Our analyses and findings in more detail

Supplementary file with details of our findings presented in the paper

Nordhagen E and Flydal E. 2024. *WHO constructs its neglect of RF-EMF exposure hazards on flawed EHC reviews: A case study demonstrating how a "no hazards" conclusion is drawn based on underlying data showing hazards*

Table of Contents

Introductory comment

This supplementary material describes in detail our findings of flaws in the following two documents commissioned by the World Health Organization (WHO) in an initiative to assess the evidence of associations between (human made) radiofrequency electromagnetic radiation (RF-EMF) and adverse health effects in the general and working population. This initiative will publish a series of reports in "the Environmental Health Criteria (EHC) series" of which we analyzed the following two, the latter describing the protocol for the former, a systematic review:

> Cordelli, E., Ardoino, L, Benassi, B., Consales, C., Eleuteri, P, Marino, C., Sciortino, M., Villani, P., Brinkworth, M.H., Chen, G., McNamee, J.P., Wood, A.W., Belackova, L., Verbeek, J., Pacchierotti, F., *Effects of Radiofrequency Electromagnetic Field (RF-EMF) exposure on pregnancy and birth outcomes: A systematic review of experimental studies on non-human mammals, Environment International*, 2023, 108178, ISSN 0160-4120, https://doi.org/10.1016/j.envint.2023.108178
>
> Pacchierotti, F., Ardoino, L., Benassi, B., Consales, C., Cordelli, E., Eleuteri, P., Marino, C., Sciortino, M., Brinkworth, M.H., Chen, G., McNamee, J.P., Wood, A.W., Hooijmans, C.R., de Vries, R.B.M., *Effects of Radiofrequency Electromagnetic Field (RF-EMF) exposure on male fertility and pregnancy and birth outcomes: Protocols for a systematic review of experimental studies in non-human mammals and in human sperm exposed in vitro*, Environment International, Volume 157, 2021, 106806, ISSN 0160-4120, https://doi.org/10.1016/j.envint.2021.106806

The former will in this file be denoted EHC2023 and the latter "the protocol".

To precisely describe our findings, the following terms will be used:

- *Paper*: a published paper which reports results for one or more control groups and one or more exposure groups of animals.
- *Experiment*: Each exposure group reported in a paper is denoted "an experiment". They may, or may not, share a common control group.
- *Study*: an entry into a pooled effect size calculation in EHC2023 (presented in forest plot figures), usually consisting of one exposure group compared with one control group, or the average from several exposure groups compared to a single, shared control group.
- *Meta-study:* the analysis of the "Low or Some Concern" RoB rated studies selected for an endpoint in line with the use of this term in EHC2023.
- *Thermal effects:* biological effects from RF-EMF exposure caused by thermal heating of tissue.
- *Thermal studies:* include experiments looking for thermal effects.
- *Nonthermal effects:* effects from RF-EMF exposure not causing thermal heating in tissue.
- *Nonthermal studies/experiments*: are studies/experiments of effects not caused by thermal heating.
- *Thermal exposure:* "High SAR" exposures with intensities assumed to cause tissue heating in the exposed animals.
- *Subthermal exposure:* "Low SAR" exposure of RF-EMF which is below the threshold assumed to cause tissue heating.

Our analysis is particularly detailed as to the analysis of the fetal weight endpoint presented in Fig. 6 of EHC2023. References to the 44 papers referenced in the fetal weight endpoint analysis are in the

following written without brackets or space, i.e., just name and year: e.g., Sharma2017, and listed under “Papers referenced in the EHC2023 fetal weight endpoint analysis” at the end of this document.

A: More than half of the data used for the fetal weight meta-analysis are thermal studies, and thus irrelevant for humans in daily life and work conditions

Among the 44 papers accepted by the EHC2023 and listed in the forest plot of the meta-study for the "fetal weight" endpoint (EHC2023, Fig. 6), we found 17 papers containing thermal studies only. These studies were published in the early period 1977 – 1991. In addition, there were two papers reporting experiments with SAR both above and below the thermal threshold. These two were published in 1978 and 2009 (Ogawa 2009, co-authored with ICNIRP-member Soichi Watanabe and finding no significant change in fetal weight on average). Thus, 19 papers reporting from *thermal* studies are used for the "fetal weight" endpoint meta-study.

EHC2023 ranks all but one of these 19 papers as of "Low or Some Concern" (LSC). They are thereby accepted by EHC2023 for its meta-analysis for the endpoint "fetal weight", although they report on experiments with SAR above 5 W/kg, i.e., thermal exposure levels prohibitively high for any RF-EMF exposures of humans in daily or occupational life, and so by any RF radiation protection standard anywhere in the world since the first rules of thumb were set in the 1950-ies.

These 19 "Low or Some Concern" (LSC) papers containing 33 *thermal* experiments are listed in Table 1 below, with SAR values, temperature increase, and exposure length specified as presented in Fig. 3 of EHC2023.

Table 1 List of the 19 papers, containing 33 thermal experiments with SAR values ranging from around 2 to 31, but mostly "high SAR" – and rated by EHC2023 as "Low or Some Concern" (LSC) as to Risk of Bias and included in for the fetus weight meta-analysis.

1. Berman1978, describing 3 thermal experiments with SAR = 7; 8.1 and 22.2, and 1 relevant experiment with SAR = 2 W/kg.
2. Berman 1981, describing 1 experiment reporting a 2 °C temperature increase in the pregnant animals.
3. Berman1982a, describing 1 thermal experiment with SAR = 16.5 W/kg.
4. Berman1982b, describing 2 thermal studies with SAR = 6 and 9 W/kg.
5. Berman1984a, describing 1 thermal experiment with a 2 °C temperature increase in the pregnant animals.
6. Berman1984b, describing 1 experiment with SAR = 16.5 W/kg.
7. Berman1992, describing 2 experiments, where 1 was assumed to be thermal. The temperature was not measured, but the highest exposure level was considered to likely be hyperthermal.
8. Chernovetz1977, describing 1 experiment with SAR = 31 W/kg.
9. Jensh1984a, describing 1 experiment with SAR = 7.28 W/kg.
10. Jensh1984b, describing 1 experiment with SAR = 7,28 W/kg.
11. Lary1982, describing 3 experiments with SAR = 11 – 112.5 W/kg.
12. Lary1983b, describing 4 experiments with SAR = 10.8 W/kg, and single dose exposures of varying length, with 2.9 to 3.9 °C temperature increases.
13. Lary1986, describing 4 experiments with SAR = 10.8 W/kg and single dose exposures of varying length, with 2.6; 3; 3.6; 4 and 4.5 °C temperature increases.
14. Mercickiewicz1986, describing 1 experiment with SAR = 17 W/kg and 1 nonthermal experiment with SAR = 4.5 W/kg.
15. Newrot1985, describing 2 experiments with SAR = 20.2 W/kg.
16. Newrot1981, describing 5 experiments with SAR = 6.7 and above.
17. Nelson1991, describing 30-minute single exposures with varying SAR and a 3.7 °C temperature increase.
18. Ogawa 2009, describing 1 experiment with exposures of pregnant females' brains with peak

SAR of 7.0 W/kg, and 1 experiment with SAR = 3.1 W/kg.

19. Brown-Woodman1988a is the only thermal paper given RoB rating "High Concern" by EHC2023 and therefore not part of the meta-analysis. This paper reports from 6 experiments with SAR = 11.2 W/kg, and 2.5 – 5 °C temperature increases.

Contained in the 44 papers used for the EHC2023 fetal weight analysis, there are in total 61 experiments considered "Low or Some Concern" (LSC) contributing data used for the EHC2023 meta-analysis. This means that 54% of the experiments constituting the data base for this EHC2023 meta-analysis are *thermal*, and therefore not relevant. We found a similar ratio for all the studies included the review, i.e. for all the meta-studies for all endpoints as a whole (183 studies, where 98 are thermal).

B: Equations impacting all meta-analyses are neither documented nor discussed. Choice of equation can skew the results in any direction

When calculating the pooled effect size for a meta-study, each included study is assigned a relative weight which determines its impact on the resulting pooled effect size.

The general purpose of weighting studies when pooling results, is to describe findings in a more compact way while assuring each of the respective studies an impact in line with its importance. This may be achieved along different paths, more or less complex and intuitively understandable, yielding results of significant difference. While qualitative approaches may simply assess the studies' relative importance and sum up the findings, in statistics, equations for weighting are used to handle larger data sets and to assure a more objective approach.

However, small adjustments to equations used for weighting might have considerable impacts on the relative weights, and thus on the overall results. Choosing equations is therefore no "objective" or neutral exercise, but implies making a choice as to which properties and results from the studies to lift forward, and thereby which overall picture of findings to convey.

In EHC2023, relative weights attributed to the studies selected for the meta-analysis are reported as percentages (%) in forest plots. However, we have found no documentation as to how weights are attributed or calculated, neither in the paper itself, nor in its precursory protocol paper (Pacchierotti 2021), nor in any supplementary files.

Due to this lack of documentation decisive for the outcome of the study, we tested several standard methods of calculating weights to identify the equation used. The Cochrane Handbook (Higgins et al. 2022) – referred to by EHC2023 as to methods for combining studies sharing a common comparator – recommends using weight = $1/\sqrt{SE_d}$, where SE_d is the *standard error* for the study. We therefore tested this approach first, and found when comparing, that small standard error values gave lower weights to the studies than the weights listed in Fig. 6, and high standard error values gave higher weights than the ones listed in Fig. 6.

In other words, the weights attributed to the studies in EHC2023 Fig. 6 are more similar than the weights from using the equation weight = $1/\sqrt{SE_d}$. Hence, the Cochrane recommendation of using weight = $1/\sqrt{SE_d}$ seems not followed.

In the hope of finding the right equation, we gathered alternative weighting equations from various standard literature. Among the alternatives for weighting, we found basing the weighting on the *number of participants* in the study, *in casu* the number of animals. When using this approach, we got extremely different results from those in Fig. 6, with a total domination of old, irrelevant thermal studies using more than hundred mice, while newer studies, typically using 6 rats, had in practice no influence on the overall pooled result. If such an approach had been used in EHC2023, the newer and relevant studies could just as well be left out from the study, making the study clearly illegitimate.

Other suggestions from the literature are using the *variance* in the studies as the key for weighting. However, with such varied weights (from mice to rabbits) using the variance would not be feasible. The literature also mentions using *methodological quality* and *risk of bias* as other approaches for relative weighting, however pointing out that this method is more subjective and requires a well-defined and validated quality assessment method.

In fact, EHC2023 does present a method for quality assessment, used for its Risk of Bias assessment. As we have shown, that method and the resulting assessment are highly biased, attributing high

scores to old, irrelevant thermal studies and low scores to new, relevant ones. We do not see this bias reflected in the weights presented in Fig. 6.

In our experiments to identify what equations have been used for the weighting in EHC2023, we looked for an approximation for the equation actually used for the fetal weight analysis. We halted our search when we found a rough approximation (less than 3% divergence in weight for all papers in Fig. 6). Hence, this equation may tell which parameters have influenced the weighting:

$$weight\_i = \sqrt{\log(n_1 + n_2) / \sqrt{SE_d}}$$

where n_1 and n_2 are numbers of animals in the control group and in the exposure group(s) and SE_d is the Standard Error.

We also tested this for the brain weight calculations. In this case the following equation gave the best approximation:

$$weight = \sqrt{\sqrt{n_1 + n_{2_1}} / \sqrt{SE_d}}$$

We tested for two more endpoints, arriving at similar results. Even though we have not been able to identify one equation providing results identical with those of EHC2023, and no single approximation was found to be best in all cases, we can draw some conclusions about the EHC2023 weight equations: Our approximate equations all significantly reduce the weights attributed to studies with high numbers of animals, compared to using number of animals only. At the same time, our approximate equations give an increased impact to studies with large standard error and less impact to studies with small standard error compared with using SE_d by itself. The effect being that more uncertainty is added to the final result.

Overall, the EHC2023 relative weight equation "shrinks the distribution" in making all weights fall withing a quite small interval, and thereby gives quite similar weights to all studies while still giving slightly more weight to large studies and/or studies with a small standard error.

From this we infer that the weighting used in EHC2023 mainly takes the standard error (SE_d) values and number of animals into account, and does not consider e.g. quality or other characteristics of the papers. We also found that just a slight variation in the formula for weighting can have significant influence on the calculated pooled effect size. Thus, by careful selection of the weighting equation, one may skew the result in any direction wished.

Without a documented, well-defined and validated weight equation, the meta-analysis cannot be properly validated. We have actively searched all files and supplementary material provided for the EHC2023, without being able to find any information about the weighting method used.

The lacking documentation and discussion of the weighting applies to *all* endpoints in the EHC2023 meta-analysis, not only to "fetal weight". This leaves all calculated results of the EHC2023 meta-study difficult – if not impossible – to validate.

We have also tried to find an approximation for calculations of the confidence interval (CI) for the pooled effect size. This turned out to be an even more difficult task, as the four meta-studies we have investigated show quite different results. We therefore left this for further study. However, both in our discussions of whether a flaw skews the confidence interval in any specific direction, and when comparing ECH2023 results with the confidence intervals from our calculations, the relative sizes of the confidence intervals are consistent and can therefore be used.

Some of the meta-analyses in EHC2023 do not use Cohen's d but weighted averages or odds ratios. We have not investigated such calculations.

C: How the Risk of Bias assessment is biased

When assessing the risk of Bias analysis, we found a clear bias towards attributing older, thermal studies high scores, while newer, relevant studies are given low scores. Below we also show that this bias skews the findings in the direction of lower overall effect size and/or more uncertainty:

C.1: The Risk of Bias analysis is highly biased and several sorting criteria are inappropriate for relevant studies

Only studies classified as of "Low or Some Concern" (LSC) have been used in the EHC2023 meta-analysis. To identify such studies, and to discard the studies of "High Concern" (HC), the EHC2023 systematic review includes a comprehensive Risk of Bias (RoB) analysis:

Each of the 88 papers is attributed a score ("++", "+", "-" or "- -") on 10 different questions. Each plus score decreases, while each minus score increases the Risk of Bias score. This is repeated for each endpoint for which the paper is used. The scores are presented in several of the EHC2023 supplementary files.

