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Interactive comment on "The aerosol-climate model ECHAM5-HAM" *by* P. Stier et al.

Anonymous Referee #2

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This paper describes a new version of the ECHAM general circulation model, including an improved aerosol scheme. The aerosol scheme uses a series of fixed-size log-normal modes to describe the aerosol distribution, with mass and number being the prognostic variables for each mode.

The model is generally well-described, though does require reading the related paper by Vignati et al (M7: a size-resolved aerosol mixture model for the use in general circulation models, J. Geophys. Res., 2004) in order to fully understand the aerosol scheme. The paper is generally well written and clear.

Major points

1. There are, of course, many assumptions made in choosing the model param-

eters. The paper often does a poor job of justifying them or referencing them. For example, section 2.3.2 is almost entirely unjustified assumptions. Please go through the paper and locate the very large number of statements made about model parameters and either justify the choices made or reference them.

2. The processing of aerosol in clouds is not well described. You describe very briefly the SO2 aqueous chemistry in section 2.4, but not how this affects the aerosol distribution. SO2 oxidation in cloud drops is a major factor in the production of accumulation mode particles and in the creation of a gap between Aitken and accumulation particles. It is not clear whether the modal assumption made in M7 already implicitly assumes the existence of such a distribution, or whether online aqueous chemistry acts to generate it. Please explain clearly how cloud chemistry affects the aerosol size distribution and how realistic it is.

   As described, the SO2 oxidation appears not to be ever H2O2 limited. Please clarify.

3. The treatment of the competition between nucleation and condensation gives cause for concern (p5567). Nucleation rates vary in a highly non-linear way on sulfuric acid concentration, so by always calculating nucleation rates after condensation has occurred you will underestimate the rate. It is not sufficient to say that condensation is an energetically favourable process (this is irrelevant for what you are trying to calculate). The validity of your assumption can be demonstrated by showing that the production rate of particles is insensitive to the timestep used. You should assure yourself that this is the case.

4. My most serious criticism of the paper is the lack of evaluation of the new features of the model; namely the size distribution and number concentrations. A lot of the evaluation refers to particle masses. While this is necessary, it is not really what is of greatest importance in this paper. The number concentrations are evaluated against your own (as yet unpublished) CN concentrations, missing a

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great number of other data sets (for example, measurements over many years in the MBL from the likes of Tony Clarke and Jost Heintzenberg and others). Given the statements made at the start of the paper about explicit simulation of the size distribution, readers ought to be able to judge the quality of this important aspect of the model. This paper is incomplete without an evaluation against observed aerosol size distributions. I am not suggesting that this evaluation needs to be exhaustive, but it does need to be included. This evaluation needs to focus on two issues: Firstly, the model describes the size distribution in terms of modes, which move in radius space in response to changes in number and mass. It is not obvious that the shifts in modal radius (and the remapping that is done) produces modes at the right size. Secondly, the CCN spectrum is no doubt one quantity that you would like this model to be able to calculate (and thereby develop better parametrisations of cloud drop concentration). This requires that you can get the shape of the size distribution correct above about 50 nm dry diameter. Is this achieved, at least broadly?

Minor points

5553.12. What is meant by 'long-term' simulations? It means different things to different people.

5554.12. The term 'interactive approach' is used several times. What do you mean by it?

5554.26. You say that the aerosol size distribution is predicted explicitly. This isn't strictly true because you assume the distribution has a fixed functional form. Perhaps you need to say 'certain parameters' of the distribution are predicted.

5556.15. What is meant by 'soluble' - in water? Non-solid? Miscible?

5556.17. What is 'relative composition'? Relative to what?

5557.2. Physical assumption. Do you mean physically realistic?

5557.2. I don't understand the meaning of the sentence beginning 'ExtendingĚ'.

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5562.15. It sounds like the sedimentation speed is restricted to prevent numerical instability. Don't you mean that the timestep is restricted?

5570.14. It's not clear what is meant by inter-modal coagulation being treated as operator-split. I don't see that this is described in the next section. What is meant here? It seems to be important.

5566.17. You talk about the 'dominating processes' of coagulation being simulated. What are these?

5570.22. Sentence starting 'The total condensableĚ' is unclear. I understand that aerosol are transferred to the mixed mmode once they have absorbed a monolayer equivalent of sulfate. Is this right? This must happen very quickly. How do you justify this assumption? What do you do about mixed sea salt/sulfate?

5578. I suggest to replot the number concentration as observable CN (i.e., > 3 nm) to be accessible to observationalists.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5551, 2004.