Response to Referee 2

1. Agreed.
2. Agreed. This mistake occurs in leading texts such as Greene (2012) and Maddala & Kim (1998). In a previous version we had Geweke, Porter-Hudak estimates of d, which exceeded 1.5 for GHGs. We can restore the discussion of fractional d.
3. Agreed.
4. Agreed. The deviation from trend is I(0), which means that Y cannot be I(0).
5. Our understanding is that PP does not change the mean. In Stata and Eviews the parameter estimates are identical for DF and PP, but the PP statistic differs from the DF statistic because the variance is different. Have we misunderstood?
6. Agreed. Detail unnecessary.
7. Agreed. We intended to say that even if there is reverse causality the parameter estimates are super-consistent. We will redraft.
8. Agreed. Type 1 error is false positive and type 2 false negative.
9. Yes. Super-super consistency with I(2) variables is T^2.
10. Done: reference for SEBM.will be included in the manuscript's revised version
11. Agreed: t instead of infinity.
12. Agreed, the exposition needs improving. The bottom line, with which ref 2 agrees, is that we do not need to specify F_B (as claimed by Stern).
13. Agreed.
14. The null is "I(2) v I(1) with a single unknown break-point."
15. Table 2. See the Response to ref 1.
16. Multicointegration, as originally suggested by Granger & Lee (1989), refers to I(1) variables in stock-flow models. Subsequently, Engsted, Gonzalo & Haldrup (1997) interpreted multicointegration in an I(2) context because a cumulative I(1) variable must be I(2). In this way the concepts of multicointegration and polynomial cointegration have become confused in the literature. Hence, the reference to Enders (2010). We will redraft footnote 4 accordingly.
17. Agreed. We should refer to g as an "anthropogenic anomaly" rather than an "anthopogenic trend". For example. $rfCO_2$ = "GHG trend" + g. We will clarify all this.
18. Agreed. Tables are better than graphs.
19. Agreed. The statistic is slightly below its critical value.
20. In equation (12) the sign of g2 should be positive.
21. Yes. It should be +0.6S.
22. Agreed. This is the cointegration test in Shin (1994). We will refer to "KPSS test" for variables and the "Shin test" for cointegration.
23. Agreed. We can show this result.
24. It should be equation 14, not 15.
25. Agreed. Handle nonlinear transformations with caution. Ref 2 needs to understand that because CFMs are highly nonlinear, we need to check

nonlinear alternatives. Maybe we should refer here to Granger & Hall (1991) on nonlinear transformations of integrated variables.

26. Banerjee et al (1993) pp 30-32 show that ΔNNP is nonstationary in UK during 1872-1975, whereas ΔlnNNP is stationary.

27. As we have just seen in #27 the log difference of a variable may be stationary. We agree that products of I(0) and I(2) cannot be I(1). The main point is that we were unable to find a nonlinear transformation that was I(1). We prefer to retain this in the light of comments received from climate scientists at various seminars.

28. Yes. CH_4.

29. Agreed. Maybe we should mention fractional cointegration in a footnote, and relate the discussion to comment #2.

30. Agreed. Engle & Yoo (1991).

31. Agreed. We should report the robustness checks in tabular form.

32. Agreed. We will use the critical values for DOLS given by Stock & Watson (1993).

33. Agreed. See comment #8.

34. Agreed. See reply to referee #1.

35. We have plotted delta $rfCO_2$ in our Response to ref #1.

36. Agreed. Results in tables.