The questions posed are:

1. Selection bias
- *RoBQ1.1 Was exposure level adequately randomized?*
- *RoBQ1.2 Was allocation to study groups adequately concealed?*

2. Performance bias
- *RoBQ2.1 Were experimental conditions identical across study groups?*
- *RoBQ2.2 Were the research personnel blinded to the study group during the experiment performance?*

3. Detection bias
- *RoBQ3.1 Can we be confident in the exposure characterization?*
- *RoBQ3.2 Has possible RF-EMF induced temperature increase been adequately considered and assessed?*
- *RoBQ3.3 Can we be confident in the outcome assessment?*

4. Attrition/Exclusion bias
- *RoBQ4.1 Were outcome data complete without attrition or exclusion from analysis?*

5. Selective reporting bias
- *RoBQ5.1 Were all measured outcomes reported?*

6. Other sources of bias
- *RoBQ6.1 Were there any other potential threats to internal validity?*

As EHC2023 is a meta-study of experiments, these questions relate to experimental situations, i.e., exposures and observations in laboratories under conditions where parameters are rigorously controlled, or at least attempted to be so. Hence, the issue arises whether these questions also favor specific types of experiments: Do they favor methodologies used when doing thermal studies, i.e., only testing the thermal causation hypotheses, over experiments testing how variations in other more complex properties of RF-EMF from wireless communications might have detrimental effects?

We tested whether the questions were biased by favoring papers containing thermal studies, while disfavoring relevant papers:

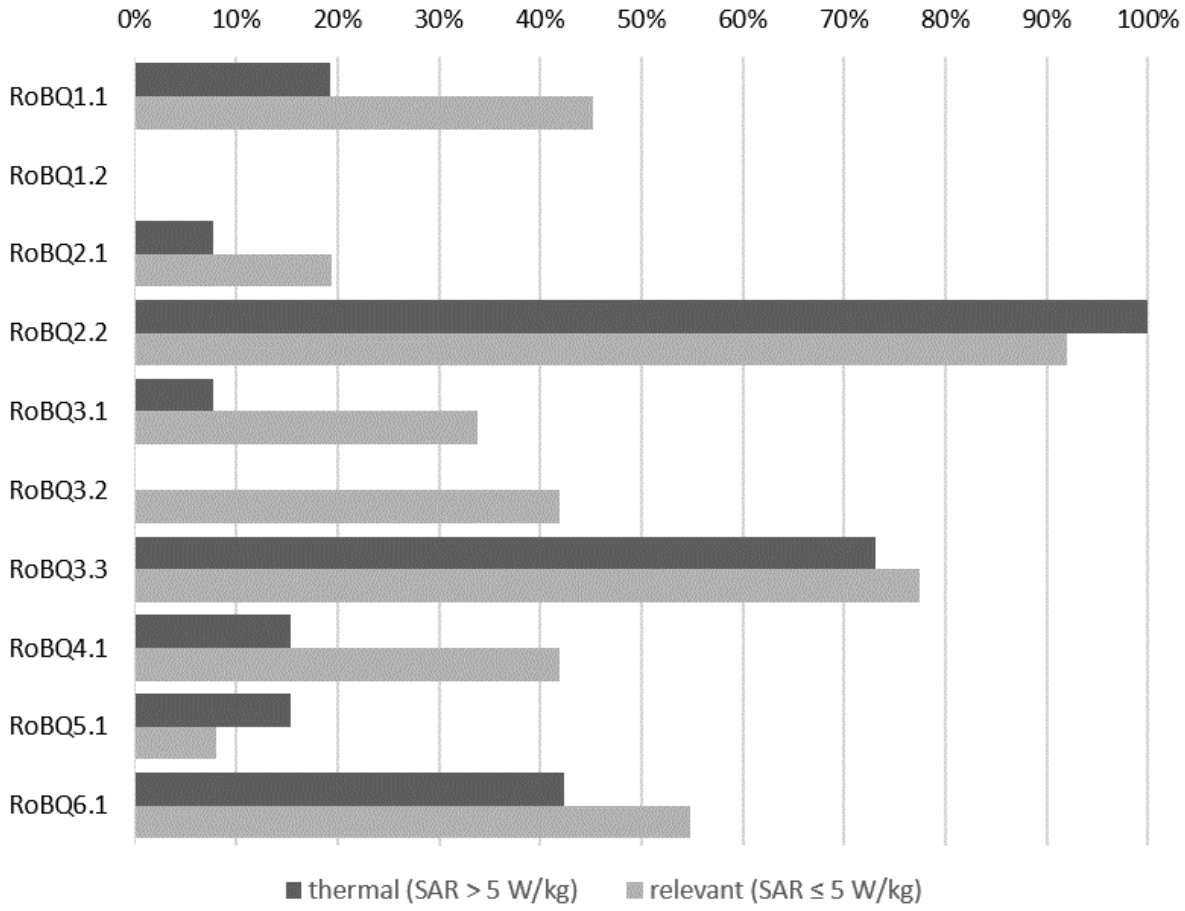

Figure 1 Percentages with negative score for the Risk of Bias questions, thermal (dark) and relevant (light) SAR papers separately.

Figure 1 visualizes the percentages of negative scores for each of the questions in the EHC2023 Risk of Bias analysis given to thermal "high SAR" vs. relevant "low SAR" papers.

If both relevant and thermal papers selected for the EHC2023 meta-analysis were roughly similarly distributed as to their assessed quality, and if the questions favored neither relevant nor thermal papers, the percentages of negative scores should, irrespectively of the papers' findings, be roughly similar for both groups. Markedly differing scores would indicate bias. Very low scores for both groups on a specific question, or very high, would indicate that the question is not well fit for quality assessments, as it does not sort out poor studies, nor irrelevant ones.

However, from Fig. 1 we see:

*RoB*Q1.1 does not discriminate well between the thermal papers, as only a small fraction gets negative scores. The only cases of negative rating are cases where papers *"[do] not mention random allocation"*.

*RoB*Q1.2 gives no further assistance in the RoB ranking, as all selected papers pass this question without a negative RoB rating.

*RoB*Q2.1 does not differentiate well, as only a small fraction gets negative scores.

*RoB*Q2.2 does not differentiate well, as blinding is not used in any "high SAR" studies and only in a small fraction of the "low SAR" papers.

*RoB*Q3.1 is only relevant for thermal experiments where control of exposed energy intensity is closely related to the quality of the experiment. For the low SAR experiments, where the intensity of the exposure is well below the thermal threshold and other (even unknown) properties of the radiation might be relevant, knowing the exact exposure characterization might not be related to the quality of the experiment. This may explain why hardly any of the papers with high SAR experiments are negatively rated, while so many of papers with the low SAR experiments are negatively rated. Thus, it is questionable if a precise exposure characterization is a RoB issue for papers with SAR values well below the thermal threshold.

*RoB*Q3.2 is even more closely related to thermal studies, as a positive evaluation requires that the temperatures of the dams, supposed to reflect exposure intensities, i.e., SAR, are monitored. In practice, this question is a differentiator between thermal and nonthermal studies: No high SAR studies score negatively, while a substantial portion of the low SAR do.

*RoB*Q3.3 Negative score is mainly attributed where outcome assessment was not done blinded or information on blinding is not given. Hence, this question closely overlaps with Q2.2 as a differentiator, although very different in content.

*RoB*Q4.1 seems to punish the low SAR, and mainly newer, nonthermal studies, which are more open to complex, even unknown, causation.

*RoB*Q5.1 does not discriminate well, as only a small fraction gets negative results.

*RoB*Q6.1 For this question, we investigated more closely which properties of the studies lead to EHC2023 attributing them a negative score. In most cases, what leads to negative scores is few experimental animals. Hence, as practiced in EHC2023, this question favors studies on mice, as such studies in general use more animals than studies with e.g. rats, rabbits or monkeys. Mice are mainly used in *thermal* studies.

Summing up and commenting on these findings:

The question, which is the least biased, with around half of the studies from each SAR group given negative score, is Q6.1. This question seems to differentiate well. However, examining the cause of low score for this question, the overall reason seems connected to low number of experimental animals. Hence, the reason for a low score could just as well be that assessments connected to Q6.1 directly relate to the number of animals in the considered study. Such a bias favors old, thermal studies which are done on large amounts of mice, and by using simple measurements to evaluate effects, disfavoring modern studies of more complex, nonthermal causation, using fewer animals.

Some questions result in strongly different scores for thermal and for relevant studies, indicating significant bias. In particular, RoBQ3.1, RoBQ3.2 and RoBQ4.1 favor thermal studies, which largely are experiments testing hypotheses based on temperature increase and the single relevant detrimental effect tested for, and done with varying intensity (high SAR) of the RF-EMF exposure. These questions systematically attribute low scores, and thereby punish, studies open to more varied effects not caused by temperature rise, thus performed at exposure levels well below thermal heating thresholds and with low focus on SAR-values and research animals' temperature.

It should also be noted that all studies getting a negative evaluation as to RoBQ3.2 are relevant, non-thermal papers, and that nonthermal papers only are attributed negative scores for that question. In fact, this is a natural consequence of such papers not being preoccupied with risks for thermal

heating and SAR values. Hence, for the relevant, nonthermal experiments, not controlling for animal temperatures is *not* a sign of poor quality, which leaves this question inappropriate for assessing the quality of such relevant studies.

As demonstrated, we find the questions posed to test for Risk of Bias, to favor thermal studies, i.e. the irrelevant ones, over the studies with relevant exposure conditions.

We also identified questions which do not offer much value to the RoB analysis (RoBQ1.1, RoBQ1.2, RoBQ2.1, RoBQ2.2, RoBQ5.1), as they either give (almost) all papers high scores, or give them all low score, i.e., do not differentiate well.

We have not checked for other biases or for the scores being correctly reported in the RoB assessment. Anyhow, with such strong findings as reported, the RoB analysis is of little value, apart from reducing the impact of relevant studies by giving many of them high RoB scores and most thermal studies better scores: low RoB.

C.2: Risk of Bias questions correlate

If scores for different questions are strongly correlated, they may be measuring correlated properties, which they should not. We therefore checked for correlations between scores for different questions, as well as for publishing year and SAR levels (SAR levels being a proxy for relevance).

For questions RoBQ3.1 and RoBQ3.2, we found significant correlations between scores, as well as with publishing years and SAR levels: Out of the 41 papers published after 2000, there are 19 papers with a negative RoB rating for RoBQ3.1. 17 of these papers are also found among the 21 papers with a negative rating for RoBQ3.2. Hence, these two questions are almost identical as to how they discriminate between the papers.

It follows that the EHC2023 Risk of Bias analysis is biased also when checking for correlations between questions: *The correlation between questions RoBQ3.1 and RoBQ3.2 adds to the bias towards giving relevant studies low RoB scores, since only relevant papers get a negative score for both, and often do so.*

C.3: Older thermal studies are considered of less RoB than newer, nonthermal studies

To check further how the outcome from the EHC2023 RoB analysis is correlated with the year of publishing, we selected those questions in the RoB analysis which are shown in Figure 1 to differentiate most clearly in the EHC2023 Risk of Bias classification: RoBQ3.1, RoBQ3.2, RoBQ4.1, RoBQ5.1 and RoBQ6.1.

We sorted the RoB classification data, gathered from the EHC2023 Supplementary file 5, and ranked the papers according to their year of publishing (Figure 2). We found a clearly visible pattern of strong bias towards positive rating of irrelevant, older thermal studies, and strong bias towards negative rating of newer, relevant studies. This bias is easily visible in Figure 2: an almost total domination of positive ratings for older papers published before 1990 (left table, with blue cells denoting “Low” or “Some” concern) and a domination of negative ratings for newer papers published after 2009 (right table, red and orange cells denoting “High” concern). Yellow cells in the left columns indicate thermal studies, 22 of 39 of the older papers, while just one single paper among the newer ones.

Name of paper	Pub. year	RoB	Q3.1	Q3.2	Q4.1	Q5.1	Q6.1
Chernovetz 1975	1975	HC	+ +	+ +	+ +	+	- -
Rugh 1976	1976	HC	+ +	+ +	+ +	+ +	- -
Chernovetz 1977	1977	SC	+ +	+ +	+	+ +	+
Rugh 1977	1977	SC	+ +	+	+ +	+	-
Berman 1978	1978	SC	+	+	+ +	+	+ +
Rugh 1978	1978	SC	+ +	+ +	+	+ +	-
Albert 1981	1981	HC	+	-	+ +	+	- -
Berman 1981	1981	SC	+	+	+	+ +	+ +
Nawrot 1981	1981	SC	+ +	+ +	+ +	+	+
Smialowicz 1981	1981	SC	+ +	+	+ +	+ +	+ +
Berman 1982a	1982	SC	+	+	+	+ +	+
Berman 1982b	1982	SC	+	+ +	+	+ +	+ +
Jensh 1982a	1982	SC	+	+ +	+ +	+ +	+
Jensh 1982b	1982	HC	+	+ +	+	+ +	- -
Kaplan 1982	1982	HC	+ +	+ +	+	+	- - -
Lary 1982	1982	SC	+	+ +	+ +	+	+ +
Smialowicz 1982	1982	SC	+ +	-	-	+ +	-
Chazan 1983	1983	SC	+ +	+	+ +	+	+
Galvin 1983	1983	HC	+ +	+ +	+	+ +	- -
Inouye 1983	1983	SC	+ +	+	+ +	+	-
Jensh 1983a	1983	HC	+	+ +	+ +	+ +	- -
Jensh 1983b	1983	SC	+	+ +	+	+ +	+
Lary 1983a	1983	SC	+ +	+ +	+ +	+	+ +
Lary 1983b	1983	SC	+	+ +	+ +	+ +	+
Berman 1984a	1984	SC	+	+ +	+ +	+ +	+ +
Berman 1984b	1984	SC	+	+	+	+ +	+ +
Jensh 1984a	1984	SC	+	+ +	+ +	+ +	+
Jensh 1984b	1984	SC	+	+ +	+	+ +	+
Merritt 1984	1984	SC	+ +	+	+ +	+ +	-
Schmidt 1984	1984	SC	+ +	+	+ +	+ +	+
Nawrot 1985	1985	SC	+ +	+ +	+ +	+	+
Galvin 1986	1986	SC	+ +	+	+ +	+	+
Lary 1986	1986	SC	+	+ +	+ +	+	+
Marcickiewicz 1986	1986	SC	+	+	-	+	-
Tofani 1986	1986	SC	+	+ +	+	+ +	+ +
Brown-Woodman 1988	1988	HC	-	+ +	+ +	+ +	- -
Brown-Woodman 1988	1988	SC	+	+ +	+ +	+ +	-
Chiang 1988 (CS, 3xHC)	1988	SC	-	+	-	+	-
O'Connor 1988	1988	HC	-	+	+	+	- -

