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Interactive comment on "Change in tropical forest cover of Southeast Asia from 1990 to 2010" *by* H.-J. Stibig et al.

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The manuscript's topic is very timely, given the considerable forest area dynamics in SE-Asia.

It is clearly written and well understandable.

Methodology and results are well presented and explicitly explained so that specific methodological observations are possible (see below). That is very good.

Not only are results of forest area changes presented but also a discussion of causes and drivers of these changes; and this discussion is not speculative but based on expert consultations in the region. This combination of socio-economic assessments and biophysical results is straightforward and excellent; it does make this contribution

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not only scientifically credible but also relevant in practice and with a high degree of legitimacy.

⇒ Looking forward to see this manuscript published soon !

Technical observations:

1. The authors may wish to be more explicit about the forest definition on which they based their forest cover change analyses. They use the FAO definition; but then, the "stable minimum mapping unit" in image interpretation is 5ha. It should be explained how the 0.5ha minimum area criterion of the FAO definition is then dealt with. Also the 5m minimum height criterion of the FAO definition deserves more explanation; it is not straightforward from coarser remote sensing imagery. While this point is briefly addressed in the discussion section (Chapter 4 L20f) it could be more explicitly explain how the authors dealt with this difficult criterion.

2. The "sampling intensity" issue when using lat-lon intersections is being addressed. And "weighting with the co-sinus" of the corresponding latitude is mentioned to explain how the estimation went This is not enough to understand the estimation entirely. The authors should explicitly give all estimators used (point estimates and interval estimate) and explain thereby how they dealt with the issues of (1) systematic sampling of (2) increasing sampling intensity with increasing latitude and of (3) combining subsets of sample locations for point and interval estimates.

3. Chapter 2.3, L10: the formulation "change of natural forest canopies to tree plantations" sounds strange: sounds as if "canopies" develop to "forest". Why not saying "natural forest" and explain what is meant by that (probably forests with diverse canopy structures that embrace different natural looking forest types, including selectively logged, virgin forest, alder secondary forest …)?

4. It is very welcome that the authors carry out an explicit accuracy assessment and discuss the inherent difficulties. Would be good (much clearer) to present the results in

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a table. Also – while it is understandable that tree cover classes are analysed – it would be useful for forest monitoring people to see a quantification of different accuracies for *different forest types* (if possible ...); it is likely, for example, that open dry forest formations are estimated with lesser accuracy than closed humid forests.

5. Chapter 3.1. L7: I guess that SE stands for standard error. Should be mentioned, then. If standard errors are given, their estimators must also be given in the methods' section (see observation 2.). Also: if the point estimate, 0.67%, for example, is given with two decimals, so should the standard error (as done in L28 then). Also: it may be mentioned that the SE as specified here is given in absolute terms (as opposed to relative to the point estimate); although it is somewhat obvious.

6. What is given in chapter 3.1 L1-25 is essentially what is given in much clearer terms in Table 3. The text may be considerably shortened.

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