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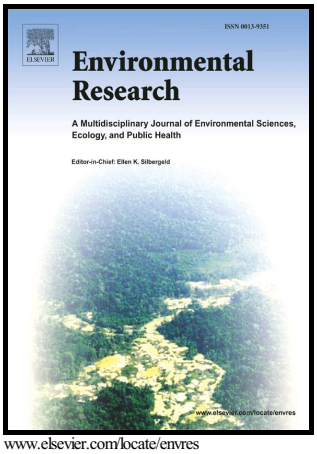

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Response to Pall, "Wi-Fi is an important threat to human health"

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Pall (2018) criticizes our 5-year-old review of studies related to Wi-Fi and health (Foster and Moulder 2013). We respond to his critique, and also note weaknesses in his selection and interpretation of studies on biological and health effects of Wi-Fi type signals.

Pall writes "The Foster and Moulder (2013) paper argues that there are no and cannot be any health effects of Wi- Fi". That was not our argument at all. We wrote: "while some effects have been reported [from Wi-Fi – type exposures], technical limitations in the studies make them difficult to interpret, and artifacts cannot be excluded…". Impossibility arguments are difficult to sustain in biology and we refrain from making them.

Pall writes: "The first 7½ pages of the paper are, however, largely irrelevant to that issue [of health effects of Wi-Fi]". Irrelevant? The standard model of health risk assessment, deriving from the famous "Red Book" of 1983 (National Research Council 1983) and adopted by the World Health Organization[1] and other health agencies, includes four main components: hazard identification (Pall's focus), hazard characterization, exposure assessment (the main topic in those first pages), and risk characterization. These are separate elements of the risk assessment process. Assessing RF exposures from Wi-Fi technology, in particular from wireless local area networks (WLANS), is a complex problem, and we reviewed major research on that topic in addition to bioeffects research.

If the goal is to identify possible health effects of electromagnetic field exposures from Wi-Fi technology, it is important to quantify the exposure levels to the subject, which is not a trivial matter. Simple proximity to Wi-Fi clients is not sufficient both because of the very uneven radiation pattern and because of the variable nature of the transmissions. Wi-Fi is not a distinctive physical agent but a brand name for wireless networking devices that are compliant with the IEEE 802.11 family of standards. Wi-Fi devices currently on the market operate using several modulation characteristics in bands near 2.45, 5.2 and 5.9 GHz, but other frequency bands are defined by IEEE 802.11x Wi-Fi enabled devices transmit pulsed RF signals at peak power levels that are generally similar to those used by cell phones and other wireless communications devices, but typically at very low duty cycles.

While the RF exposure levels from Wi-Fi clients and WLANs are invariably far below accepted international exposure limits, they are highly variable. Because of their low potential exposure to RF energy compared to cellular telephones, very few studies have examined possible bioeffects of RF fields produced by Wi-Fi technology. This contrasts with thousands of bioeffects and exposure assessment studies related to cell phones,[2] most commonly at frequencies between 800 and 1950 MHz.

We focus on Pall's Table 1 ("Summary of health impacts of Wi-Fi EMF exposures") and related discussion, which extends our 2013 review. We do not critique his biophysical theories, which were not discussed in our 2013 review and would require a lengthy analysis to consider fairly.

[1] http://www.who.int/foodsafety/micro/riskassessment/en/
[2] https://www.emf-portal.org/

Our 2013 review was based on as complete a search as we could do to identify bioeffects studies using Wi-Fi signals. We initially "focus[ed] on studies that were peer-reviewed and that had well-defined exposure systems and dosimetry". Finding few such studies, we expanded the scope to include studies "that were not peer-reviewed, or which had apparent technical deficiencies". We divided the studies into two groups according to our judgment of the quality of exposure assessment. In addition, we applied risk-of-bias criteria that are typically used by health agencies in reviewing the RF bioeffects literature.

The first group of 7 studies (with relatively adequate exposure assessment) reported no statistically significant (alternatively, in some studies, no deleterious) effects. These conclusions were abbreviated in our Table 4 as "no effects", which perhaps Pall misinterpreted as asserting the truth of the null hypothesis, which is unprovable in any event. By contrast, every one of the second group of 8 studies (with generally much poorer methodological quality) reported effects of some sort for some endpoints. The studies were generally small and appeared to be exploratory in nature; many had obvious deficiencies in statistical analysis and did not measure dose-response relations for reported effects.

In his Table 1, Pall cites an additional 16 studies involving Wi-Fi exposures that appeared after our review. Most were animal studies, but the group included one human and one epidemiology study as well. He characterizes their conclusions in a table of "health impacts of Wi-Fi EMF exposures" (his Table 1).

