Field evidence of significant effects of radiation on wildlife at chronic low dose rates is weak and often misleading. A comment on "Is non-human species radiosensitivity in the lab a good indicator of that in the field? Making the comparison more robust" by Beaugelin-Seiller et al.

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The intention of the paper by Beaugelin-Seiller and co-workers (Beaugelin-Seiller et al., 2018) is to make the comparison of wildlife radiosensitivity in the field with that in the laboratory more robust. The paper aims to evaluate the hypothesis that animals in the natural environment are much more radiosensitive than those in laboratory settings. It draws heavily on a previous paper by Garnier-Laplace et al. (2013) and supports the hypothesis of these authors that organisms in the field are much more radiosensitive than those studied under laboratory conditions. This earlier study has been well cited in the scientific literature (with currently 96 Google Scholar citations) and both of these papers are providing data and methodological approaches to the work of the International Commission on Radiological Protection (ICRP). I believe, however, that both the Garnier-Laplace et al. (Garnier-Laplace et al., 2013) and the current paper (Beaugelin-Seiller et al., 2018) are based on limited and often flawed available field data and a flawed methodological approach. In both papers, the authors note the high uncertainties of the approach and limitations of the data they use. However, I believe that these problems are so severe that they are leading to the wrong conclusions being drawn on the important issue of radiation effects on ecosystems. The purpose of this letter is to highlight the deficiencies in much of the key field data used and in the approach taken in these studies.

Scepticism about findings of major radiation effects on organisms at Chernobyl and Fukushima is often misinterpreted as somehow suggesting that there are no radiation effects at all. So it needs first to be stated clearly that there is little doubt that chronic radiation in the most contaminated parts of the Chernobyl Exclusion Zone (CEZ) and at Fukushima is likely to be causing some radiation effects (Baker et al., 2001; Baker et al., 2017; Lerebours et al., 2018). The “hot spots” at Chernobyl, comprising perhaps a few percent (at most) of the surface area of the CEZ, can give rise to dose rates to organisms > 40 μGy h^{-1}. It is accepted by most scientists in the radiation protection community that radiation potentially damages DNA at all dose rates without a lower threshold. The key question is: at what dose and dose rate does significant damage to wildlife populations occur ?

Remarkable claims

In their important article (Chesser and Baker, 2006) summarising their long experience of radiation effects research at Chernobyl, Profs. Ron Chesser and Robert Baker of Texas Tech. University present a number of key lessons for the radioecological community, one of which is that “*Incredible results require incredible evidence*” in relation to claims of major radiation damage to barn swallows (at relatively low chronic dose rates) by the team of Prof. Anders P. Møller of Université Paris-Sud and Prof. Tim Mousseau of the University of South Carolina.

It is perhaps not clear to the casual reader how remarkable are the claims being made in this paper (Beaugelin-Seiller et al., 2018) and the papers on which its key conclusions are based (Møller et al., 2015; Møller and Mousseau, 2007). For example, based on re-analysis of data collected from the area around Fukushima (Møller et al., 2015), the Beaugelin-Seiller et al. (2018) study claims “*the total dose which would have led to a reduction of 50% of the total number of birds (the so-called ED50) in the study area in the same 4-yr period has been estimated at 0.55 Gy*” which, given the “exposure durations of birds in this study (from 295 to 1391 days)” gives a dose rate range of approximately 16-77 μGy h^{-1}. So, chronic dose rates in the range 16-77 μGy h^{-1} have apparently led to a 50% reduction in bird populations around the Fukushima Daiichi nuclear power plant. Further, the authors claim “*the total number of* *individuals would have been reduced by 26% with every change of one order of magnitude in total dose (in Gy)*". So, an increase in cumulative dose from about 3200 µGy to 32,000 µGy or 3.2-32 mGy (see Fig. 4 of (Beaugelin-Seiller et al., 2018)) and dose rate from between 0.1-0.45 to between 1-4.5 µGy h^{-1} is hypothesised to lead to a 26% reduction in bird abundance. If this is correct, it is indeed a remarkable result which would seriously affect the recovery of contaminated lands. Few people would want to live in an area in which birds are, apparently, dying or failing to reproduce as a direct or indirect effect of radiation (the effect of radiation was, apparently, found in both evacuated and non-evacuated areas (Beaugelin-Seiller et al., 2018)).

