



**Comment on: ‘The quest for a consistent signal in ground and GRACE gravity time-series’, by Michel Van Camp, Olivier de Viron, Laurent Metivier, Bruno Meurers and Olivier Francis**

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# Comment on: ‘The quest for a consistent signal in ground and GRACE gravity time-series’, by Michel Van Camp, Olivier de Viron, Laurent Metivier, Bruno Meurers and Olivier Francis

D. J. Crossley,<sup>1</sup> J.-P. Boy,<sup>2</sup> J. Hinderer,<sup>2</sup> T. Jahr,<sup>3</sup> A. Weise,<sup>3</sup> H. Wziontek,<sup>4</sup> M. Abe<sup>5</sup> and C. Förste<sup>5</sup>

<sup>1</sup>Department of Earth and Atmospheric Sciences, St. Louis University, St. Louis, MO 63108, USA. E-mail: [crossleydj@gmail.com](mailto:crossleydj@gmail.com)

<sup>2</sup>EOST-IPGS (UMR 7516 CNRS/Université de Strasbourg), 5 rue Descartes, F-67084 Strasbourg, France

<sup>3</sup>Institute of Geosciences, General Geophysics, Friedrich-Schiller-University, Burgweg 11, D-07749 Jena, Germany

<sup>4</sup>Federal Agency for Cartography and Geodesy (BKG), Richard-Strauss-Allee 11, D-60598 Frankfurt/M., Germany

<sup>5</sup>Helmholtz Centre Potsdam, Research Centre for Geosciences GFZ, Telegrafenberg, D-14473 Potsdam, Germany

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## SUMMARY

The paper in question by Van Camp and co-authors [MVC] challenges previous work showing that ground gravity data arising from hydrology can provide a consistent signal for the comparison with satellite gravity data. The data sets used are similar to those used previously, that is, the gravity field as measured by the GRACE satellites versus ground-based data from superconducting gravimeters (SGs) over the same continental area, in this case Central Europe. One of the main impediments in this paper is the presentation that is frequently confusing and misleading as to what the data analysis really shows, for example, the irregular treatment of annual components that are first subtracted then reappear in the analysis. More importantly, we disagree on specific points. Two calculations are included in our comment to illustrate where we believe that the processing in [MVC] paper is deficient. The first deals with their erroneous treatment of the global hydrology using a truncated spherical harmonic approach which explains almost a factor 2 error in their computation of the loading. The second shows the effect of making the wrong assumption in the GRACE/hydrology/surface gravity comparison by inverting the whole of the hydrology loading for underground stations. We also challenge their claims that empirical orthogonal function techniques cannot be done in the presence of periodic components, and that SG data cannot be corrected for comparisons with GRACE data. The main conclusion of their paper, that there is little coherence between ground gravity stations and this invalidates GRACE comparisons, is therefore questionable. There is nothing in [MVC] that contradicts any of the previous papers that have shown clearly a strong relation between seasonal signals obtained from both ground gravity and GRACE satellite data.

**Key words:** Spatial analysis; Satellite gravity; Time variable gravity; Hydrology; Europe.

## 1 INTRODUCTION

Although this subject has received geodetic attention for more than a decade, there are several recent publications that are the most relevant here, *viz.* those using empirical orthogonal function (EOF) analysis: Neumeyer *et al.* (2008), Crossley *et al.* (2012), Abe *et al.* (2012) and Weise *et al.* (2012). In these papers, comparisons are made using the EOF decomposition of GRACE satellite gravity fields (computed as if at the Earth's surface), surface gravity as measured by a sparse network of superconducting gravimeters (SGs) that are part of the global geodynamics project (GGP) and global hydrology models. These papers came to the same conclusions,

first that the annual variations in all the data sets are clearly revealed in the dominant modes of the EOF decomposition, in some cases the first mode being able to explain more than 70 per cent of the variance reduction. Secondly, that although there can be a difference between the amplitude of the SG modes compared to those from GRACE and the global hydrology, this is caused by the complex effects of hydrology at stations where the SGs are below the soil moisture layer (but still above the groundwater). A number of other publications that did not use EOF analysis, for example, Neumeyer *et al.* (2006), Weise *et al.* (2009) and Naujoks *et al.* (2010), all succeeded in showing that by paying careful attention to the local hydrology contributions, it was possible to reconcile the

observations of SG stations with the GRACE data. We discuss the various issues involved in roughly the same order as presented in Van Camp *et al.* (2014), henceforth referred to as [MVC].

