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Interactive comment on "EUROSPEC: at the interface between remote sensing and ecosystem CO_2 flux measurements in Europe" *by* A. Porcar-Castell et al.

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We really appreciate the detailed and constructive comments by the Editor and the two Reviewers. We respond below to each of the comments, indicating how they have been integrated into the revised version of the manuscript. Main comments are numbered (1), (2), (3),. . . and our response is indicated by the sign (->).

**Comments by the Editor (G. Wohlfahrt) and Response by the Authors

(1) The authors nicely work out the crucial role that in situ measurements play in up-scaling by linking between eddy covariance flux tower footprints and remote sensing. However, how to achieve this link receives much less attention in the paper (altogether

around 20 lines on p. 13093-13094), compared to technical, technological and deployment issues of in situ measurements. The major conclusion I draw from this section is that the present approach of in situ flux tower sampling actually is unable to provide this link due to the mismatch in footprints and that new tools are required to establish this link. Does that mean that all previous and ongoing measurements on flux towers where in situ measurements were made on just a small fraction of the flux tower footprint are useless in providing this link except for the most homogeneous sites? Is there a way/are there examples of quantifying the associated uncertainty? The authors push UAVs as the 'silver bullet' to resolve the footprint mismatch dilemma. However, UAVs share some of the problems of satellite remote sensing, that is typically measurements from UAVs will provide data with poor temporal resolution compared to in situ continuous measurements. How to link between periodic UAV and continuous in situ measurements and further to the eddy covariance flux footprint?

In summary, I think that the issue of linking between in situ spectral and eddy covariance flux measurements, which is one of the central questions of EUROSPEC, provides much more which could and should be discussed and encourage the authors to think about how to expand the corresponding section in the paper. Ideally, this would be shown on the basis of some case study, but a conceptual treament of the necessary steps would also be useful.

-> We thank the Editor for this critical and justified comment. We agree that the issue of upscaling deserved more explanation. Also, the drawbacks posed by the mismatch between footprint and optical data were unjustly presented in a too negative tone in the previous version. The mismatch between footprint and optical data may add noise or bias to the seasonal relationship between flux and optical data but the relationship will still remain a powerful tool for upscaling activities. We have completely rewritten this section and clarified these points.

In turn, UAVs represent a tool that can be used to characterize the footprint/pixel variability, the BRDF of the target surface, and to conduct robust statistical sampling.

These are issues that do not require high temporal resolution but rather, spatial versatility. Linking flux data with satellite data is complicated both by the temporal and spatial mismatch between datasets. Subsequently, while in situ spectral measurements help us elucidate the link between optical and flux data across time, punctual UAV measurements will help us study the impact of scale, complementing in situ spectral data. We present these points in the edited Section 3.4 and included also a new figure that we hope will help clarify the role of in situ spectral measurements and UAVs in upscaling activities.

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**Comments by Referee 1 (J. Moreno) and Response by the Authors

The paper represents a review of achievements and conclusions derived from the EUROSPEC COST activity, and contains relevant recommendations for implementation of networks of proximal optical measurements in flux towers to complement/validate satellite data in the context of global carbon cycle research.

The content of the paper can probably be taken as a reference in the field, particularly due to the useful recommendations provided towards the implementation of a network of optical spectral measurements, operated in continuous mode in association with flux tower sites and other ecological monitoring sites, serving then as a tool for the validation of modelling approaches and other type of observations, particularly satellite data. The review character of this contribution makes this paper quite relevant and of general interest.

(2) The structure of the paper is adequate and in general it is quite well written. The main aspects in the paper are the identification and description of the sources of variability in the measurements and the derived recommendations. Three sources of variability are described, but one source of variability which is only indirectly addressed, and which is critical for this type of measurements, is the temporal stability in the measurements and temporal stability in calibration. Issues such as the heating of