Name of paper	Pub. year	RoB	Q3.1	Q3.2	Q4.1	Q5.1	Q6.1
Guler 2010	2010	SC	-	-	-	+	-
Sambucci 2010	2010	LC	+ +	+	+ +	+ +	+
Takahashi 2010	2010	SC	+ +	+	+ +	+ +	+
Ait-Aissa 2012	2 012	SC	+ +	-	+ +	+	+
Aldad 2012	2012	HC	- -	-	+ +	+	+ +
Poulletier de Gannes 2012	2012	LC	+ +	+	+	+ +	+
Bas 2013	2013	HC	- -	-	-	+ +	- -
Haghani 2013 2xHC	2013	HC	- -	+ +	-	+ + (1, "+" 2)	-
Ikinci 2013	2013	HC	+	-	+	+	-
Odaci 2013	2013	HC	-	-	-	-	- -
Shirai 2014	2014	SC	+ +	+	+ +	+ +	-
Sangun 2015	2015	HC	+	+	-	+ +	- -
Shibkova 2015	2015	HC	- -	- -	+	+	+
Zhang 2015	2015	SC	-	-	-	+ +	-
Alchalabi 2016	2016	HC	- -	- -	-	+	+
Erdem-Koc 2016	2016	SC	-	-	+ +	+	-
Odaci 2016	2016	SC	-	-	-	+	-
Razavinasab 2016	2016	HC	-	-	-	+	- -
Rifat 2016	2016	HC	+	-	-	-	-
Stasinopoulou 2016	2016	SC	+	+	+	-	-
Türedi 2016	2016	HC	-	-	+	+ +	- -
Alchalabi 2017	2017	HC	- -	- -	+	+	+ +
Sharma 2017	2017	SC	+	-	-	+	-
Shirai 2017	2017	SC	+ +	+	-	-	+
DastAmooz 2018	2018	HC	- -	-	-	+ +	-
Petitdant 2018	2018	HC	+ +	-	-	-	-
Wang 2018	2018	HC	- -	-	+ +	+	-
Wyde 2018	2018	SC	+ +	+	+	-	+
Calis 2019	2019	SC	-	-	-	+	+
Azimzadeh 2020	2020	HC	- -	-	-	+	+
Li 2020	2020	HC	- -	-	+	+ +	- -
Keles 2021	2021	HC	+	+	-	+	-

Figure 2. The figure shows part of the EHC2023 rankings for the RoB questions Q3.1, Q3.2, Q4.1, Q5.1 and Q6.1. To the left are the rankings for papers published before 1990, and to the right for papers from 2010 onwards. Yellow background for paper names indicates "thermal paper". Such papers dominate the older papers (left table) while being almost totally absent among the newer papers (table to the right). The dominant blue color to the left shows that the majority of the elder papers get positive ratings for the RoB questions. The dominating colors to the right, orange and red, visualize the domination of negative ratings for papers from 2010 onward. High Concern (HC) overall rating is shown as red in the Risk of Bias (RoB) columns. We find only two "LC studies", and these are newer and nonthermal. Both these studies have ICNIRP-members as co-authors and report no detrimental effects found.

For papers published in the years not shown in Fig. 2, 1990 – 2010 (20 papers) the scores are more varied, as the "thermal only" RF-EMF studies in that period got stark competition from the as to dosimetry less stringent methodologies needed for testing nonthermal effects from mechanisms often still unknown. The more varied approaches used in that interim period reflect both currents.

Thus, the simple sorting by year of publication, seen together with Risk of Bias scores, strengthens our previous findings: *The EHC2023 RoB analysis is heavily biased towards attributing low Risk of Bias to old, thermal, and therefore irrelevant studies, in effect classifying them as unbiased, while newer, relevant studies with low SAR get higher RoB scores, in effect classifying them biased.*

This skewedness in the EHC2023 Risk of Bias assessment towards thermal experiments may explain why such experiments are allotted such a strong representation (54%) in the EHC2023 meta-analysis for "fetal weight", while so many *relevant studies (newer, with "low SAR") have been left out from the meta-study due to their RoB score showing "High Concern".*

C.4: The flawed Risk of Bias assessment is used to strengthen the argument that no conclusion can be drawn of RF-EMF being a health hazard

To add additional doubt to there being any negative health effects from RF-EMF exposure, the EHC2023 authors come forward with arguments which indicate that the authors have a preconceived "thermal only" position:

They write (EHC2023, page 27):

> *The values relative to the group of "low or some concern" studies and the group of "high concern" studies are separately reported. For some of the most relevant endpoints, like litter size and incidence of dead fetuses, it is shown that the pooled effect size of studies at "high concern" for RoB is much greater or much more uncertain than the value for studies at "low or some concern" for RoB. In other cases, this is not so evident, but this may be due to the small number of studies. These observations support the assumption that studies at high risk of bias report biased and exaggerated results or are more variable, which reduces the robustness of the pooled effect size. These considerations support our decision of using only the set of studies at "low or "some concern" for RoB for the final assessment of the body of evidence. [Emphasis added]*

In the quoted paragraph, the authors of EHC 2023 argue for the greater pooled effect size for the studies of "High Concern" – almost exclusively nonthermal and relevant ones – to be a result of bias and exaggeration. When knowing the bias of the Risk of Bias analysis, and the reasons for that bias, analyzed above, one can see that such a prejudiced view is highly ill-founded, and makes sense only from a preconceived "thermal only" position. In fact, the inverse chain of arguments seems as likely, or even more so: The effects found in the newer "nonthermal" studies, dismissed as of "High Concern", might as well arise from these studies being of *higher methodological quality* and more apt than the older "thermal only" methods for identifying relevant (nonthermal) effects.

Therefore, the argument for their RoB analysis being correct, and for their decision being correct to therefore exclude the "High Concern" studies from the body of evidence, seems only defendable within the biased logic of the thermal only world view.

At page 31, the authors of EHC2023 repeat the speculative idea that authors of "High Concern" papers be more prone to exaggerate their findings. By "extrapolating" this assumption, they speculate that also the middle category, the "Some Concern" papers, probably overstate the effects compared to "Low Concern" papers. Hence, they speculate further that the lack of "Low Concern" papers in the evidence base lead to exaggerated effects in the meta-analysis and that therefore "*the evidence of an RF-EMF effect on pregnancy and birth outcomes could be overstated to a certain degree*" (SIC!). By this chain of arguments, ill-based in the same way as the one just explained above, the authors of EHC2023 imply that if they had only found more "Low Concern" papers, the calculated overall effect size would have been even smaller and more uncertain (EHC2023, page 31):

> *Finally, it cannot be excluded that the pooled effect sizes resulting from our meta-analyses were exaggerated in the direction of an RF-EMF effect. The comparison of pooled effect sizes between "low or some concern" and "high concern" studies (Table 4) shows a higher effect for some of the most relevant endpoints in the "high concern" studies. Unfortunately, so few studies could be classified as "low concern" that such a comparison could not be done between "low" vs "some concern" and we were forced to include all these studies in the one category "low or some concern" studies. Nevertheless, the trend observed in Table 4 suggests*

> *that the evidence of an RF-EMF effect on pregnancy and birth outcomes could be overstated to a certain degree.*

The point that there are so few studies of “Low Concern” among the ones included for the EHC2023 study, is as such correct, but, as explained above, this is a result of the biased RoB analysis: From Figure 2 we see that among the more recent papers in the EHC2023 meta-analysis, which are all nonthermal, only two papers are labelled “Low Concern”: Sambucci2010 and Poulletier de Gannes2012, both reporting no effects found and both having ICNIRP members as co-authors. 12 other papers are ranked “Some Concern”.

In this manner, the authors of EHC2023 manufacture a rationale suitable to defend a “thermal only” or “minimal effects" position.

D: The "Fetal weight" endpoint meta-analysis is of unacceptable low quality due to the many errors and flaws

As a further quality control of EHC2023, we focused on details in the EHC2023 meta-analysis of the "fetal weight" endpoint.

To perform these checks required going into minute details of the papers and the values presented in EHC2023 Fig. 6. To make the work manageable we therefore focused on the 33 relevant studies (all RoB scores). We analyzed how data from the studies had been selected, reproduced, and treated in calculations.

We found various flaws and irregularities at all levels, all undermining the reliability of the meta-analysis. They do so by skewing the findings in the direction of lower overall effect size and/or increasing the confidence interval, i.e. all adding uncertainty to the overall results. The following was found:

- Errors in copying data from papers: 5 of 33 studies, one of them with two separate errors
- Undocumented deviation in equations influencing confidence intervals (CI): 4 of 33 studies
- Unscientific choices of approaches for handling experiments sharing a common control group: 12 of 33 studies

A few studies are affected by more than one of the flaw types, so in total we find reasons for correcting the values for 19 of 33 studies (58%). Details of the findings are presented below.

D.1: Uncertainty added by incorrect reproduction of data from papers

While checking if data from relevant experiments have been correctly cited when used in the meta-analysis, we also checked relevant experiments rated "High Concern" as to Risk of Bias. We did so in order to get a larger number and more representative selection of experiments to investigate. We found that data from 5 of the 33 relevant experiments, whether of "Low and Some Concern" or "High Concern", are wrongly cited in EHC2023 Fig. 6.

The flaws vary substantially and taken together have a significant impact on the calculated effect size and/or the reliability of the study. A list of these flaws is found here as Table 2.

Table 2: Flaws in EHC2023 Fig. 6 from misreporting which we found to have a significant impact on *the calculated effect size or the reliability of the study.*

Papers (RoB scores)	**Misreportings**	***Effect from correcting flaws***
Dasdag2000 (LSC)	Number of dams is doubled in Fig.6: Number of dams is reported as 12 both for control and intervention groups. Correct number is 6 in both cases, as the study started with 12 dams in each group, but 6 were sacrificed before the end of pregnancy. Hence, fetal weight was reported only for the 6 remaining dams.	*Increases the confidence interval, i.e., decreases the reliability of the study, but with no impact on the overall effect size, as measured with Cohen's d.*
Jensh1982b (HC)	The number of control dams is set to 3, while the paper says 4. This is a study finding decreased fetal weight.	*The overall effect size as well as the confidence interval are slightly increased, i.e. a stronger, but more uncertain effect.*
Merrit1984 (LSC)	Numbers are cited wrongly: Weight of sham-exposed fetuses is cited as 0.96 in meta-	*Increases the effect size considerably.*

	analysis, while 0.969 in the study; weight of exposed fetuses is cited as 0.94 in meta-analysis, while 0.89 in study.	
Sangun2015 (HC)	The values for exposed and sham-exposed have been switched in Fig. 6. Mistakenly cited, the values in Fig. 6 show increased fetal weight, while the study found *reduced* fetal weight.	*The effect size switches from clearly negative to clearly positive (from -0.81 to +0.81).*
Shama2017 (LSC)	Average standard deviation (SD) for the two exposure experiments is wrongly set as 0.7, while correct SD-value is 0.07.	*The effect size is made ten times stronger.*
Also Shama2017 (LSC)	The two experiments finding significant effects with high certainty, each with its separate control group, are wrongly pooled together. They should be cited as two separate studies, doubling the number of control animals.	*The overall effect size increases.*

It is highly remarkable that in the EHC2023 meta-study, all flaws from misrepresenting the papers listed above contribute to reducing the overall effect size from RF-EMF exposures and/or increase the overall confidence interval, i.e. adding uncertainty to the findings.

We found no such flaws skewing the results in the opposite direction.

D.2: Uncertainty increased by unjustified choice of deviant equation for confidence interval (CI), unveiling one more level of flaws

A confidence interval (CI) is a purely statistical concept to express a range of values which most probably (e.g., with 95% purely statistical certainty) contains the "true", or "correct", value.

While checking the calculations used in EHC2023 for the confidence intervals, we found they have been done in a deviant way for the four studies with the fewest animals: Stasinopoulou2016 1 to 3, and Jensh1983a. As a consequence, the confidence intervals are decreased, which means that these studies get a relatively higher impact than other studies, as explained below.

Generally, all studies' confidence interval are calculated for the EHC2023 meta-study using Equation 1, which gives the pooled Standard Error (SE):

Equation 1 for pooled SE used for calculating confidence interval for Cohen's d, where n_i is the number of animals in each study and d is Cohen's d.

$$SE_d = \sqrt{\frac{(n_1+n_2)}{n_1 n_2} + \frac{d^2}{2(n_1+n_2-2)}}$$

However, for Stasinopoulou2016 1 to 3 and Jensh1983a, we found that the "-2" at the end of the equation has been omitted. This is a known variation of the pooled SE equation, but we have found no justification or explanation in EHC2023 as to why this variation has been used for just these four studies and not for all.

The effect of choosing this other variation, is that the confidence intervals decrease, which means that the studies will get higher weight. Hence, this unexplained choice of equation for calculating confidence intervals impact the EHC2023 meta-analysis as follows:

1. The three Stasinopoulou2016 studies find *increased* fetal weight from exposure. The deviant equation produces reduced confidence intervals and attributes these studies more weight, thus giving more credibility to increased fetal weight resulting from exposure (in contrast to other studies finding reduced weight), *which reduces the overall effect size found in the EHC2023 meta-study.*
2. Jensh1983a finds *decreased* fetal weight from exposure. The choice of the deviant equation produces a (slightly) reduced confidence interval, i.e., attributes slightly more credibility to this study finding *decreased* fetal weight.

These two papers might seem contradictory, as the one finds increased and the other decreased fetal weights from exposure. The use of the deviant equation might be intentional to amplify this supposed contradiction between the two studies, thus adding uncertainty to the findings. However, these opposite results are easy to reconcile, as was done by Andrew Marino (Marino 2010, page 130): The common and essential finding is that RF-EMF exposure may impact weights in both directions – sometimes upwards (well known as *hormesis* in the literature on ionizing radiation), at other times downwards – in both cases for reasons not well known. Presupposing that changes from exposure can only result in *decreased* weights, is unfounded. Therefore, a better indication of effect would be degree of change in weight for exposed vs. control groups, not just loss of weight.

D.3 Impact on reproduction errors and deviation in equations, in total 27% of the investigated studies

Summing up, out of the 33 relevant studies found in EHC2023 Fig. 6, including both “Low or Some Concern" and “High Concern” Risk of Bias papers, we found 9 studies, i.e., 27%, cited erroneously or with unjustified divergences. This is a surprisingly high portion. All these flaws contribute to reducing the overall effect size and/or increase the confidence interval, although in two cases these effects stem from a superficial interpretation and simplistic notions as to possible effects.

These flaws, too, add uncertainty to the meta-study’s overall reliability. They also weaken the reliability of the findings in the studies, being that offspring of mothers having been exposed to RF-EMF with SAR < 5 W/kg, in general have lower, or deviant, fetal weight.

