Reply to reports on article JBES-P-2022-0567

First of all, we would like to thank the editor, the associate editor and the reviewers for their many comments and suggestions which were very helpful to improve the paper. In the revision, we have addressed all comments of the referees and have rewritten the paper accordingly. Please find our point-by-point responses to the referees below. In our responses, we have indicated where alterations to the manuscript have been made. Please also note that we have tried to add as much of the new material as possible to the main body of the paper. However, to meet the 35 pages limit, we had to relegate parts of the material, in particular, the revised simulation study (which comprises many additional simulation exercises) to the Supplement. If required, we can of course rearrange the material, shifting part of the simulations to the main body of the text while moving other parts to the Supplement.

Reply to the editor

1. *The reviewers have diversified opinions, and they raise many issues. Although I think you can address many of them, there is one main issue I don't see how to handle: As the AE points out, you do not have a compelling case for the null hypothesis that time trends are identical across multiple time series. Like the AE, I can't think of different economic time series sharing the same trend. I appreciate your discussions on clustering much more, but the same issue arises within a group.*

   There is a small econometric literature on co-trending and co-breaking where common time trends and breaks are used to capture comovements of multiple time series. Rather than time trends being identical, they are proportional to each other. That might be more plausible for modeling multiple economic time series. Because this may be difficult in your framework, I do not mean to impose it. Instead, I am pointing out that such literature may or may not be helpful.

   Although I doubt the plausibility and usefulness of the null hypothesis, I would like to give the benefit of the doubt. I am willing to consider a revision of your paper, though with no guarantee that it will ultimately be accepted. I ask you to address all of the reviewers' concerns. Most importantly, please make a much more compelling and convincing case for the null hypothesis, which may be very difficult.

   We fully agree with you and the associate editor that the null hypothesis

   $$H_0: \text{ All time trends are the same}$$

   is not very interesting by itself. In most applied cases, it is quite far-fetched to assume that all time trends are exactly the same. Hence, there is usually no need

to test this hypothesis formally. In particular, rejecting H_0 by a statistical test does not generate additional useful information.

The main aim of our paper is to go beyond a dull statistical test of H_0 and to come up with an approach that is able to generate interesting information in practice. However, quite obviously, we have not done a good job of conveying this in the paper. In the revision, we have rewritten large chunks of text to make a more compelling and convincing case for our approach (see in particular the revised Introduction and Section 2.2 on the model details). Let us briefly summarize the two main points here:

(a) We in fact consider a more general null hypothesis than H_0, in particular,

$$H_0' : m_i = m + c_i \text{ for some } c_i \in \mathbb{R} \text{ and all } 1 \leq i \leq n,$$

where m_i denotes the time trend of the i-th time series and m is a common time trend. Under H_0', the time trends m_i need not be exactly the same. They are only required to be parallel to each other. Put differently, the time series are co-trending.
In our model, we can reduce H_0' to H_0 by normalizing the model components appropriately. To do so, we proceed as follows:

- The time trends m_i are not fully identified in our model

$$Y_{it} = m_i\Big(\frac{t}{T}\Big) + \boldsymbol{\beta}_i^\top \boldsymbol{X}_{it} + \alpha_i + \varepsilon_{it} \tag{1}$$

  without further constraints. In particular, model equation (1) is equivalent to

$$Y_{it} = m_i^\diamond\Big(\frac{t}{T}\Big) + \boldsymbol{\beta}_i^\top \boldsymbol{X}_{it} + \alpha_i^\diamond + \varepsilon_{it},$$

  where $m_i^\diamond = m_i + c_i^\diamond$, $\alpha_i^\diamond = \alpha_i - c_i^\diamond$ and $c_i^\diamond$ is an arbitrary real constant. We can thus freely shift additive constants between the trends m_i and the fixed effects α_i. For identification, we impose the constraint $\int_0^1 m_i(u)du = 0$ on equation (1) for all i.
- Now suppose that H_0' holds true. In this case, the trends m_i are parallel to each other, i.e., $m_i = m + c_i$ with some constants c_i and a common trend m for all i. The constraint $\int_0^1 m_i(u)du = 0$ implies that $c_i = c := -\int_0^1 m(u)du$ for all i. Put differently, it implies that the trends are exactly the same. Hence, under our identification constraint, the null hypothesis H_0' that the trends are parallel to each other coincides with the hypothesis H_0 that the trends are identical.

Without loss of generality, we can thus restrict attention to H_0 in our framework. We apologize: we should have spelt this out clearly already in the old version of the paper.

(b) The associate editor writes: *Would one really want/need to test for the exact "sameness" of time series trends? Or is it the case that the null hypothesis is uninteresting, but the alternative is, especially if I am able to see which trends are different and where?* This is exactly the point! We want to have a statistical approach which is not only able to tell us (with a certain statistical confidence) whether the null is violated or not. We rather want a method which gives us as much information as possible on which types of violations occur. Specifically, we want a method which allows us to say which trends have a different form and in which time intervals the trends are different. We think that this is exactly the information which is important in practice. Take for example our application on house price trends in different countries (or an application on temperature time trends at different spatial locations, on volatility trends of different stocks, ...). Most probably, the trends are not the same in all countries. Hence, rejecting H_0 does not give us much valuable information. However, proper confidence statements about (i) which countries have different trends and (ii) where, i.e., in which time periods, the trends differ may provide very valuable information for practitioners. It is exactly such confidence statements that are produced by our approach.

We hope the above summary and the corresponding discussion in the revised paper make a convincing case why our approach is much more useful and informative than competing methods for the comparison of time trends.

Reply to the associate editor

1. *The less enthusiastic referee wrote: "Perhaps it is my unfamiliarity with the problem (or my tendencies toward Bayesian methodologies...), but I do not find it to be a particularly compelling research question. The goal is to test whether a time trend—after adjusting for covariates—is identical across multiple time series. This does not seem to be a high priority for multiple time series and dynamic regression analysis, and it's not clear whether a hypothesis test generates much useful information in this context." This is a comment I broadly agree with: would one really want/need to test for the exact "sameness" of time series trends? Or is it the case that the null hypothesis is uninteresting, but the alternative is, especially if I am able to see which trends are different and where? I am thinking aloud here, but overall I don't think the testing problem, as stated, is interesting enough for JBES readers.*

   Please see our reply to the editor's comments above for a detailed response to your criticism.

2. *There are two prior papers by the submitting author, which consider a similar problem but in the absence of external covariates. I don't think the current paper makes it clear early enough what is different between the current work and those earlier papers.*

   As the current paper, Khismatullina and Vogt (2023) is concerned with the comparison of trends. However, their model setting is tailored to epidemic count data and thus quite specific. In the revision, we discuss the connections to their model and methodology directly in the Introduction (in more detail than in the previous version of the paper); see p.4. Even though there are connections on the methodological level, our proofs are utterly different from those in Khismatullina and Vogt (2023). Since a discussion of the differences is quite technical, we have not added such a discussion to the introduction but think it fits best into Section 4 on the theory. Please see Remark 4.3 therein for the details.
   Khismatullina and Vogt (2020) is concerned with a problem quite different from ours. Only a single time series is considered there and the aim is to test for local increases/decreases in the trend function. The main connection of the current work to Khismatullina and Vogt (2020) is on a purely technical level. In particular, our proof strategy can be regarded as an extension of theirs. We think the details are best discussed in the context of Section 4 on the theory (please see Remark 4.2 therein).
   We hope you find the revisions described above appropriate. However, if you think the technical connections/differences should be discussed even earlier than in Section 4, we are of course willing and happy to make further changes.

