revised the paper to improve the positioning of the paper.  we have now more explicitly positioned the work as one that can impact on the inventory management of retailers similar to related work in the domain of forecasting  Overall, based on the above I am inclined to reject the paper due to its lack of solid contribution, convincing results, presentation and academic rigour. | |
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