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instruments along a diurnal cycle can bias the measurements, as well as long-term drifts in instrument behaviour of instrument calibration. Given the experience of the authors during EUROSPEC and related studies, it would be adequate to show more explicitly (maybe through a new dedicated Section 3.2.4) how relevant are such stability effects, quantifying the changes experienced.Temperature-controlled equipment is complex and expensive, and guidelines can be provided on when such temperature stability becomes more critical and when other simple methods can be used, giving guidelines about the amount of error that would be expected. At the same time, it would be adequate to indicate the different stability effects on absolute terms and on relative inter-band calibration (i.e., for multispectral sensors, indicate which bands or spectral ranges are more sensible to instabilities). Some indication about the frequency needed for crosscalibration of instruments, typical order of magnitude of the accuracy in radiometric calibration achieved for these types of instruments, typical temporal drift of calibration, etc. can be details than can provide more quantitative information useful for the readers.

-> We thank the reviewer for raising this very relevant point. We agree on the importance of the sensor stability and in that we had not dealt with it with sufficient detail. Unfortunately, we were not able during EUROSPEC COST Action to conduct a systematic and quantitative analysis of the multiple stability effects and accordingly we cannot go into the subject with the level of detail that the topic deserves. This is probably a subject for a Review paper by itself which hopefully comes out sometime in the future. We have however followed the advice of the Reviewer and written a whole new section (3.2.4) introducing the subject. Although the section does not present a detailed analysis with a checklist and guidelines, due to the reasons mentioned earlier, we try to introduce the main tradeoffs and list a number of questions that the users should keep in mind when planning and designing long-term in situ spectral measurements, as well as calibration and subsequent data processing protocols.

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(3) Section 3.2.3 is named “Field-of-view heterogeneity”, which seems to be related to the heterogeneity of observations inside the field of view of the instrument. In fact, the description is not about surface heterogeneity within the field-of-view, which is unavoidable at all observation scales and depends very much on the objective of the measurements. What is addressed in Section 3.2.3 corresponds in fact to two different issues: (a) the knowledge of the actual instantaneous field-of-view (optical) of the instrument, related to the PSF/MTF and related optical characterization, basically given by a 2D response function over the observation area; and (b) the case of sensors having focal-plane separation of spectral bands (either by the spatial disposition of detector in the focal plane, or by aberrations in the optical acquisition of the signal), causing that the instantaneous field-of-view is different for each spectral band. The first aspect (a) can be compensated by a dedicated laboratory characterization of the instrument for any kind of design, while the second aspect (b) requires a proper design of the instrument. Implications are different and have different impact on the data processing and interpretation.

->The reviewer is right in that the title was not consistent with the contents of this section. The title is now changed to “The fields-of-view of field spectrometers and multispectral sensors”.

The purpose of this section was to highlight that the FOV of spectrometers needs more than cursory consideration when instruments and fore optic combinations are being selected for field use and to direct readers’ attention to Mac Arthur et al 2012 and Eklundh et al 2011 for a full discussion of the subject.

Field spectrometer manufacturers normally specify an included solid angle for an instrument/fore optic combination FOV inferring that it is conical and (from the references provided in the text) it is usually assumed that all points from within the surface area delimited by the FOV are weighted equally in the integrated measurement recorded. As field spectrometers are not imaging instrument PSF or MTF are not normally discussed or used in spectrometer specifications. A more appropriate term is the Directional Response Function (as defined in “Methods of characterizing illuminance meters and luminance meters,” Comm. Int. L’Eclairage, Vienna, Austria, Tech. Rep. 69, 1987). The use of this term recognizes that points within the FOV (as the reflectance form each point within the FOV will be received by the instrument from different angles) will have different weightings in the integrated measurement recorded. This weighting is a function of the fore optic and the optical design of the spectrometer as explained in detail in the (Mac Arthur et al 2012). The revised version has been modified accordingly to clarify that we are dealing with non-imaging spectrometers and give more details on the DRF. This was clearly confusing in the previous version as the Reviewer was referring to terminology usually applied to in imaging systems.

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(4) An aspect which is not discussed in the paper, and that becomes essential to analyse scaling issues from single flux tower measurements to spatially continuous remote sensing data, is the role of spatial statistics and statistical sampling issues. Ground sampling following statistical basis is essential to properly formalize a mean value for the averaged measurement and the associated statistical error, which depends on the sampling strategy followed. The opportunity to use UAVs in such ground sampling provides a method to follow different possible statistical sampling procedures of the ground area (tower footprint) or the remote sensing pixel. Some comment about statistical background should be included when analysing upscaling aspects in Section 3.4.