3. *Due to the various approximations, the size control is only approximate. I don't see it as a "state of the art" way of thinking in these types of FWER control problems; please see e.g.* `https://arxiv.org/abs/2009.05431`*, where size control, in a different but related multiscale testing problem, is exact.*

   As you point out, the size control in our results is only approximate (in the sense of being asymptotic). The reason is that our proofs rely on strong approximation theory which is asymptotic in nature. For simplified versions of our model (e.g. for the most simplistic version $Y_{it} = m_i(t/T) + \varepsilon_{it}$ with errors that are i.i.d. both across i and t), it would be possible to get results on exact size control by using techniques from Chernozhukov et al. (2017). However, in our general setting, we were not able to use these techniques (which is mostly due to the quite complicated dependence structures present in the model). We thus resorted to strong approximation techniques which allow us to get at least approximate size control. From an applied point of view, this is however not a big problem as far as we can see: usually, results on exact size control (as the ones in the linked

paper) cannot be used directly in practice as they depend on certain distributions which are unknown in practice. Hence, one usually needs to resort to asymptotic approximations to derive critical values in practice (as in Section 2.3 of the linked paper), making the size control only approximate at the end of the day.

4. *I suspect the procedure must be really difficult to use in practice with confidence, as it depends on so many tuning parameters including the bandwidth. The authors say their software is at* `https://github.com/marina-khi/multiscale_inference`, *but the link is broken.*

   In response to your comment (and to comment 5 of referee 1), we have carried out a number of robustness checks in the revised simulation study where we consider different choices of tuning parameters (see the new Section A.4 in the Suppementary Material as well as our reply to comment 5 of referee 1 for further details).

   The main tuning parameter of our method is the grid $\mathcal{G}_T$ which in particular specifies the bandwidths that are taken into account by the procedure. Most nonparametric tests in the literature depend on one or more bandwidth parameters. Usually, the bandwidth is picked adhoc as there is virtually no theory for optimal bandwidth selection in nonparametric testing (as opposed to optimal bandwidth selection for nonparametric curve estimation). With our multiscale approach, we go one step into the direction of a bandwidth-free test: we consider various bandwidths simultaneously, thus avoiding the need to pick a single bandwidth adhoc. However, our procedure is of course not fully bandwidth-free as we still need to pick a set of bandwidths. Nevertheless, as long as this set is chosen sufficiently rich (in the sense of including a variety of bandwidth values ranging from very small to very large), its particular choice can be expected to have a negligible effect on the procedure. This is supported by the robustness checks in Section A.4 where we consider different grids $\mathcal{G}_T$.

   Apart from the grid $\mathcal{G}_T$, our method depends on (i) the number of bootstrap samples L to compute the Gaussian quantile, (ii) the kernel K and (iii) secondary tuning parameters for the computation of the long-run error variance. As long as L is chosen large enough (say $L \geq 1000$), the precise choice of L should have a negligible effect. We use $L = 5000$ throughout the paper. As a robustness check, we have re-run everything with $L = 1000$, which (as expected) yields almost identical results. (We briefly comment on this in Section A.4 of the Supplementary Material but do not report the exact results.) As suggested by classical nonparametric theory, the choice of kernel is much less crucial than the choice of bandwidth. Thus, we have not carried out robustness checks w.r.t. the choice of kernel. For the estimation of the long-run error variance, we can take an estimator off-the-shelf which will of course depend on further tuning parameters.

In the paper, we work with the estimator from Khismatullina and Vogt (2020) where extensive robustness checks w.r.t. to the choice of tuning parameters have been carried out. However, it is of course possible to work with other long-run variance estimators. As a rule of thumb, the stronger restrictions we make on the dependence structure of the error process, the easier it gets to estimate the long-run variance and the less tuning parameters are needed. (In the most extreme case where we assume the errors to be i.i.d., the long-run variance coincides with the short-run variance which is very easy to estimate in comparison with the long-run version.)
Finally, many thanks for pointing out to us that the link to the code was broken. We have fixed this and the full code for simulation studies and for the application analysis can be found at `https://github.com/marina-khi/multiple_trends_code`.

5. *Both referees, including the more enthusiastic one, mention several further issues with the paper, including issues related to the practicalities of the method, the simulation study and the asymptotic nature of the method.*

   Please see our reply to referees 1 and 2 for a detailed point-by-point response to the issues raised.

Reply to referee 1

Thank you very much for the careful reading of our manuscript and the interesting suggestions. In our revision, we have addressed all your comments. Please see our replies to them below.

1. *The assumptions and requirements for the variance σ^2 deserve further consideration.*
   (i) First, it is claimed that the variances are assumed to be constant across series, but that a different estimator is used for each series. Which is the correct assumption for practice and theory?
   (ii) Second, given the economic and potential financial applications, how might volatility (or time-varying variance) be incorporated into the testing procedure? Is this plausible within the proposed framework, even if additional assumptions are required? If it is not plausible to account for volatility explicitly, then is the procedure robust in the presence of volatility?

   (i) We assume that the long-run error variance σ^2 is the same for all time series i. However, rather than constructing a single estimator $\hat{\sigma}^2$ from the pooled (pre-processed) sample $\{\widehat{Y}_{it} : 1 \leq t \leq T, 1 \leq i \leq n\}$, we construct a separate estimator $\hat{\sigma}_i$ for each i which depends only on the i-th (pre-processed) time

series $\{\widehat{Y}_{it} : 1 \leq t \leq T\}$. The reason for this is purely technical: since the estimator $\hat{\sigma}_i^2$ only depends on the data of the i-th time series, we can apply the strong approximation results from Berkes et al. (2014) separately to each time series i. If we used a pooled estimator $\hat{\sigma}^2$, this would not be possible. In the revision, we make this point clearer on p.10.

(ii) It should be possible to extend our methods and theory to allow for a time-varying (short-run and long-run) error variance. In particular, we conjecture that we can allow for locally stationary error processes whose autocovariance structure varies smoothly over time. However, such an extension would be highly non-trivial with many technical difficulties occurring on the way.
Whether the assumption of a time-constant error variance – or more generally, the assumption of a stationary error process – is plausible depends of course on the application context at hand. Our methods should be robust to mild deviations from stationarity (e.g. when faced with a locally stationary process whose autocovariance structure varies only slightly over time). However, if one expects the deviation from stationarity to be very strong (e.g. massively changing error variance), then one should treat our methods with caution.
Let us also mention that sometimes, time-varying error variance can be dealt with by a model transformation. Consider for example the locally stationary volatility model

$$r_{it} = s_i\Big(\frac{t}{T}\Big)\xi_{it}, \tag{2}$$

where r_{it} denotes the return of stock i at day t, s_i is a smoothly varying volatility function and ξ_{it} is a stationary process (e.g. a GARCH-type process) with mean 0 and variance 1. Such a model has been considered e.g. in Feng (2004) and Hafner and Linton (2010). The squared returns satisfy the model equation

$$r_{it}^2 = s_i^2\Big(\frac{t}{T}\Big) + \varepsilon_{it} \quad \text{with} \quad \varepsilon_{it} = s_i^2\Big(\frac{t}{T}\Big)(\xi_{it}^2 - 1).$$

Hence, the squared returns can be written as the sum of a deterministic trend function which specifies the time-varying variance $\mathrm{Var}(r_{it}) = s_i^2(t/T)$ and an error term u_{it} whose variance is also changing over time. Rather than analyzing this model which involves a nonstationary error process, we can apply a log-transformation to the squared returns. This yields the equation

$$\log(r_{it}^2) = \log\Big(s_i^2\Big(\frac{t}{T}\Big)\Big) + \log(\xi_{it}^2).$$

After centering the error term $\log(\xi_{it}^2)$, this transformed model fits into our framework. In particular, the error term $\log(\xi_{it}^2)$ is stationary. Note that it

is possible to incorporate covariates in the volatility model (2). For instance, one may consider the model

$$r_{it} = s_i\Big(\frac{t}{T}\Big)\exp(\boldsymbol{\beta}_i^\top \boldsymbol{X}_{it})\epsilon_{it},$$

which leads to the log-transformed model equation

$$\log(r_{it}^2) = \log\Big(s_i^2\Big(\frac{t}{T}\Big)\Big) + \boldsymbol{\beta}_i^\top \boldsymbol{X}_{it} + \log(\epsilon_{it}^2),$$

which is a special case of our model framework.
In the revision, we briefly discuss the extension to locally stationary error processes in the new Section 7.

