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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Estimation of vegetation cover resilience from satellite time series" *by* T. Simoniello et al.

T. Simoniello et al.

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Replay to Referee #2

We are grateful to the referee for the highlighted points that allowed us to better explain non clear passages. The text has been modified by carefully addressing the referee's comments.

General comments

Reviewer Comment (RC): There are a few statements which can not be easily understood without knowing the paper of Lanfredi et al. of 2004. In order to make the paper self-explanatory it is suggested to add a number of explanatory remarks rather than refer to Lanfredi et al.

The original sign-time distribution methodology focuses on the probability that a scalar

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field at a given site has never changed sign. The functional form of this probability is directly related to the dynamics ruling the fluctuating field. In particular, exponentially decaying persistence is associated to short range correlated fluctuations whose mean life time can be estimated from the distribution of the surviving times at the different sites. Thus, in its standard form, the technique concerns the evaluation of the first passage times across a reference level, usually the mean value of the field, and assumes that data are not noised. NDVI interannual data are noised instead and their mean values are determined by the land cover type. The application of the standard technique to such data automatically would evidence the persistence of soil use (e.g., forests continue to be forests) and noise. Neither the time series can be time averaged to eliminate erraticity because of their limited length. In order to look at long term persistence and, contemporaneously, to eliminate the mean land cover contribution, we focused on positive and negative "trends" rather than on positive and negative "deviations". We revised the paper according to the referee's remarks in order to better clarify rational and technical points of our procedure.

Detailed issues

RC: The principal question is what one has to understand under "resilience of vegetation cover" in view of the result of other investigations that the recovery time of the NDVI after a drought or fire is about three years (which does not imply that the same vegetation is grown during this time!).

First of all it is important to underline that external disturbances, such as fires, affect the vegetation cover locally. The resilience properties we estimate do not refer to single pixels, which could have experienced shocks of different origin and sign, but are obtained from their collective statistics (per land cover classes, altitude ranges or climatic regions). They have a regionalized character and therefore are suited for enhancing average recovery ability features of vegetation cover that could be conditioned by regionalized factors, such as climate.

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Perhaps the reviewer refers to a comment on the reliability of recovery times reported in Lanfredi et al. of 2004, where we said that some estimated mean life times were compatible with the recovery of natural vegetation, e.g. maquis, from fire. Such a comment only supported the consistency of the estimated values from an ecological point of view.

The discussion on drought is a little bit different since it generally affects quite large areas, even entire countries or basins. Therefore, it induces a collective vegetation dynamics that is what we searched for and what is relevant for climate studies.

RC: In fact, what is determined is the duration of the deviation of the greenness of the earth surface from a reference data set or to be more precise the deviation from a trend prescribed by a reference data set of ten years. This can have different reasons: It could be that the vegetation recovers from a shock, maybe the reference years were favourable for the vegetation, maybe what we see is the change of precipitation or a change of land-use at the end of the reference period.

As already specified, our procedure does not look at single pixels neither at effects of specific shocks or environmental conditions but at the global statistics of the induced NDVI fluctuations, whatever their origin may be. We selected a long reference period (see the reply to referee # 1) just for minimizing local (in time) effects of favourable or unfavourable initial conditions. If we start from an initial configuration of trends at chance, we can count for how many years these tendencies confirm their direction. Then, we can study the distribution of the counts so estimating the mean timelifes of the trends over selected regions. Such an analysis should average out local details and the right interpretation of these times should provide right information on the general conditions of land cover. Figure 1 reported as "additional materials" could help to better understand how the method works. Figure 1a) presents the initial trend, which is slightly negative. The successive ones show how the trend varies when successive fluctuations are progressively added. Interannual variations modify the slope that continues to be negative even when NDVI starts to systematically increase (from 1995 to

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1997) since these fluctuations (which could be a noise effect) are not sufficiently strong for clearing the trend. Only in 1999, when the accumulation of these fluctuations is able to balance the previous ones (destructive interference), the global tendency of NDVI becomes positive. At this time, NDVI recovers from the negative initial trend and the first passage is observed.

RC: If we look at the final product, fig. 5, we see that the Basilicata remains negative for twelve years. But how much negative is not indicated neither is clear when precisely the deviation started within the ten years reference set. It could even been that within these twelve years there is an upward trend though the NDVI never reached the level of the reference years again. There certainly is no continuous downward trend of vegetation cover. If this would be so, then after twelve year there would be a desert.

There is no doubt about the general importance of trend strength. However, this variable is mainly useful within change detection studies; on the contrary, our analysis concerns resilience and therefore recovery. Trends can keep the negative sign for all the observational period, as occurs in Basilicata, without implying a systematic decrease of photosynthetic activity. The slope of the total trend can have magnitude lower than the initial one (see Fig. 2 in additional materials) but we cannot decide if a recovery process is really started or instead vegetation has experienced temporary effects of random interannual variability (e.g., rainfalls), just as the referee noted. There is continuity in the negative sign of the trend from the initial time, which is always directed downward but there is not the significant accumulation of negative fluctuations that is implied by desertification. Therefore we keep track that recovery does not occur within the elapsed time and this information is included into the statistics: pixels showing a behaviour similar to that illustrated in Fig. 2 (in additional materials) contribute by increasing the probability of very long recovery times.