-> We treat the statistical sampling issues and the role of UAVs in connection with comment (1) above.

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(5) Box 1 is particularly relevant, but some potential changes can be implemented: (a) Change “high signal to noise” by “High signal-to-noise ratio” (b) Change “Ideal cosine directional response function” by “Optimal cosine directional response function” (the

ideal one is probably not achievable) (c) The “Low temperature-sensitivity” condition probably means thermal stability in the measurements (for a temperature sensor one would like to have high temperature sensitivity!) (d) “Operating temperature range matching the wide thermal distribution of terrestrial plant species” means in fact the range of environmental conditions, but probably for a given site the overall range is more limited than the overall range when all potential sites are considered together. No necessarily every single instrument will operate in the full range of conditions. This can be clarified.

-> Changes (a), (b) and (c) have been made to Box 1 as suggested by the Reviewer. For point (d) we have edited the text as follows: For global networks: Operating temperature range matching the wide thermal distribution of terrestrial plant species, from -50°C to 50°C. For local measurements: temperature range matching local variation.

The purpose of considering a wide temperature range during sensor design is to cover the demands of standardized measurements across global networks. For such applications, the same sensor design/type would need to be deployed across sites, which may include arctic and tropical sites.

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(6) In Table 1, for SFOV it is stated “Need for temperature control to derive radiometric quantities in absolute units”, but in fact the temperature control does not guarantee calibration in absolute units. Here reference should be to consistent time series of measurements, but not necessarily absolute units, which imply calibration to a reference. The same is true for DFOV given as “Need to temperature control to derive both radiometric quantities in absolute units and reflectance ratios”. Difference between absolute units (calibration to a reference) and temperature control for stability (consistency in calibration) should be clarified.

-> We thank the reviewer for picking up this inconsistency. We have rephrased these points as:

SFOV (disadvantages): “Either temperature control, or characterization of the temperature sensitivity of the instrument and post-processing, are needed to acquire consistent time series of radiometric measurements”.

DFOV (disadvantages): “Either temperature control, or characterization of the temperature sensitivity of each of the instruments and post-processing, are needed both for derivation of reflectance factors and to acquire consistent time series of radiometric measurements”.

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(7) Also in Table 1, it is stated that hyperspectral systems have “Option to process the data with radiative transfer models”, but this is true for all data type, multispectral or hyperspectral, and not exclusive of hyperspectral.

-> The reviewer is right. Our point here was to indicate that hyperspectral data provides more information, compared to multispectral data, which can be used to better constrain the inversion of radiative transfer models. We have reformulated this sentence as: “Increased range of possibilities for inversion of radiative transfer models”

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(8) In page 13078, lines 22-23 it is stated that “Spectral measurements can be classified into multispectral or hyperspectral depending on the number of bands.” In fact, the number of bands is a bad criteria to classify sensors, and has been misleading sometimes. Better refer to spectral resolution versus spectral range covered, contiguous spectral coverage versus discrete coverage, etc.

and

(9) Figure 2 is not particularly appropriate. As it is, it looks like multispectral data (discrete data) is a subset of the continuous sampling (same Gaussian response, but different number of Gaussians). In fact, discrete cases tend to be wide filters that show a response far from a Gaussian. The continuous case shown corresponds typically

to a spectrometer, but the discrete case looks like the case where some bands of the spectrometers are used (i-e, case of MERIS or OLCI). The general case, however, is not the one illustrated in the figure. -> The terminology we used is consistent with previous work cited in that section, in particular that of Balzarolo et al. (2011). However, we agree that the differences between multispectral and hyperspectral measurements can be sometimes misleading as they may overlap. We have clarified these aspects in the revised version and formatted also Figure 2 following the advice from the reviewer, which we hope will help to clarify this point.

"Accordingly, spectral measurements can be classified into multispectral or hyperspectral depending on the number of bands."