2. *There are several issues with the simulation study.*
(i) Setting the fixed effect to zero and including a single covariate both make for a much simpler design than considered in the theory. More challenging scenarios, including nonzero fixed effects and multiple predictors (e.g., using the estimated values and/or covariates from the application) would better demonstrate the capabilities of this approach.
(ii) The data from the null fix $m_i = 0$ and claim this is WLOG. However, this is also quite a simple case: the shared $m_i()$ curve could be quite complex under the null, which only maintains that the trends are shared among the series.
(iii) There are no competing methods considered; some alternative approach or benchmark must be added. A reasonable alternative might consider an additive model and compute confidence intervals (or bands) for the trends, with a simple heuristic to determine whether the functions are identical. The proposed approach should do better, but demonstrating improvements over a reasonable alternative is important.
(iv) Only a small number of series is considered. How does the approach perform when n is large?

   (i) We have taken your suggestions into account and consider the following more challenging simulation setup:

   - As before, we choose $n = 15$ and $T \in \{100, 250, 500\}$.
   - We include 3 covariates and model them by the following VAR(3) process:

$$\underbrace{\begin{pmatrix} X_{it,1} \\ X_{it,2} \\ X_{it,3} \end{pmatrix}}_{=:\boldsymbol{X}_{it}} = \begin{pmatrix} a_1 & 0 & 0 \\ 0 & a_2 & 0 \\ 0 & 0 & a_3 \end{pmatrix} \begin{pmatrix} X_{it-1,1} \\ X_{it-1,2} \\ X_{it-1,3} \end{pmatrix} + \underbrace{\begin{pmatrix} \nu_{it,1} \\ \nu_{it,2} \\ \nu_{it,3} \end{pmatrix}}_{=:\boldsymbol{\nu}_{it}}.$$

   We choose $a_1 = a_2 = a_3 = 0.25$. The innovations $\boldsymbol{\nu}_{it}$ are drawn i.i.d. from

a multivariate normal distribution $N(0, \Phi)$ with

$$\Phi = \begin{pmatrix} 1 & \varphi & \varphi \\ \varphi & 1 & \varphi \\ \varphi & \varphi & 1 \end{pmatrix},$$

where $\varphi \in \{0.1, 0.25\}$ specifies the correlation between the 3 covariates.

- We set $\boldsymbol{\beta}_i = (\beta_{i,1}, \beta_{i,2}, \beta_{i,3}) = (1, 1, 1)$ for all i.
- We assume that the errors ε_{it} follow the AR(1) model $\varepsilon_{it} = a\varepsilon_{i,t-1} + \eta_{it}$, where $a = 0.25$ and the innovations η_{it} are i.i.d. normal with mean 0 and standard deviation 0.25.
- We let $\alpha = (\alpha_1, \ldots, \alpha_n)$ be a normally distributed random vector. In particular, $\alpha \sim N(0, \Sigma)$ with

$$\Sigma = \begin{pmatrix} 1 & \rho & \cdots & \rho \\ \rho & 1 & \ddots & \vdots \\ \vdots & \ddots & \ddots & \rho \\ \rho & \cdots & \rho & 1 \end{pmatrix},$$

where $\rho \in \{0.1, 0.25\}$ gives the correlation across i.

- To generate data under the null H_0, we let $m_i = 0$ for all i as before. This is without loss of generality as we explain in detail below (see our response to the second part of your comment). To produce data under the alternative, we use the bump function $m_1(u) = b \cdot \vartheta(u, 0.3, 0.1) - b \cdot \vartheta(u, 0.7, 0.1)$ with $\vartheta(u, c, d) = \mathbb{1}\{\frac{|u-c|}{d} \leq 1\}\big(1 - \frac{(u-c)^2}{d^2}\big)^2$ for $b \in \{0.25, 0.5, 0.75\}$ (depicted in Figure 1) and $m_i = 0$ for $i \neq 1$.
- We take the grid $\mathcal{G}_T$ to be the same as before: $\mathcal{G}_T = U_T \times H_T$, where $U_T = \big\{u \in [0,1] : u = \frac{5t}{T} \text{ for some } t \in \mathbb{N}\big\}$ and $H_T = \big\{h \in \big[\frac{\log T}{T}, \frac{1}{4}\big] : h = \frac{5t-3}{T} \text{ for some } t \in \mathbb{N}\big\}$.
- As before, in order to estimate the long-run error variance σ^2, we follow the procedure described in Khismatullina and Vogt (2020) with the following tuning parameters: $q = 25$ and $r = 10$.
- As before, we calculate the Gaussian quantiles based on $L = 5000$ samples.
- The number of simulation runs is 5000.

Figure 1 illustrates that the described simulation setup is quite challenging already without covariates and fixed effects. The black lines in the plot show the bump function m_1 for different heights of the bump $b \in \{0, 0.25, 0.5, 0.75\}$, where the value $b = 0$ corresponds to the null H_0 (as in this case, $m_1 = 0$ and we assume that $m_i = 0$ for all $i > 1$). The red line shows one realization of the time series $\{Y_{it}^\circ : 1 \leq t \leq T\}$ for $T = 250$, where

