Is it possible to test the significance of linear trends using a t-test if the considered data are moving averages?

In the article „Does climate change affect period, available field time and required capacities for grain harvesting in Brandenburg, Germany?“ by A. Prochnow et al. (Agricultural and Forest Meteorology 203, 2015, 43–53), the authors calculate (simple) moving averages of the considered quantities („number of hours per year available for harvesting grain with defined moisture content“; coefficient of variation; total sunshine duration; etc.) for time periods of 30 years in steps of one year (i.e., 1961-1990, 1962-1991, …, 1984-2013) first. After that, they derive the trend of these averaged values and use these values to estimate the significance with the help of the „t-test for the regression slope“ (see their section 2.4 and Table 5 and Table 6). This way most of these trends are proofed to be significant with $p<0.01$.

I am convinced that this procedure is wrong. I learned that the values y(i) and y(j) (or the residuals) which are entering the regression procedure (especially if the significance is supposed to be tested with a t-test) must be statistically independent (see, e.g., http://en.wikipedia.org/wiki/Student%27s_t-test#Slope_of_a_regression_line). But the moving averaged values are highly correlated and not at all independent. This violation of the precondition results in a much to small estimate for the standard deviation s of the y(i). The reduction factor is even smaller than 1/sqrt(30)=1/5.5 (because the computation interval is shorter than the correlation length). My own tests (see attached figures) showed that the standard deviation is reduced by a factor of about 1/15. The computation of the significance level with this very small standard deviation gives very high significant results (i.e., very small p-values). But the truth of the matter is that there is no significance at all and all trends could be emerged by pure accident.

Does anybody agree with my belief? (I can not believe that four authors and two or three reviewers/referees did not recognize this pitfall!) I also appreciate hints of the authors in case that I understood anything wrong.

I have written a review for the above mentioned article because there are more shortcomings in this paper (e.g., no consideration of multiplicity at p=0.05 but 12 or 16 test in one table (see their Table 3 and 4) with only a few significant results // very sophisticated regression functions (their Table 1) for estimating the „hours within classes for grain moisture contents“ with r^2 up to 0.99 (given references are not downloadable); I assume that these regressions are derived by stepwise

multiple regression but are overfitted and could not withstand an external validation) // The probabilities of the results of Figure 4 („inclusive all more severe cases“) can be derived by means of the Binomial distribution and are not small enough (all greater than 0.1) to indicate any significance // et cetera).
See: https://www.researchgate.net/publication/270793652_Does_climate_change_affect_period_available_field_time_and_requiredcapacities_for_grain_harvesting_in_Brandenburg_Germany/reviews/72069