It should be noted that current ICRP recommendations allow dose rates to humans of this order or higher: the current occupational effective dose limit is 20,000 µSv y^{-1} averaged over 5 years with a maximum of 50,000 µSv allowable in any one year (ICRP, 2007). The 50,000 µSv y^{-1} level translates to an average of 31.25 µSv h^{-1} (approximately equivalent to µGy h^{-1} for low LET radiations) for a 1600 hour working year, obviously allowing much higher dose rates for shorter periods of time. Further the ICRP (ICRP, 2007) concludes that

"in the absorbed dose range up to around 100 mGy [100,000 µGy] *(low LET or high LET) no tissues are judged to express clinically relevant functional impairment. This judgement applies to* ***both single acute doses and to situations where these low doses are experienced in a protracted form*** [my emphasis] *as repeated annual exposures."*

Human workers are only exposed for part of their time, and cumulative dose rates are lower. Given, however, that significant radiation effects on wildlife populations are hypothesised to be deterministic, the comparison of dose rates is relevant. The findings of the study above, and the very low Predicted No Effect Dose Rate for vertebrates of 2 µGy h^{-1} quoted in (Beaugelin-Seiller et al., 2018) is in direct contradiction to ICRP recommendations for radiological protection of humans. The contradiction is further emphasised by the fact that the human system of radiological protection aims to protect the individual: the system for the protection of the environment only aims to protect wildlife populations. Though it would be a mistake to assume that the current ICRP recommendations for human radiation protection are infallible, the contradiction illustrates how remarkable are the claims being made by some studies of wildlife at Chernobyl and Fukushima.

Weak and misleading evidence

There appears to be a high level of quality control over the studies used in these assessments (Beaugelin-Seiller et al., 2018; Garnier-Laplace et al., 2013):

"*All the publications that we evaluated were subjected to a grading system based on dosimetry, experimental design, and statistical details (similar to what was done in the PROTECT project; Garnier-Laplace et al., 2010). The quality criteria analysis permitted a scoring of each individual paper, with 80 as the maximum value. Only data sets from papers with total scores greater than 35 were used in our subsequent analyses. A score of >35 corresponds to A, B or C category score in FREDERICA, as defined in Copplestone et al. (2008)*." (Garnier-Laplace et al., 2013)

This appears on the surface to be a high level of quality control, but a careful reading and critical analysis of the papers themselves suggests that **it is in no way sufficient**. Careful reading of the studies used in the Garnier-Laplace (2013) paper shows that at least seven out of a total of eleven data sets used in this meta-analysis either clearly should have been rejected for use in the study, or are highly suspect (I haven't checked the remaining four). Figure 1 reproduces the figure presented in (Beaugelin-Seiller et al., 2018; Garnier-Laplace et al., 2013) showing the comparison of field (CEZ) and laboratory studies.

The Jackson et al. (2005) study (providing three of the eleven data sets for the Garnier-Laplace (2013) SSD paper) clearly shows that these three data sets should not have been used. As noted by Jackson et al. (2005) themselves, theirs was a "preliminary" study from which it is not possible to draw conclusions on chronic dose effects at Chernobyl:

"*although the highest number of individual organisms was recorded in the low contamination site (Paryshev) this coincided with the lowest overall biomass" and "it seems reasonable to conclude that acute exposure to high levels of radiation may have denuded invertebrate populations immediately after the Chernobyl accident. Subsequently, recolonisation has been slower in regions subject to continuing high levels of soil contamination with ^{90}Sr and ^{137}Cs. In part, this may be linked to habitat changes (e.g. loss of tree canopy cover in areas of more extensive early die-back). Nonetheless, some niche expansion by remaining invertebrate populations appears to have occurred as there is little evidence for any overall loss of biomass when comparing high contaminated sites with relatively low contaminated sites*" (Jackson et al. 2005)

Thus the paper concludes that changes are likely to be due to initial high dose rates shortly after the accident and presents **no evidence whatsoever** on effects of later much lower dose rate chronic radiation on invertebrate populations. I also note that one (apparent) positive effect of radiation (higher invertebrate biomass) was wrongly included as a negative effect in the Garnier-Laplace (2013) paper. This lack of evidence for effects is supported by more recent studies of invertebrate activity in Chernobyl contaminated soils which show little evidence of impacts even at very high dose rates (and in an area previously severely damaged by extreme dose rates shortly after the accident) (Lecomte-Pradines et al., 2014). The difficulty of distinguishing between long term effects of chronic dose rates and effects of habitat changes due to initial radiation damage from high dose rates shortly after the accident is a general problem in field studies (Beresford et al., this Special Issue).

