Reviewer 1: Based on email from Dag E. Helland dated 13 October 2011, my assignment was to undertake a peer review of the paper by Dempster et al. (2011) for ”Den nasjonale forskningsetiske komité for naturvitenskap og teknologi” (NENT).

General comments:
The paper by Dempster et al. (2011) deals with the effects of fish farms on wild gadoid fish. The main objective of the study is to evaluate if salmon farms along the Norwegian coast act as ecological traps, i.e. attract wild gadoid fish to ecologically inferior artificial habitats, or if they act as population sources by promoting better growth, condition, and eventually supporting higher total offspring survival of local wild gadoid fish. These investigations were carried out by sampling cod and saithe in the vicinity of salmon farms, and in local control sites considered to be far enough away from fish farms. The fish were sampled during the summer months by standardised hook and line fishing gear, and in total 570 saithe and 349 cod were sampled in three regions (Ryfylke, Hitra and Øksfjord). Of these, 526 fish were considered farm associated (FA) and 393 un associated (UA). The sampled fish were subject to standard measurements such as fork length and body weight, used to estimate Fulton condition index (FCI), and liver and gonad weight used to calculate hepatosomatic index (HSI) and gonadosomatic index (GSI). In addition the sex was determined in most cases as well as the occurrence of external and internal parasites. The FA and UA fish were contrasted for the various measures using Generalized linear models (GLM), and their stomach content (diet) were compared using a non-parametric multivariate MDS technique. The authors documented a generally higher condition of FA cod and saithe compared to the UA control fish as well as an uptake of fish feed in FA fish. There were in addition some differences in parasitic load, although these were not as consistent across sites and groups. The authors concluded that there was no evidence that salmon farms act as ecological traps, but rather had a potential as population sources due to documented elevated fish condition of FA fish and presumed higher reproductive output. A major premise of the paper is that fish condition is a proxy measure of fitness since under normal circumstances, fish fecundity is positively related to condition.

The assumption that fitness relates to condition (or “fatness”) as in this study, is somewhat troublesome. Although the authors undoubtedly are right in suggesting that wild fish with higher condition typically have higher fecundity than other wild fish with lower condition (everything else being equal), and as such should produce more eggs than those with lower condition, the linking of wild fish condition with semi-cultivated fish condition to fitness is in doubt for several reasons. First it implies that the survival potential of the offspring for both groups of fish should be similar. This is not at all documented by the collected data nor by references presented in the paper. The authors could have used examples from the literature on salmonids to illustrate the differences in fitness related measures between (fatter) farmed salmon (including escapees), and (leaner) wild salmon. In this case, it is doubtful that they would have obtained much support for their assumptions. Secondly, it implies that the reproductive behaviour of the semi-cultured fish will be as adapted as the wild counterparts, and that they indeed will be maturing at a suitable time period, undertaking necessary spawning migrations and movements, and displaying appropriate courtship behaviour. Cod for example display an intricate spawning behaviour involving energetically intense paired swimming bouts. It is not at all evident that cod with higher levels of liver lipids will have more success in these behaviours. Although the authors do mention some of the assumptions relating to offspring survival explicitly in their discussion (p. 6 in Discussion), a stricter editorial practice on the title and abstract should have been in place.

One other problematic issue with the paper is the choice of sampling method. By using hook and line, the authors have to some extent precluded the documentation of differences in disease related status between FA and UA groups. This because one of the first behavioural changes during the development of a disease will be a reduction, or even a complete loss of appetite. Hook and line sampling methods are typically targeting actively feeding fish and without any documentation of the fraction of non-feeding fish in both groups, this will provide an incomplete picture of the health status in FA and UA groups. This limitation of the study is not properly acknowledged by the authors.

Other specific comments:
- The section on parasite sampling in the Materials and methods is limited to only two references. More references could have been included here, also some that refer to the risk of disease transfer from fish in densely populated farm pens to wild fish in the immediate surroundings. This is mentioned on p. 7 in the Discussion, however, but it is not stated why this was not followed up since this obviously has the potential to negatively affect the FA fish.
- The analysis of parasitic load is carried out using GLM analysis after log transformation. It is unclear why the authors did not use a poisson-type error distribution (suitable for count data) and thus avoid the transformations (and some underlying assumptions) altogether. Many of the tests regarding parasitic load where marginally significant, and the use of inappropriate distributions and assumptions in these analysis may have influenced the outcome of the tests.
- Further, by carrying out repeated tests in a site specific manner, the authors have missed out on some of the advantages of an ANOVA-type analysis (e.g. documentation of site and group specific interactions), and may as well have inflated the occurrence of type I errors. This will lead to more significant test results than what is really supported by the data, and could easily have been avoided by implementing a Bonferroni correction (in this case the critical p-value for each variable tested should be 0.05/3).

Conclusion:
Dempster et al. have documented a significantly higher condition among farm associated (FA) cod and saithe compared to farm un-associated (UA) cod and saithe. This was in line with their hypotheses as outlined at the end of the Introduction. A rather uncritical use of fish condition as a proxy of fitness, led them to conclude in the title and abstract that fish farms act as population sources rather than ecological traps. It would have been appropriate for the authors, as well as the reviewers and journal editors, to have sought a more modest wording in the title and paper itself. It is much less likely however, that this paper represents a serious breach of misconduct of proper scientific practice, but rather an over-interpretation of the obtained results. The basic underlying data are available for the readers to judge by themselves, and the appropriate arena for the a continued discussion of the matter should therefore be in the scientific literature and not, in my opinion, in a national science ethics committee. **I therefore conclude that the paper by Dempster et al. in spite of its examples of over-interpretation, and inadequacies, does not represent a serious breach of acceptable scientific practice.**

References:
Dempster, T., P. Sanchez-Jerez, D. Fernandez-Jover, J. Bayle-Sempere, R. Nilsen, P. A. Bjorn, and I. Uglem. 2011. Proxy Measures of Fitness Suggest Coastal Fish Farms Can Act as Population Sources and Not Ecological Traps for Wild Gadoid Fish. PloS one **6**. DOI 10.1371/journal.pone.0015646