In his section 9, Pall (2018) presents a long critique of our review. He criticizes several of the negative studies we cited as not using "genuine" Wi-Fi. However, those studies used signals derived from Wi-Fi devices and thus were Wi-Fi signals by definition (although they may have been amplified and used in better controlled exposure systems than possible with Wi-Fi clients). Pall cites a French study (Poulletier de Gannes et al. (2012)) as using continuous-wave RF signals despite its title indicating Wi-Fi exposure. That study did not state the precise source of the RF energy (a companion paper by the same authors described the source as a Wi-Fi client.) We confirmed with the senior author (Lagroye) that both studies in fact used signals derived from Wi-Fi clients.

However, Pall's criticism applies to his own review. About half of the 16 studies in his Table 1 used 2.45 GHz energy modulated with 217 Hz pulses, which is not used in Wi-Fi networks, but rather is characteristic of GSM cellular transmissions and is characterized by much higher duty cycles of transmission than typical of Wi-Fi clients.

Pall draws much stronger conclusions from the same set of studies; namely that the negative studies that we cited had insufficient power to detect real effects, and the positive studies demonstrated real health effects of Wi-Fi.

Apart from its inconsistency, Pall's critique fails on several accounts:

Inaccuracies

For example: Pall cites Lee et al. (2014) as showing that Wi-Fi causes "growth stimulation of adipose stem cells (role in obesity?)". That study exposed human adipose-derived stem cells to Wi-Fi radiation for 5 days from a smartphone mounted just beneath the culture dishes. But the authors drew opposite conclusions to Pall's: "we could not find any harmful effects of Wi-Fi electromagnetic signals from

smartphones". Lee et al. did report an increased growth rate in the exposed cells, which they attributed to the 2 ^{o}C temperature increase produced by the cell phone; thermal controls produced similar effects.

Another example: Pall cites Yildirim et al. (2015) (the only epidemiology study in the group) as showing that Wi-Fi EMF exposure causes "sperm/testicular damage, male infertility". That study compared sperm parameters from men attending a fertility clinic as related to self-reported Internet and cell phone usage. It did not evaluate "sperm/testicular damage, male infertility" and there is no basis to conclude anything about "Wi-Fi EMF exposures" (which were not assessed in the study) as related to these problems.Nor does Yildirim et al. (2015) demonstrate of any sort of harm associated with Wi-Fi usage. Table 4 of that study compares 8 demographic and sperm parameters. Two (total motile sperm count, and progressive motile sperm counts) showed statistically significant ($p<0.05$) differences as related to Wi-Fi vs landline Internet access. However, the differences in the group means in both cases were much smaller than the standard deviations. We calculated Cohen's d of 0.16 for both results (a standard statistical test indicating a small effect). Both sets of sperm counts were within the normal range (Hamilton et al., 2015). Moreover, Yildirim et al did not correct for multiple comparison issues, which further clouds the interpretation of their results.

Confirmation bias and cherry picking: Selective use of sources, colloquially known as cherry picking, is a major fallacy in public debate. Pall does not state his criteria for including or excluding studies from his Table 1 or for evaluating the studies. It appears that he selected studies reporting biological effects of some kind while disregarding negative studies. He is hypercritical of "negative" studies that we cited (which were generally superior in their methodological quality despite limitations including small size) and far less critical of the "positive" studies that we or Pall cited in our respective reviews.

An example of cherry picking: Pall's Table 1 includes Papageorgiou et al. (2011) that reported effects of Wi-Fi on the amplitude of the P300 evoked responses in humans, but not a comparatively much stronger study by Zentai (2015) that failed to find effects of Wi-Fi signals on spontaneous EEG activity. His Table 1 omits roughly half of the studies that we cited in our review as well as some relatively strong recent studies (Woelders et al. 2017; Zentai 2015) – none of which reported statistically-significant effects of exposure.

Lack of distinction between biological and health effects: The World Health Organization noted (in its environmental criteria report of biological effects of power frequency fields):

> Before identifying any actual health hazards, it is useful to clarify the difference between a biological effect and an adverse health effect. A biological effect is any physiological response to… fields. Some biological effects may have no influence on health, some may have beneficial consequences, while others may result in pathological conditions, i.e. adverse health effects. WHO, 2007).

One standards setting committee noted "An adverse health effect causes detectable impairment of the health of the exposed individual or of his or her off-spring; a biological effect, on the other hand, may or may not result in an adverse health effect." (Ahlbom, 1998). Few if any of the studies that either we or Pall cited are standard risk assessment studies, and the endpoints examined are difficult or impossible to relate to human health effects. The one epidemiology study (Yildirim (2015) yields no information about health effects of Wi-Fi for reasons stated above.

Acceptance of scientifically and statistically weak studies as proof of real effects. Throughout his review, Pall bases his arguments on "statistically significant" differences ($p<0.05$) between exposed and unexposed groups. For years, statisticians have been pointing out the unreliability of null hypothesis significance testing and the p-value for identifying real phenomena.