The bandwidth of these sensors (in terms of full width at half the maximum response, FWHM) is in the order of 10 nm or greater and the sampling across a specific spectral range is typically discrete (Fig. 2)

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Minor Comments by Reviewer 1:

Page 13071, line 21: change "carbon cycle" by "terrestrial components of the carbon cycle", or "terrestrial carbon balance" or more directly "GPP". The problem comes from the fact that carbon cycle includes other temporal and spatial scales and contains also carbon exchanges in oceans, etc., just to be more precise.

->Rephrased: ". . .estimates of GPP over terrestrial ecosystems."

Page 13074, line 20: change "recent technological and technical advances" by "recent technical advances"

->Done

Page 13078, line 12: change "down-welling and up-welling reflectance" by "down-welling and up-welling radiances"

->Done

Page 13080, line 22: change “in situ field measurements to measure: : :” to avoid word repetition.

->“measure” replaced by “quantify”

Page 13081, line 1: change “at or off-nadir” (not clear the meaning, should be “at nadir or off-nadir” ?)

->Changed by “at nadir or off-nadir”

Page 13083, line 21: change “Because the situation where the same sensor is used in all sites is not: : :” by “Because the use of the same type of sensor in all sites is not: : :”

->Done

Page 13084, line 25: change “instrument readout” by “instrument readout time”

->Done

Page 13085, line 14: change “angular-dependent time degradation” by “angulardependent time degradation at such wavelengths”. ->Done Page 13086, line 7: change “the response across that area is the same at all points” by “the response across that area is the same for all points inside the given FOV”.

->Done

Page 13086, lines 14-15: change “Even when less optically complex spectrometers, measuring only across the VNIR region are considered, the Earth Surface: : :” by “Even when less optically complex spectrometers are considered, for instance measuring only across the VNIR region, the Earth Surface: : :”

->Done

Page 13087, line 13: change “science and industry was suggested” by “science and industry was suggested, particularly to produce prototypes for new instruments"

->Done

Page 13094, line 22: change "resolution" by "resolutions" (plural)

->Done

Page 13095, line 13: change "Network" by "network" (no need for capital)

->Done

Page 13095, line 25: change "Tools" by "tool" (no need for capital)

->Done

Page 13096, line 3: change "Networking" by "networking" (no need for capital)

->Done

Page 13097, line 1: change "has" by "have"

->Done

Page 13098, line 24: here the word "drones" is used, when previously they were referred as UAVs or RPAs. Better use a consistent wording or clarify differences, if any, between drones and UAVs.

->Replaced it by UAVs for consistency

Page 13099, line 5: change "community" by "communities"

->Done

Figure caption of figure 2: change "spectra" by spectral".

->Done

In Table 1, Configuration hemispherical-conical: change "Small Sampling area" by

“Small sampling area”.

->Done

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**Comments by Referee 2 (J. Gamon) and Response by the Authors

This review of recent EuroSpec advances represents a valuable contribution to the emerging field of integrated optical - flux sampling and to the larger topic of ecosystem monitoring within the context of the global carbon cycle. The paper provides a broad context, nicely summarizes the recent history of EuroSpec and similar efforts elsewhere, and describes the recent formation of the Optimise program to carry the work forward. The manuscript includes a useful and insightful discussion of recent technical advances, helpful critiques of current limitations (e.g. gaps in current international efforts as well as technical challenges), and suggestions for future progress in the field of proximal remote sensing. It also provides a solid argument for developing a concerted sampling and data approach to better link the flux tower network to remote sensing.

Overall, this is a well-written and comprehensive review. A few points to consider for possible addition/clarification:

(10) Page 13073, line 6 - The statement that optical estimates of fAPAR are affected by canopy structure is not uniformly true for all optical methods. Broadband measurements clearly have trouble, but spectral methods can distinguish green from non-green, using transmitted or reflected light, and this is largely the basis for using vegetation indices like NDVI. However, it is true that ground validation methods of fAPAR (e.g. using light bars based on PAR that do not distinguish color) are often strongly confounded by non-green canopy materials, and the literature has often been vague on this point. So this is probably more of a problem for our ground validation than for our satellite indices.