The study of fertility of laboratory mice exposed in cages in the CEZ (Pomerantseva et al., 1990) also should not have been used in the SSD (Fig. 1) for the obvious reason that they were laboratory mice fed regularly and not subject to predation pressure. The hypothesis that wild animals are more vulnerable to radiation than animals in the laboratory is not tested using these (otherwise very interesting) data. Three study sites were used at dose rates to the testes of 166, 5,000 and 42,000 μGy h^{-1} (cumulative doses of 0.1, 3 and 25 Gy over the 25 day study period). A further problem with using these data is that it is not possible to determine an unambiguous dose response curve given that there are few sites, little replication and very wide dose rate differences between sites. In addition, the lowest dose rate value (166 μGy h^{-1}) showed no significant difference from control. The error bars shown in Figure 1 are likely to be a significant underestimate for the uncertainty in this data point.

Four of the eleven datasets supporting the conclusions of the Garnier-Laplace (2013) paper should obviously not have been used. Other key studies used in the field SSD (Fig. 1) show a huge contradiction, which was not properly considered in the Garnier-Laplace (2013) and the present paper (Beaugelin-Seiller et al., 2018): studies of forest birds (Møller and Mousseau, 2007) and invertebrates (Møller and Mousseau, 2009) apparently show significant population-level effects at dose rates much lower than other organisms show individual-level effects (Fig 1.).

The paper by Garnier Laplace et al. (2013) rejected use of the study of invertebrates by (Møller and Mousseau, 2009), stating that "*sampling strategies and confounding factors are more likely explanations for the "apparent" drastic decrease of the species abundance and numbers of individuals reported by the authors at incredibly low dose rates. Therefore, those data from aboveground invertebrates were considered as outliers* [see Fig. 1] *and were not used to interpret the comparison of the range of variation of radiosensitivity of terrestrial species between controlled exposure conditions and real field situations*."

This paper is rejected because, in the light of what we know about the biological effects of radiation on invertebrates, its conclusion (of dramatic population declines at dose rates which are within the range of natural background radiation) makes no sense. It is very reasonable to reject the paper on this basis, but this leaves key questions unanswered:

1. The study observed apparently highly statistically significant negative effects on five separate species whilst (according to the authors) having "controlled for confounding environmental variables". How, then, did the authors get this remarkable result ? Is it a huge statistical coincidence ? Or is there (as Garnier Laplace et al. 2013 speculate) some unknown confounding variable unrelated to radiation which reduces insect abundance at relatively higher dose rate sites ?

2. If there is some confounding variable (unrelated to radiation) which reduces insect abundance at relatively higher dose rate sites, surely this reduced insect abundance and/or the unknown confounding variable would also impact on populations of other animals, particularly birds. This unknown factor (not causally linked to radiation) would therefore invalidate apparent findings of dose response relationships between bird abundance and radiation level (Møller and Mousseau, 2007).

3. If there is no unknown confounding factor but instead data behind this finding is flawed in some systematic way in which abundance is found lower at higher radiation dose rate sites, how did such a systematic bias appear in the data on **five** different species ? All data were collected by A.P. Møller.

It makes no logical sense to, on the one hand reject the Møller and Mousseau insect paper (Møller and Mousseau, 2009) but, on the other, accept the bird abundance paper (Møller and Mousseau, 2007). Beaugelin-Seiller et al. (2018) and Garnier-Laplace et al. (2013) must either accept both (given that both of the original studies claim to have measured confounding variables) and use them in their analysis, or reject both.

I think there is good reason for the radioecological and radiation protection communities to **treat the remarkable claims by Anders Pape Møller and Tim Mousseau as highly suspect** until their hypotheses are thoroughly tested by independent research. In Table 1 I have summarised six remarkable claims by these authors and their collaborators. For each one I have shown that there is very significant counter-evidence against these claims. Although this is just a small part of the huge publication output of these authors, I think it constitutes sufficient evidence to be **highly suspicious of the remarkable claims they make** on the ecosystem impacts of low-level radiation.

It is also important for the radioecological and radiation protection communities to note that **a number of eminent biologists do not cite work by A.P. Møller** (Prof. Chris Thomas, University of York, pers. comm.; Prof. Richard Palmer, University of Alberta, pers. comm.) following the ruling against him by the Danish Committee on Scientific Dishonesty, as recorded in a news article in *Nature* (Vol. 427, p 381, 2004). Prof. Richard Palmer (University of Alberta, pers. comm.) has stated "I had the impression that it was more interesting to him [A.P. Møller] to get the paper published than to be correct". Prof. Andrew

Pomiankowski of University College London, a former collaborator of A.P. Møller has stated (pers. comm.) that "I never cite and stopped reading any research papers produced by Anders P. Møller some years ago. I simply don't trust the research he does. In my eyes he failed to adequately address the criticisms levelled against his research".

