We would like to thank the reviewers for their pertinent and constructive remarks which allowed us investigate the subject in greater depth.

Responses to reviewer 1

1) «*The authors fail to carefully distinguish between the occurrence of overturns and the existence of turbulence*.»

Clearly, we made, throughout the submitted version of the manuscript, a one to one association between overturn and turbulence. We followed the conclusion of Thorpe in his seminal paper of 1977: Thorpe addressed this issue and concluded that «in the absence of any other test, we used this [the detection of overturns] as a criterion for the presence of turbulence.» He based his conclusion on a threshold Rayleigh number Ra which can be resolved above noise level. By applying the same reasoning, and by using typical values for superadiabatic temperature differences (0.01 K) and vertical scales (L_{tropo}=100 m, L_{strato} = 10 m), we found Ra ranging from 10^8 (in the lower stratosphere) to 10^{11}-10^{12} (in the troposphere), far above the critical Ra number for the onset of turbulent convection ($Ra_c \sim 5x10^4$).

Another reason for relating overturns to turbulence is the fact that developed turbulence within a stratified medium will produce overturns owing to the stirring of air parcels. But one cannot, from the in-situ temperature measurements only, distinguish these turbulent regions from unstable layers before the onset of turbulence.

A third argument comes from observations. During one MUTSI flight, a persistent turbulent layer was observed by the MU radar (Luce et al., 2002). The horizontal distance between the radar and the air masses encountered by the balloon within the heights of interest (11.5 to 12.3 km) ranged from 1.5 to 16 km. Within the turbulent altitude domain, the balloon observed several overturning layers as well as an almost neutralized layers possibly due to turbulent mixing processes. Although not fully conclusive (the observation were not exactly colocated), they support our initial interpretation.

Thus, we have good reasons to think that overturns are associated with turbulence. But we agree with the reviewer that radiosondes do not detect turbulence per se. And that overturns can exist and disappear before turbulence is fully developed. So, as suggested, we take care throughout the paper to avoid any confusion between overturns and turbulence. We also add a few sentences in the revised version (end of the first paragraph and second paragraph of the introduction) in order to clarify this issue.

2) *About "first differences"*: we explicitly explain the meaning of "first differences" at first occurrence of this expression (which is widely used in time series analyses ans elsewhere).

3) *About the fraction of atmosphere which is unstable:*

From Tables 1 and 2, and by taking a tropopause height at 12 km and a lower-stratosphere up to 25 km, we found that 56% of the troposphere and 37% of the stratosphere is statically unstable (at the vertical resolution of HR flights after denoising, i.e. ~ 1 m). In a previous paper (Wilson et al, 2010), we found, from 3 MUTSI flights, that 45% of the profile (troposphere+stratosphere) was unstable. The former value, for tropospheric data, is in excellent agreement with the one obtained

by Cho et al (54%).
Although beyond the scope of this manuscript, we will add a few sentences about this result in the conclusion.

Responses to reviewer 2

1) *Specific comment*.

To our knowledge, simultaneous observations in the free atmosphere from both in-situ and additional instrument (such as radars, lidars or aircrafts,...) of overturns has not been reported, until now, without ambiguity. A good example is the observation reported by Luce et al.(2002) of strong radar echoes as well as of large overturns from an in-situ temperature profile. Such an observation support the interpretation of turbulent processes related to potential temperature overturns. However, some ambiguity remains since the distance of the air masses sounded by the radar and the balloon ranges from 1.5 to 16 km with a time lag of 10 to 16 min. Always with the aim of clarifying such an issue, we have planned a field campaign this fall including the MU radar, a lidar, and numerous balloon flights.

We will include a short paragraph in the introduction to mention the observations of Luce et al.. And to acknowledge that further validation is still needed.

2) *About the term «inversion»*: we will add a sentence in order to avoid confusion with the meteorological sense.

3) About the word «sample»: In a statistical sense, a “sample” is a subset of a population and not a single item of that population (common sense). The term “sample” seems thus relevant in the present context. However, we understand the possible confusion, the same ambiguity existing in French. We added a short definition at the first occurrence of this term.

4) As suggested, we took care throughout the paper to avoid useless ambiguity between the terms «raw», «low resolution» «standard» and so on.

5) We thank again the referee for correcting the (too) numerous typos and mistakes throughout the paper. Also, we corrected the figures.

Reference: Luce**,** Fukao, Dalaudier and Crochet, Strong Mixing Events Observed near the Tropopause with the MU Radar and High-Resolution Balloon Techniques, JAS, 59, 2885-2896, 2002