-> We apologize for the lack of clarity in our previous version. What we had in mind

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in this sentence was indices such as the NDVI obtained with multiband sensors rather than fAPAR estimates using PAR sensors. We agree with the reviewer that hyperspectral methods will be less affected by canopy structure compared to broadband measures using PAR sensors or other broadband sensors. Yet, NDVI estimates are still affected by the reflectance properties of non-green elements in the canopy, like bark or senescing leaves, (e.g. Campbell & Borden, 2005, Can. Entomol. 137:719-722; di Bella et al. 2004, Int J Rem Sens, 25:5415-5427) or wavelenght-dependent scattering effects within the canopy (e.g. Knyazykhin et al. 2012). The effect of structure on VI-based estimates of fAPAR is also mentioned in Gamon 2015. We have reformulated the paragraph as:

“It is important to note that fAPAR in Eqn 1 corresponds to green fAPAR, in contrast to total canopy fAPAR where both photosynthetic and non-photosynthetic elements such as wood contribute to PAR absorption. Green fAPAR has been widely estimated using reflectance-based vegetation indices as proxy, notably the Normalized Difference Vegetation Index (NDVI) derived from red and near-infrared (NIR) reflectance (Rouse et al., 1973; Tucker, 1979). These vegetation indices correlate better with green fAPAR than with total fAPAR because their spectral formulation can significantly discriminate green from non-green elements (Gamon et al. 1995). However, canopy structural factors, background properties, or sun-target-sensor geometry can complicate the estimation of green fAPAR with vegetation indices (Di Bella et al. 2004; Gamon 2015, Knyazikhin et al.2012)”

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(11) Page 13073, line 9 – vegetation indices (e.g. NDVI) are more closely related to green fAPAR (i.e. the fraction of PAR absorbed by green canopy material) than total fAPAR, and this should probably be clarified.

-> See Response to comment (10)

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(12) Page 13073, lines 22-23 – It may be true that other indices besides NDVI are better related to LAI, but are these any better related to green fAPAR? Green fAPAR, not LAI, is the real concern in the LUE model, so these discussions of saturation and LAI may not so terribly important from this perspective. Since LAI is non-linearly related to fAPAR, LAI may not be a meaningful metric for the LUE model. Given that LAI is ill-defined for much of the world's vegetation (think bryophytes, evergreen conifers, or most desert plants), it may be best to avoid a reliance on LAI-based approaches, at least when determining canopy light absorption. Optical approaches like fAPAR (or NDVI) that can provide a more direct measurement of light absorption by green tissues may actually be more relevant and useful than this discussion of LAI non-linearity suggests.

-> It is true that if we stick to the formal LUE paradigm green fAPAR is the main concern and we should not talk about LAI. However, we cannot rule out the fact that LAI and spectral indices related to LAI may be more efficient for estimating GPP particularly in dense canopies. This illustrates the need to keep the way open for alternative interpretations, models and hypothesis to relate optical data to flux data in addition to the LUE model. To avoid confusion, we rewrote and moved the following paragraph so that it is separated from the description of the LUE model:

"Noting that the relationship between NDVI and fAPAR tends to saturate at high canopy densities (Myneni and Williams, 1994; Olofsson and Eklundh, 2007) also other approaches have been used to estimate vegetation carbon uptake. For example, the Enhanced Vegetation Index (EVI) (Huete et al., 2002) efficiently describes the seasonal variability in GPP across both dense and sparse vegetation canopies (Schubert et al., 2010, 2012; Sims et al., 2006; Sjöström et al., 2011; Xiao et al., 2004a, 2004b; Xiao et al., 2010). More recently, the plant phenology index (PPI) (Jin and Eklundh, 2014) has been shown to be linearly related to green leaf area index (LAI), and better related to seasonal GPP variations than NDVI and EVI of coarse-resolution MODIS data at northern latitudes. This illustrates the value of investigating the relationship between

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carbon uptake and spectral information in flux footprint areas beyond the LUE model depicted in Eqn 1."