## 2 DATA PROCESSING

### 2.1 Trend removal

In [MVC], Section 2.1, the authors fit a second-order polynomial for each SG ‘... to remove possible non-linear instrumental drift or other long-term geophysical effects, which are outside the scope of this study ...’, but they do not provide these functions explicitly. In Section 2.3, they state that a ‘... second-degree polynomial was applied to all SG, hydrology and GRACE series, in order to remove any possible bias that may be caused by non-linear slopes caused by instrument drift or by residual long-period geophysical signals that are beyond the scope of this paper’. It is not clear why a second-order polynomial is chosen to remove SG drift, especially as Van Camp & Francis (2006)—this is the same paper quoted by [MVC]—show explicitly that the SG drift should be either a linear or exponential function. The argument that other long-period components can be removed by a second-order polynomial seems to us weak, and hardly very effective, as can be seen, for example, from the series for station WE in their Fig. 2. We consider the removal of anything beyond a linear trend from these data sets to be unnecessary for the purpose of this data comparison.

### 2.2 Composite annual cycle

Throughout [MVC], the authors use the terms composite, common, annual and seasonal to mean slightly different things, but never really define the differences. In Section 2.1, a ‘composite annual cycle’ is obtained for each station individually by stacking the data set with a pre-defined period (1 yr in this case). But such a reduction does not reflect a truly *common signal* for the study region, and hence cannot be used to bring out any common non-seasonal signal. Only by removing a common annual signal for all stations, can similarities at sub- or inter-annual periods be analysed, as was attempted in Neumeyer *et al.* (2008) using the first principal component of an EOF analysis. If one removes a signal with broad frequency content (the composite annual cycle is not a single frequency wave) containing a common part as well as site-specific local parts for each station, it is not surprising that little further agreement across the series can be expected. Because the seasonal signal from hydrology is not expected to be equal either in amplitude or phase or with a constant period  $T$  at every station, the residual after subtraction may contain artefacts that are not real. This wrong assumption is one of the main sources of the confusion in [MVC].

That the removal of a composite seasonal cycle from the SG data suppresses a great deal of the annual signal in the SG data can be seen by comparing Figs 2(a) and (b). The reader should not be misled in assuming that Fig. 2(b) represents the series used subsequently in the paper, for this is not the case. At various stages later on, the series are treated both with and without this composite signal. For example, the common signal is restored in Figs 5 and 6, which concentrates on the annual signal. This makes it difficult to follow the logic of the paper.

### 2.3 Global hydrological calculations

In their Section 2.2, the authors first state that they use the global hydrological loading from one of the online loading services, and

that is very familiar to us. But then they state that the hydrology grids were decomposed using spherical harmonic (SH) coefficients and then converted into ground gravity using load Love numbers. For what they want, this is not appropriate. The difference lies in the way the loading is used. In previous papers, for example, Crossley *et al.* (2012), the goal was to extract continental-scale coherence (EOF modes) from the total signal at all SG stations in order to compare with GRACE. But when [MVC] do the loading for site-by-site comparisons using pairwise correlations, it is crucial to choose the appropriate sign for the local contribution depending on the location of the local masses with respect to the gravimeter. We show here in Section 3 that their procedure gives incorrect results.

### 2.4 GRACE processing

One positive feature of the [MVC] paper lies in the extension of the SG series to a longer time span than previous studies, and the inclusion of stations Pecny and Walferdange (the latter data are not available online). In addition, they include a larger number of different GRACE solutions, and isolate the non-tidal ocean loading contribution as one area where GRACE data processing includes a model and the SG series often do not. By re-processing three of the GRACE solutions by subtracting this correction, this generates additional GRACE solutions for consideration. This multiplicity of GRACE options, however, is not discussed in any detail in their Fig. 6, because the authors are clearly more anxious to emphasize the deficiencies of the SG data rather than examine the disparities between the different GRACE solutions or the different global hydrology models.

