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Comments on the paper “The initial conditions of isolated star formation - VI. SCUBA mapping of prestellar cores” (Kirk et al. 2005)

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Abstract. In their survey paper of prestellar cores with SCUBA, Kirk et al. (2005) have discarded two of our papers on L183 (Pagani et al. 2003, 2004). However these papers bring two important pieces of information that they cannot ignore. Namely, the real structure of L183 and the very poor correlation between submillimeter and far infrared (FIR) dust emission beyond $A_V \approx 15$ mag. Making the erroneous assumption that it is the same dust that we are seeing in emission at both 200 and 850 μm , they derive constant temperatures which are only approximate, and column densities which are too low. In fact dust temperatures do decrease inside dark clouds and the FIR emission is only tracing the outer parts of the dark clouds (Pagani et al. 2004).

Key words. ISM: dust, extinction – ISM: Structure – ISM: individual: L134N – ISM: individual: L183

these mis-identifications, most or all of the subsequent results discussed in that paper are not valid.

1. The L183 case

1.1. Historical background

Ward-Thompson et al. (1994) reported a first submm source in L183 centered at $15^{\text{h}}54^{\text{m}}-2^{\circ}51'$ (J2000) and Ward-Thompson et al. (2000) reported a second submm source centered at $15^{\text{h}}54^{\text{m}}09.-2^{\circ}52'38''$ (J2000), about $90''$ further south. The existence of the first source is not mentioned in the second paper. Lehtinen et al. (2003) combined these two detections as two separated sources which they identified with FIR peaks from an ISOPHOT 200 μm strip despite a difference of $30''$ in the separation of the two sources between the submm and the FIR identifications.

We then showed (Pagani et al. 2003) from a large MAMBO map and from an ISOCAM absorption map that there was no northern submm source compatible with the position reported in Ward-Thompson et al. (1994). This was also confirmed by recent SCUBA maps at 850 μm from our own work (unpublished) and from Kirk et al. (2005) present work.

We also showed that the 200 μm sources found by Lehtinen et al. (2003) were in fact artefacts from their data reduction process and that no point source could be clearly identified (Pagani et al. 2003, 2004). Because of

1.2. Present paper (Kirk et al. 2005)

Kirk et al. (2005) report one point source at 850 μm and two point sources at 450 μm , one in common with the 850 μm peak and which coincides also with our own ISOCAM and MAMBO detection and a second, further north for which they have no explanation apart from a possible temperature gradient related to the fact that the 200 μm peak is situated even further north than this second peak. Though this second peak could be approximately consistent with their first detection (Ward-Thompson et al. 1994), or with Lehtinen et al. (2003) source FIR1, they do not discuss the validity of their detection with respect to these works. However, in their introduction, they indicate that the results found by Lehtinen et al. are consistent with their own findings.

Can we check the possibility of a source appearing at 450 μm (with a 10σ detection) with no or at most weak counterpart at 850 μm ? First, let us evaluate the 450/850 μm ratio: the highest contour is 1170 mJy/beam at 450 μm and 120 mJy/beam at 850 μm , these contours have approximately the same size except that the 850 μm contour is obviously not a closed contour around this putative source. If we suppose that the dust emissivity law varies with λ^{-2} , we find that this ratio of ≈ 10 is indicative of a 35 K source. This is a large value for a dark cloud

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which could indicate the presence of an embedded protostar. However this hot spot is not seen with IRAS at $100\ \mu\text{m}$ nor with ISOPHOT at the same wavelength despite its higher resolution ($45''$). Keeping the same dust dependency with wavelength, the flux at $100\ \mu\text{m}$ should be $\approx 10^4\ \text{MJy sr}^{-1}$ if we extrapolate from the $450\ \mu\text{m}$ estimate. If we take a more pessimistic dust emissivity law proportional to $\lambda^{-1.5}$ or varying from λ^{-2} to λ^{-1} between 450 and $100\ \mu\text{m}$, this would lower the flux only by a factor of 2. We must take into account the dilution of this hot spot in the ISOPHOT beam. The hot spot is $15''$ wide (i.e. the SCUBA resolution. The contours are slightly more extended in fact but let us take this value as a lower limit) and the ISOPHOT resolution is $45''$ thus even if the remaining dust in the ISOPHOT pixel has a negligible emission flux, the hot spot emission should still be $10^3\ \text{MJy sr}^{-1}$ at $100\ \mu\text{m}$ and still $500\ \text{MJy sr}^{-1}$ if we lower the emissivity of the dust in the FIR. It is obvious that such a source could not be missed in the ISOPHOT $100\ \mu\text{m}$ map which yields $27\ \text{MJy sr}^{-1}$ including the diffuse dust emission and only $5\ \text{MJy sr}^{-1}$ after the diffuse emission has been subtracted. One should remember that SCUBA is totally insensitive to diffuse extended emission and there should be no reason to consider it here. Even if we lower the dust temperature to $20\ \text{K}$ instead of $35\ \text{K}$, we find a final flux (including dilution) of $120\ \text{MJy sr}^{-1}$ (or 60 for a lower emissivity), several times more than the measured value. Thus the northern $450\ \mu\text{m}$ source is most probably an artefact and the north-south temperature gradient is not a correct explanation for this detection.