RC: The EUROKADS (1km resolution data set) is not fully evaluated with respect to the Basilicata area and as long as we do not exactly know, how the GIMMS is used to compile the annual data values it is also not legitimate to draw any conclusions or

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to make any comparisons, but sporadic results obtained for a part of the Basilicata shown in the following table do so far not underscore that there is a tendency towards desertification or instability though the peak values seem to shift.

We do not know the EUROKADS time series, and we found no information somewhere in the web. Anyway it is no matter of fact, the important issue is that the data of few years (three) reported in the reviewer's table are not sufficient for determining the long term tendency. The reported values are in agreement with our data: in the areas with negative trends we can select some years with similar NDVI values, but having a general tendency that is negative. Moreover, in some part of Basilicata there are areas with persistent positive trend over 12 years. By considering the rationale of our method, it is easy to understand that, the presence of long surviving negative trends is not indicative of desert but suggests vulnerability to desertification, in agreement with many independent studies and with the Italian National Environmental Agency (see references reported in the paper).

RC: In section 3.1 it is said that the data are reprojected. Here the question arises how representative the new pixels are for the area and how accurate they fit the CORINE data set. Already by averaging (or selecting one out of ?) the 64 original 1 km data it is questionable how accurate the new pixels are registered. A comparison of the data set with the 1 km data set used by Lanfredi would be interesting.

We tested different rescaling methods for CORINE data and we found that the accurate one accounts for a first reclassification in few land cover classes (from the original 44 to 8, as already indicated in the text) and, then, for a two-stage rescaling based on a median filter in order to control the aggregation process of pixels, i.e. to verify the correspondence between the original and rescaled data. For the registration of the GIMMS data set, after the reprojection we manually checked the accuracy on the border line with a vector of the coast and we performed some adjustments with GCPs until the coastline overlapped the first half of border pixels. In addition, in order to guarantee the correct overlap between GIMMS data and CORINE, at the end we evaluated the

NDVI of contiguous pixels at the passage of different covers. For the comparison with 1km data see the replay to referee #1.

RC: Then the data are composed into annual maps. What does MVC stand for? The lines 16-19 are not clear with respect to data set which is constructed: Is this a sequence of annual sums of the NDVI, of annual averages of the NDVI, or a sequence of the annual maxima of the NDVI (as suggested by the line 19)?

MVC stands for Maximum Value Composite. The dataset is the sequence of the annual maxima, as suggested by the line 19. We clarified in the text that trends, and then persistence, were estimated from annual maxima.

RC: Obviously at line 7, page 519, by NDVI(x,y,t) [would it not be better to write NDVI(x,y;t)?] it is meant by the phrase "time t" the year t (t = 1, 2, 3, ... 24, respectively 1982 ... 2003).

Yes, as far as the specific application discussed in the paper, the sampling time t indicates the year (t = 1, 2, 3, ... 22, indicates 1982, 1983 ... 2003 respectively). In the new version we have followed the suggestion of the referee: NDVI(x,y;t) instead of NDVI(x,y,t).

RC: What follows in lines 9-14, page 519, needs a little bit of sharpening. If it is understood correctly, first a reference data set is established using the trend of the years from 1982 to 1991 (compare also page 521, line 15). Then: "The sign .... lasts until interannual variations do not integrate destructively". Please explain the phrase "destructive integration".

The variable we consider is the sign of the trend slope. At any step of our procedure, we add new fluctuations. If they are positively correlated with the previous trend, then their integration contributes to reinforce it (constructive integration) otherwise they tend to destroy it (destructive integration).

RC: Referring to line 1 at this page (519) "the signs lasts as long as the starting value

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(within the reference period?) is not reached again. In some lines it is suggested that the reference period provides a "level" or even a "value", in others a "trend". The question which follows is, whether from one year to the other the trend is "broken" or whether the "level" of the reference period is reached (both changes may occur due to a fluctuation, line 13).

The referee is right. Presumably, the confusion about the use of the words "level", "trend", or "deviation" arises from the unclear separation in the text between the discussion of the classical sign-time distribution procedure and our adapted algorithm. In the first case (line1, page 519), persistence concerns deviations from reference levels that stand until the reference value is restored. In the second one, which refers to our method, persistence concerns trends that tend to move vegetation far from the initial state. The return time is the time we have to wait for the series appears to be stationary and we can assume that the current status is "statistically" equivalent to the initial one (recovery).

RC: Line 12-14 would, maybe, read better: "... the trends of those sites are classified as persistent, for which the current inter-annual change does not break the previously estimated trend (which means: reach the level or the trend of the reference period again?) or change the sign of the deviation from the reference value". If it is written " ... change its sign" (line 13) then this would occur in the year of maximum deviation from the reference value not at the return to the reference value. Please explain what is precisely meant here.