$$Y_{it}^\circ = m_i\Big(\frac{t}{T}\Big) + \varepsilon_{it} \tag{3}$$

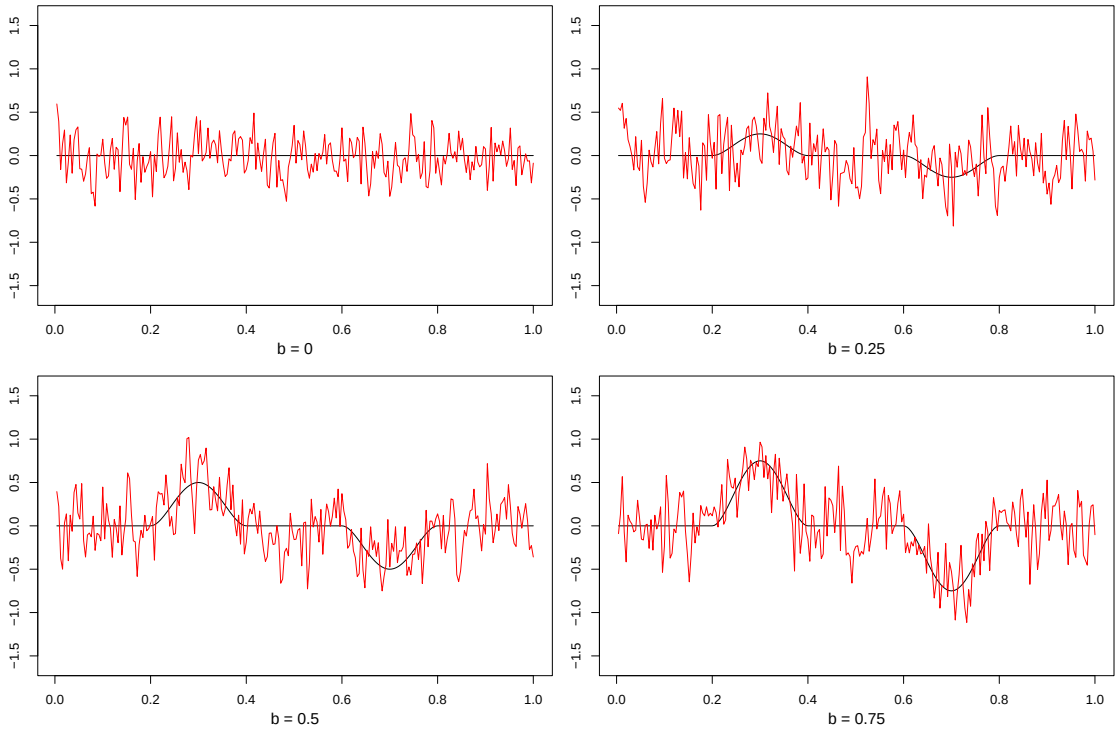

Figure 1: In black, the bump function m_1 is plotted for different heights of the bump: $b \in \{0, 0.25, 0.5, 0.75\}$ with $b = 0$ corresponding to the null H_0. In red, we depict the time series $\{Y_{it}^{\circ} = m_1(t/T) + \varepsilon_{1t} : 1 \leq t \leq T\}$ for one simulation run and $T = 250$.

is the sum of the trend function and the noise term. Put differently, equation (3) is an idealized version of our simulation design without covariates and fixed effects. The values $b = 0.25, 0.5, 0.75$ correspond to three different alternatives. In particular, as the size of the bump b gets larger, we move further and further away from the null. For the smallest value $b = 0.25$, the bump function is extremely difficult to detect as the noise level is very high, or put differently, as the signal-to-noise ratio is very small (as can be seen from the upper right panel of Figure 1). For $b = 0.5$ and $b = 0.75$, the signal-to-noise ratio slowly gets bigger, making it less hard to detect the bump function. Nevertheless, even for $b = 0.75$, there is substantial noise around the bump function, meaning that detection of the bump is still non-trivial.

The simulation results for the new more challenging design are reported in Section A.1 of the Supplement.

(ii) We can assume that $m_i = 0$ under H_0 without loss of generality for the following reason: First of all, note that given our normalization constraint $\int_0^1 m_i(u)du = 0$ for all i, the functions m_i must be the same for all i under

H_0. Now consider the multiscale test statistic

$$\widehat{\Psi}_{n,T} = \max_{(u,h)\in\mathcal{G}_T, 1\le i<j\le n} \Big\{ \Big| \frac{\widehat{\psi}_{ij,T}(u,h)}{(\widehat{\sigma}_i^2+\widehat{\sigma}_j^2)^{1/2}} \Big| - \lambda(h) \Big\}$$

with

$$\widehat{\psi}_{ij,T}(u,h) = \sum_{t=1}^{T} w_{t,T}(u,h)(\widehat{Y}_{it} - \widehat{Y}_{jt}).$$

Suppose we are under the null, that is, $m_i = m$ for all i and an arbitrary trend function m. Since

$$\widehat{Y}_{it} = m\Big(\frac{t}{T}\Big) + \varepsilon_{it} + (\boldsymbol{\beta}_i - \widehat{\boldsymbol{\beta}}_i)^\top \boldsymbol{X}_{it} + (\alpha_i - \widehat{\alpha}_i),$$

the function m cancels out exactly (not only approximately!) in the difference $\widehat{Y}_{it} - \widehat{Y}_{jt}$. Moreover, it has a negligible effect on the estimator $\widehat{\boldsymbol{\beta}}_i$ as it essentially gets eliminated by the differencing operation. A similar point applies to the estimator $\widehat{\alpha}_i$ where m shows up in the form of $\frac{1}{T}\sum m(t/T)$, which for large T is very close to $\int_0^1 m(u)du = 0$. Hence, if we simulate data under the null with different functions m, we get (almost) identical values for the multiscale statistic $\widehat{\Psi}_{n,T}$. We can thus safely resort to setting $m = 0$.

(iii) The main merit of our method is that we can perform inference uniformly over all pairs of time series (i,j) and over all locations u and bandwidths (or scales) h in the grid $\mathcal{G}_T$. Simple competitors (like the one you propose) are not uniform over the curves (i,j) and bandwidths h (as uniform confidence bands for nonparametric curves available in the literature depend on a specific choice of bandwidth). This implies that these simple competitors will be much too liberal. One could of course make a simple Bonferroni correction to fix this. However, as the underlying statistics will be strongly correlated across different (i,j) and bandwidths h, this will result in extremely conservative procedures. We demonstrate this by comparing our approach with the SiZer method from Park et al. (2009) which is the closest competitor to our method (being a multiscale test which however is not uniform over scales h) Please see the new Section A.5 in the Supplement for the details.

(iv) In order to examine the performance of our method for larger n, we rerun our simulation exercises for $n \in \{25, 50, 100\}$ and T as before, i.e., $T \in \{100, 250, 500\}$. We work with precisely the same simulation setup as specified in (i) except for the fact that we use a smaller grid $\mathcal{G}_T$ to decrease the computational burden a bit. Specifically, we take the grid

$$\mathcal{G}_T = \Big\{(u,h) \subseteq [0,1] : (u,h) = ((2s+1)h, h) \text{ for } s = 0, \dots, \Big\lfloor \frac{h^{-1}-1}{2} \Big\rfloor \text{ and } h \in \mathcal{H}_T \Big\},$$

that is, we work with a dyadic scheme (as in Wavelet analysis) with scales in the set

$$\mathcal{H}_T = \{h = 2^k h_{\min} \text{ for } k = 0, \dots, K\},$$

where $h_{\min} = \frac{\lceil \log T \rceil}{T}$ and K is such that $2^K h_{\min} \leq \frac{1}{4}$, i.e.,

$$K \leq \left\lfloor \log\left(\frac{T}{4\lceil \log T \rceil}\right) \right\rfloor \frac{1}{\log(2)}.$$

Overall, the simulation results show that the method performs well for larger n. Please see Section A.3 in the Supplement for the details.

3. *Similarly, there are many related clustering methods, including (Bayesian and non-Bayesian) methods for clustering functional data. The proposed approach is reasonable, yet should be placed in a broader context and evaluated against appropriate competitors.*

   We compare our clustering approach with the following benchmark:

   Step 1. For each i, estimate the trend function m_i by a standard local linear estimator $\widehat{m}_i = \widehat{m}_{i,h}$ with bandwidth h.

   Step 2. Estimate the distance between $\widehat{m}_i$ and $\widehat{m}_j$ by taking the supremum metric

   $$d_{ij}^{\text{bmk}} = \sup_{u \in [0,1]} |\widehat{m}_i(u) - \widehat{m}_j(u)|.$$

   Step 3. Construct the following dissimilarity measure from these supremum distances:

   $$\widehat{\Delta}^{\text{bmk}}(S, S') = \max_{i \in S, j \in S'} d_{ij}^{\text{bmk}}.$$

   Step 4. Run a HAC algorithm with the computed dissimilarities.

   This procedure is a very simple and natural benchmark, with our approach being a refinement of it. In particular: our approach replaces the simple distance measure d_{ij} by a more advanced multiscale distance measure and provides a way to estimate the number of clusters, which is not part of the simple benchmark procedure. A comparison study is presented in Section A.6 of the Supplement. Most importantly, it shows that our method provides robust results whereas the benchmark depends very strongly on the choice of bandwidth.