To spot the impact of flaws described above, we did our own calculations of effect sizes and confidence intervals. We restrained our analysis to the relevant studies of effects on fetal weight presented in EHC2023 Fig. 6. We calculated the values for all the relevant studies independently of their Risk of Bias scores, as we had found the Risk of Bias analysis to be highly biased towards attributing negative scores to new and relevant studies, irrespective of their quality. It does therefore not seem justifiable to leave the “High Concern” papers out of a meta-study *en bloc*.

To compare the values used in EHC2023 Fig. 6 with correct values and consistent use of formulas we did the following two calculations for Cohen’s d with 95% confidence interval for all *relevant* studies in Fig. 6, i.e., fetal weight studies with SAR below 5 W/kg and with more than one single exposure dose:

1. A calculation true to Fig. 6, i.e., using the values and variations in formulas. See the left table in Figure 3 below.
2. A calculation where we used the correct values from the papers and the same formula for all studies. See the right table in Figure 3 below.

We see that the pooled effect size increases from 0.24 with flaws, to 0.54 with no flaws, a significant difference when using standard interpretations of Cohen’s d.

	As in WHO2023 Fig. 6			Correct data and consistent equations:		
Study	Cohen's d	CI lower	CI upper	Cohen's d	CI lower	CI upper
Berman1978-1	-0,08	-0,34	0,19	-0,08	-0,34	0,19
*Brown-W1988b-1	0,21	-0,66	1,09	0,21	-0,66	1,09
*Brown-W1988b-2	0,03	-0,93	0,99	0,03	-0,93	0,99
Dasdag2000	2,66	1,54	3,78	2,66	1,03	4,28
Galvin1986	-0,51	-1,41	0,38	-0,51	-1,41	0,38
Guler2010	-0,09	-1,02	0,83	-0,09	-1,02	0,83
Kubinyi1996-1	0,10	-0,45	0,65	0,10	-0,45	0,65
Kubinyi1996-2	-0,08	-0,78	0,61	-0,08	-0,78	0,61
Lary1983a	-0,14	-0,63	0,34	-0,14	-0,63	0,34
Lee2009 -1	0,84	0,10	1,58	0,84	0,10	1,58
Lee2009 -2	0,27	-0,35	0,90	0,27	-0,35	0,90
*Marcickiewicz1986	1,45	1,02	1,87	1,45	1,02	1,87
Merrit1984	0,06	-0,81	0,94	0,25	-0,63	1,13
*Ogawa2009	-0,08	-0,61	0,46	-0,08	-0,61	0,46
Sambucci2010	0,00	-0,82	0,82	0,00	-0,82	0,82
Schmidt1984	-0,05	-0,67	0,57	-0,05	-0,67	0,57
*Sharma2017	0,22	-0,76	1,21	*2,21*	*0,72*	*3,70*
				7,06	*3,77*	*10,35*
Smialowicz1981	-0,50	-1,13	0,13	-0,50	-1,13	0,13
Smialowicz1982	-0,52	-1,67	0,64	-0,52	-1,67	0,64
Stasinopolou2016-1	-0,44	-2,25	1,37	-0,44	-2,27	1,38
Stasinopolou2016-2	-1,16	-2,78	0,45	-1,16	-2,83	0,50
Stasinopolou2016-3	-2,91	-5,05	-0,76	-2,91	-5,20	-0,61
Tofani1986-3	0,06	-0,70	0,82	0,06	-0,70	0,82
*Alchalabi2016	3,20	2,07	4,33	3,20	2,07	4,33
Galvin1983	0,36	-0,34	1,06	0,36	-0,34	1,06
*Inaloz1997	1,25	0,33	2,18	1,25	0,33	2,18
Jensh1982a	-0,53	-1,69	0,63	-0,53	-1,69	0,63
Jensh1983b	-0,44	-1,59	0,70	-0,44	-1,59	0,70
Jensh1982b	0,18	-1,09	1,46	0,19	-0,96	1,34
Jensh1983a	1,44	0,06	2,83	1,44	0,04	2,85
*Li2020	0,88	-0,19	1,95	0,88	-0,19	1,95
Sangun2015	-0,87	-2,34	0,60	0,87	-0,60	2,34
*Wang2018	-0,19	-1,17	0,80	-0,19	-1,17	0,80
Meta-analysis	0,24	-0,06	0,55	0,54	0,24	0,84

Figure 3: To the left are calculations of Cohen's d and confidence intervals for all relevant studies in EHC2023 Fig. 6 "fetal weight" endpoint with no errors corrected. To the right reproduction errors are corrected and the same equations is used for all studies. The pooled effect size is changed from the original 0.24 to 0.54, a significant difference when using the standard interpretation of Cohen's d.

D.4: Uncertainty added by approaches chosen for handling shared control groups

Several of the relevant papers selected by EHC2023 for its meta-analysis contain more exposure groups sharing a common comparator, i.e., control group. While examining how EHC2023 reports effects found in such studies, we found several flaws and a practice diluting the quality of the meta-analysis by adding uncertainty to the overall result:

Aggregation of effects from studies where several exposure groups share the same comparator is a general issue with no intrinsically "true" or "correct" solution. The aim is to aggregate data in ways that as faithfully as possible convey, in a single, standardized expression, the essence from the more detailed findings, i.e. the properties considered of interest for the overall analysis.

Thus, just lumping together effect sizes from experiments and averaging may be the right thing to do when properties in the various groups and their exposures are reasonably similar, but not justifiable in other contexts where they are not. Sums, averages, means or other aggregate characterizations across experiments are not justifiable when relevant properties of the various groups and their exposures differ significantly, as such procedures necessarily mean not only loss of information, but also loss of validity and reliability. The resulting values might mask essential information, become nonsensical or clearly misleading.

Also, listing a study containing several experiments sharing a common control group as if they were separate studies, gives that study a disproportionate weight. However, if the experiments are not similar, aggregating them might not be justifiable for reasons just mentioned, while no aggregation means no single, standardized expression, thus a more fragmented picture.

In full accordance with this general view, the Cochrane Handbook (Higgins et al. 2022) section 23.3.4 prescribes five ways of including multiple groups from one study sharing a common comparator. Of these, we find that EHC2023 uses the two first options mentioned in section 23.3.4, but shiftwise in ways that decrease the studies' overall reliability:

1. *Combine groups to create a single pair-wise comparison (recommended).*
2. *Select one pair of interventions and exclude the others.*

D.4.1: Selection of one pair of interventions: EHC2023 selects the ones showing overall least effect

When selecting an exposure group for comparison with the control group, the selection should either be done in a truly random way, or "*be based on a scientifically defensible rationale*" (Turner 2006). In EHC2023 the selection is neither random, nor defensible, and EHC2023 does not give a reason for or defend the choices made*. If for a purpose, the shifting approaches seem chosen to add uncertainty to the findings:*

For three of the papers where a single exposure group is selected (Galvin1983 & 1986; Nelson1991), EHC2023 selects the results for the male offspring, not the female – in a review on effects on mothers and their offspring. Why so? In the EHC2023 Supplementary file 2, «Data Collection Comments", the reviewers argue in favor of leaving out the female data in order not to duplicate information, but with no explanation as to why just female data instead of male data should be excluded:

> *"When data on fecundity and on adverse effects on the offspring health at birth were separately reported for male and female offspring, only the former were extracted to not duplicate information from the same litters".*

The effect of excluding female data, instead of male, from the two Galvin studies for the meta-study, is a slightly increased confidence interval and reduced effect size. (For Nelson1991 we found no effect from this choice.) Hence, *choosing male data in these cases adds uncertainty, and seems neither truly random, nor due to a scientifically defensible rationale.*

From the fourth of these studies, Tofani1986, the EHC2023 authors have selected the exposure group with the longest exposure duration for the meta-analysis, no rationale given. As this is the group with the least decreased fetal weight, *selecting this exposure group reduces the overall effect size more than any of the other exposure groups in Tofani1986 would do, and more so than when averaging across the groups.*

So, a*gain, the authors of EHC2023 make choices adding uncertainty to the findings, with no rationale as to why.*

D.4.2 Aggregating and averaging across heterogenous cases gives more uncertainty and less relevance to the result, as well as significant loss of relevant information

EHC2023 claims aggregating and averaging across heterogeneous experiments sharing a control group to be the method with the "less bias", but with no explanation provided (EHC 2023 page 31):

> *We chose the last option* [i.e., averaging, alternative 1 above] *because, in our opinion, it introduced less bias and we had decided to explore the dose–effect relationship through dose–effect meta-analysis.*

As mentioned above, aggregating by averaging are defensible on certain preconditions. The Cochrane Handbook recommends averaging, provided the "interventions", i.e., in our context the exposure conditions, are sufficiently similar for the aggregated data to convey what is relevant from the study for the question asked. Hence, the method to choose when aggregating data, is the one which best enlightens the question that the EHC2023 meta-analysis is meant to address: the possibility of health effects on the fetus.

In its section 2.10 Synthesis methods, EHC2023 makes it clear that it lumps together effect sizes from different exposure groups with a common control group by averaging both the different exposures (measured as SAR values) as well as the effect sizes and standard deviations, and by weighting the effect sizes according to the sizes of the groups exposed – i.e., in accordance with the alternative 1 recommended above (EHC2023, page 5):

> *When a study had several exposure groups matched to the same comparator, the means and standard deviations of these exposed groups were combined into one exposed group using the formulas provided in the paragraph 6.5.2.10 of the Cochrane Handbook (Higgins et al., 2022), so that each study was entered only once into the meta-analysis. The exposure level assigned to that combined exposed group was calculated as the average SAR of the exposed groups in that study weighed by the number of animals in each exposed group.*

Here EHC2023 fails: First and foremost, averaging across thermal and subthermal exposure levels is inappropriate, if for no other reason, because the supposed mechanisms causing effects are so fundamentally different. This problem is not discussed in EHC2023.

As also stated in the Cochrane instructions, neither is it appropriate to lump together and average data differing widely (Higgins et al. 2022, MECIR Box 10):

> *C62: Ensuring meta-analyses are meaningful (Mandatory)*
>
> *Undertake (or display) a meta-analysis only if participants, interventions, comparisons and outcomes are judged to be sufficiently similar to ensure an answer that is clinically meaningful. … Clinical diversity does not indicate necessarily that a meta-analysis should not be performed. However, authors must be clear about the underlying question that all studies are addressing.*

The nonthermal studies selected for the EHC2023 meta-study present several such issues where aggregating data is not legitimate: Of the 8 nonthermal studies represented by averaged values in the meta-analysis for "fetal weight", the "interventions", i.e. in our context exposure conditions (listed below), are simply not sufficiently similar to ensure that the averaged and aggregated data provide an "*answer that is clinically meaningful*": Neither the averaging between single studies'

experiments, nor the aggregation of the averaged values between studies reflect the relevant findings in a way improving the understanding of effects from exposures of mothers and offspring. Most of the nonthermal studies are studies searching for effects from causations very different from heating and thus not identifiable by concepts like SAR, and in large parts even unknown. In the search for causations, the animals are administered exposure conditions varying along several dimensions in order to find correlations between the exposure conditions and variations in effects. Such significant differences are certainly also found as to the "fetal weight" endpoint, as demonstrated by the differences between the following studies used by EHC2023:

- Alchalabi2016: This study investigated *whether different durations of daily exposures had different effects*. They found that the shorter the exposure, the higher the reduction of fetal weight.
- Brown-Woodman 1988b-1: The aim of the study was to investigate *whether differences in pulse rates, as measured in Hz, had any impact on the mothers and their offspring*. They found that the 10 Hz pulse combined with the lowest SAR alternative, distributed as 1 hour daily, resulted in the most reduced fetal weight. The summarized values used for the EHC2023 meta-analysis are averages of data across four exposure conditions, two of 1 hour duration and two of 45 minutes duration per day, with two different pulse frequencies resulting in four different SAR levels, one for each of four exposure groups, all sharing the same control group being sham-exposed for 1 hour (i.e., not a proper sham control for the two 45 minutes studies). By averaging the data for the EHC2023 meta-study, the 45 minutes studies included, the effect size is reduced by 2/3, thus making aggregated findings less significant and adding uncertainty to the findings.
- Brown-Woodman 1988b-2: This is a further analysis of data from the previous paper, here *investigating effects of pulse rates different from those above*. From this study, EHC2023 uses averaged data from two 30 minutes per day exposure studies under different exposure conditions, including different SAR and pulse rates, compared to a 30 minute sham exposed group.
- Inalotz1997: This study cites data from experiments with *various SAR and exposure lengths:* simulating being close to a microwave oven for a shorter time (3,9 W/kg in 15 min) compared to being at a longer distance over a longer time (1,9 W/kg for 30 minutes), both in terms of reduced fetal weight. A clear difference was found: The closer and shorter stay had more effect than the more distant and longer stay.
- Li 2020: The paper *compares the effects of very different exposures: 1800 MHz continuous wave (CW), WiFi modulated 2,4 GHz, and, thirdly both sources simultaneously*, i.e., three exposure conditions with one exposure group for each. The WiFi had much lower power density than the 1800 MHz CW. They found that the combined exposure had clearly the most detrimental effects, among them reduced fetal weight, even though the SAR values were quite similar to the 1800 MHz exposure.
- Marcickiewicz1986: Here there were *two exposure groups, one thermal and one nonthermal*, this difference in exposure intensities being the focus of the study. A statistically significant difference in effects on fetal weight was found (reduced fetal weight).
- Sharma2017: Here the experiments *exposed the pregnant animals at different periods during pregnancy*. Clear differences as to exposures and effects were found (reduced fetal weight).
- Wang 2018: In this study, in Japanese, the aim was to *explore if different exposure durations have different effects*. The English abstract reports significant differences as to effects, but since we only have available the average values of an unknown number of experiments with unknown exposure conditions, we have done no further assessments.

As we see from this list, the heterogeneity is high as to interventions, comparisons and outcomes. To aggregate such data does not contribute to "an answer that is clinically meaningful". On the contrary, aggregating such data derails the readers' attention from evidence for nonthermal causations and the limitations of SAR as a relevant measuring stick, towards the idea that "there can be no effects, since SAR values are low".

Hence, the selection strategy chosen for the EHC2023 in order to aggregate data from the selected studies, ignores the preconditions and warnings as to such aggregation, and cannot yield meaningful results. Averaging such data means averaging *ad absurdum* and amounts to averaging temperatures from the North pole and the Equator to tell about biological effects from sunlight. Due to the massive loss of relevant information, the selection strategy and the way the EHC2023 handles studies with multiple experiments and a common control, *the EHC2023 meta-analysis and the sub-group analysis on fetal weight are simply useless as to the study's purpose – at best nonsensical, if not grossly misleading.*

E: Few of the severe detrimental effects from subthermal RF-EMF exposure reported in the papers are truthfully presented in EHC2023's overview and presentation of each study's experiments and findings.