2. The dust temperature constancy

We have shown in both our papers, (Pagani et al. 2003, 2004) that the $200\ \mu\text{m}$ was not tracing the coldest dust but only its outskirts. The dust peaks are not seen and this is not a question of beam dilution with ISOPHOT as we have shown specifically in Pagani et al. (2004) by smoothing our dust map obtained from near- and mid-infrared measurements to ISOPHOT resolution and comparing it to the $200\ \mu\text{m}$ map (see our Figs 5, 6 and 7 in Pagani et al. 2004). A north-south cut of both maps (NIR+MIR map and ISOPHOT FIR map) to follow the profiles makes the case very clear. We reproduce here the Fig. 6 of Pagani et al. (2004) to show this cut (Fig. 1). The most plausible explanation for this discrepancy is that the temperature drops inside the cloud below $10\ \text{K}$. This is predicted by Zucconi et al. (2001), Evans et al. (2001) and Stamatellos & Whitworth (2003) and is contradictory with the assertion that constant temperature is consistent with the observations. Our result is directly obtained from the observations and does not rely on any of these models until we want to quantify more precisely this effect. The criticism addressed by Kirk et al. about the Zucconi model being inconsistent with the observations is only partially right : deep inside the cores, the temperature does not vary very much in any of these models, and the variation is slow enough (or even constant, albeit at a level of

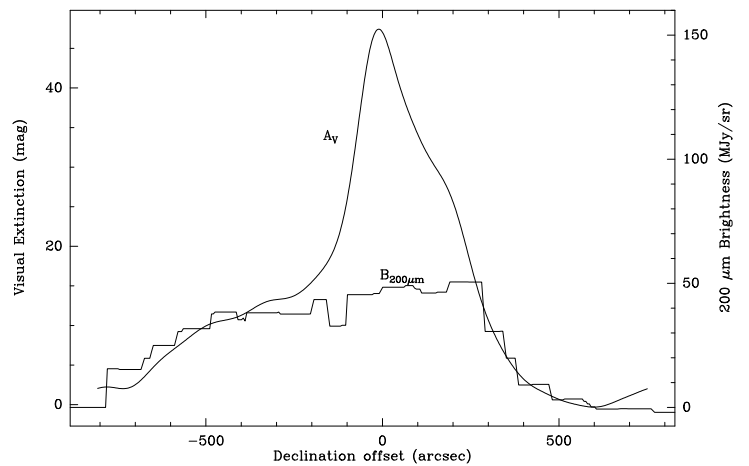


Fig. 1. ISOPHOT $200\ \mu\text{m}$ emission (labeled $B_{200\mu}$) seen along a north-south cut through the main dust peak and compared to the dust measured in absorption (labeled A_V) and degraded to $90''$ resolution. The $200\ \mu\text{m}$ emission perfectly matches the extinction due to the dust on the outer parts of the cloud but quickly flattens out in the middle. The loss of correlation between the dust in absorption and in emission is striking (from Pagani et al. 2004)

about $7\text{--}7.5\ \text{K}$, after having dropped from $13\text{--}15\ \text{K}$ outside the cloud, following Stamatellos & Whitworth 2003) to let absorption profiles measured in the NIR and the emission profile in the submm look similar. From our observations, it is clear that the $200\ \mu\text{m}$ is tracing dust up to $A_V \approx 15\ \text{mag}$ (total along the line of sight) and that beyond, the dust temperature has dropped to such low temperatures that its contribution to the total $200\ \mu\text{m}$ emission is negligible. Thus the dust traced by submm and FIR emission are not the same and deriving temperatures and subsequently dust column densities from spectral energy distributions including both sets of wavelengths is wrong. For example, in L183, we find a dust temperature varying from $\approx 13\ \text{K}$ to $\approx 7\ \text{K}$ and a peak opacity of $150\ \text{mag}$, instead of $10\ \text{K}$ and $85\ \text{mag}$ for Kirk et al. (2005). Though the authors could argue that we are more or less within the error bars they mention, we are at the edge of these bars and the error is probably systematic in an overestimate of the dust temperature and an underestimate of the dust column density and mass for most of their sources for which they have combined submm and FIR data.

3. Conclusions

Probably the main results of the paper of Kirk et al. (2005) are not much changed by these corrections we bring here and thus there is all the less no benefit to discard works which partly contradict one's results. At least our papers should have been discussed to say why they disagree with our results, we are open to discussion. This would help everybody to test and improve his/her arguments. There is also no shame to recognize one's mistakes especially in the very difficult domain of submm continuum observations

and if the authors would do so themselves it would avoid other people to use wrong data which always adds noise to the debate and induce extra effort from other people to correct and fight against the propagating mistakes.

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