

Interactive comment on “Identification of reworking in Eocene to Miocene pollen records from offshore Antarctica: a new approach using red fluorescence” by Stephanie L. Strother et al.

Anonymous Referee #1

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Identification of reworking in Eocene to Miocene pollen records from offshore Antarctica: a new approach using red fluorescence.

Stephanie L. Strother, Ulrich Salzmann, et al.

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This paper approaches a really important problem in post Eocene Antarctic terrestrial palynology, which is the great difficulty in distinguishing contemporaneous pollen grains from those that may have been reworked from sediments deposited during previous epochs. This difficulty results in significant uncertainty in reconstructions of post-Eocene Antarctic vegetation.

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This is not a problem unique to this time or region, but is particularly pronounced in this setting due to compounding factors of 1) likely low contemporary pollen flux following large scale reduction of vegetation cover after ice sheet growth and 2) large scale reworking of sediments (and the pollen therein) due to glacial processes.

The authors use fluorescence of pollen grains as a proxy for burial history, underpinned by the observation that fluorescence intensity decreases irreversibly with a combination of temperature and time. They suggest that measurement of fluorescence parameters will allow in situ assemblages to be distinguished from those assemblages that are dominated by reworked specimens.

A really exciting advance on the use of fluorescence on Antarctic pollen, which they note has been explored before, is their extraction of RGB data and other parameters from digital images – which generates significantly richer and potentially more robust data than visual estimates of fluorescence.

I believe there are two significant problems with this paper as submitted:

1) a lack of consideration of burial history and conceptual models for reworking. If this was included, it could lead to clear and testable hypotheses to demonstrate the presence and extent of reworking (or otherwise) in this setting, and

2) sample sizes.

Burial History and conceptual models

An explicit consideration of source areas and burial history/burial depth of source material the authors have examined is lacking.

Their paper would be significantly improved by at least a conceptual model of the source of the reworked grains, and a clear hypothesis of how their results would look if there was (for example) no reworking, 50% reworking, increased reworking through time. In other words, to make the paper really useful, the reader needs to be guided more clearly on how to determine whether a new sample collected from (for example)

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Site1356 contains an assemblage that is predominately reworked or contemporaneous.

At the moment, there is a significant leap of logic to get from the observation that there is a change in fluorescence parameters with depositional age, to the application of a tool to identify reworking. Demonstrating that mean fluorescence values differ between Eocene and Miocene pollen is fine, but this does not exclude the possibility that much of the Miocene assemblage is reworked.

For example, a most simple burial history possible would look something like the attached jpeg (Review Figure 1).

If Review Fig 1 was the true history, and the contemporaneous pollen flux was unchanged through time, and there was no reworking, expected fluorescence parameters down the core would decrease in a linear way (i.e. red values would decrease), due to increasing burial depth/temperature and duration down the core. The variance of the fluorescence measurements at each time step should be the same. Depending on the accumulation rate/burial depth, the mean red values from each time step could be significantly different.

But, there could be a range of effects of introducing reworked pollen grains at each time step, depending on the source and nature of these reworked grains.

The most basic reworking model is if we assume some constant proportion of grains are introduced from only the previous time step into each new sediment package (e.g. during the Miocene, 60% of the pollen deposition is contemporaneous, thus with a red value of 50, the remaining 40% is reworked from buried-then-exposed Oligocene sediments with a red value of 40). We would still expect a linear change in red values with age in the final core, and also constant variance, but variance would be larger than the first example, and the mean values slightly lower.

A more complicated example would be that a constant proportion of grains were re-

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worked from the entire sediment column into each new sediment package (e.g. during the Miocene, 60% of pollen might be contemporaneous, thus have a red value of 50, 20% is reworked from buried-then-exposed Oligocene sediments with a red value of 40, and the final 20% is from Eocene sediments with a red value of 30). The result of this scenario would still be a slightly non-linear change in mean red value with age (slightly non-linear as some grains might get reworked more than once), and an increase in variance up the core (which incidentally is approximately what is illustrated in ms Figure 3a).

A fourth, (slightly) more realistic example would be for an increasing proportion of reworked grains into each time step, as the amount of contemporaneous input (vegetation) decreased in a step-wise fashion. The result would be (I think) an exponential increase in red values up the core, also with increased variance – which perhaps even better reflects the patterns in Ms Figure 3, particularly when the individual taxa are examined in ms Figure 3b.

These four hypothetical examples suggest that an important parameter for the identification of reworking, and the testing various hypothesised mixing/reworking models, is not identification of a strong correlation between mean fluorescence values and age, but the changes in mean and variance between time steps. It seems to me this would require much larger sample sizes than have so far been generated, but it is also not clear to me how “the presence of in situ pollen and spores” promised in the abstract can be identified without it.

A related question, one that perhaps could be answered by exploring changes in mean values between time steps as opposed to consideration of variance, is whether there is a point up the core at which all of the pollen becomes reworked (i.e. all vegetation expires). In the simple model illustrated above, assuming the removal of vegetation was abrupt and large-scale, one might expect to see a change in the slope of the line that reflects the lack of input of specimens with high red values . . . in that case, one would be looking for a “break point”, rather than a regression of the entire data. As

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an aside, in the analysis of comparison of mean values between time steps of the style the authors have already completed, one would imagine that one characteristic of this point could be that there was no significant difference in the means between adjacent time steps. One stated assumption, that the pollen grains that have been sampled/measured are in situ based on their preservation characteristics under light microscopy, also needs to be reframed if the authors rewrite their hypotheses more clearly.