This does not definitively prove that A.P. Møller's work on radiation effects cannot be relied on, but I believe that this evidence, together with the evidence presented in Table 1, must make us highly sceptical of the remarkable claims by this group of researchers.

Laboratory studies are likely to be more sensitive than field studies

It is necessary to state that laboratory studies are better able to control for confounding factors than field studies. It may (or may not) be true that animals in the field are much more sensitive to radiation than those in the lab. But what is certain is that the many confounding factors in the natural environment make it very difficult to detect (likely subtle except at extremely high dose rates) radiation effects in the field. Field irradiator experiments were conducted from the 1960's -1980's giving much valuable information (e.g. (Mihok, 2004)), though this approach may not be feasible in the present day. Clearly, both field and laboratory studies can be valuable. Since causal relationships are so difficult to establish in complex natural environments, however, claims of causal effects of radiation need to be supported by additional independent field studies and hypothesis testing in the laboratory: this is very often lacking in radioecological studies.

Problems with the methodological approach

Aside from the problems with data discussed above, there are a number of other important problems with the Species Sensitivity Distribution (SSD) approach as it is used in this context:

(1) The comparison of species sensitivity must be based on some consistency of environmental endpoints between species; from the previous Garnier-Laplace (2013) paper it is clear that a wide variety of endpoints were used, ranging from individual to population level;

(2) As the authors note (Beaugelin-Seiller et al., 2018), studies reporting no effects are ignored; nine papers are cited in Table 1, all presenting counter-evidence to studies apparently finding effects: none of these negative findings can be included in a SSD;

(3) The calculation of EDR10 value (the dose rate at which an individual species would suffer 10% effects) has to assume a particular shape of the dose-response curve **which is usually not at all supported by the data and analysis presented**. At low dose rates, the shape of the dose response curve is likely to be impossible to determine in the field studies cited by Beaugelin-Seiller et al. (2018) and Garnier-Laplace et al. (2013).

Maybe the remarkable claims are right ?

Scientific knowledge is always provisional (Popper, 2014) and hypothesis testing in complex ecosystems is difficult (Peters and Peters, 1991; Smith, 2000). As detailed in Table 1, a number of studies have provided important counter-evidence to the apparent findings of very large radiation effects at very low dose rates. However, it should be acknowledged that there has been no systematic and large scale independent study of bird populations at Chernobyl or Fukushima which can adequately test the hypothesis of Møller, Mousseau and their collaborators. It is possible (though I think very unlikely) that there is some mechanism by which birds are much more radiosensitive than other species. Independent studies on birds would be very valuable, though these need to acknowledge the limits on statistical power of all such studies in a hugely variable natural environment (Beresford et al. this Special Issue).

What if the remarkable claims are wrong ?

It could be argued (using the Precautionary Principle) that the potential over-estimation of radiation risk to the environment does little harm, since it encourages us to be highly cautious in use of radiation which is, after all, a known genotoxin and carcinogen. But over-estimation of radiation risk can also be damaging. The public and political debate over the environmental costs and benefits of nuclear power clearly needs to be based on the best available scientific evidence. Perhaps more importantly, there are hundreds of thousands of people at Chernobyl and Fukushima currently living with chronic, very low level anthropogenic radiation. The wholly understandable but (to the vast majority of the radiation protection community) unfounded fear of significant radiation health effects has caused major economic, social and psychological damage to communities living in Chernobyl affected areas (UNDP and UN-OCHA, 2002). Apparent findings of major radiation effects on animal populations at very low dose rates have a large media and public impact. If these are wrong (and I think they are), they severely hinder the very difficult process of recovery of the communities affected by the Chernobyl and Fukushima accidents and, I believe, **do real damage to people's health and wellbeing**.

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Figure Captions

Figure 1. Reproduction of the comparison of field and laboratory studies using a Species Sensitivity Distribution SSD (Beaugelin-Seiller et al., 2018; Garnier-Laplace et al., 2013) with my comments in boxes. The outlier data on invertebrates (Møller and Mousseau, 2009) was rejected for use in the SSD.

