

The paper by Jelavic et al. focuses on the adsorption of DNA by two carbonaceous materials, namely soot and charcoal. The subject bears some environmental relevance and, in that regard, this could be an interesting study.

Unfortunately, the manuscript in its present form is in my opinion scientifically rather poor and does not deserve publication.

General remarks.

The authors present a few adsorption isotherms of DNA on soot and charcoal with less than 10 data points per isotherm and then they compare these isotherms to various models, compute some kind of degree of merit and from the best fit, supposedly deduce adsorption mechanisms. Such an approach is extremely reductive and furthermore is scientifically unsound. The fact that isotherm data are described by a certain model does not provide any evidence that the assumptions underlying the model are true. There are hundreds of examples where, for instance, isotherms can be described by a Langmuir model whereas the basic assumptions of this model, i.e. monolayer adsorption, homogeneous adsorption energy and absence of lateral interactions are not fulfilled. Furthermore, the fact, which is the case here, that the Freundlich model provides a good fit does not provide any meaningful information about adsorption mechanisms. The fact that natural surfaces are energetically heterogeneous is a generality. It is truly a pity that the authors do not try to get more information from the data they gathered as I think that there are a few tendencies, notably concerning the role of the background electrolyte that could be useful.

Detailed remarks.

- Line 17. The statement that a comprehensive insight is lacking does not take into account the large body of literature on the subject including the various studies where nucleosides and nucleotides have been adsorbed on various environmental surfaces.
- Line 43. The term CM is not defined.
- Line 185 and after. See general remarks.
- Line 194 and Figure 1A. Labelling the axis as 2θ without indicating the wavelength is somehow meaningless.
- Line 202 and Figure 1B. The role of surface oxygen that could represent polar groups is not discussed at all.
- Line 218. What does an increase in surface charge density in divalent electrolyte mean? What would be the mechanism?
- Line 221. The increase in IEP with increasing ionic strength may well be due to ion adsorption. This is not even mentioned.
- Line 222. What are the so-called internal and external surfaces? How were they determined? The strong difference between IEP and PZC is really strange.
- Line 248. What are inner particle surfaces??
- Line 254 and after. Binding through cation bridges is never considered whereas this could well occur.
- Line 275 and after. As said in the general remarks, this whole part is meaningless. For instance the transition from multilayer to monolayer deduced from the slightly best fit of Sips equation does not make any sense.
- Line 304 and after. Same remark as before. The fit to a model does not imply that the assumptions underlying the model are verified.

- Line 319. Each DNA occupies three active sites????
- Line 338 and after. Using a longer DNA could be an interesting idea. Still, no information about the various conformations of DNA is provided (in particular what role does ionic strength play on that conformation?). Furthermore, comparing particle size with DNA size and configuration could have provided relevant information.
- Line 389 and after. How does DNA behave in mixtures of water and alcohol?