### 2.5 Data sampling

In [MVC] prior to further analysis, the GRACE solutions ‘... were linearly interpolated to 5 d to compare directly with the SG data ... which ... avoids losing information’. But this treatment of the different temporal resolutions of the various data series is inadequate. All previous papers used the much preferred method of downsizing and properly filtering the SG series (from GGP 1 min data) to the original (and carefully designed for) resolution of the GRACE solutions, which is a 10 d sampling for GRGS and a 1 month sampling of the Level 2 series derived from either the CSR or GFZ data centres. The method used by [MVC] inevitably means that for GRACE solutions with monthly resolution, some of the shorter period information at 5-d interpolation will come from the interpolation itself. A linear change of gravity within 1 month cannot reflect natural variability, especially after removing most of the seasonal signal. Obviously, this must distort the comparisons based on EOF analysis.

Our criticism, however, is not simply one of a sampling issue. It is important because it assumes that we can fairly compare two data sets with different spectral contents. We cannot bridge both spatial and temporal gaps at the same time. We do not know for sure what the relative fraction of the periods lying between 5 and 10/30 d is in the energy spectrum, but the authors should have filtered them out to make the two data sets consistent. It is obvious that the SG series will have much higher energy at 5 d than the GRACE data and this will affect all their conclusions. It is a matter of having both data sets detect the same physical processes that are characterized by specific scales in both time and space, that is, in our case, the mesoscale processes in the atmosphere and hydrosphere. We suspect that no one familiar with GRACE data would try to simulate satellite data at 5-d sampling by the method in [MVC].

### 3 LOADING CALCULATIONS

We note first that in [MVC], page 4, the description of how the GRACE time-series were obtained is misleading. From GRACE observables, sets of SH coefficients are usually derived directly, except for Mascon-type solutions which were not used in this study. Therefore, the statement ‘GRACE time-variable gravity was decomposed into SHs, and then reconstructed at the SG station location as ground gravity values, using the appropriate combination of load Love numbers’ is quite confusing, since no decomposition is necessary, but simply a synthesis of the SH coefficients provided by the data centres.

More importantly, the hydrological loading calculations themselves are wrong. Loading computations can be performed either in the spectral (i.e. SHs) or the spatial (i.e. using Green’s function) domains. De Linage *et al.* (2009) provided the mathematical derivation of the surface gravity changes due to a surface load:

$$\delta g(\theta, \lambda, t) = \sum_{n=0}^{+\infty} g_n \sum_{m=0}^{+\infty} w_n^m(t) Y_n^m(\theta, \lambda), \quad (1)$$

where  $w_n^m(t)$  and  $Y_n^m(\theta, \lambda)$  are the degree  $n$  and order  $m$  harmonic coefficient of the water equivalent depth and Associated Legendre function, respectively. Eq. (1) is equivalent to eq. (6) of de Linage *et al.* (2009) where the gravity change is expressed as a function of the surface density  $\sigma_n = \rho_w w_n^m(t)$  with  $\rho_w$  as the water density. The coefficient  $g_n$  depends on the relative position of the gravimeter with respect to the ground:

$$g_n = -4\pi G \rho_w \frac{n + 2h'_n - (n+1)k'_n}{2n+1}, \text{ if the instrument is below the ground, or}$$

$$g_n = -4\pi G \rho_w \frac{-(n+1) + 2h'_n - (n+1)k'_n}{2n+1}, \text{ if the instrument is above the ground}$$

where  $G$  is the gravitational constant and  $h'_n$  and  $k'_n$  are the load Love numbers of degree  $n$ .