4. *A related Bayesian strategy is to use simultaneous band scores (simBaS) to assess whether a function differs from zero. This could be applied pairwise to the differences between functions to establish a Bayesian competitor to the proposed approach, and simply requires posterior draws from an analogous Bayesian model.*

   We mention simBaS as a possible competitor in Section ?? of the Supplement where we compare with the SiZer method from Park et al. (2009) (as discussed

in our reply to your comment 2(iii)). In view of the many other extensions and robustness checks carried out, we have decided not to run simulations on any other competitors besides SiZer. We hope you are fine with this.

5. *The application includes numerous tuning parameters (including kernels, intervals, etc.). Are the results robust to these choices? Further details are needed.*

   In the revision, we have carried out a number of robustness checks on the choice of tuning parameters. Please see the new Section A.4 of the Supplementary Material for the details. In addition, please also see our reply to comment 4 of the associate editor.

   The main tuning parameter of our methods is the grid $\mathcal{G}_T$. As a robustness check, we have rerun the simulation exercises from Section A.1 for three different grids: the grid defined in the description of the simulation setup above as well as a sparser and a denser grid (see p. 5 in the Supplement, respectively, for a precise definition). All three grids are sufficiently rich in the sense of incorporating a wide range of bandwidths (from very small to very large ones) and many time points that are scattered rather densely over the unit interval. The simulation results are very similar across the different grid specifications, indicating that our methods are insensitive to the precise choice of grid (as long as it is sufficiently rich).

   Further tuning parameters of our methods are (i) the number of bootstrap samples L to compute the Gaussian quantile, (ii) the kernel K and (iii) tuning parameters to estimate the long-run error variance σ^2. The number of bootstrap samples L is set to 5000 throughout the simulation study. As a robustness check, we have rerun everything with $L = 1000$. As expected, the results are essentially identical. We have added a short remark on this to p. 6-7 of the Supplement, but we have not reported the results. However, we are happy to do so if you think this is needed. As suggested by classical nonparametric theory, the Epanechnikov kernel (which is used throughout the paper) has good properties and the choice of kernel K is of minor importance, in particular, much less important than the choice of bandwidth. We have thus decided not to run any robustness checks on the choice of kernel. Finally, our procedure can be combined with any method off-the-shelf for long-run error variance estimation. In the simulations and the empirical applications of the paper, we use the procedure from Khismatullina and Vogt (2020), where extensive robustness checks w.r.t. to the choice of tuning parameters have been performed. We have thus not included further robustness checks on these secondary tuning parameters in the paper.

6. *The multiscale tests are designed to control the FWER. Why is that the right criterion for the types of applications in mind (compared to e.g., FDR)? Given*

that other reasonable choices exist, additional motivation for this objective is warranted.

Let α be a given value, say $\alpha = 0.05$.

(i) Controlling the FWER allows us to make the following type of confidence statements: We can claim with statistical confidence at least $1-\alpha$ (formally, with asymptotic probability at least $1-\alpha$), that the differences between the trends detected by our test method reflect true differences in the underlying curves.

(ii) Controlling the FDR would only allow us to make statements of the following form: We can claim that (on average) $100(1-\alpha)\%$ of the differences detected by our test correspond to true differences.

We think that in economic and financial applications, one is usually interested in statements of the form (i) rather than (ii). Take a financial example where one aims to compare volatility trends of different stocks. Practitioners want to make interpretations based on the differences found by the test. For instance, they would like to connect them to certain market events. For this, they want to be rather confident that the detected differences are worth interpreting (i.e. are not spurious but correspond to real differences). It is not enough to be confident that (on average) a certain percentage of the detected differences are worth interpreting. Hence, in our opinion, confidence statements of the form (i) are more apt than statements of the form (ii) in this context.
We have added these remarks on FWER vs. FDR control to Section 3.3 of the paper. See in particular Remark 3.2 therein.

7. *I'm wondering if there might be some clarification about the independence of ε_{it} across series i. In particular, suppose the intercepts α_i were instead considered random, like in mixed modeling (or Bayesian inference). Then marginally, the "new" errors $(\alpha_i + \varepsilon_{it})$ would be dependent across series i. Similar reasoning might apply to the covariates. From this perspective, the class of models might be considered more general.*

First of all, please note the terms α_i are not restricted to be deterministic. They can be either deterministic or random, we do not impose any distributional assumptions on them, and they can be correlated with the covariates $\boldsymbol{X}_{it}$ in an arbitrary way. Following the panel data literature in economics, we refer to them as fixed effects (which one may find extremely misleading beacuse "fixed" sounds like they are restricted to be deterministic). In the revision (see p.5-6), we have added some additional remarks on the terms α_i to avoid any confusion. Following your suggestion, we point out in the revised paper (see p.??) that we

allow for dependencies in the error terms $(\alpha_i + \varepsilon_{it})$ across time series i via the inclusion of the fixed effects α_i.

8. *It is claimed on p.6 that the mean function integrating to zero is "required" for identification of the intercept. I think this is a sufficient, not necessary, condition, since others might suffice.*

   We agree that the wording is a bit misleading. We have thus changed it. We use the constraint $\int_0^1 m_i(u)du = 0$ for identification as it yields a convenient normalization. However, it is of course possible to work with other constraints instead.

Reply to referee 2

Thank you very much for the careful reading of our manuscript and the interesting suggestions. We have addressed all your comments in the revision. Please see our replies to them below.

1. *Although you correctly cite Khismatullina and Vogt (2020, 2021) on which quite a bit of this new work seems to be based can you please summarize more in particular about the test proposed in Khismatullina and Vogt (2021, Journal of Econometrics), and explain where and how your proofs differ from that (e.g. by the complexity in needing to treat the covariates).*

   Khismatullina and Vogt (2023) consider the following setting: Let Y_{it} be the number of new Covid infections on day t in country i and suppose we observe a data sample $\{Y_{it} : 1 \leq t \leq T\}$ for n different countries i. A simple way to model the count data Y_{it} is by means of a Poisson distribution. In particular, we may assume that Y_{it} is Poisson distributed with time-varying intensity parameter $\lambda_i(t/T)$, that is, $Y_{it} \sim \mathcal{P}_{\lambda_i(t/T)}$. This model can be formulated as

$$Y_{it} = \lambda_i\Big(\frac{t}{T}\Big) + u_{it} \quad \text{with} \quad u_{it} = \sqrt{\lambda_i\Big(\frac{t}{T}\Big)}\eta_{it}, \tag{4}$$

   where η_{it} is a noise term with zero mean and unit variance. Equation (4) is a nonparametric trend model, where the trend and the (time-varying) variance of the error term u_{it} are given by the intensity function λ_i. For applications, however, the assumption that the trend and the error variance are exactly the same is usually too restrictive. Khismatullina and Vogt (2023) thus propose the quasi-Poisson model

$$Y_{it} = \lambda_i\Big(\frac{t}{T}\Big) + u_{it} \quad \text{with} \quad u_{it} = \sigma\sqrt{\lambda_i\Big(\frac{t}{T}\Big)}\eta_{it}, \tag{5}$$

   which extends the model in equation (4) by including the scaling factor σ. This allows the variance to be a multiple of the trend function. This model is not

nested in our framework as the error terms u_{it} have a (very particular) nonstationary structure. Nevertheless, it is much simpler in most other respects: The noise terms η_{it} are assumed to be i.i.d. both across i and t, implying that there are no dependence structures at all in the model. Moreover, there are no covariates and fixed effects included.

The multiscale test in Khismatullina and Vogt (2023) is similar in spirit to the one analyzed in the current paper. However, their theoretical analysis is completely different. In particular, their proofs build on high-dimensional CLTs for hyperrectangles developed in Chernozhukov et al. (2017), while our proofs rely on results from strong approximation theory. Hence, even without the inclusion of covariates and fixed effects, our proof strategy would be completely different. The issue is that the arguments in Khismatullina and Vogt (2023) rely heavily on the i.i.d. structure of the data and can thus not be carried over to the current model setting as far as we can see.