Sample Size.

This work is under-sampled in two respects. In the first instance, 30 grains per time step is only barely sufficient to observe changes in mean values, and is certainly insufficient to explore the changes in variance from one time-step to the next required to understand the likely mixed pollen populations sampled here (Yes, 30 observations are used as a cut-off for the shift from “small” to “large” sample size in some formal statistical tests, such as the students-t distribution, but so many of the underpinning assumptions about unbiased sampling from a uniformly distributed population are just not met in the present study!).

This is compounded by the demonstration in Figure 3 that there is a clear taxonomic effect on fluorescence values – given this demonstration of such a heterogeneous pollen population (and not even considering other possibly significant causes of heterogeneity, for example facies), the previous statistical treatments in Tables 1a and 2, where all the pollen is considered as a single group, are not useful.

At the very least, the authors should ensure that the different taxa are sampled proportionally/evenly through time, but since these taxonomic effects have been identified, the authors really need to make sufficient additional observations of each taxa (or perhaps one or two target taxa) before they do further analysis.

My feeling is that a more appropriate data set for this sort of analysis would be in the order of ~ 500-1500 grain measurements of a single taxa, i.e. ~200-300 specimens

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per time step (*N. lachlaniae* might be best as it is the most common, and long lived). If this is not possible (and it may well not be – there is likely not much pollen there!), more effort needs to go into describing and acknowledging the effects and implications of small sample size.

Summary

I reiterate that this is really interesting and potentially very valuable work, but it would be significantly improved by a more coherent hypothesis or conceptual model of how to apply these results, and a significantly larger data set.

Line comments

L67 suggest “are subjected to” instead of “confronted”

L70 to follow from the previous point, this needs to be qualified with something along the lines of “if burial histories are the same, fluorescence change could be used as an indicator of age”

L83 “each should come with” = “we hypothesise”?

L105 A summary of what is known of the the burial history would be helpful here – is there any constraint or estimate of the amount eroded at eh disconformities – i.e. is there any possibility the Eocene pollen was buried to a greater depth before Oligocene time etc. . . if these sort of effects relate to only 10s of meters of extra burial, this is useful for the reader to know

L132 modern name of *Nothofagus fusca* type trees has been changed to genus *Fuscospora*

L147 This seems sensible. So why do you then combine them for your statistics?

L157 suggest remove “in situ”. . . All you can infer is they are “not obviously reworked”.. that distinction is critical for this paper!

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L160 I suggest that a clear description of conceptual models of reworking is really important about here – to provide some context and reason for the statistics in the next section. . . the reasons for wanting to know why correlations against age and significant differences between mean values must be laid out.

L171 what do you mean “set” the p-value? Is this a threshold you have adopted to accept or reject a hypothesis? If so, at least this should be acknowledged/highlighted in Table 1 – perhaps bold the results with acceptable p-values?

L175 the meaning of U-values this test generates should be explained. . . . If this is a threshold score, describe what is is, where it is from and what it means, and make this clear in your Table 2 – including same comments on p-values as above

L175 Once you get into multiple sequential significance tests of this sort, perhaps describe why some sort of Bonferroni – type correction is not appropriate?

L180 Are these results tabulated?

L200 could you plot these visual data, to demonstrate there really is an advantage to using the digital data? The ranges you quote seem to overlap about as much as the fluorescence red values? The visual data does not appear in your supplementary data?

L230 The visual fluorescence data are not shown or plotted – how can you demonstrate that then that the digital measurements are better or worse at allowing differentiation of mean values between epochs?

L240 where are these results shown?

L245 following burial models discussed above – it is really not clear to me how demonstrating a linear relationship as you have done is an indicator or otherwise of reworking. This needs to be described more clearly.

L252 how? What is your threshold value or test to concluded that the sample or stage

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has enough in situ pollen for reconstruction?

L259 “applied” rather than “adhered”?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-391, 2016.

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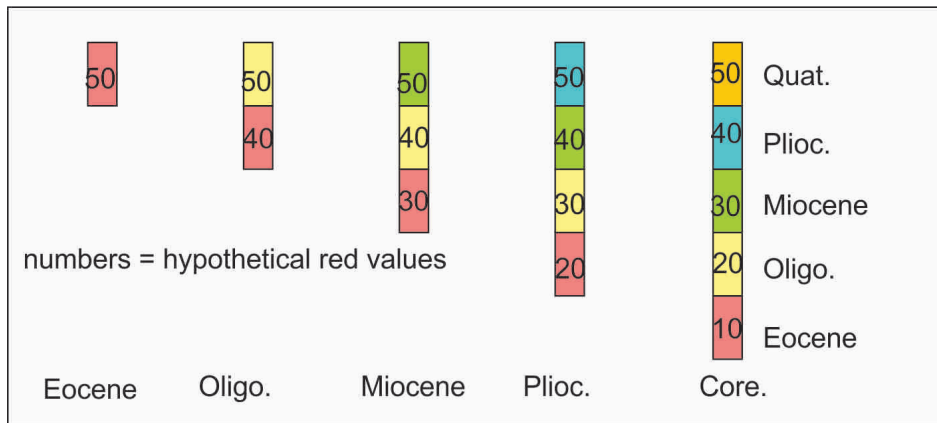


Fig. 1. conceptual deposition model discussed in review