A small p value means that observed differences between two groups are improbable assuming the null hypothesis, i.e. that there is precisely no effect. However, as Gelman and Carlin (2017) point out:

> A common conceptual error is that researchers take the rejection of a straw-man null as evidence in favor of their preferred alternative. A standard mode of operation goes like this: $p < 0.05$ is taken as strong evidence against the null hypothesis… [however] a low p-value is not necessarily strong evidence against the null, a high p-value does not necessarily favor the null (the strength and even the direction of the evidence depends on the alternative hypotheses), and p-values are in general not measures of the size of any underlying effect. But these errors persist… (Gelman and Carlin 2017).

Colquhoun (2014) remarked: "If you use $p = 0.05$ to suggest that you have made a discovery, you will be wrong at least 30% of the time. If, as is often the case, experiments are underpowered, you will be wrong most of the time". He notes that this is "the most optimistic view possible" since it assumes an unbiased study design. The criteria he lists for unbiased design (all negative results are published, and there is a single pre-specified outcome of a study to rule out multiple comparison issues) are hardly or not at all addressed by most of the studies he cites, few of which correct for multiple comparisons.

Pall complains about "low statistical power" in the negative studies listed in our Table 4. But that is true to a greater or lesser extent of virtually all of the studies listed in both our and Pall's reviews. That criticism cuts both ways. An underpowered study has an increased risk of missing a real effect of a stated size. But it also has increased risk of a false positive, incorrectly rejecting the null hypothesis (no effect). For a simple discussion of this problem see Christley (2010).

Inadequate experimental design is another significant problem with many of the studies that we and Pall cite. Health agency reviews of the RF bioeffects literature typically give little weight to studies with poor exposure assessment, lack of concurrent sham controls, lack of blinding, lack of control for thermal effects, or other deficiencies.

For example, the Swedish Radiation Safety Authority's Scientific Council on Electromagnetic Fields (Danker-Hope, 2017) states:

> Without dosimetric information, any effects cannot be related to an exposure level and without a sham-exposed group it is not possible to attribute any effects to the actual EMF exposure.
>
> As in previous years, a number of studies had to be excluded from the evaluation due to poor quality and missing information. Most of the excluded studies provided no, or incomplete, dosimetric information, or failed to include sham-exposed controls. Without dosimetric information, any effects cannot be related to an exposure level and without a sham-exposed group it is not possible to attribute any effects to the actual EMF exposure. It is very unfortunate that investigators are not adhering to international standards concerning the reporting of their studies, and that journals often do not have an adequate peer-review system that corrects such omissions. There can

> also be a risk that doing bad quality studies and making people afraid may have some impact on their health and well-being and is another reason why only studies with high quality protocols should be funded, performed and published.

Or, as Health Canada explains,

> "Poorly conducted studies (e.g. inadequate exposure evaluation, lack of appropriate control samples or inadequate statistical analysis), receive relatively little weight, while properly conducted studies (e.g. all controls included, appropriate statistics, complete exposure evaluation) receive more weight."[3]

Many, if not most, of the additional studies that Pall includes in his Table 1 (and elsewhere in his paper) have been given little weight by health agencies in their expert reviews. For example, a 2017 review under auspices of the Swedish Radiation Safety Authority considered Yildirim's 2015 study to be "uninformative in terms of a potential causal RF-EMF effect on semen quality" due to lack of adequate correction for confounders and other problems (Danker-Hopfe et al. 2017).

Health agencies have emphasized the need to consider studies in a broader context :

> Obviously, the presence or absence of statistical significance is only one of many factors in this evaluation. Indeed, the evaluation considers a number of characteristics of the study. Some of these characteristics are rather general, such as study size, assessment of participation rate, level of exposure, and quality of exposure assessment. Particularly important aspects are the observed strength of the association and the internal consistency of the results including aspects such as exposure-response relation. Other characteristics are specific to the study in question and may involve aspects such as dosimetry, method for assessment of biological or health endpoint, the relevance of any experimental biological model used ." (Danker-Hope, 2017).

By contrast, Pall appears to accept experimental findings without critical review for statistical and methodological quality.

Having examined the additional papers that Pall cites, we reaffirm our earlier conclusion: a number of studies have reported bioeffects of Wi-Fi exposures, but technical limitations make many of them difficult to interpret and artifacts cannot be excluded. We are not aware of any health-agency warnings about health risks of Wi-Fi technology. Despite some level of public controversy and an ongoing stream of reports of highly variable quality of biological effects of RF energy (*e.g.* articles in a recent special issue of the Journal of Chemical Neuroanatomy, Volume 75, 2016) health agencies consistently conclude that there are no proven hazards from exposure to RF fields within current exposure limits (even as they consistently call for more research).

We repeat our recommendation from our 2013 review: if studies are to be done using the small exposure levels characteristic of Wi-Fi technology, they need to be done well, with experimental models relevant to human health, with meticulous exposure assessment and with careful attention to good study design. Such efforts, however, are expensive and need to be adequately supported.

[3] https://www.canada.ca/en/health-canada/services/environmental-workplace-health/reports-publications/radiation/understanding-safety-code-6.html

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