We haved added a condensed version of the above discussion to the revision (see p.4 in the introduction and Remark 4.3 in Section 4).

2. *As your results are of asymptotic nature, it would be good to discuss limitations – even give an example where the procedure would cease to work.*

   As we discuss in response to your comment 4 below, our method will in general have poor small sample performance when the error and covariate processes are very persistent. For example, when the error process is autoregressive of order 1 with parameter close to the unit circle (say 0.95 or 0.99), the procedure will break down in small samples (even though our asymptotic theory allows for an AR(1) error process with parameter 0.95 or 0.99). We discuss this limitation in Remark 2.1 in the revision. Please also see our answer to your comment 4 for more details.

3. *Moreover, can you at least sketch out if by something like a Bootstrap procedure (cf. Zhang et al, 2012) more of the "asymptotic flavour" of your test/cluster procedure could be remedied?*

   To be honest, it is not so clear to us how to set up a good bootstrap procedure. In what follows, we sketch some ideas and discuss the implied issues. To make things easier, consider the simplified model

$$Y_{it} = m_i\Big(\frac{t}{T}\Big) + \varepsilon_{it}$$

   without covariates and fixed effects and assume that the errors ε_{it} are i.i.d. both across i and t. In this simplified model, one may set up a multiplier or wild

bootstrap by resampling the statistics

$$\widehat{\psi}_{ij,T}(u,h) = \sum_{t=1}^{T} w_{t,T}(u,h)(\widehat{Y}_{it} - \widehat{Y}_{jt}),$$

which are the main building block of our multiscale statistic $\widehat{\Psi}_{n,T}$. In particular, one may proceed as follows: Draw a large number L of samples $S^{(\ell)} := \{\xi_{it}^{(\ell)} : 1 \leq t \leq T, 1 \leq i \leq n\}$ for $\ell = 1, \ldots, L$, where $\xi_{it}^{(\ell)}$ are i.i.d. standard normal random variables. (Note that other distributions are possible here.) Then compute the resampled statistics

$$\widehat{\psi}_{ij,T}^{(\ell)}(u,h) = \sum_{t=1}^{T} w_{t,T}(u,h)(\widehat{Y}_{it}\xi_{it}^{(\ell)} - \widehat{Y}_{jt}\xi_{jt}^{(\ell)})$$

and the corresponding multiscale statistic $\widehat{\Psi}_{n,T}^{(\ell)}$ for $\ell = 1, \ldots, L$. Finally, take the empirical $(1-\alpha)$-quantile of the bootstrap sample $\{\widehat{\Psi}_{n,T}^{(\ell)} : \ell = 1, \ldots, L\}$ as the critical value of the test.

We guess that this bootstrap approach should work. However, it will be conservative because under the alternative, the variance level of $\widehat{Y}_{it}\xi_{it}^{(\ell)}$ is in general higher than the target $\mathrm{Var}(\varepsilon_{it})$. One could fix this by subtracting an estimator of $m_i(t/T)$ from $\widehat{Y}_{it}$ before multiplying with $\xi_{it}^{(\ell)}$. However, to estimate $m_i(t/T)$ nonparametrically, we'd need to introduce an additional bandwidth parameter (for each i), which goes somewhat against the philosophy of our multiscale approach). Moreover, when the errors ε_{it} are not independent across t, one would have to correct the approach to take into account the time series correlation in the errors. Apart from these issues, it is not clear whether such a wild bootstrap approach has better theoretical properties than our simulation approach. We have thus decided not to include specific suggestions for bootstrap procedures in the paper. However, we fully agree with you that it is an interesting issue how to set up a good bootstrap in our case. We thus briefly mention this as an open question in the new conclusion section (Section 7).

4. *I am having a slight (finite sample) identification concern with (not only your) model(s) mixing deterministic (nonparametric) trends with covariate (and also error) structure which is allowed to be positively serially dependent, e.g. autoregressive (as in your examples): I think that for "any" fixed sample size it might always occur that the trajectory of a stochastic trend, an autoregressive process with roots relatively close to the unit circle, say, cannot be distinguished from the deterministic trend. Wouldn't that be potentially a problem for your (and any related) test procedure? As a follow-up on this, wouldn't you need (or to say it differently, wouldn't it be perhaps beneficial to add) some extra conditions on*

the nature of your covariates (and potentially also your errors ε?) to avoid this problem?

We fully understand the issue you point out. One may model a time series trend by a deterministic function plus noise (as we do), by a very persistent stationary process (such as an AR process with roots close to the unit circle) or by a nonstationary process (such as a unit root process). In small samples, it is essentially impossible to reliably tell how the trend is generated (whether it is deterministic or stochastic, and if stochastic, whether it is produced by a stationary or a nonstationary process). Asymptotically, it is of course possible to discern these cases by a formal statistical test. However, such a test will have power only for quite large sample sizes and will thus be rather useless in smaller samples.

What does this mean for our procedure? To obtain good performance in small samples, we require that the error (and covariate) process in our model does not produce strong stochastic trends. For our asymptotic theory to work, it is sufficient to assume that the error (and covariate) process is stationary and short-range dependent. However, this includes very persistent processes like AR processes with roots close to the unit circle. Hence, the conditions for our asymptotic theory are not sufficient to guarantee good performance in small samples. Loosely speaking, to guarantee good small sample performance, we require the persistence of the error (and covariate) process to be sufficiently mild. In our simulations where $T = \{100, 250, 500\}$, we can for example deal well with AR(1) errors with autoregressive coefficient 0.25 or 0.5. However, if we considered an AR(1) error process with 0.95, the procedure would clearly break down.

We discuss these points in Remark 2.1 in the revision.

5. *What about a naive competitor that is just based on the second derivative (= change of the slope parameters) rather than the distance based on the curves and the first derivative (as in your local linear estimator)? I believe that this could also work rather well on your economic example data in Figures 3–6? Maybe you can "benchmark" your procedure against such a simple competitor (as such a comparison is somewhat missing explicitly – although you orally compare sometimes with Zhang et al (2012)).*

   TO DO. Not sure what is meant here! Any ideas, Marina?

6. *Can your proposed test procedure be considered somehow to be equivalent to constructing a uniform confidence region where you would need to control if two (or more curves) are within the same tube (not just pointwise)? If so, it would be perhaps interesting to explain the link, and why for your test procedure it is suf-*

ficient/adequate to control the "familywise" error (does this correspond to what one does for a "uniform" region?).

One can make the following connection to uniform confidence regions:

- The main building block of our test statistic is the local average

$$\widehat{\psi}_{ij,T}(u,h) = \sum_{t=1}^{T} w_{t,T}(u,h)(\widehat{Y}_{it} - \widehat{Y}_{jt}).$$

  Since the weights $w_{t,T}(u,h)$ are rescaled versions of the kernel weights of a standard local linear kernel estimator (Fan and Gijbels, 1996), $\widehat{\psi}_{ij,T}(u,h)$ is nothing else than a rescaled difference between local linear estimators of $m_i(u)$ and $m_j(u)$ with bandwidth h.