We use the Green’s function formalism introduced by Farrell (1972), but note that the complete formula including the local (Dirac) term can be found in Boy *et al.* (1998, eq. 7), so the gravity variations become:

$$\delta g(\theta, \lambda, t) = \int \int G(\psi) w(\theta', \lambda', t) \cos \theta' d\theta' d\lambda', \quad (2)$$

where  $G(\psi)$  is the Green’s function and  $\psi$  the angular distance between the gravimeter at  $(\theta, \lambda)$  and the water-depth load  $w()$  at  $(\theta', \lambda')$ . The Green’s function again depends on the relative position of the instrument with respect to the ground:

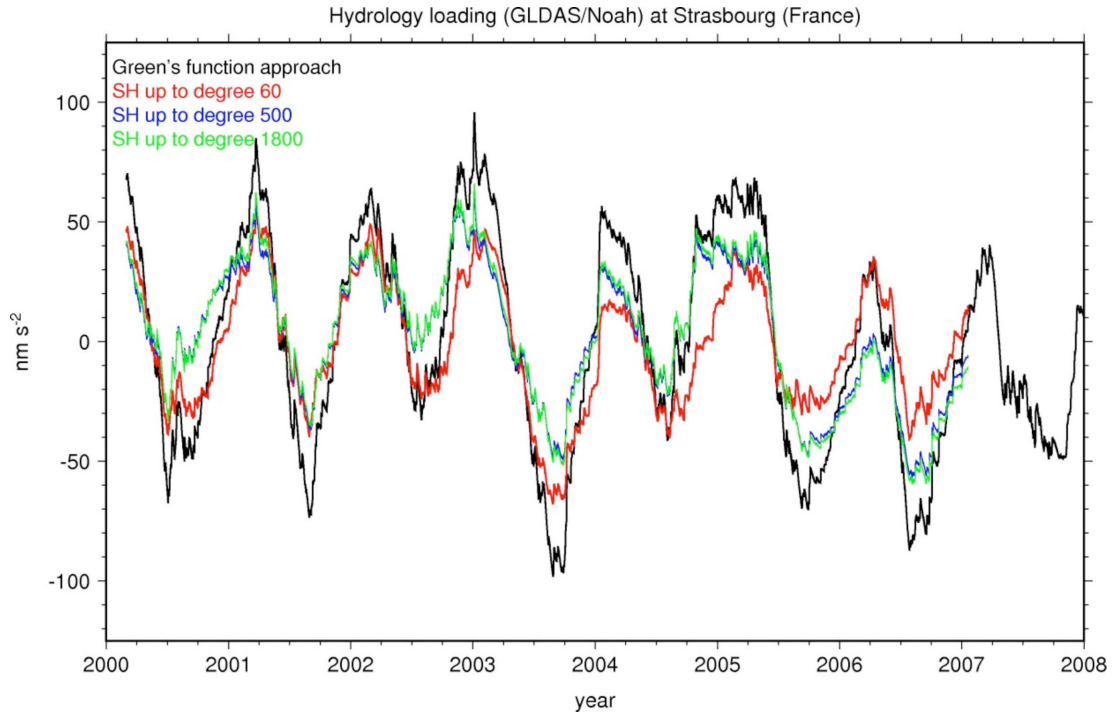
$$G(\psi) = -G \rho_w \sum_{n=0}^{+\infty} (n + 2h'_n - (n+1)k'_n) P_n(\cos \psi) + 2\pi G \rho_w \delta(\psi),$$

$$G(\psi) = -G \rho_w \sum_{n=0}^{+\infty} (n + 2h'_n - (n+1)k'_n) P_n(\cos \psi) - 2\pi G \rho_w \delta(\psi),$$

for locations above and below the ground, respectively, with  $P_n$  the degree  $n$  Legendre polynomial and  $\delta(\psi)$  the Dirac function. The last term corresponds to the Bouguer approximation that can be used as the local hydrological effect if no precise model of the observatory surrounding is available. In their first paragraph on page 4, [MVC] wrote that the Green’s function formalism can only be considered for stations above ground. As shown above, this is incorrect.

Eqs (1) and (2) are strictly identical if the sums are performed up to  $+\infty$ . In practice, the SH approach must be truncated at a maximum degree  $N$ , and due to the Kummer method (Farrell 1972), the infinite sum in the Green’s function expression is approximated.