List of Tables

Table 1 Hypotheses by the group of Prof. A.P. Møller and Prof. T. Mousseau and counter-evidence.

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Hypothesis by Anders Pape Møller, Tim Mousseau and collaborators	**Counter-evidence**
Apparently highly significant reduction in abundance of five invertebrate species at radiation dose rates (for EDR_{10}) in the range 2.9×10^{-2} to 3.4×10^{-2} µGy h^{-1} (Møller and Mousseau, 2009).	Simply not a plausible causal effect of radiation given understanding of biological effects of low dose radiation. Natural background terrestrial and cosmic (weighted) dose rate to a "reference" bee in the UK is in the range 11-140 $\times 10^{-2}$ µGy h^{-1} (Beresford et al., 2008). Our studies on invertebrates in aquatic systems at Chernobyl have not observed significant population level (Murphy et al., 2011) or individual (Fuller et al., 2018; Fuller et al., 2017) effects at dose rates up to about 30 µGy h^{-1}.
Apparently highly significant reduction in mammal abundance at radiation dose rates in the range about 0.1-200 µGy h^{-1} external dose rate (Møller and Mousseau, 2013).	Inadequate sampling methods: survey tracks were too short and too close together given the home range of many of the species studied. Actually found a very high mammal abundance in the CEZ: observations of wolves, for example, was reported to be 44 tracks/10km compared to 13 tracks/10 km reported in the much larger (Deryabina et al., 2015) study. Hypothesis not supported by Deryabina et al. and other studies of mammals in the CEZ (Baker et al., 1996; Webster et al., 2016), though only the small mammal studies (Baker et al., 1996) tested for effects at very high dose rates in small "hot spots". Field irradiator experiments (Mihok, 2004) in Canada found no significant effect on vole populations at dose rates of approximately 1800 µGy h^{-1}.
Approximately 70% of voles at Chernobyl have cataracts at cumulative dose rates from around 20 µSv to 80,000 µSv (1 µSv approximately equals 1 µGy for low LET radiations) (Lehmann et al., 2016).	Samples were not properly preserved likely leading to a huge overestimation of cataract incidence (Smith et al., 2016 comment on (Lehmann et al., 2016)). No dose response in male voles and only a weak response in females (over a range in cumulative dose from 20-80,000 µGy) is not plausible. Induction of large numbers of cataracts in voles at cumulative dose rates of < 1000 µGy (< 1 mGy) is not plausible.
Major reduction in leaf litter decomposition by soil-dwelling invertebrates in a dose range between 0.09 and 240 µGy h^{-1} (Mousseau et al., 2014).	As observed by Bonzom and coworkers (Bonzom et al., 2016), the leaf litter decomposition rate **at the highest dose rate sites** of Mousseau et al. (Mousseau et al., 2014) was "at rates comparable or higher to what is reported in the literature for the same or similar tree species at sites without any radioactive contamination". No significant effects found (at lower dose rates in range 0.22 to 29 µGy h^{-1}) in a leaf litter decomposition study (Bonzom et al., 2016); no significant effects of chronic doses in range 0.7 - 220 µGy h^{-1} on soil nematode assemblages at Chernobyl (Lecomte-Pradines et al., 2014).
Reduced reproduction of barn swallows: 40-60% of barn swallows were non-breeding at dose rates between approx. 5 and 60 µGy h^{-1} compared to < 20% at "control" and low-dose rate sites (Møller et al., 2005).	Very likely confounded by absence of human population at Chernobyl (Smith, 2008): sites at high dose rate areas are abandoned; those at low dose rate and "control" sites are not. Møller and coworkers very clearly and seriously mislead the reader on the crucial question of whether key sites were abandoned or not ((Smith, 2008), Supplementary Information). At 6 µGy h^{-1} ((Møller et al., 2005) observed 60% on barn swallows were non-breeding at approx.. this dose rate), a field irradiator experiment found no effect of radiation on tree swallow breeding performance or nestling growth rate (Zach et al., 1993).
"Elevated frequency of abnormalities in barn swallows at Chernobyl" at dose rates up to approx. 60 µGy h^{-1} (Møller et al., 2007).	Very likely confounded by absence of human population at Chernobyl (Smith, 2008): sites at high dose rate areas are abandoned; those at low dose rate and "control" sites are not. Previous work by A.P. Møller himself (Møller, 2001) has shown the cessation of farming practice (dairy farming) to significantly negatively influence barn swallow abundance, reproduction and nestling quality.

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