however infected organs or carcasses are usually not condemned and consequently these findings are not officially reported. This may explain why cysts of *T. s. asiatica* have never been reported in Vietnam. To our knowledge, this current survey of helminth-induced lesions in pigs' livers is the first of its kind in Vietnam.

In their comments on our paper, Galán-Puchades and Fuentes mention that Vietnamese people usually eat raw or undercooked pork. It should, however, be noted that only a few traditional Vietnamese dishes contain raw pork. Pork livers are usually fried, but no investigation has been undertaken to evaluate the effects of Vietnamese cooking methods on the viability of cysts.

Galán-Puchades and Fuentes raised the possibility that *T. s. asiatica* may cause human cysticercosis. The high incidence of subcutaneous cysts among cysticercosis patients in Vietnam and other Asian countries together with the presence of *T. s. asiatica*, a species that is only found in these areas, may support this hypothesis. Subcutaneous cysticercosis is uncommon in America and Africa. However, no conclusive evidence was found to confirm this hypothesis. PCR-RFLP analysis on three subcutaneous cysts, collected by biopsy from Vietnamese patients at the National Institute for Malariology, Parasitology and Immunology, all showed a *T. solium* pattern (unpublished results). Differences in pathology of human cysticercosis may also be caused by genetic differences within the *T. solium* species. Based on genetic polymorphism analysis of two mitochondrial genes, Nakao et al. (2002) were able to differentiate an African/American and an Asian genotype of *T. solium*. However, more work on the genomic DNA polymorphism is necessary to determine whether this genetic difference in this parasite may explain pathological differences (Ito et al., 2003). An important proportion of *T. solium* tapeworm carriers develop cysticercosis through self-contamination. To examine this possibility in *T. s. asiatica*-infected individuals in our study, Galán-Puchades and Fuentes suggested the use of imaging techniques at hepatic level and/or liver ultrasound. We will investigate whether this study and blood sampling for electroimmunotransfer blot analysis can be organised in Vietnam, but realise that even after tracing back these individuals in the remote provinces it will not be easy to convince them to undergo these examinations. We can add that none of the *T. s. asiatica*-infected individuals in our study showed clinical signs suggestive of neurocysticercosis or subcutaneous cysticercosis.

Conflicts of interest statement

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

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Comment on: What do community-based dengue control programmes achieve? A systematic review of published evaluations

A systematic review of the achievements of community-based dengue control programmes has been long overdue, and Heintze et al. (2007) made a most welcomed contribution. They conclude that community-based strategies appear to have some effect on *Aedes* larval indices but that there is no evidence of their impact on dengue transmission. The authors also report methodological shortcomings in all assessed studies and pinpoint their lack of breadth. However, the review itself suffers from some methodological limitations and conceptual inconsistencies and seems, in general, to rush the bend too sharply.

Our first quibbles are methodological and mainly related to the scoring system used for assessing a paper's quality. Amongst others, the study designs were appraised on very formal grounds only, without paying much attention to the actual underlying quality. For instance, many epidemiologists would argue that a 'randomised controlled trial' (score 3 points) with four units allocated to an intervention and four to a control arm is fairly equivalent to a 'controlled before and after trial' (1 point) where six intervention and four control units were purposively but carefully chosen. Also, the authors fail to appreciate that *P*-values (1 point if reported) and confidence intervals (1 point if reported) are essentially reciprocal statistics and that the latter are to be preferred (Gardner and Altman, 1986).

Second, and more importantly, Heintze et al. (2007) define community-based dengue control as 'any intervention in which at least one component targets the community'. This is conceptually questionable since it leads to a strange amalgam of interventions in which people are merely recipients of technocratic control efforts, of strategies that aim at individual behavioural change and of approaches that truly involve the community in (decision-making on) control activities. Differentiating between them has been strongly advocated by scholars in the fields of dengue (Gubler and Clark, 1996) and of participation in health (Rifkin, 1996). Furthermore, separating target-oriented from empowerment-oriented approaches might have helped to identify 'which...components of...interventions have...impact' and to sort out whether indeed 'tailoring interventions...is important'. This is now left over to 'future research'.

Our final reserve is again methodological. The authors deplore that the reviewed studies inform insufficiently on strategy developments, on implementation processes and on reasons for successes or failures, but they fall short of questioning their own inclusion criteria. Randomised trials are generally not suited to capture the riches of complex interventions, and evaluations of quasi-experiments do not often explore the underlying mechanisms that lead to outcome (Magalhães de Oliveira and Valla, 2001; Pérez et al., in press). We acknowledge the importance of appropriate designs (given specific objectives), of rigorous methods (a function of the discipline) and of eventually measuring outcome, but when one sets out to comprehensively review the achievements of community-based dengue control strategies one cannot, with impunity, exclude all studies but 'trials evaluating effect'.

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Reply to comment on: What do community-based dengue control programmes achieve? A systematic review of published evaluations

Systematic reviews require a strict definition of terms and selection criteria, which means that other interesting and important aspects of a paper are filtered out. In our review we found—and mentioned in the discussion—rich information regarding the complexity of community interventions (without having included approaches where 'people are merely recipients of technocratic control efforts'); we identified the challenges of measuring behaviour change and related variables, but our main objective was to assess the efficacy of such interventions in terms of reduction of vector density and eventually disease transmission. Our argument is that, particularly in dengue vector control, you might achieve an excitingly active community participation and empowerment but, as the control tools are often laborious and demanding or the required leadership is difficult to maintain, the expected (sustained) reduction of the vector population may not be achieved. What we found is weak evidence for 'community effectiveness' in dengue vector control, which may be due to a lack of well designed trials or because community-based interventions deliver less than expected.

This does not mean that analysing the evidence of cost-effective strategies for achieving and maintaining community participation is less important (for example, Toledo et al., 2007); on the contrary, such reviews are urgently needed, taking into account the challenges of operationalising complex variables such as 'target behaviour', 'skill deficits', 'performance' (Elder and Lloyd, 2007), 'community empowerment' and others. Even more urgently needed is research on alternative delivery models of dengue vector control, looking at staff requirements, costs and partnerships, using an adequate methodology (controlled before—after trials are, in our view and following the Effective Practice and Organisation of Care Group (EPOC) checklist, not 'fairly equivalent to randomized controlled trials') and taking into account novel vector control tools. Such studies have been initiated and it is hoped that funding agencies will increasingly be aware of the importance of implementation research.

The question of using a scoring system for assessing methodological quality in systematic reviews has no clear-cut answer, as such scores are to a certain extent arbitrary. For this reason, we presented the methodological details in