[MVC] modelled the surface gravity variations directly from GRACE monthly Stokes coefficients (page 4) using the SH decomposition up to  $N \approx 60$ . We show in Fig. 1 the differences when computing the gravity variations from the GLDAS/Noah hydrology



**Figure 1.** Hydrological loading computed at Strasbourg using GLDAS/Noah model, using SH decomposition with different truncation degree (60, 500 and 1800) or the Green’s function approach.

**Table 1.** Linear trend ( $\text{nm s}^{-2} \text{ yr}^{-1}$ ) and annual amplitude ( $\text{nm s}^{-2}$ ) and phase (in days) of hydrological loading computed using SH decomposition or the Green’s function formalism.

Station	Method	Trend	Amplitude	Phase
MO	SH1800	7.026	32.180	71.588
MO	SH500	7.273	31.248	71.451
MO	SH60	0.950	31.522	66.329
MO	Green’s function	8.106	61.076	66.977
ST	SH1800	− 6.061	24.781	48.808
ST	SH500	− 5.508	23.804	46.866
ST	SH60	− 1.522	31.078	63.314
ST	Green’s function	− 3.807	49.957	55.032
VI	SH1800	0.411	21.841	54.513
VI	SH500	0.527	21.497	55.438
VI	SH60	4.087	21.195	70.648
VI	Green’s function	0.375	49.272	60.287

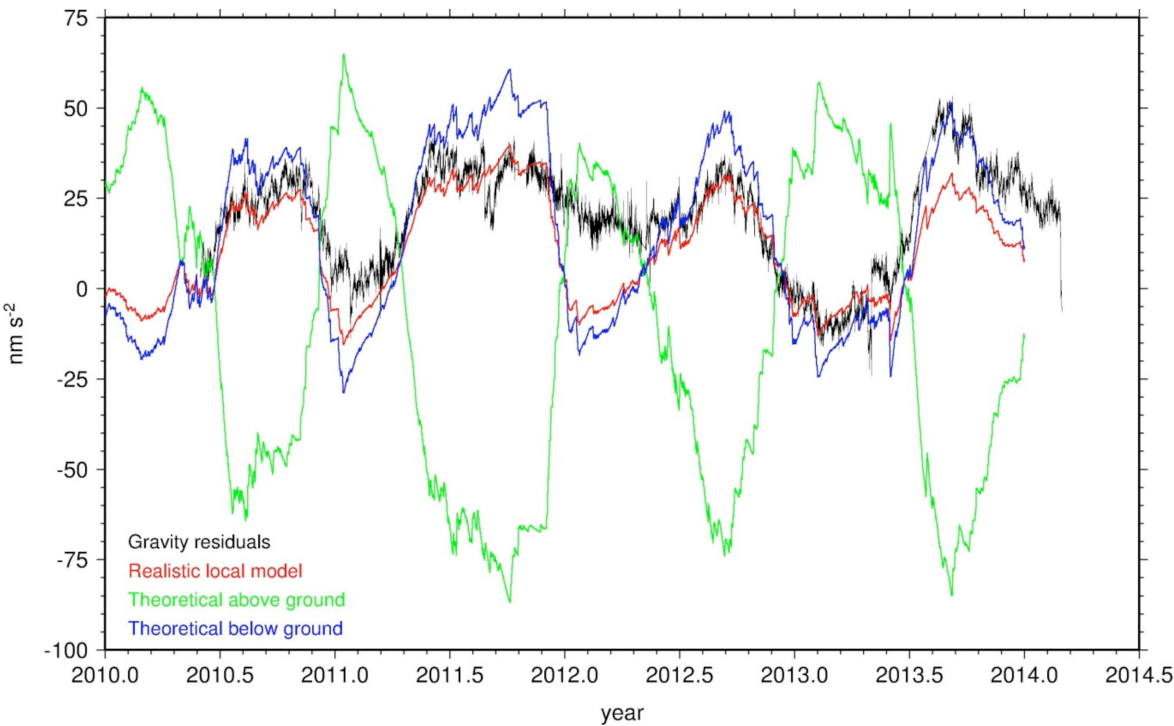
model in the spectral domain (with different truncation degrees) and the spatial domain. This model has a time sampling of 3 hr and a spatial resolution of  $0.25^\circ$  ( $n \approx 500$  at  $45^\circ$ ). Because the truncation of the infinite sum, even at a high degree (here 1800), cannot correctly approximate the Bouguer asymptotic value, there are large differences between the two approaches. In effect the local terms appear as a Dirac term, and cannot be approximated by a truncated SH approach because its expansion has equal energy at all degrees. [MVC] compared annual amplitude and phase of SG residuals with loading estimates from GRACE-retrieved equivalent water height using SHs and loading estimates from hydrology models computed using the Green’s function formalism (see <http://loading.u-strasbg.fr/GGP/>). As shown previously, the first method will underestimate the local Newtonian attraction. We compare in Table 1 of this comment the linear trend, the annual amplitude and phase computed for the 2000–2007 period of surface gravity variations for three stations in Europe (Moxa in