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(13) Page 13073, lines 5-6 – Efficiency variation is considered indirectly through meteorologically-based variables (which may not be available for a given site).

-> We agree that a significant part of the inter-seasonal variation in LUE can be reproduced via the instantaneous effect of an environmental scalar. What we wanted to emphasize is that the slow dynamics in physiology and phenology are not considered. We have rephrased accordingly:

"while inter-seasonal variability due to plant phenology and photosynthetic dynamics (Lagergren et al., 2005) is only considered via the instantaneous effect of the environmental scalar, which cannot reproduce the slow response dynamics of vegetation."

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(14) Page 13081, lines 19-21 - By "averaging out" these effects, aren't bi-hemispherical measurements LESS sensitive to BRDF effects than hemispherical-conical by? Can you provide a citation to support this point?

-> We are aware of a few studies providing a comparison of the sensitivity to BRDF effects of bi-hemispherical and hemispherical-conical measurements. In Meroni et al. (2011) the bi-hemispherical reflectance collected with the HSI was compared with the hemispherical-conical reflectance collected with a traditional field spectroscopy set-up with nadir viewing geometry (FOV f 25° and white reference panel to estimate incident irradiance). The bi-hemispherical and hemispherical-conical reflectance differed both in intensity and shape of the diurnal cycle. In particular the bi-hemispherical reflectance was less affected by BRDF effects for measurements collected around solar noon but reflectance markedly increases at large illumination zenithal angles. The effect of large illumination angles on the bi-hemispherical reflectance is also reported in Strub et al. (2003, Geoscience and Remote Sensing, IEEE 41:1034-1042, doi: 10.1109/TGRS.2003.811555).

We thank the reviewer for this justified comment. The text in the previous version of the manuscript was confusing because the comparison in Meroni et al. (2011) refers only to hemispherical-conical measurements collected with a nadir view. We have modified the text as:

“bi-hemispherical measurements tend to be more sensitive to variations in illumination geometry compared to hemispherical-conical measurements collected with a nadir view particularly for large illumination zenith angle , as affected by the bi-directional reflectance function (BRDF) of the surface.”

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(15) Page 13085, lines 12-13 - Some brief mention of the panel options here (and their pros/cons) would be helpful. For example, most are made of teflon (PTFE) and users can purchase Spectralon (for a high price) or can make panels from virgin white teflon (at considerable cost savings but slightly reduced performance).

-> Thanks for pointing out to this improvement. We have added the following paragraph:

“Selection of reference panel material is also very important. Manufacturers of reference panels for spectroscopy such as LabSphere (Spectralon ®) or SphereOptics (Zenith polymer ®) use a sintered fluoropolymer manufactured to have a very high reflectance, possibly in excess of 96% and approximate a Lambertian reflectance across the 400 nm to 2,500 nm spectral. Alternatively, low cost PTFE sheets (i.e. Teflon) can be purchased at lower cost. However, PTFE sheets have lower reflectance, approximately 80%, and have higher specular reflection. Also, because PTFE sheets are not manufactured to be used as ‘references’ there may be variability between individual sheets and wavelength dependent reflectances may be unknown. Overall, PTFE sheets are not recommended as field spectroscopy reference standards.”