Since time series are discrete and noised, we generally do not detect the return (slope=0) but rather the passage. This occurs mainly when the slope of the trend is already near to zero and a small fluctuation is sufficient to clear it and to induce another small trend in the different direction (see the passage from 1998 to 1999 in Figure 1 of additional metrials).

RC: With the "surface s(x,y;t) is it meant the average of the years 1982-1991? Then it

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should be written s(x,y;0,t). So the trend within the years 1982-1991 forms the surface s(x,y;ti) and determines the initial value +1 or -1.

The surface s(x,y;t) is the current (at time t) map of the trend signs. It assumes values +1 and -1 according to the signs of trends estimated in the period [0,t]. In particular, as far as the application reported in this work, the trend in (x,y) over the 1982-1991 period determines the value +1 or -1 of s(x,y;ti).

RC: In line 22 is ti identical with the initial time (line 18), the period 1982-1991, or is it the year prior to the present year t (then it would be clearer to write tj ; same in line 25)?

It is the initial time ti for which we computed the reference trend.

RC: Is line 18-20 to be understood as follows: First a surface s is constructed of the years 1982-1991 and it may be called s(x,y;0,ti) = s(x,y;1982,1991). Then P(x,y;ti) = s(x,y;0,ti). Then a surface s(x,y;0,ti+1) is constructed and compared with P(x,y;ti). If s(x,y;0,ti+1) equals P(x,y;ti) then P(x,y;ti+1) is set to P(x,y;ti). If not, P(x,y;ti+1) = 0 notwithstanding whether the new value is smaller or larger than the old one. So one obtains a series of values starting with [P(x,y;ti)]1992, ...{P remains constant as long as the deviation remains in the same direction} .... [P(x,y;ti)]1992+m, {change of the sign of deviation} 01992+m+1, {zero continues as long as the new trend remains the same because always the previous P was zero}... 01992+m+n {change of trend} {zero continues because the following P is identical with the preceding one} .... 02003. The n(t) sequence would be 1, 2,3, ... m+1, m+1 .... and finally N(2003) = P2003 P = m+1. This result would be obtained for all years after 1992+m. But how are the N(ti) constructed to obtain a decreasing q? Is N(ti) increasing by one each year a new surface s is generated?

For the sake of generality and simplicity, we preferred do not render explicit the years (1982-2003) in the discussion of our methodology. We referred to:

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ti as the initial time interval (1982-1991)

ti+1 as the initial time interval plus one year and so on

T as the whole time series period (1982-2003)

Both the surfaces s(x,y;t) and P(x,y;t) are symbolic maps. They are compared logically. Thus, there are only two possibilities: "equal" or "different" (not "smaller"; or "larger"). Two values are possible for s(x,y;t): -1 (negative trend), +1 (positive trend). Three values are possible for P(x,y;t): -1 (persisting negative trend), +1 (persisting positive trend), and 0 (cleared trend). Any other symbols can be used. Perhaps the discussion of the referee refers to slopes rather than directly on signs, as in our algorithm.

As far as the persistence probability, N(t) (pag 520 line 5) counts the number of pixels that maintain the trend sign at the time t so it "cannot" increase with time. It intrinsically describes a decay. In the new version of the paper, we have better clarified that N(t) is the number of non cleared trends having the same sign in P(x,y;t). In other words, the number of non cleared positive (negative) trends can be obtained by counting how many +1 (-1) are present in this map. Being q(t) =N(t)/N(ti), by definition q(ti)=1 and q(t) decreases as N(t) decreases for $t > ti$.

RC: Some typing errors must be corrected. The capitalization of words is not strictly done. In line 3 and 4 of page 518 some words are written with a capital first letter, some with a lower case letter (see also Climate on page 514, line 4, page 523, line 19 and 27 "temperate").

We adopted the capital letters for the name of the classes, e.g. Temperate sub-oceanic, and lower case letters for common words. We checked for misprints to address the selected convention.

RC: Also page 514, line 10, "potentially"; line 12, "grow"; line 16, "these"; page 524, line 12 "thermo"; page 521, line 18, "indices".

Misprints have been corrected.

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RC: Experience with the EUROKADS has shown that for each decade entering the time series it has to be visually determined that there are no spurious cloud remains in the image. Automatic de-clouding often is not sufficient to get rid of all cloud remains (see above table: decade 13, 1998, the dip is questionable).

The use of maximum annual values (MVC) eliminates the problem of cloud contamination since clouds tend to lower the NDVI value; therefore, by keeping the maximum we selected the NDVI values within the image with clear pixels. The problem could arise if there are many following decades that are cloudy during the period of maximum photosynthetic activity. By considering that for the analyzed ecosystems the maximum is mainly during summer, there is very low probability of so prolonged cloudy period.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 511, 2008.