The comments from the reviewers were exceedingly helpful. Any hope that I had to *put one over* on the audience was immediately and effectively dispelled. My responses to the reviewers' comments are not fully incorporated into the submitted second year paper. I can, however, address the comments in this response, hoping to outline my continuing attempt to refine the paper along the lines of the reviewers' comments.

The reviewers note that my model assumes that only one processing plant be built within each cluster. This comment has prompted more research into the cost structure of palm oil plantations. Conversations with collegues in ESPM have yielded a much better understanding, albeit undocumented, of the operating costs of large palm oil plantations. The cost of the processing facility is by far the largest cost, accounting for 40-60% of the upfront investment. There are almost certainly no more than one processing facility per cluster. The reviewers' question did, however, prompt another study. Namely, how many of the clusters have a processing facility at all? Many of the smaller clusters may be near large thoroughfares, which may mitigate the need for a processing facility that is immediately within the cluster. This should not change the broad results of the paper, but may complicate the interpretation. The roads within clusters are almost certainly of lower quality than roads between clusters. The theoretical model, then, may be enhanced with basic graph theory or operations research to suggest optimal number of centers in a polycentric graph. In fact, this structure may help to explain the power laws observed in the sizes of clusters, a rank-size distribution that is not altogether different from the rank-size distribution of urban population centers.

The comment on the theoretical model is also well-received, and reveals my own lack of understanding of Weitzman's models. I have modeled the policy shock as a simple increase in the relative price of one capital input. Within Weitzman's framework, the effect of the differential price increase is clear, and is (hopefully) reflected in my paper's structure. However, the question remains about why the policy should not have impacted any of the other parameters that determine the placement of the iso-net-gain line, rather than just movement along it. I believe that here, too, I can make an argument or explain the intuition that will leave the broad results unchanged; but this has not been fully described in the paper.

Finally, I have added a little more on the link between the theoretical model and the empirical model. This could definitely be fleshed out more, however. The only purpose of the theoretical model is to show in painful detail that the standard results in production theory are robust up to dynamic considerations, assuming a stationary solution. The results are, namely, that (a) an increase in an input's price will reduce

the intensity of that input in the production process and (b) the overall level of inputs used depends on the elasticity of substitution in the derived demand for inputs. In my case, the rise in the long-term price of remote deforestation will reduce the intensity of remote clearing but increase aggregate clearing because of the assumed cost functions of peripheral and remote clearing. The empirical specifications fall directly out of this insight; but it is clear that is poorly expressed in the paper.

On the empirical side, I have taken many of the suggestions to heart, especially the placebo/falseification test. I have run a simulation wherein I split the treatment group into two, testing for a spurious treatment effect. It is clear, however, from the simulation that the national-level policy did not induce a systematic difference in the intensity of remote cluster creation. The variance and mean of the simulated differences remained stable over time, indicating that neither the policy nor variation in commodity prices did not substantially change the generating process of clusters between provinces (or kabupatens) in Indonesia. This empirical result is consistent with a national-level policy. A question for future work is whether the same exercise should be peformed for subsets of the treatment *and* control groups, instead of just subsets of the control group. We are testing whether the national-level policy shifted deforestation. Is it necessary to assume that the national level policy was homogenous across sub-provinces in the treatment group? Or does the aggregate impact remain noteworthy, even if the distribution of clearing activity shifted within the country? These questions will be further explored in future versions of the paper.