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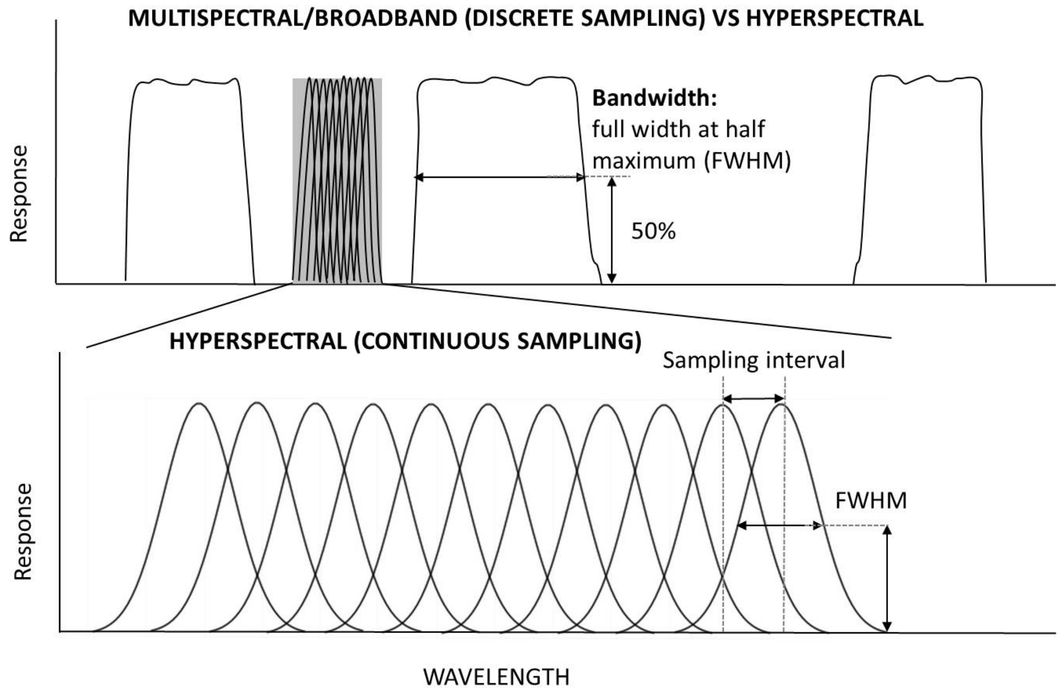

Fig. 1. Modified Figure 2

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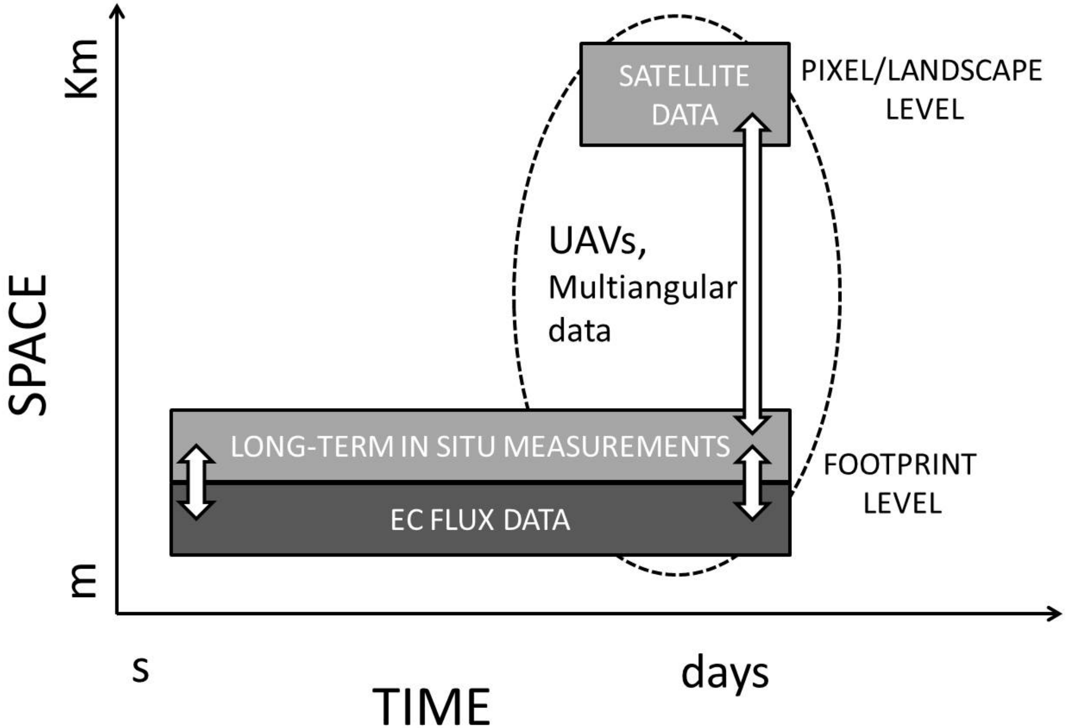

Fig. 2. New Figure 6

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