In EHC2023 Table 3 results from each of the papers selected for review are presented under the heading "Summary of paper results". We would expect such a summary to be correct and present all statistically significant and relevant findings reported in the papers as to effects from RF-EMF exposure on the mothers and their offspring. Our check as to the 29 relevant (i.e., nonthermal) papers reveals they are not, as we compared the presentations in Table 3 with the results reported in the papers' abstracts. Several of EHC2023's summaries are clearly erroneous, misleading and/or simply omitting detrimental effects reported in the papers:

The summary of one of the papers in EHC2023 Table 3 is clearly erroneous, and, as well, the presentation omits *all* detrimental effects reported in that paper.

For 7 papers, the summaries in EHC2023 Table 3 claim no effects were found, while serious detrimental effects reported in the papers are not mentioned, neither in the overview Table 3, nor elsewhere in EHC2023.

In addition, from 5 papers the EHC2023 summaries only mention some relatively modest effects like pup weight, while the studies themselves report more severe effects. EHC2023's rationale for these omissions seems to be that EHC2023 only reports on detrimental effects within the narrowed scope of the meta-study (EHC2023 page 8), i.e. it should ignore all detrimental effect except those from the defined endpoints for meta-analysis (SIC!), although these endpoints were chosen to provide proxies for the overall health risks of women and their fetuses:

> *"All papers that met the inclusion criteria were reported in tabular form (Table 3). In this table, characteristics of the studies regarding populations, exposure and outcomes are presented. Additionally, a very brief description of the main results in scope for the systematic review is reported, based on the authors' interpretation and discussion."*

By this flawed logic, EHC2023 filters out detrimental effects that are outside the proxies chosen for the systematic review, thereby hiding away findings of many serious, nonthermal effects, conveying the impression that only few, if any, effects were found, as documented in the following. This is of course fundamentally wrong and misleading, and unacceptable as science.

E.1: Paper reports reduced fetal weight, in EHC2023 Table 3 erroneously reported as "no effect on pup weight"

For one of the relevant (i.e., nonthermal) papers, the summary in EHC2023 claims "no effect on pup weight", while reduced pup weight in addition to other significant effects are explicitly reported in the paper (citations in *italics*):

Paper	**Presentation in EHC2023 Table 3**	**Results reported in the paper**
Galvin1983	*No effect on pup weight.*	*No effects were noted on pregnancy rate or litter size; however, pup weight was reduced. Also, the white blood cell numbers were lower in male and female pups exposed to microwave radiation and the differential cell counts were altered in the female pups at 10 d postpartum.*

E.2: "No effects" reported in EHC2023, while the papers report serious and significant effects

We here list the 7 relevant papers summarized in EHC2023 as "no effects found", while the papers describe detrimental effects in their abstracts (citations in *italics*, our comments in normal font). Note that this also applies to Galvin1983 presented above, resulting in a total of 8 such misrepresented relevant papers.

Paper	**Presentations in EHC2023 Table 3**	**Results reported in the paper**
Guler2012	*No effect on pup weight.*	*Change in mothers' free radicals, leading to oxidative destruction in lipids and DNA molecules.*
Inaloz1997	*No effects on litter size, fetal weight and fetal length.*	*Pronounced necrotic renal tissues and adrenal glands* [in litters of exposed mothers].
Kubinyi1996	*No effect on pup weight. At PND 24 no effect on brain weight.*	*The activity of enzyme isolated from the brain showed a significant decrease after CW MW exposure, but the changes were not significant after 50 Hz AM MW exposure … The activity of the enzyme isolated from liver increased under CW and 50 Hz modulated MW*.
Sangun2015	*No effect on pup weight.*	*Brain and ovary TOS and OSI values in the prenatal group were significantly increased (p<0.05) compared to the control group. … Increased TOS and OSI values in the brain and ovary tissues can be interpreted as a sign of chronic stress induced by EMF.*
Smialowicz1981	*No effect on pup weight.*	*… significant decreases in the activity of cetylcholinesterase were observed in the striatum and medulla oblongata of 22-day-old rats and in the midbrain of 40-day-old rats but not in 97-day-old animals*
Smialowicz1982	*No effect on pup weight.*	*Significant increases in the response of lymph-node but not of blood lymphocytes from irradiated rats following stimulation with mitogens was observed in two of four experiments. … These rats, born to irradiated dams, showed a similar increased response of node but not of blood lymphocytes to T cell mitogens at 42 days of age.*
Wan2918	*No effect on litter size and on pup weight.*	*The morphological changes of pyramidal cells in the polymorphic layer and DCX-positive cells in the dentate gyrus were obvious in rat offspring of long term maternal exposure group. There were less PCNA-positive cells in dentate gyrus and decreased expression of DCX and BDNF in hippocampus by Western blot in long term maternal exposure group compared with control and short term maternal exposure group (all P<0.05). Conclusion: Long term prenatal mobile phone exposure might inhibit the expression of PCNA and DCX in dentate gyrus of rat offspring by down-regulating BDNF.*

E.3: EHC2023 understates significant detrimental effects found in the papers

For the following papers, the summaries in EHC2023 Table 3 are grossly understating significant developmental, neurological and/or other detrimental effects found in the studies (citations in *italics*, our comments in normal font):

Paper	Presentations in EHC2023 Table 3	Results reported in the paper
Li2020	*Some effects on pup weight but no on pup length. At PND 21 no major effect on learning and memory by Y-maze test; at PND 49 no major effect on motor activity by open field test; pups as the experimental unit.*	The paper finds significant ($p<0.05$) lower fetal weight in the exposed groups compared with control. It also presents other findings. E.g.: *The bodyweights and tail length of the 1800MHz exposure group and the 1800 MHz + WiFi exposure group were smaller than the control group ($P < 0.05$). The eye opening time of the 1800 MHz + WiFi exposure group was earlier than the WiFi exposure group ($P < 0.05$).* They also report significant differences for several aspects of the maze test and open field test. In addition, they report significant neurological changes: *In both the 1800 MHz + WiFi and WiFi groups, NR2A and NR2B expression was down-regulated, whileNR2D, NR3A andNR3B were up-regulated. Moreover, NR1 and NR2C in the WiFi group were also up-regulated.*

E.4: Only a few of the effects reported in the papers are mentioned in EHC2023

From the following 4 papers, the EHC2023 summaries include a few effects, but not the more serious ones which are the main focus of the studies reported in the papers (citations in *italics*, our comments in normal font):

Paper	Presentation in EHC2023 Table 3 (only findings copied below)	Results (also) reported in the paper
Alchalabi2016	*Decrease of implantation sites after the longest daily exposure during preimplantation stage. Decrease of litter size and fetal weight after the whole gestation exposure.*	"*Malformation, haematoma, and oedematous foetuses"* and *"Postnatal observations showed haematoma, congestion, short tail, malformation and growth restriction and delay in some growth markers were observed. In-utero irradiation for 2 and three weeks induced oxidative stress in pregnant rats."*
Jensh1983b	*Increase of pup weight; increase of female fertility by F2 litter size.*	*There were significant differences in activity among the irradiated and control offspring between the sexes, the irradiated offspring being more active. These results are indicative of possible radiation-induced behavioral alterations.*
Sambucci2010	*No effect on litter size and pup weight.*	*Stress-associated effects as well as age- and/or sex-related differences were observed for several parameters.*
Stasinopoulou2016	*At PND 21 decreased hippocampal neuron density.*	Heart rate increase in the embryos and *"Significant changes in newborns' somatometric characteristics were noticed. Pyramid cell loss and glia fibrilliary acidic protein (GFAP) over-expression."*

E.5: The EHC203 "Summary of paper results" misreport from the studies selected

Summing up the figures and tables presented above, we find that out of the 28 relevant papers for "fetal weight" reporting nonthermal effects, the "Summary of paper results" in EHC203 Table 3 does not report significant and detrimental health effects reported for 13 of these papers. Of the 15

relevant papers correctly presented, 8 of these papers report “no [nonthermal] effects found”, i.e., no effects to report and therefore no effects to leave out of the presentation in Table 3.

Hence, EHC203 Table 3 only mentions all nonthermal effects reported in 7 out of the 28 papers. However, we also see that findings are understated, typically leaving out the wording “significant”, e.g. Table 3 reports “Decrease of fetal weight”, while the paper reports “The birth weight … was significantly lower … (p < 0.001))” as for Dasdag2000. Hence, also in most of these cases EHC203 underplays findings highly relevant to mothers and offspring.

F: Correlating factors not taken into account resulting in flawed endpoint analysis: Two examples

Several of the papers reporting on fetal weight endpoint selected for the EHC2023 meta-study, discuss the evident fact that when the number of fetuses in mother's womb is higher, the weight of each fetus is lower. I.e., a lower fetal weight is normal when there are more fetuses in the litter, as well as the other way around. Nonetheless, we found that this important factor which should be taken into account in EHC2023 before drawing conclusions as to RF-EMF effect on fetal weight, has not been considered, nor discussed.

The resulting analysis in EHC2023 on the effects of RF-EMF on fetus weight is therefore too simple and shallow for drawing any conclusions at all. Our closer look shows that *when corrected for litter size, RF-EMF as cause of reduced fetal weight gets increased confidence.*

To check if similar issues might also arise as to other endpoints in EHC2023, we chose "brain weight" and found a similar oversimplified and shallow analysis for this endpoint, as litter size and related fetal weight has not been taken into account when considering effects on brain weight.

Moreover, EHC2023 chose to use brain weight as indicator of changes in brain pathology, although brain weight is well proven to be a parameter totally independent of whether there are changes in brain pathology or not. This is even demonstrated in the analyzed studies. Hence, *also the EHC2023 results as to brain weight are simply useless and bewildering.*

This raises serious concerns, elaborated in the following:

F.1: Details as to the impact of litter size as cause of reduced fetal weight

As both "fetal weight" and "litter size" are endpoints investigated in EHC2023, litter size should have been considered a covariate to RF-EMF exposure, due to its effect on the fetal weight. This should have been discussed in the "fetal weight" meta-study, but is not.

The well-known inverse co-variation between litter size and fetal weight is the topic of, e.g., [Romero1992]. We cite from the abstract:

> *[..] the correlation coefficient (r = 0.677) was highly significant (P = 0.002), which made evident that there was an inverse relationship between fetal weight and litter size. If fetal weight/litter size inverse relationship is not taken into account when toxicity on the fetal weight is analyzed, wrong conclusions may be reached if the test substance reduces the litter size, provoking embryofoetal mortality. The iatrogenic decrement in fetal weight can be masked by an increment due to the litter size reduction. We suggest that in all three segments of reproductive toxicity studies, litter size must be considered as a covariate to the effect of the test substance on the fetal weight, in order to perform a correct analysis of covariance (ANCOVA), in addition to the dose factor commonly used in common ANOVA.* [Emphasis added.]

Our closer examination reveals that in EHC2023, both "litter size" and "fetal weight" are reported for 28 studies, of which 11 are thermal. We checked for correlations between litter size and fetal weight in these 28 studies to see if such an inverse relationship would help when assessing the impact from RF-EMF exposures on fetal weight.

The matrix of possible interpretations is such:

- If there is a
    - decrease in both litter size and fetal weight, or
    - no change in litter size, but decreased fetal weight, or
    - decreased litter size and no change in fetal weight – (When the litter size decreases, the normal outcome is increased fetal weight, hence, if no change in fetal weight, the EMF exposure has caused decreased fetal weight relative to the expected weight.),

  then the RF-EMF exposure is the probable cause of the *decreased* fetal weight. Litter size might then not be a co-variate, but together with fetal weight an effect of the exposure.

- Inversely, if there is a
    - decrease in litter size together with an increase in fetal weight, or
    - no change in litter size but increased fetal weight, or
    - increased litter size and no change in fetal weight – (When the litter size increases, the normal outcome is decreased fetal weight, hence, if no change in fetal weight, the EMF-exposure caused increased fetal weight relative to the expected weight.),

  then the exposure is probably a good indication that the RF-EMF exposure caused fetal weight increase.

For the remaining three combinations, no conclusions can be drawn as to the role of RF-EMF exposure:

- If a study reports increased litter size and decreased fetal weight, some or more of the reduced weight may – or may not – be caused by the increased litter size.
- Inversely, decreased litter size might explain none or more or less of an increase in the weights of the fetuses.
- If the study reports no changes, neither in litter size nor in fetal weight, no effect is found from RF-EMF exposure.

When adding the alternative “No change”, which means effect size less than 0.09, we get the following table (Table 3) showing how fetal weight and litter size correlate, and how this correlation may be used to indicate whether RF-EMF exposure is found to have an effect on fetal weight, given the information on litter size found in the mentioned studies.

We see from Table 3 below, that a significant share, 12 out of 28 studies, are found in the “Undetermined” cells, i.e., they cannot be used to support any conclusion as to the impact from RF exposure on fetal weight.

Of the *thermal* studies, 7 support *reduced* fetal weight, while no studies support *increased* fetal weight. Of the *relevant* studies, however, 6 studies support *reduced* fetal weight, while 3 relevant studies support *increased* fetal weight.

Taken together, these studies underpin that low SAR exposures can, and may, cause reduced fetal weight. This finding is in line with our calculation of effect size for relevant studies. However, as long as the issue of inverse co-variance between fetal weight and litter size is not addressed in more detail, also not taking the inverse effect into account (named *hormesis* in the context of weak ionizing radiation), *the reliability of the effect size found in EHC2023 and in our own meta-analysis for the “fetal weight” endpoint, is highly questionable also for this reason*.

Thus, our litter-size-to-fetal-weight analysis, together with our effect size results, *increases, from the studies selected for investigation by the EHC2023 authors, the calculated probability that pregnant mother's exposure to subthermal RF-EMF will lead to reduced fetal weight. Thereby, this study adds confidence to our results as to the overall effect size.*

Also these findings underpin that, considering the importance of the topic, a more solid analysis should have been carried out by EHC2023, i.a. by taking into account the inverse co-variance between litter size and fetus weight, and that therefore, the present analysis is not acceptable.

Table 3: Correlations between fetal weight and litter size. Blue cells for the combined data indicating effects from RF-EMF exposure, and green cells for indications of decreased fetal weight.