Germany, Strasbourg in France and Vienna in Austria) modelled with GLDAS/Noah using the Green’s function formalism and the SH approach truncated at degree 60, 500 or 1800.

There is a clear factor of about 2 between the loading computed with the Green’s function formalism and the SH approach truncated at degree-60, although the same hydrology model was used here. In their fig. 6, [MVC] had to divide the loading series computed with GLDAS/Noah and ERA interim hydrology models (Green’s function formalism) to plot them with their loading estimates from GRACE monthly Stokes coefficients. One of their conclusions is that global hydrology models are inadequate to estimate the local attraction, compared to GRACE for example. The truth is rather the complete opposite, because their factor 2 between the different approaches comes from their misunderstanding of the mathematical derivation of eqs (1) and (2).

Further, in their fig. 6, [MVC] claimed to compare hydrology loading estimates with SG residuals. As they only consider the case of above ground stations in their loading estimates, they chose to invert the sign of the SG records for below ground stations. This is clearly too simplistic. Basically, the hydrology loading is the sum of the local Newtonian attraction (about 80 per cent of the total signal) and the non-local signal. Only the sign of the local effect has to be inverted when considering both above/below ground station, and certainly not the non-local contribution.

To illustrate our point, Fig. 2 of this comment shows gravity residuals from the underground station of Strasbourg for the period 2010–2014 compared with computed (using the correct Green’s function formalism) hydrology loading effects with GLDAS/Noah model. In green, we model the loading effects assuming a station above the ground, and in blue an underground station. Clearly, the correct modelling shows variations of the same order of magnitude. If we invert the sign of the SG residuals, as done by [MVC], the annual amplitudes no longer match. Consequently, their conclusion, that is, that hydrology models (or GRACE) cannot be used for



**Figure 2.** Gravity residuals at Strasbourg (black) and hydrological loading estimates from GLDAS/Noah model with different geometry for the local contribution (green: station above ground, blue: station below ground and red: a more realistic geometry).



precise computation of loading effects, is again false. To be explicit, [MVC] computed hydrology loading from GRACE-retrieved equivalent water height using the SH approach, truncated at a degree-60 (resolution of GRACE solution); they also downloaded hydrology loading time-series from the <http://loading.u-strasbg.fr> loading service, computed from different global hydrology models but using the Green's function approach. Their mismatch between SG residuals, GRACE derived loading (using the SH approach) and loading estimated from global hydrology models (using the Green's function formalism) is due to the fact they used wrong mathematical derivations.

The local attraction (Bouguer term) can be improved by taking into account the actual topography in the vicinity of an instrument. For example, in Strasbourg, Longuevergne *et al.* (2009) found a local admittance of  $-0.305 \text{ nm s}^{-2} \text{ mm}^{-1}$  of water with a precise high-resolution digital elevation model (DEM), compared to the default value of  $-0.42 \text{ nm s}^{-2} \text{ mm}^{-1}$  for a plane-layer approximation. Although the global GLDAS/Noah hydrology model cannot explain all the remaining gravity variations, the differences between observed and modelled hydrology loading are not as large as claimed by [MVC]. Thus, precise hydrology modelling is required to fully explain gravity observations, as demonstrated by Longuevergne *et al.* (2009), Creutzfeldt *et al.* (2010) and Naujoks *et al.* (2010).

