

Unfortunately, at the time of the study the team had focused only on recording simple standardised outcomes at a fixed endpoint in time (point outcomes) through a manual audit of patient cards. Specific efforts to ascertain and record the exact dates of death, loss to follow-up or stopped ART were not made. Thus, we are unable to report on them in this reply.

There are a number of observations that are worth airing for discussion. First, albeit within the limits of a relatively crude analysis, the magnitude of the differences between the communities, i.e. the clinical significance between the two groups, is very large, suggesting that a powerful effect was taking place. Although staggered entry into ART was occurring, it is likely to have been similar in both groups, thereby making the comparison fairer.

Second, although desirable in a real-world programme setting, a randomised study design is often not possible or ethically acceptable. This was the case for this study in which we had to base our analysis on simple observational data. As Muula rightly points out (and as we had also highlighted in our paper), it is thus indeed impossible to know the exact reasons for the differences observed in death rates (or other outcomes) between the two groups that were compared.

Third, in a situation where randomisation could not take place, it is important to try to describe the two groups clearly in order to get a sense of their similarity. The basic clinical characteristics of the two groups did not show exceptional differences, except that those without community support had lower CD4 baseline levels. This might have been linked to delayed presentation for whatever reason(s) or it might have been a social consequence of the absence of community and family support. Although not mentioned in the paper, the great majority (80% or more) of those placed on ART at the time of the study were inhabitants of traditional authorities within the proximity of Thyolo Hospital and so we do not think that distance as such would explain this difference. True enough, we did not aim to control for potential confounders such as cost of transport or distance to health facilities, which might merit further study.

We do, however, believe in the suggestion that there might indeed be social differences between the two groups that would explain the favourable impact of community support, and this is precisely the focus of this paper.

Success of treatment for any chronic illness, whether with ART or any other form of difficult treatment, would stand to gain if there is a supportive environment that fosters treatment compliance, empowerment and treatment literacy. Although public health services, including the ART clinic services in Thyolo, try hard to reach this objective through repeated counselling sessions at the hospital level, this cannot in any way replace the enhancing effect of a well prepared and supportive family and community environment, the possibility of decentralised and 'accessible' assistance in case of complications through volunteers or community nurses, or continuing adherence support. These components, including active defaulter tracing, were built into community support activities for which none of the health facilities in Thyolo had the human resources or capacity to fulfil. It is noteworthy that the hospital ART team systematically used the existing community networks to

trace and assist patients who do not turn up for their clinic appointments.

In summary, it may be said that the findings of this study tend to reflect the possible gaps in both 'access' and 'continuum of care' that are being filled, with significant effect, by the presence of a community network. In this regard, the results, despite the limitations, appear to confirm the strong positive effects of community support in scaling-up ART.

Conflicts of interest statement

The authors have no conflicts of interest concerning the comments in this correspondence.

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Comment on: Human tapeworms in north Vietnam

The interesting results obtained by Somers et al. (2007) regarding the high prevalence of *Taenia saginata asiatica* found among 65 north Vietnamese patients deserve some comments related to the diagnostic methods used and the epidemiology of this parasite. Although we and many other authors believe that *T. asiatica* should be considered a separate species (Galán-Puchades and Mas-Coma, 1996), we call it a subspecies of *T. saginata*, respecting the opinion of Somers et al. (2007).

The high proportion of *T. s. asiatica* identified using a PCR-RFLP assay in north Vietnam is in sharp contrast to previous epidemiological findings in humans and pigs. Only one human case had previously been diagnosed (De et al., 2001) and no metacestodes of *T. s. asiatica* have so far been reported in pigs in Vietnam. This suggests that the absence of *T. s. asiatica* in humans and pigs, not only in Asia, could possibly be due to misdiagnosis and that molec-

ular techniques such as PCR-RFLP or multiplex PCR need to be included in the diagnostic procedures so that it is possible to identify *T. s. asiatica*.

From an epidemiological point of view, it is remarkable that *T. s. asiatica* is found in Vietnam even though in that country it is uncommon to eat undercooked pork liver, which is the most usual location of the cysticerci of *T. s. asiatica*. A possibility is that alternative locations for the parasite exist, which is reinforced by the fact that Vietnamese usually eat raw or undercooked meat. With this in mind, it might be concluded that the spread of *T. s. asiatica* to countries outside Asia could be facilitated by culinary habits such as eating raw and undercooked food, including viscera or meat.

In view of the results obtained by Somers et al. (2007), we suggest the investigation of two relevant questions in the Vietnamese patients, i.e. the possibility that *T. s. asiatica* may cause human cysticercosis and the specificity of the electroimmunotransfer blot (EITB) assay for *T. solium* cysticercosis.

The WHO has practically and, we believe, incautiously discarded *T. s. asiatica* as a possible cause of human cysticercosis only because *T. saginata* does not cause the disease (Murrell, 2005). *Taenia saginata* does not cause human or pig cysticercosis, so the same should be valid for *T. s. asiatica*, but *T. s. asiatica* clearly causes pig cysticercosis. Therefore, the possibility that *T. s. asiatica* may cause human cysticercosis does indeed currently exist (Galán-Puchades and Fuentes, 2004). Somers and colleagues have the unique chance to investigate the possible presence of cysticerci in the 36 patients who harboured *T. s. asiatica*. Considering the most common location of cysticerci in the intermediate host, we suggest the use of imaging techniques mainly at hepatic level and/or liver ultrasound.

In addition, serological analysis of these patients should also be a tool to demonstrate whether the EITB is in fact 100% specific for *T. solium* or cross-reacts with *T. s. asiatica*, as previous reports suggest (Furrows et al., 2006; Galán-Puchades and Fuentes, 2007; Pilcher et al., 1991).

In the case of *T. solium* taeniasis, not all patients develop cysticercosis and the same might be true for *T. s. asiatica*. However, the possible presence of cysticerci and/or positive bands in the EITB assay in some Vietnamese patients might have an enormous impact on the epidemiology, pathology and diagnosis of human taeniasis and cysticercosis.

Conflicts of interest statement

The authors have no conflicts of interest concerning the comments in this correspondence.

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Response to comment on: Human tapeworms in north Vietnam

The high proportion of *Taenia saginata asiatica* among tapeworms collected from patients in north Vietnam is surprising because Asian tapeworm had been identified only once (De et al., 2001) and because the larval stage of this cestode has never been reported in pigs in Vietnam. Although *T. solium* is endemic in rural areas in Vietnam (Somers et al., 2006), earlier reports indicated that based on morphological differentiation only 20–22% of tapeworms belonged to this species (cited by Willingham et al., 2003). In that study, 78–80% of the human tapeworm infections were attributed to *T. saginata*, which, however, could not be differentiated from *T. s. asiatica* based on the morphological methods used and therefore could have been misdiagnosed.

Recently, the National Institute of Veterinary Research in Hanoi conducted a slaughterhouse-based study in pigs to investigate the prevalence of helminth-induced lesions in the liver as well as cysts in the meat and viscera. Preliminary results showed a high proportion of cysts in the viscera as well as lesions in the pigs' livers, which were indicative of cestode infections. All cysts in the viscera and most lesions in the liver were due to metacestodes of *T. hydatigena*, a common tapeworm of dogs; however, some liver lesions resembled cysticerci of *T. s. asiatica* (unpublished results). These lesions are currently being examined using molecular techniques for species identification. The frequent occurrence of visible cysts in the pigs' livers and viscera is not unknown by butchers and meat inspectors,