28 studies in all, 11 thermal, 17 relevant **Fetal weight:**	**Litter size:** **Increased** - 8 studies 4 thermal, 4 relevant	**No change** - 6 studies 1 thermal, 5 relevant	**Reduced** - 14 studies 6 thermal, 8 relevant
Increased 7 studies 0 thermal studies 7 relevant studies	*Increased fetal weight from exposure* Thermal: 0 Relevant: 1	*Increased fetal weight from exposure* Thermal: 0 Relevant: 1	*Undetermined* Thermal: 0 Relevant: 5
No change 4 studies 1 thermal studies 3 relevant studies	*Increased fetal weight from exposure* Thermal: 0 Relevant: 1	*Undetermined* Thermal: 0 Relevant: 1	*Decreased fetal weight from exposure* Thermal: 1 Relevant: 1
Reduced 17 studies 10 thermal studies 7 relevant studies	*Undetermined* Thermal: 4 Relevant: 2	*Decreased fetal weight from exposure* Thermal: 1 Relevant: 3	*Decreased fetal weight from exposure* Thermal: 5 Relevant: 2

F.2: "Brain weight" meta-analysis subject to similar flaw

A similar analysis to our analysis of the relation between fetal weight and litter size, should have been carried out also as to brain weight:

It seems evident that as for fetal weight, litter size should also have an effect on brain weight: The higher the litter size, the lower the brain weight of each fetus must be. As noted, this impact is neither discussed, nor corrected for in EHC2023, and undermines the reliability of any findings as to impacts on brain weight in the review.

F.3: "Brain weight" chosen as proxy for physiological effects erroneously produces "no effect found" and is contradicted by conclusions in reviewed papers

Our check of "brain weight" led us to the following question: Is "brain weight" actually a fruitful endpoint at all for an analysis of health risk on fetuses? By examining the studies used for the "brain weight" endpoint, we found it is not. Nonetheless, in the EHC2023 meta-analysis the endpoint "brain weight" is considered to be a good marker for the endpoint "brain pathology".

EHC2023 writes:

> *3.5.2.3.1. Brain pathology. The weight of the brain or the cerebellum was considered the most representative marker of a possible RF-EMF impact on the central nervous system.*

On page 30 the authors underpin as follows their choice of basing their analysis of a possible RF-EMF impact on the central nervous system by using brain weight measurement as a proxy:

> *We decided to base the meta-analysis of delayed pathological effects only on the offspring brain upon weight measurement, because we considered the count of the cell number in histopathological sections a less standardized method.* [Emphasis added.]

We checked all the papers selected for the brain pathology meta-analysis in EHC2023 to see if any of them consider decreased brain weight a good indicator of changes in brain pathology. The papers do not support such an assumption:

- Bas2013 reports *increased* brain weight and significant changes in brain pathology in the form of statistically significant reduced number of pyramidal cells.
- Jensh1982a and b do not analyze brain pathology, but just report brain weights together with weight of other internal organs.
- Kubinyi1996 reports from two experiments, finding that enzyme isolated from the brain showed a significant decrease after exposure. Neither of the two experiments find any change in brain weight.
- Odaci2016 reports *increased* brain weight and statistically significant reduction of Purkinje cells.
- Sharma2017 is the only nonthermal study finding *decreased* brain weight, but did also report pathological changes in the form of cytoarchitechure of hippocampus and cerebellum and reduction in Purkinje cell number.

Based on these papers, which are the ones EHC2023 consider relevant for assessing the impact on brain physiology based on brain weight only, there is reason to believe that reduced brain weight is ***not*** a good indicator of detrimental changes in brain pathology – unless "no effect found" is an intended outcome: *Basing a final assessment on brain weight, as EHC2023 does, provides a useless result, and does so by the use of papers which report other serious brain related effects which are left unreported in the EHC2023.*

G: Subgroup analyses prescribed in the protocol as to important properties of RF-EMF exposure are *not* followed

The protocol prescribed for the EHC2023 meta-analysis is found in [Pacchierotti 2021], authored by the same 12 people as EHC2023, except for two co-authors. In the protocol, not only the topic of carrying out sub-group analyses for various biological endpoints is mentioned (such as those carried out in the papers selected for EHC2023), but also that sub-group analyses should be carried out as to such factors as specified here:

> *We will conduct the subgroup analysis if there are at least 3 studies per subgroup and we will assess whether the pooled effect sizes are statistically different […for …] the frequency, duration, modulation and level of exposure (e.g., acute vs protracted exposure or low vs high exposure level)*

EHC2023 diverges from the protocol by *not* doing sub-group analyses for any of these factors except level of exposure. In EHC2023 page 5, the authors argue for the sub-grouping chosen by referring to what they "considered the most likely to affect" the outcome, i.e., a preconception heavily based on "thermal thinking". Thus, what they describe as "a slight deviation from the protocol", is actually a prejudice restricting the methodology such that it effectively blocks the view from discovering nonthermal effects:

> T*o explore possible causes of heterogeneity, we conducted sub-group analyses according to animal species, exposure levels (SAR less than 0.1, 0.1 ≤ SAR less than 5, SAR ≥ 5 W/kg) and measurements of animal core temperature increase below or above 1 °C. We limited the subgroup analysis to these 3 variables, because they were considered the most likely to affect a possible association between exposure and outcomes and to keep the work manageable. This is a slight deviation from the protocol (see Section 4.5.2).* [Emphasis added.]

In EHC2023, Section 4.5.2, the authors comment as follows on their "slight deviation" limiting their investigation to their preconceived ideas of temperature increase as the "most likely" mechanism:

> *"Among the factors envisaged in the Protocol, we limited our heterogeneity analyses to exposure levels and dam core temperature increase, because these are the variables most likely affecting RFEMF biological effects, and experimental animal species, because inter-species consistency of results was to be considered as an upgrading factor for the certainty of evidence. We did not explore sources of heterogeneity by differences in tested radiofrequencies because only 3 endpoints (with no more than 3 studies each) were assessed at frequencies above 6000 MHz. This was the upper range in which a different mechanism of biological interaction might be expected because of short penetration depth into superficial tissues (a few mm or less)."* [Emphasis added.]

Here, circular reasoning and highly biased or uninformed statements are clearly expressed:

Firstly, it is obvious that high SAR and temperature increase are important factors in producing detrimental effects. Therefore, the present regulation of RF-EMF exposure sets limits to protect human mothers (and others) from such hazardous exposure. Hence, such subgroup analyses are worthless in assessing health risks for pregnant women: At real exposures in daily life and workplace situations, there are under normal circumstances no such exposures.

Secondly, it is circular reasoning to choose the parameters "exposure levels" and "core temperature increase" (SAR) in order to increase interspecies consistency and thereby higher certainty of

evidence: The reliability lies in choosing parameters for which one already knows there is a simple dose-effect relationship as to exposures and detrimental effects at thermal levels. By definition, such a narrowed perspective makes the study unable to find any simple dose-effect relationship at subthermal levels, neither to spot causes unrelated to (averaged) exposure intensities.

Thirdly, we find no explanation for not doing any sub-group analyses for "modulation and duration of exposure": Both these factors occurring in all real life RF exposure are since long well known to impact biology and health (Kostoff 2020, Lai & Lewitt 2022, Panagopoulos 2021) and are therefore relevant when assessing health risk for pregnant women. The impact on the endpoints from such varying properties of RF exposure is also seen in many of the nonthermal papers selected for the EHC2023's meta-analysis. The following papers analyzed by EHC2023 as to the "fetal weight" endpoint find that variations as to modulation, frequencies and/or durations have substantial impacts on the outcomes:

- Berman1978 report malformations at lowest SAR, i.e. 2 W/kg, not 7, 8.1 or 22.2.
- Brown-Woodman 1988b - from Abstract: "*Reabsorbed fetuses, in particular lower frequency pulses: "This effect was greatest in rats irradiated for 60 min at 10 Hz [pulses] (SAR=2.8 W/kg). ... Longer durations of exposure at lower pulse repetition frequencies resulted in a greater percentage of resorptions than shorter durations at higher frequencies.*" The study also reports reabsorbed fetuses, in particular for the mothers exposed to the *lowest* SAR-intensity and the *lowest* pulse frequency. In the meta-analysis such data were concealed by averaging the values.
- Kubinyi1996 - from Abstract: "*The activity of enzyme isolated from the brain showed a significant decrease after CW MW exposure, but the changes were not significant after 50 Hz AM MW exposure, The activity of the enzyme isolated from liver increased under CW and 50 Hz modulated MW*."
- Sharma2017 reports reduced Purkinje cell numbers, most so in the dams exposed with the longest duration.
- Alchalabi2016 - from Abstract: *"In-utero irradiation for 2 and three weeks induced oxidative stress in pregnant rats."*
- Berman1992 tested the effect of three SAR levels: 0.07, 2.4 and 4.8 W7kg, reporting effects found only at 4.8 W/kg, which they *assume being hypothermal.*
- Inalotz 1997 tested two exposure conditions: daily exposures during pregnancy of 30 min. at SAR 1.9 W/kg and 15. min. at 3.9 W/kg. They report differences in results, with the most effects found after the longest exposure with the lowest SAR, i.e., 30 min. at 1.9 W/kg daily exposure.

Some of the papers also report different effects for male and female pups. No such sub-group analyses are mentioned in the protocol, nor in EHC2023.

H: Regulatory bodies addressed with misinformation concealing "moderate and large detrimental effects"

The findings of detrimental effects on brain physiology, not mentioned in EHC2023 Table 3, are so strikingly clear that they should be mentioned elsewhere in EHC2023. They are mentioned for studies not included in the meta-study. Under the heading *"3.5.2.3.1. Brain pathology",* discussing the "brain weight" endpoint, we find the following (EHC2023 p.25; for the references see EHC2023; emphasis added):

> *In addition to brain or cerebellum weight, 7 papers reported data on the number or density of neural cells in specific portions of the central nervous system (Albert et al., 1981, Bas et al., 2013, Erdem Koç et al., 2016, Keles and Sut, 2021, Odaci et al., 2016, Sharma et al., 2017, Stasinopoulou et al., 2016). Four studies were classified at "some concern" and 3 at "high concern" for overall study RoB.* ***All these papers reported a significant decrease in cell number or cell density at whole body average SARs ranging between 0.01 and 2 W/kg.*** *When organ weight was also measured, two papers reported no impact on brain (Bas et al., 2013) or on cerebellum (Odaci et al., 2016), while 1 paper (Sharma et al., 2017) reported a decrease of brain weight. Considering the difference in the methods used to assess brain pathology, the difference between the results of the meta-analysis on brain weight and the results of the studies measuring neuronal cell numbers is not a surprise.*

Note that these are all nonthermal studies, and together with the 4 papers (also nonthermal) in the meta-analysis finding reduced cell numbers, they amount to a total of 11 papers, all reporting significant physiological changes, *all* of the newer, nonthermal studies selected for this endpoint!

We investigated how these important findings are handled further in the EHC2023 discussions leading to the EHC2023 conclusion of "no conclusion" and recommendations for policy decisions:

The "brain pathology" endpoint is reported in a group of five analyzed endpoints handled as a whole under the heading *"Delayed effects on the offspring health".* Several of these endpoints have meta-analysis results showing high probability of detrimental effects. However, in section 4.1.3 EHC2023 presents a summary of the findings for these different endpoints (p. 30). There it is claimed that no indication of effects on brain weight is found. The reported physiological changes are not mentioned. For the endpoint "Female fertility" there are mainly thermal studies finding no effect. The remaining three out of the five endpoints are presented as follows (bullet point formatting and emphasis added):

- *RF-EMF exposure during pregnancy may* ***have a moderate detrimental effect*** *on learning and memory functions as measured by the maze escape latency time of the offspring (SMD 0.54, 95% CI - 1.24 to 0.17, 2 studies). However, we attributed a* ***very low certainty*** *to this result.*
- *RF-EMF exposure during pregnancy* ***may have a large detrimental effect*** *on motor activity functions as measured by the endurance time of the offspring in any type of test (SMD 0.79, 95% CI 0.21 to 1.38, 4 studies) but, again,* ***we are uncertain*** *of the result.*
- *RF-EMF exposure during pregnancy* ***may have a moderate to large detrimental effect*** *on motor and sensory functions as measured by the magnitude of the startle response of the offspring (SMD -0.66, 95% CI - 1.18 to- 0.14, 2 studies), but* ***we are uncertain of this result****.*

We observe that the uncertainty is attributed to “High Concern” RoB and that the large majority of these studies are newer, nonthermal studies finding effects, except from some older studies typically from 1982/84 and all with Jensh as first author.

The above reported results and the fact that all newer studies of brain pathology find detrimental effects, are clear indications that there is good reason to take precautionary steps to prevent *“delayed effects on the offspring health”*.

With the expressed uncertainty claimed in EHC2023, we expected such an advice on precaution to be uttered together with other results in the final conclusion of EHC2023: *”4.4 Implication for policy and research”* – together with the need for further research. We found absolutely no such mention. However, several of the endpoints finding no effects were presented, some even with arguments for why there is no reason to expect effects in everyday situations. E.g., as for the “female fertility” endpoint, which found no effect on the litter size of mothers who were exposed before they were themselves born, EHC2023 claim (emphasis added):

> ***The whole body average SAR values in the included experiments are well above the recommended human exposure limit values for the general public set by international bodies (ICNIRP 2020)****. Actual SAR values experienced by the public in the general environment are below, and in most cases, well below, the recommended human exposure limit values. The dose effect meta-analyses contributed to support the results of the meta-analysis but were not supposed to define the shape of the dose–effect relationship or find a minimum exposure level at which a clear effect could be discerned.*

This is a typical downplaying of results and an assurance of no effects in everyday situations. Tthis statement does in no way hold for the other studies on delayed effects on the offspring, as the majority of these studies use exposure conditions far below the exposure limits of ICNIRP2020, and do find detrimental effects. However, these findings are omitted in total.

An overview of the section *”4.4 Implication for policy and research”* (EHC2023 p. 31) shows the above citation to be representative for the presentation and discussion of findings, arguing only for the uncertainty of the probability of detrimental effects as to the small effects found, while none of the endpoints with larger effects are even mentioned. Again, we see a clear downplaying of results finding detrimental effects, and here it is not even mentioned that such detrimental effects exist for the endpoints where the more serious effects are reported and precautionary measures are highly appropriate: the physiological and psychological detrimental effects on offspring from exposures far below exposure limits. By omitting and underplaying those findings which are the most relevant to the aim of the study - *“the adverse effects of RF-EMF exposure during pregnancy in offspring of experimental animals”* (citation from abstract EHC2023), EHC2023 misinforms the regulators, the target group for the WHO EHC project.