#### 4 COMMON VARIABILITY AND EOF ANALYSIS

We note that comments here about EOF analysis also, of course, apply to its derivatives such as multichannel singular spectral analysis (MSSA), for example, Rangelova *et al.* (2012).

Section 3 of [MVC] argues that any common variability in a set of 2-D (x,y) time-series negates the techniques of cross-correlation analysis and EOF decomposition. This claim would seem to have far reaching implications if applied to the numerous papers that have used the EOF techniques for GRACE data, for example, de Viron *et al.* (2006); Schmeer *et al.* (2012). It is somewhat ironic that de Viron *et al.* (2006) state explicitly '... to search for an ENSO signal in the GRACE data, the EOF decomposition is particularly well-suited'. As discussed by Kim & Wu (1999), the EOF technique is indeed well-suited for the examination of signals that contain both on the ground-satellite gravity and hydrology signals referred to above. Simply put, we do not believe that there is any evidence that periodic components disqualify the EOF technique. In support of their point of view, they refer to Von Storch & Zwiers (1999), which is a general textbook quoted without an explicit page reference, and hence of limited use.

Crossley *et al.* (2012) showed explicitly (in their Appendix) that an EOF analysis when applied only to the annual components of the SG series made perfect sense when interpreting the modes in terms of the phases and amplitudes of the individual series. EOF decomposition is able to satisfactorily combine individual series even where pairwise correlations may be weak or even partly reversed (due to some stations being underground). We should also point out that EOF analysis should not be confined (as in [MVC]) to only the first mode; more than two modes are needed to represent the variability of all the GGP stations as recognized in the previous EOF publications. Crossley *et al.* (2012) found no large difference in the PC1 solutions whether using the three above-ground stations or the full set of stations, but the second and higher modes clearly

reveal the contribution of opposite signals from the below-ground stations, a point missed by [MVC].

By comparison, [MVC] prefer to consider pairwise correlations to make the point that there can be uncorrelated signals between specific stations. We might all agree that the coverage of stations in Central Europe is too sparse to show convincingly that high spatial correlation between nearby stations indeed must be commonplace. Rather fortuitously, Zhang *et al.* (2014) recently analysed two SG residual series at Strasbourg and Black Forest Observatory, separated across the Rhine valley by a distance of 58 km—a factor 10 smaller than the GRACE averaging radius. They found a coherence higher than 0.8 for periods between 5 and 25 d, despite the very different local situation of the two instruments.

A significant contradiction arises in their EOF treatment, starting after their equation (3): '... Let us take the seven stations ... note that the annual signal was not filtered out' followed two sentences later by '... Here, after filtering for the seasonal cycle we computed the eigenvectors and associated time-series'. Which is correct? This problem arises because of the unnecessary confusion generated in [MVC] about the role of a common signal. If one were to follow the approach in [MVC] of determining the significance (or not) of correlations between two data sets with a dominant annual signal, then the results of many studies should be discarded as not significant, in particular studies comparing GPS to GRACE (see Tregoning *et al.* 2009). Why should methods that are commonly used for GPS/GRACE comparison not also be applied to GGP/GRACE?

[MVC] claim that their analysis shows a very poor SG coherence, that is, figs 5 and 6, but this is not altogether convincing. Despite all of their processing problems, the SG series are overall quite coherent with the numerous GRACE and global hydrology solutions (excepting the hydrology is a factor 2 in error, as discussed above). One should note again the problem in the caption to Fig. 5; it is not correct just to invert the sign of the SG series for underground stations, but only the local component of hydrology (as done correctly in previous publications).