- Now consider

$$\psi_{ij,T}(u,h) = \sum_{t=1}^{T} w_{t,T}(u,h)\Big(m_i\Big(\frac{t}{T}\Big) - m_j\Big(\frac{t}{T}\Big)\Big),$$

  which is a weighted local average of the difference between the functions m_i and m_j on the interval $[u-h, u+h]$. Our theoretical results imply that with asymptotic probability $1-\alpha$,

$$\big|\widehat{\psi}_{ij,T}(u,h) - \psi_{ij,T}(u,h)\big| \leq c_{n,T}(\alpha,h)$$

  for all $(u,h) \in \mathcal{G}_T$ and $1 \leq i < j \leq n$, where $c_{n,T}(\alpha,h) := (\widehat{\sigma}_i^2 + \widehat{\sigma}_j^2)^{1/2}(q_{n,T}(\alpha) + \lambda(h))$ with $q_{n,T}(\alpha)$ the global critical value of our multiscale test and $\lambda(h)$ defined in Section 3.2 of the paper.

- We can interpret the result from the previous bullet point as a uniform confidence band for the parameter $\psi_{ij,T}(u,h)$ (uniform over all (u,h) and $i<j$ under consideration). In particular, we get that with asymptotic probability $1-\alpha$, $\psi_{ij,T}(u,h)$ lies in the confidence interval

$$\mathrm{CI}_{ij,T}(u,h) := \Big[\widehat{m}_i(u,h) - \widehat{m}_j(u,h) - c_{n,T}(\alpha,h), \\ \widehat{m}_i(u,h) - \widehat{m}_j(u,h) + c_{n,T}(\alpha,h)\Big].$$

- Our test can now be equivalently formulated as follows: We reject the local null hypothesis

$$H_0^{[i,j]}(u,h) : m_i \text{ and } m_j \text{ are the same on the interval } \mathcal{I}_{u,h} = [u-h, u+h]$$

  if the uniform confidence band $\mathrm{CI}_{ij,T}(u,h)$ does not include 0.

We have added a condensed version of the above discussion to the revision. Please see the new Remark 3.1 therein.

7. *How in all of this does the number of curves (larger than two) play a role, in practice, for correctly calibrating your test (as least asymptotically as possible)? On the other hand, do your results reflect the fact that obviously they depend on the number of time series (or rather the number of series where trends are different, a number that you would have access to in an oracle situation)?*

   The critical value $q_{n,T}(\alpha)$ of the multiscale test is defined as the $(1-\alpha)$-quantile of the Gaussian statistic

$$\Phi_{n,T} = \max_{1\le i<j\le n} \max_{(u,h)\in\mathcal{G}_T} \Big\{ \Big| \frac{\phi_{ij,T}(u,h)}{(\sigma_i^2+\sigma_j^2)^{1/2}} \Big| - \lambda(h) \Big\}, \tag{6}$$

   where

$$\phi_{ij,T}(u,h) = \sum_{t=1}^{T} w_{t,T}(u,h) \left\{ \sigma_i(Z_{it} - \bar{Z}_i) - \sigma_j(Z_{jt} - \bar{Z}_j) \right\},$$

   with Z_{it} being independent standard normal random variables for $1 \le t \le T$ and $1 \le i \le n$ and $\bar{Z}_i = \bar{Z}_{i,T} := T^{-1}\sum_{t=1}^{T} Z_{it}$. As one can see, the statistic $\Phi_{n,T}$ depends on both the number of time series n and the time series length T. This implies that its $(1-\alpha)$-quantile and thus the critical value of the multiscale test depends on both n and T. Hence, the calibration of the test (or put differently, the choice of the critical value) is very "un-asymptotic" in the sense of reflecting the size of n and T.

8. *Here is a small series of remarks towards needing to choose (u,h) – an example for a practical choice is given in Section 7, only (a bit late): Your localised multiscale method requires to discretize the continuous (u,h). I am wondering if the way to do this plays a role for the properties of the resulting practical procedure. Can you please also compare with wavelet-based multiscale methods which are based somehow on a "built-in" way of choosing the location-scale parameters (u,h)?*

   In the revision, we give an example of such a practical choice much earlier (see the last few sentences in Section 3.2).
   Our methods take into account all points (u,h) in the discrete grid $\mathcal{G}_T$ (which is finite but grows with sample size). As one can see from equation (6), the Gaussian statistic depends on the grid $\mathcal{G}_T$. Hence, the test is calibrated differently depending on the grid. In that sense, the test adapts to the choice of grid: if the grid is quite sparse, the critical value will tend to be smaller, if it is quite fine, it will be larger. One can show formally (at least for a very simplified version of our multiscale test; also see the results in Dümbgen and Spokoiny (2001)) that the critical value becomes stable if we make the grid finer and finer. This means that the exact choice of the grid (i.e. the exact way of discretizing) should play

a negligible role as long as the grid is not chosen too sparse. In the revision, we support this claim by simulation evidence. Please see the new robustness checks in Sections A.3 and A.4 of the Supplement where we run our methods with different grids. We in particular include a dyadic grid which is (to use your words) the "built-in" way of choosing the location-scale parameters (u, h) in wavelet analysis. Please also see our reply to comment 5 of referee 1.

9. *page 12, around equation (3.6): it took me a moment to understand that you are talking about the standard local linear estimator (of Fan and Gijbels) here, you might want to make this clearer.*

   We have made this clearer now. Please see the beginning of Section 3.2 in the revision where we write: "Note that the weights $w_{t,T}(u, h)$ are rescaled versions of the kernel weights of a standard local linear estimator (Fan and Gijbels, 1996). Hence, the statistic $\widehat{\psi}_{ij,T}(u, h)$ is nothing else than a rescaled difference between local linear estimators of $m_i(u)$ and $m_j(u)$ with bandwidth h."

10. *I understand the heuristics behind using the Gaussian version (3.12) of the test statistics in the "idealised" situation but what about the "non-idealised" situation of unknown variances σ^2 and unknown parameters β? Is the Gaussian-based MC simulation method still valid when you need to estimate those parameters?*

    Yes, the Gaussian-based MC simulation method is still valid when σ^2 and $\boldsymbol{\beta}_i$ are estimated. In particular, as stated in Proposition 4.3, our multiscale test has (asymptotically) the correct size $(1 - \alpha)$ under the null if we take the $(1 - \alpha)$-quantile of the Gaussian statistic (3.12) to be the critical value of the test. In the proof of Theorem 4.1 (which underlies Proposition 4.3), it is formally shown that the effect of estimating σ^2 and $\boldsymbol{\beta}_i$ is negligible (see Step 1 of the proof for σ^2 and Step 4 of the proof for $\boldsymbol{\beta}_i$).

11. *Again about the choice of (u, h): what happens with expressions (such as in equation (4.1)) which depend on $\max_{(u,h)}$ in practice where you have to discretize this (u, h)? I do not think that a maximum over a continuous location-scale parameter can be treated the same way as one over a discrete one? Does the choice of the grid $\mathcal{G}_T$ influence the results here, don't you need some (additional) conditions on the grid (its spacing etc)? This refers, e.g. to the simulation section 6, page 24 where – in passing – you might want to change the strange wording there where you say "for some t in N" and rather detail the specification of the grid in t here as you do later in Section 7.*

    All maxima in the paper are taken over a finite number of points. For example, the maximum in equation (4.1) is taken over all pairs (i, j) with $1 \leq i < j \leq n$ and over all (u, h) in the finite grid $\mathcal{G}_T$. Hence, we consider a finite grid of points

(u, h) in both theory and practice. (Note that in our theoretical considerations, we let the grid size grow with the sample size.)

Please see our reply to your comment 8 on how the choice of grid $\mathcal{G}_T$ influences our methods.

We do not need extra conditions on the grid $\mathcal{G}_T$ because the family of hypotheses $\{H_0^{[ij]}(u, h) : 1 \leq i < j \leq n, (u, h) \in \mathcal{G}_T\}$ that we test depends on the chosen grid $\mathcal{G}_T$. Hence, if we have a small/large grid, we test a small/large number of hypotheses. As an extreme (admittedly a boring one, but it nevertheless helps to make the point), if the grid $\mathcal{G}_T$ only contains a single point, say $(u, h) = (0.5, 0.1)$, then we can only make confidence statements about the trend functions m_i on the interval $[0.5 - 0.1, 0.5 + 0.1]$.