I: Summing up: The effect of correcting all flaws results in a huge increased confidence in the existence of detrimental effects from low SAR RF-EMF exposure

In addition to correcting errors, we investigated the impact on correcting the pooled effect size by choosing the experiments being most relevant to realistic exposure conditions for humans in their everyday life.

To select an approach for handling experiments with a shared control group, we followed the overarching advice from the Cochrane handbook, which is to arrive at a scientifically meaningful result. We therefore choose to add studies sharing a control group into the meta study by comparing the control group *with the experiment showing the most detrimental effect*, instead of averaging the studies:

The formal purpose of the EHC2023 meta-analysis is to assess if pregnant women could be at risk from wireless communication technology, not the average health risk inflicted on laboratory animals. It is obvious that women are and will be exposed to many different wireless technologies deployed and in use today and in the future. The exposure conditions are poorly known, not the least since there are and will be all kinds of interactions and interferences. We may expect, however, that their energy intensities are and will be *subthermal* and that exposures will be u*biquitous, frequent, varying and persistent*, and do and will not consist of just one single dose. In such a scenario, where pregnant women might be exposed to any of the exposure conditions found in the nonthermal experiments the most relevant cases are the worst cases, not the averages. The scientifically meaningful and ethically defensible approach would therefore be to include those experiments with subthermal exposure conditions having the most significant results. We therefore choose to extract the effects from studies sharing a control group by selecting the experiments showing the most detrimental effects, *not* by averaging.

When recalculating, we had to introduce relative weight to studies with other parameters than the original ones in EHC2023. As discussed above, relative weights assigned to individual studies is a highly important parameter when aggregating data in the form of a pooled Cohen's d and confidence interval: These weights determine how much the result from any specific study will impact the pooled result. For our analysis to be comparable to the one in EHC2023 Fig. 6, we used the relative weights assigned in Fig. 6. However, as correcting errors and flaws in some cases resulted in changes in the number of experimental animals and in fetal weight values, the relative weights of these studies were no longer applicable, and weights had to be re-calculated. However, since EHC2023 does not document how relative weights have been calculated, an exact recalculation could not be done. We therefore did a pragmatic adjustment, assigning relative weights when appropriate, trying to approximate a correct value.

To compare results and thus see the impact of the choices made in EHC2023, we did the following calculations for Cohen's d with 95% confidence interval for all *relevant* studies in Fig 6., i.e., fetal weight studies with SAR below 5 W/kg and with more than one single exposure dose:

1. A calculation true to EHC2023 Fig. 6, i.e., using the values and variations in formulas and approaches for the shared comparator studies as done in the EHC2023 meta-analysis. See left table in Figure 3 above and Figure 4 below. In Figure 6, represented by a red diamond (A).
2. To check the effects from correcting data and formulas, we did a repetition of the above calculation, but now with no errors in the cited data, and identical formulas used for all

studies. See right table in Figure 3 above and middle table in Figure 4 below. In Figure 6 below represented by the upper green diamond (B).

3. Then, for studies with common control groups, we chose the ones with the most reduced fetal weights (for reasons of simplification leaving out the abovementioned topic of *hormesis*, which, if handled, would utterly increase the effect). See right table in Figure 4 below. In Figure 6 below represented by the lower green diamond (C).
4. To exclude research not relevant to humans in daily exposure situations, we did a re-calculation of our second calculation, now without studies reporting SAR above 2 W/kg. See Figure 5 below. In Figure 6 below represented by the blue diamond (D).

As in EHC2023 Fig. 6				Correct data and consistent equations:			… and our choices for shared control:		
Study	Cohen's d	CI lower	CI upper	Cohen's d	CI lower	CI upper	Cohen's d	CI lower	CI upper
Berman1978-1	-0,08	-0,34	0,19	-0,08	-0,34	0,19	-0,08	-0,34	0,19
*Brown-W1988b-1	0,21	-0,66	1,09	0,21	-0,66	1,09	*0,20*	*-0,96*	*1,35*
*Brown-W1988b-2	0,03	-0,93	0,99	0,03	-0,93	0,99	*0,25*	*-0,77*	*1,27*
Dasdag2000	2,66	1,54	3,78	2,66	1,03	4,28	*2,66*	*1,03*	*4,28*
Galvin1986	-0,51	-1,41	0,38	-0,51	-1,41	0,38	*-0,16*	*-1,04*	*0,71*
Guler2010	-0,09	-1,02	0,83	-0,09	-1,02	0,83	-0,09	-1,02	0,83
Kubinyi1996-1	0,10	-0,45	0,65	0,10	-0,45	0,65	0,10	-0,45	0,65
Kubinyi1996-2	-0,08	-0,78	0,61	-0,08	-0,78	0,61	-0,08	-0,78	0,61
Lary1983a	-0,14	-0,63	0,34	-0,14	-0,63	0,34	-0,14	-0,63	0,34
Lee2009 -1	0,84	0,10	1,58	0,84	0,10	1,58	0,84	0,10	1,58
Lee2009 -2	0,27	-0,35	0,90	0,27	-0,35	0,90	0,27	-0,35	0,90
*Marcickiewicz1986	1,45	1,02	1,87	1,45	1,02	1,87	*1,20*	*0,72*	*1,67*
Merrit1984	0,06	-0,81	0,94	0,25	-0,63	1,13	0,25	-0,63	1,13
*Ogawa2009	-0,08	-0,61	0,46	-0,08	-0,61	0,46	-0,08	*-0,70*	*0,54*
Sambucci2010	0,00	-0,82	0,82	0,00	-0,82	0,82	0,00	-0,82	0,82
Schmidt1984	-0,05	-0,67	0,57	-0,05	-0,67	0,57	-0,05	-0,67	0,57
*Sharma2017	0,22	-0,76	1,21	*2,21*	*0,72*	*3,70*	*2,21*	*0,72*	*3,70*
				7,06	*3,77*	*10,35*	*7,06*	*3,77*	*10,35*
Smialowicz1981	-0,50	-1,13	0,13	-0,50	-1,13	0,13	-0,50	-1,13	0,13
Smialowicz1982	-0,52	-1,67	0,64	-0,52	-1,67	0,64	-0,52	-1,67	0,64
Stasinopolou2016-1	-0,44	-2,25	1,37	-0,44	-2,27	1,38	-0,44	*-2,27*	*1,38*
Stasinopolou2016-2	-1,16	-2,78	0,45	-1,16	-2,83	0,50	-1,16	*-2,83*	*0,50*
Stasinopolou2016-3	-2,91	-5,05	-0,76	-2,91	-5,20	-0,61	-2,91	*-5,20*	*-0,61*
Tofani1986-3	0,06	-0,70	0,82	0,06	-0,70	0,82	*0,39*	*-0,43*	*1,22*
*Alchalabi2016	3,20	2,07	4,33	3,20	2,07	4,33	*8,38*	*5,51*	*11,26*
Galvin1983	0,36	-0,34	1,06	0,36	-0,34	1,06	0,36	-0,34	1,06
*Inaloz1997	1,25	0,33	2,18	1,25	0,33	2,18	*3,16*	*1,63*	*4,68*
Jensh1982a	-0,53	-1,69	0,63	-0,53	-1,69	0,63	-0,53	-1,69	0,63
Jensh1983b	-0,44	-1,59	0,70	-0,44	-1,59	0,70	-0,44	-1,59	0,70
Jensh1982b	0,18	-1,09	1,46	0,19	-0,96	1,34	*0,19*	*-0,96*	*1,34*
Jensh1983a	1,44	0,06	2,83	1,44	0,04	2,85	*1,44*	*0,04*	*2,85*
*Li2020	0,88	-0,19	1,95	0,88	-0,19	1,95	*1,31*	*-0,19*	*2,80*
Sangun2015	-0,87	-2,34	0,60	0,87	-0,60	2,34	*0,87*	*-0,60*	*2,34*
*Wang2018	-0,19	-1,17	0,80	-0,19	-1,17	0,80	-0,19	-1,17	0,80
Meta-analysis	0,24	-0,06	0,55	0,54	0,24	0,84	0,78	0,48	1,07

Figure 4: Recalculation of pooled effect size and confidence intervals, doing various corrections. Left table original values for relevant studies in the fetal weight endpoint analysis of EHC2023 found in EHC2023 Fig. 6. Middle table: values when correctly cited and same equation used for all. Right table also include more relevant selection of experiments to include in the studies. Gray cells are changed values. Red names for papers indicate errors, while blue indicate changes in selecting studies. In total there are changes for the values of 18 of the 33 studies.

Fetal weight studies with SAR ≤ 2 W/kg

Study	Cohen's d	CI lower	CI upper
Berman1978-1	-0,08	-0,34	0,19
Brown-W1988b-1	0,25	-0,77	1,27
Dasdag2000	2,66	1,03	4,28
Galvin1986	-0,16	-1,04	0,71
Guler2010	-0,09	-1,02	0,83
Lary1983a	-0,14	-0,63	0,34
Lee2009 -1	0,84	0,10	1,58
Lee2009 -2	0,27	-0,35	0,90
Merrit1984	0,25	-0,63	1,13
Ogawa2009	-0,08	-0,70	0,54
Sambucci2010	0,00	-0,82	0,82
Schmidt1984	-0,05	-0,67	0,57
Sharma2017	2,21	0,72	3,70
Sharma2017	7,06	3,77	10,35
Stasinopolou2016-1	-0,44	-2,27	1,38
Stasinopolou2016-2	-1,16	-2,83	0,50
Stasinopolou2016-3	-2,91	-5,20	-0,61
Tofani1986-3	0,39	-0,43	1,22
Alchalabi2016	8,38	5,51	11,26
Galvin1983	0,36	-0,34	1,06
Inaloz1997 Group 2	1,71	0,54	2,88
Li2020	1,31	-0,19	2,80
Sangun2015	0,87	-0,60	2,34
Wang2018*	-0,19	-1,17	0,80
	1,01	0,65	1,37

Figure 5: Pooled Cohen's d with 95% confidence interval for studies with SAR <= 2 W/kg.

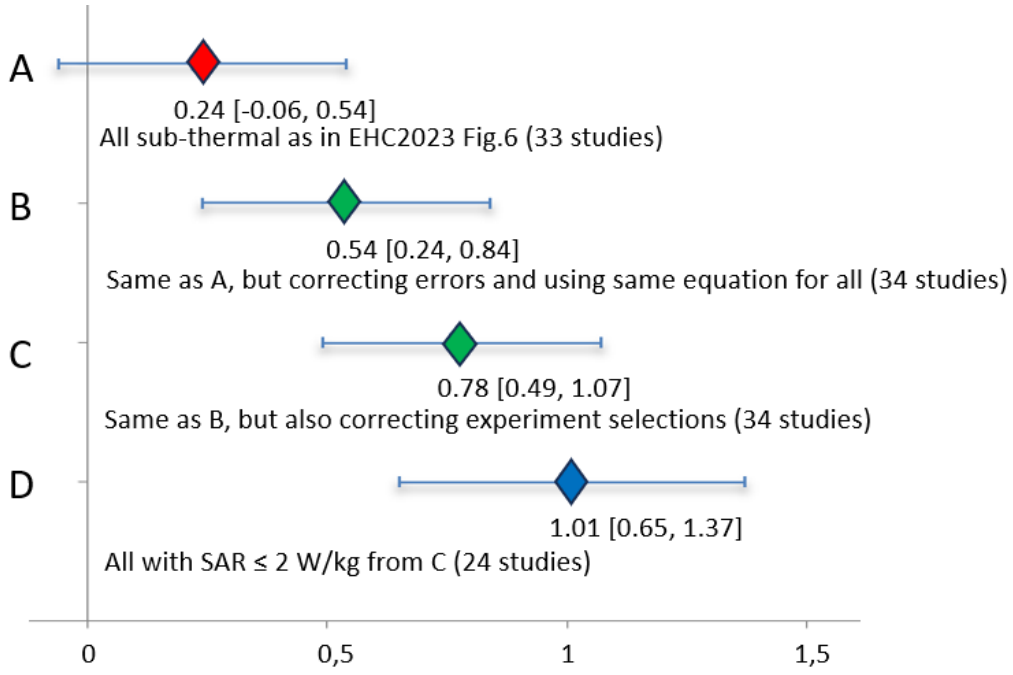

Figure 6: Forest plot of the pooled Cohen's d with 95% confidence intervals. See text for explanations.

References

Cordelli, E., Ardoino, L, Benassi, B., Consales, C., Eleuteri, P, Marino, C., Sciortino, M., Villani, P., Brinkworth, M.H., Chen, G., McNamee, J.P., Wood, A.W., Belackova, L., Verbeek, J., Pacchierotti, F., Effects of Radiofrequency Electromagnetic Field (RF-EMF) exposure on pregnancy and birth outcomes: A systematic review of experimental studies on non-human mammals, Environment International, 2023, 108178, ISSN 0160-4120, https://doi.org/10.1016/j.envint.2023.108178.

Higgins JPT, Thomas J, Chandler J, Cumpston M, Li T, Page MJ, Welch VA (editors), 2022, Cochrane Handbook for Systematic Reviews of Interventions version 6.3 (updated February 2022). Cochrane. Available from www. training.cochrane.org/handbook.

Kostoff, R.N., Heroux, P., Aschner, M., Tsatsakis, A., 2020. Adverse Health Effects of 5Gg Mobile Networking Technology under Real-Life Conditions. Toxicology Letters. Volume 323, 2020, Pages 35-40, ISSN 0378-4274, https://doi.org/10.1016/j.toxlet.2020.01.020.

Lai, H., Levitt. B.B.:, 2022.The roles of intensity, exposure duration, and modulation on the biological effects of radiofrequency radiation and exposure guidelines, Electromagnetic Biology and Medicine, 2022, Vol. 41, No. 2, 230-241

Marino, Andrew: Going somewhere – Truth about a life in science, Cassandra Publishing, 2010, ISBN: 978-0981854915

Pacchierotti, F., Ardoino, L., Benassi, B., Consales, C., Cordelli, E., Eleuteri, P., Marino, C., Sciortino, M., Brinkworth, M.H., Chen, G., McNamee, J.P., Wood, A.W., Hooijmans, C.R., de Vries, R.B.M., Effects of Radiofrequency Electromagnetic Field (RF-EMF) exposure on male fertility and pregnancy and birth outcomes: Protocols for a systematic review of experimental studies in non-human mammals and in human sperm exposed in vitro, Environment International, Volume 157, 2021, 106806, ISSN 0160-4120, https://doi.org/10.1016/j.envint.2021.106806.