#### 5 LOCAL EFFECTS ON GRAVITY

Considering the interpretation of a single SG point series, one can divide the gravity effects from either the atmosphere or the hydrosphere into local, regional and global contributions. Everyone would agree that due to the inverse square law of gravity, the major contribution of such signals comes from within a few 100 m–1 km of the station, for example, Creutzfeldt *et al.* (2008). Several studies (e.g. Weise *et al.* 2012) have shown that for some stations such as MO, ST and VI, the local hydrology effect is quite large and with a reversed sign, compared to a surface station, that consequently can dominate the regional part.

But a satellite such as GRACE does not discriminate local effects from regional or global contributions, and for most solutions, the SH coefficients are global and the field can be reconstructed over a region of perhaps 500 km. So, the averaged total hydrology (mainly the local effect) at all surface locations is indeed the signal seen by GRACE. There may still be some debate whether a station-specific local effect dominates, and how much signal must be removed before doing ground-surface comparisons using a station array. Nevertheless, the local effects are in reality shared among neighbouring stations (but not necessarily in a uniform way as the station spacing increases). Corrections for local hydrology are required only if the

site hydrology or other factors (e.g. soil, topography) would not be representative of other surface locations surrounding an SG.

Following this logic, we find a problem in the model presented by [MVC] in the Appendix. Their eqs (A3) and (A4) may be written as shown, but eqs (A5) and (A6) need changing. Even if the topography is not flat, we still have  $g_L(S) \approx g_{L\text{Smooth}}(S)$  because surface stations on an uneven terrain approximate  $g_{L\text{Smooth}}$ , especially when combined together over a region  $L$ . Fundamental potential theory says that if stations all lie on a topographic surface the gravity potential can be continued smoothly up to satellite altitudes (apart from atmospheric corrections).

Suppose we define  $g_{\text{SGL}}(U)$  as the local effect of mass above an underground gravimeter (between the SG and the topographic surface), then  $g_L(U) = g_{L\text{Smooth}}(S) + g_{\text{SGL}}(U)$ , so to compare with GRACE, we would have, replacing their (A5) and (A6),

$g_{\text{SG}}(S) - g_{\text{GRACE}}(S) = g_L(S) - g_{L\text{Smooth}}(S) \approx 0$ , for many stations, and

$g_{\text{SG}}(U) - g_{\text{GRACE}}(S) = g_{\text{SGL}}(U)$ , for underground stations,

where the last expression also incorporates the approximation  $g_R(U) \approx g_R(S)$ . Then, the only correction we need to account for is the local mass effect above a subsurface gravimeter. Hence, when [MVC] conclude their paper with the statement ‘... the feasibility of joined studies is unclear. ... because it is impossible to correct SG data for local phenomena to make them comparable with GRACE observations’ we have to emphatically disagree. We do not need the hydrology everywhere to make the correction, and it is not a futile circular problem.

## 6 DISCUSSION AND CONCLUSIONS

We conclude that the treatment of the SG data in [MVC] has led to erroneous conclusions regarding the utility of combining surface gravity measurements with satellite data. There are issues in data treatment that include the unnecessary removal of a second-degree polynomial from the data, and especially an unjustified interpolation of 1-month GRACE data to a common 5-d sampling used by [MVC].

We have also undertaken specific demonstrations to show that their hydrological loading has been done incorrectly. The first explains a factor 2 discrepancy in the global hydrology loading for the SG stations, and the second shows the error of reversing the whole sign of the loading for stations below ground. We conclude that their series are not as carefully processed as others in this field of study.

Most curiously in [MVC] the authors try, without much justification in our opinion, to disqualify the EOF technique from treating data series with periodic components. This makes little sense when measured against the hundreds of studies using eigenmodes (EOF, MSSA) for all kinds of seasonal data sets such as GRACE, GPS, atmosphere and hydrology, including papers reported by some of the authors themselves.

These issues are embedded in a paper that switches from correlation analysis between pairs of stations to the use of EOF techniques. Neither method by itself is wrong if applied correctly, but the mixture of the two is done by playing with common signals that are subtracted and added irregularly in their discussion. It is difficult to avoid the impression that the goal of [MVC] was to undermine the use of SG data in previous studies, as they conclude that it is not possible to correct SG data to make them comparable with

GRACE data. As should be clear in this comment, we could not agree less.

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