Finally, as you suggest, we have changed the wording on p.24 in the previous version of the paper (see p.?? in the revision).

12. *Section 7, page 41, lines 45-50: can you develop this conjecture a bit?*

We guess you mean the conjecture on "Section 7, page 31, lines 45-50". Let us briefly repeat this conjecture: Contrary to Knoll et al. (2017), our test finds significant differences in the trending behaviour of the observed time series, specifically, between the time trends of Belgium and Denmark and of Belgium and the USA (see Figures 5 and 6 in the paper, respectively). We conjecture that this discrepancy in the results is "due to the fact that the method used in Knoll et al. (2017) does not account for effects of other factors such as GDP or population growth, while our test allows to include various determinants of the average house prices in model (7.1)". We in particular believe that the rate of population growth is the main factor that causes the discrepancy in the results. The influence of this factor on house prices greatly varies across countries. Specifically, the estimated regression coefficient of the population growth rate for Belgium (BEL), Denmark (DNK) and the USA is $\widehat{\beta}_{\text{BEL},2} = 3.45$, $\widehat{\beta}_{\text{DNK},2} = 0.19$ and $\widehat{\beta}_{\text{USA},2} = 0.80$, respectively.

To substantiate our conjecture, we have repeated the analysis from the current Section 6 (Section 7 previously) with one distinction: we do not include the population growth rate as a regressor in the model. As conjectured, our test does not find any significant differences between Belgium and Denmark and between Belgium and the USA in this case (see Figures 2 and 3 above), which is in line with the findings presented in Knoll et al. (2017).

We have added a few more sentences on our conjecture to Section 6 on p.27-28.

13. *You might want to add a Conclusion Section which could both serve to recall the difficulties encountered in treating the more general situation of more than two curves and the presence of covariates, and also discuss some of the aforemen-*

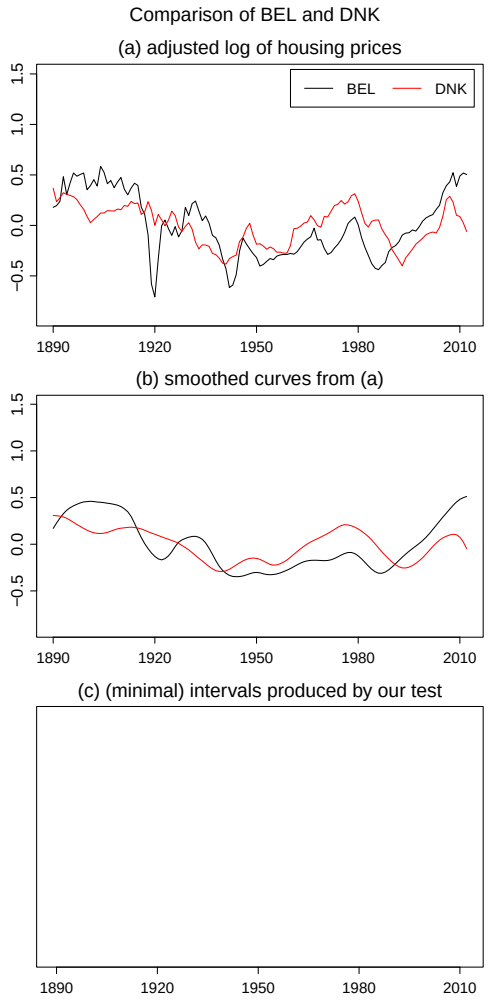

Figure 2: Test results for the comparison of the house prices in Belgium and Denmark not accounting for population growth.

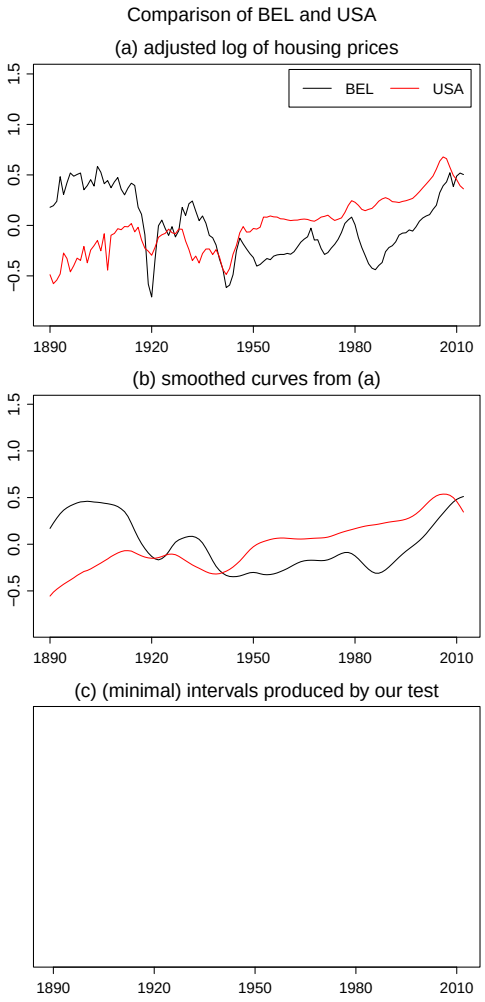

Figure 3: Test results for the comparison of the house prices in Belgium and the USA not accounting for population growth.

Note: In each figure, panel (a) shows the two augmented time series of the house prices, panel (b) presents smoothed versions of the augmented time series, and panel (c) depicts the set of intervals $\mathcal{S}^{[i,j]}(\alpha)$, which is empty in the present case.

tioned points on Bootstrap alternatives or on potential competitors.

As suggested, we have added a conclusion section (see the new Section 7) where we discuss these points.

14. *Develop more to which extent the second data application (in the Supplement) brings insights beyond the one of the first (and why you chose to present the first and not the second in the main body of the text).*

The two applications serve different purposes: The data example on GDP growth in the Supplement is taken from Zhang et al. (2012). It mainly serves as a

comparison study. In particular, it allows us to compare our approach with a previously established method on a real data set and, in particular, to highlight the advantages of our method. The application example in the main body of the text, in contrast, is (to the best of our knowledge) original rather than taken from another paper. It explores the differences between the trending patterns of house prices across countries, which is a subject of broad interest to economists, policymakers and the public alike. We thus think it nicely illustrates the usefulness of the proposed method in real-world economic analysis.
Ideally, we would have liked to keep both application examples in the main paper. However, to meet the limit of 35 pages, we had to relegate one of them to the Supplement. As we wanted to have an original application in the main paper, we kept the example on house prices and shifted the one on GDP growth to the Supplement.

15. *Supplement section page 15, line 49 – a notational detail: should the first* o_p *be* O_p *if the* $\rho_T = o(1/\log(T))$ *or vice versa?*

    We have changed the first o_p to O_p.

16. *It would be good to explain somewhere in the main body (Section 3 or 4?) the additional difficulties in proving the results in the presence of the covariates.*

    The inclusion of covariates produces additional approximation errors. In the proof of Theorem 4.1 (see in particular Step 4 therein), we show that these additional errors are negligible asymptotically. However, as the proof is very technical, we found it quite difficult to explain the underlying ideas verbally in only a few sentences. [Marina: Any ideas here?] We have thus decided to point out in Remark 4.2 in Section 4 that the inclusion of covariates produces additional approximation errors and refer to Step 4 of the proof of Theorem 4.1 for the details how to handle these errors. We hope you are fine with this solution.

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