Panagopoulos DJ, Karabarbounis A, Yakymenko I, Chrousos GP. Human-made electromagnetic fields: Ion forced-oscillation and voltage-gated ion channel dysfunction, oxidative stress and DNA damage (Review). Int J Oncol. 2021 Nov;59(5):92. doi: 10.3892/ijo.2021.5272. Epub 2021 Oct 7. PMID: 34617575; PMCID: PMC8562392. (Open Access)

Romero A, Villamayor F, Grau MT, Sacristán A, Ortiz JA. Relationship between fetal weight and litter size in rats: application to reproductive toxicology studies. Reprod Toxicol. 1992;6(5):453-6. doi: 10.1016/0890-6238(92)90009-i. PMID: 1463926.

Turner, H.M., Robert M. Bernard, R.M., 2006, Calculating and Synthesizing Effect Sizes, Contemporary Issues in Communication Science and Disorders, Vol. 33, p42-55. https://doi.org/10.1044/cicsd_33_S_42

Papers referenced in the EHC2023 fetal weight endpoint analysis

Alchalabi, A.S.H., Aklilu, E., Aziz, A.R., Malek, F., Ronald, S.H., Khan, M.A., 2016. Different periods of intrauterine exposure to electromagnetic field: Influence on female rats' fertility, prenatal and postnatal development. Asian Pacific Journal of Reproduction 5, 14–23. https://doi.org/10.1016/j.apjr.2015.12.003.

Berman, E., Carter, H.B., 1984. Decreased body weight in fetal rats after irradiation with 2450-MHz (CW) microwaves. Health Phys. 46, 537–542. https://doi.org/10.1097/ 00004032-198403000-00004.

Berman, E., Kinn, J.B., Carter, H.B., 1978. Observations of mouse fetuses after irradiation with 2.45 GHz microwaves. Health Phys. 35, 791–801. https://doi.org/10.1097/00004032-197812000-00006.

Berman, E., Carter, H.B., House, D., 1981. Observations of rat fetuses after irradiation with 2450-MHz (CW) microwaves. J. Microw. Power 16, 9–13. https://doi.org/10.1080/16070658.1981.11689226.

Berman, E., Carter, H.B., House, D., 1982a. Reduced weight in mice offspring after in utero exposure to 2450-MHz (CW) microwaves. Bioelectromagnetics 3, 285–291. https://doi.org/10.1002/bem.2250030212.

Berman, E., Carter, H.B., House, D., 1982b. Observations of Syrian hamster fetuses after exposure to 2450-MHz microwaves. J. Microw. Power 17, 107–112. https://doi.org/10.1080/16070658.1982.11689270.

Berman, E., Carter, H.B., House, D., 1984. Growth and development of mice offspring after irradiation in utero with 2,450-MHz microwaves. Teratology 30, 393–402. https://doi.org/10.1002/tera.1420300312.

Berman, E., Weil, C., Philips, P.A., Carter, H.B., House, D.E., 1992. Fetal and maternal effects of continual exposure of rats to 970-mhz circularly-polarized microwaves. Electromagn. Biol. Med. 11, 43–54. https://doi.org/10.3109/15368379209012851.

Brown-Woodman, P.D.C., Hadley, J.A., 1988a. Teratogenic Effects of Exposure to Radiofrequency Radiation (27.12 MHz) from a Shortwave Diathermy Unit. Ind. Health 26, 1–10. https://doi.org/10.2486/indhealth.26.1.

Brown-Woodman, P.D.C., Hadley, J.A., 1988b. Studies of the Teratogenic Potential of Exposure of Rats to 27.12 MHZ Pulsed Shortwave Radiation. Journal of Bioelectricity 7, 57–67. https://doi.org/10.3109/15368378809016177

Chernovetz, M.E.; Justesen, D.R.; Oke, A.F. 1977; A teratological study of the rat: Microwave and infrared radiations compared. Radio Science 12: 191-197. doi:10.1029/RS012i06Sp00191

Dasdag, S., Akdag, Z., Ayyildiz, O., Demirtas, O.C., Yayla, M., Sert, C., 2000. Do cellular phones alter blood parameters and birth weight of rats? Electro Magnetobiol 19, 107–113.

Galvin, M.J., MacNichols, G., McRee, D.I., 1983. Effects of 2450 MHz microwave radiation during the gestational period on the postnatal hematology of rats. Cell Biophys. 5, 33–41. https://doi.org/10.1007/bf02788585.

Galvin, M.J., Tilson, H.A., Mitchell, C.L., Peterson, J., McRee, D.I., 1986. Influence of pre- and postnatal exposure of rats to 2.45-GHz microwave radiation on neurobehavioral function. Bioelectromagnetics 7, 57–71. https://doi.org/10.1002/bem.2250070107.

Guler, G., Tomruk, A., Ozgur, E., Seyhan, N., 2010. The effect of radiofrequency radiation on DNA and lipid damage in non-pregnant and pregnant rabbits and their newborns. Gen. Physiol. Biophys. 29, 59–66. https://doi.org/10.4149/gpb_2010_01_59.

Inaloz, S.S., Dasdag, S., Aslan, A., Bilici, A., Yayla, M., 1997. The effects of the microwave oven on pregnant and newborn rat kidneys. Urogynaecologia International Journal 11, 9–15. Found paper here:

https://www.researchgate.net/publication/323538185_The_effects_of_the_microwave_oven_on_pregnant_and_newborn_rat_kidneys

Jensh, R.P., Vogel, W.H., Brent, R.L., 1982a. Postnatal functional analysis of prenatal exposure of rats to 915 MHz microwave radiation. J. Am. Coll. Toxicol. 1, 73–90. https://doi.org/10.3109/10915818209018019.

Jensh, R.P., Weinberg, I., Brent, R.L., 1982b. Teratologic studies of prenatal exposure of rats to 915-MHz microwave radiation. Radiat. Res. 92, 160–171. https://doi.org/10.2307/3575851.

Jensh, R.P., Weinberg, I., Brent, R.L., 1983a. An evaluation of the teratogenic potential of protracted exposure of pregnant rats to 2450-MHz microwave radiation: I. Morphologic analysis at term. J. Toxicol. Environ. Health 11, 23–35. https://doi.org/10.1080/15287398309530318.

Jensh, R.P., Vogel, W.H., Brent, R.L., 1983b. An evaluation of the teratogenic potential of protracted exposure of pregnant rats to 2450-MHz microwave radiation. II. Postnatal psychophysiologic analysis. J. Toxicol. Environ. Health 11, 37–59. https://doi.org/10.1080/15287398309530319.

Jensh, R.P., 1984a. Studies of the teratogenic potential of exposure of rats to 6000-MHz microwave radiation. II. Postnatal psychophysiologic evaluations. Radiat. Res. 97, 282–301.

Kubinyi, G.; Thuroczy, G.; Bakos, J.; Boloni, E.; Sinay, H.; **Szabo**, L.D. 1996; Effect of continuous-wave and amplitude-modulated 2.45 GHz microwave radiation on the liver and brain aminoacyl-transfer RNA synthetases of in utero exposed mice. Bioelectromagnetics 17: 497-503. doi:10.1002/(SICI)1521-186X(1996)17:6<497::AID-BEM10>3.0.CO;2-I.

Lary, J.M., Conover, D.L., Foley, E.D., Hanser, P.L., 1982. Teratogenic effects of 27.12 MHz radiofrequency radiation in rats. Teratology 26, 299–309. https://doi.org/10.1002/tera.1420260312.

Lary, J.M., Conover, D.L., Johnson, P.H., 1983. Absence of embryotoxic effects from lowlevel (nonthermal) exposure of rats to 100 MHz radiofrequency radiation. Scand. J. Work Environ. Health 9, 120–127. https://doi.org/10.5271/sjweh.2434. **[Termed 1983a in EHC2023 text]**

Lary, J.M.; Conover, D.L.; Johnson, P.H.; Burg, J.R. 1983b; Teratogenicity of 27.12-MHz radiation in rats is related to duration of hyperthermic exposure. Bioelectromagnetics 4: 249-255. doi:10.1002/bem.2250040305.

Lary, J.M., Conover, D.L., Johnson, P.H., Hornung, R.W., 1986. Dose-response relationship between body temperature and birth defects in radiofrequency-irradiated rats. Bioelectromagnetics 7, 141–149. https://doi.org/10.1002/bem.225007020 **Error in reference. Should be: https://doi.org/10.1002/bem.2250070205 (missing last 5)**

Lee, H.J., Lee, J.S., Pack, J.K., Choi, H.D., Kim, N., Kim, S.H., Lee, Y.S., 2009. Lack of teratogenicity after combined exposure of pregnant mice to CDMA and WCDMA radiofrequency electromagnetic fields. Radiat. Res. 172, 648–652. https://doi.org/10.1667/rr1771.1.

Li, Z.Q., Zhang, Y., Wan, Y.M., Zhou, Q., Liu, C., Wu, H.X., Mu, Y.Z., He, Y.F., Rauniyar, R., Wu, X.N., 2020. Testing of behavioral and cognitive development in rats after prenatal exposure to 1800 and 2400 MHz radiofrequency fields. J. Radiat. Res. 61, 197–206. https://doi.org/10.1093/jrr/rrz097.

Marcickiewicz, J., Chazan, B., Niemiec, T., Sokolska, G., Troszy´nski, M., Luczak, M., Szmigielski, S., 1986. Microwave radiation enhances teratogenic effect of cytosine arabinoside in mice. Biol. Neonate 50, 75–82. https://doi.org/10.1159/000242571. Merritt, J.H., Hardy, K.A., Chamness, A.F.,

1984. In utero exposure to microwave radiation and rat brain development. Bioelectromagnetics 5, 315–322. https://doi.org/10.1002/bem.2250050304.

Merritt, J.H., Hardy, K.A., Chamness, A.F., 1984. In utero exposure to microwave radiation and rat brain development. Bioelectromagnetics 5, 315–322. https://doi.org/10.1002/bem.2250050304.

Nawrot, P.S., McRee, D.I., Staples, R.E., 1981. Effects of 2.45 GHz CW microwave radiation on embryofetal development in mice. Teratology 24, 303–314. https://doi.org/10.1002/tera.1420240309.

Nawrot, P.S., McRee, D.I., Galvin, M.J., 1985. Teratogenic, biochemical, and histological studies with mice prenatally exposed to 2.45-GHz microwave radiation. Radiat. Res. 102, 35–45. https://doi.org/10.2307/3576427.

Nelson, B.K., Conover, D.L., Brightwell, W.S., Shaw, P.B., Werren, D., Edwards, R.M., Lary, J.M., 1991. Marked increase in the teratogenicity of the combined administration of the industrial solvent 2-methoxyethanol and radiofrequency radiation in rats. Teratology 43, 621–634. https://doi.org/10.1002/tera.1420430618.

Ogawa 2009, K., Nabae, K., Wang, J., Wake, K., Watanabe, S.I., Kawabe, M., Fujiwara, O., Takahashi, S., Ichihara, T., Tamano, S., Shirai, T., 2009. Effects of gestational exposure to 1.95-gHz W-CDMA signals for IMT-2000 cellular phones: Lack of embryotoxicity and teratogenicity in rats. Bioelectromagnetics 30, 205–212. https://doi.org/10.1002/bem.20456

Poulletier de Gannes, F., Haro, E., Hurtier, A., Taxile, M., Athane, A., Ait-Aissa, S., Masuda, H., Yann Percherncier, Ruffié, G., Billaudel, B., Dufour, P., Veyret, B., Lagroye, I., 2012, Effect of In Utero Wi-Fi Exposure on the Pre- and Postnatal Development of Rats, Birth Defects Research, Volume 95, Issue 2, April 2012, pages 130-136

Sambucci 2010, M., Laudisi, F., Nasta, F., Pinto, R., Lodato, R., Altavista, P., Lovisolo, G.A., Marino, C., Pioli, C., 2010. Prenatal exposure to non-ionizing radiation: Effects of WiFi signals on pregnancy outcome, peripheral B-cell compartment and antibody production. Radiat. Res. 174, 732–740. https://doi.org/10.1667/RR2255.1

Schmidt, R.E., Merritt, J.H., Hardy, K.H., 1984. In utero exposure to low-level microwaves does not affect rat foetal development. Int. J. Radiat. Biol. Relat. Stud. Phys. Chem. Med. 46, 383–386. https://doi.org/10.1080/09553008414551561.

Sharma, A., Kesari, K.K., Saxena, V.K., Sisodia, R., 2017. The influence of prenatal 10 GHz microwave radiation exposure on a developing mice brain. Gen. Physiol. Biophys. 36, 41–51. https://doi.org/10.4149/gpb_2016026

Smialowicz, R.J., Ali, J.S., Berman, E., Bursian, S.J., Kinn, J.B., Liddle, C.G., Reiter, L.W., Weil, C.M., 1981. Chronic exposure of rats to 100-MHz (CW) radiofrequency radiation: Assessment of biological effects. Radiat. Res. 86, 488–505. https://doi.org/10.2307/3575465.

Smialowicz, R.J., Weil, C.M., Kinn, J.B., Elder, J.A., 1982. Exposure of rats to 425-MHz (cW) radiofrequency radiation: effects on lymphocytes. J. Microw. Power 17, 211–221. https://doi.org/10.1080/16070658.1982.11689282.

Stasinopoulou, M., Fragopoulou, A.F., Stamatakis, A., Mantziaras, G., Skouroliakou, K., Papassideri, I.S., Stylianopoulou, F., Lai, H., Kostomitsopoulos, N., Margaritis, L.H., 2016. Effects of pre- and

postnatal exposure to 1880–1900MHz DECT base radiation on development in the rat. Reprod. Toxicol. 65, 248–262. https://doi.org/10.1016/j.reprotox.2016.08.008.

Sangun 2015, O., Dundar, B., Darici, H., Comlekci, S., Doguc, D.K., Celik, S., 2015. The effects of long-term exposure to a 2450 MHz electromagnetic field on growth and pubertal development in female Wistar rats. Electromagn. Biol. Med. 34, 63–71. https://doi.org/10.3109/15368378.2013.871619.

Tofani, S., Agnesod, G., Ossola, P., Ferrini, S., Bussi, R., 1986. Effects of continuous lowlevel exposure to radiofrequency radiation on intrauterine development in rats. Health Phys. 51, 489–499. https://doi.org/10.1097/00004032-198610000-00008

Wang, L.X., Huang, H.H., Lv, G.R., 2018. cts of prenatal exposure of 850–1900MHz mobile phone on the expression of PCNA and DCX in dentate gyrus of offspring rats. Zhongguo Ying Yong Sheng Li Xue Za Zhi 34, 35–38. https://doi.org/10.12047/j.cjap.5592.2018.010. **Japanese, unavailable except abstract**