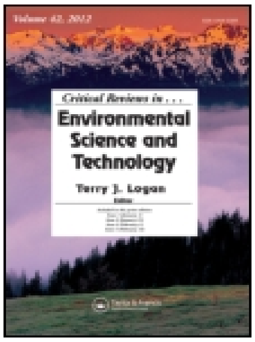

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Comments on environmental impact of radiofrequency fields from mobile phone base stations

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ABSTRACT

This article is an answer to the review paper from Verschaeve (2014). This review paper attempted to dismiss every study that shows negative effects of microwave radiation on living organisms. His conclusions are not supported by scientific data and are mostly based on his claims for "inaccurate" dosimetry. This issue is not the case, especially in studies employing real and not simulated exposures by mobile telephony (and related technologies) antennas, since this type of radiation is of highly varying nature, and its levels – regardless of any dosimetry – are simply the same with those exposing daily billions of users.

KEYWORDS

Electromagnetic fields, radiofrequency fields, mobile phone radiation, base stations, environmental impact, biological effects, health effects

1. Introduction

Dr Verschaeve, the author of the review paper entitled "Environmental Impact of Radiofrequency Fields from Mobile Phone Base Stations" (Verschaeve, 2014), criticizes a great number of biological studies that show effects of mobile telephony and other related types of radio frequency (RF)/microwave radiation on a large variety of living organisms ranging from plants to mammals. Almost all of these studies are published in international peer review scientific journals of high reputation. The author criticizes exclusively those studies with positive and alarming results instead of recognizing and underscoring the fact that these results are corroborating each other and thus it would be most unlikely to be wrong. The author attempts to underestimate the importance of these studies by "discovering" "shortcomings" in each and every one of them. Most of the "discovered" "shortcomings" are related with the "accurate" evaluation of the exposure dosimetry. The author claims that the measurements "are not correct" (although he does not mention the

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degree of correctness), and "for this reason these studies do not provide any evidence that observed biological effects are associated with exposure to the electromagnetic fields." In this way, Dr Verschaeve systematically attempts to discredit and dismiss practically all studies showing a variety of alarming effects related to animal/human health and the natural environment.

There are countless points in Dr Verschaeve's review that offend science and scientists, and it is practically impossible to respond to every single one of them. Moreover, there are numerous fundamental mistakes, and below we analyze those we consider most important.

2. The author makes arbitrary statements throughout his paper, not supported by scientific arguments, and omits significant facts

For example:

(1) In regard to effects on bees (page 1319), the author states that "evidence for adverse effects from mobile phone radiation is extremely poor." But most of the studies he discusses (six out of eight studies) show alarming effects. One of the remaining two studies that did not show effects is old (Gary and Westerdahl, 1981) and employed a very different microwave radiation type than those employed in today's mobile telephony. Making comparisons with studies performed before the existence of mobile telephony and stating arbitrary conclusions of "no effects" for mobile telephony radiations, while still six out of eight of the studies he examined show alarming effects, is unscientific and flawed methodology.

(2) Another arbitrary statement of the author in regards to studies that show effects on bees (page 1319) is as given: "Indeed, there were only a few scientific publications related to effects from mobile phone radiations and comparable frequencies on bees but, unfortunately, they are all characterized by severe shortcomings."

The author omits to note that at least one of the observed effects on bees, which was a large decrease in reproduction (egg laying) (Sharma and Kumar, 2010), was identically observed in Drosophila (Panagopoulos et al., 2004, 2007a, 2010) and birds (Balmori, 2005). This observed decreased reproduction is strongly corroborated by very similar effects in amphibians (embryonic mortality) (Balmori, 2006, 2010a), rats (decreased ovarian development) (Gul et al., 2009), and human sperm (Agarwal et al., 2008). This unique similarity of effects in different organisms found by different research groups can be further explained by the observed cell death induction in reproductive cells due to DNA damage reported for Drosophila ovarian cells by Panagopoulos et al. (2007b, 2010) and for human sperm cells by De Iuliis et al. (2009). It is evident that such a similarity of effects cannot be due to randomness. Yet nothing is noted by Dr Verschaeve who should at least mention this similarity of the reported effect on different animals. This similarity validates both the observed effect and the studies. Apart from effects on reproduction, many other significant effects reported in many other studies (Balmori, 2009; Cammaerts and Johansson, 2013; Cammaerts et al., 2011, 2012; Cucurachi et al., 2013; Favre, 2011) further support the observation that mobile telephony radiation adversely affects living organisms.

(3) The author takes into consideration studies with very low microwave intensities, such as $\leq 0.003\ \mu W/cm^2$ (Joos et al., 1988) or $0.01\ \mu W/cm^2$ (Staeger, 1989), in which the "no effect" finding was to be expected for such low exposure levels. Nevertheless, the author takes equally into consideration these studies, and studies with real radar exposures showing important adverse effects on tree growth (Balodis et al., 1993, 1996; Selga and Selga, 1996), to conclude again that overall, investigations "do not support the hypothesis that RF radiations from transmitters harm trees and forests." Therefore, this is another arbitrary "conclusion" of Dr Verschaeve.

(4) The author refers to seven studies related to effects of microwave radiation on trees. Four of them showed effects (Balodis et al., 1993, 1996; Krug, 1990; Selga and Selga, 1996), some of which were alarming such as decreased germination, inhibition of growth, and promoted senescence. Two older studies with too low radiation intensities showed "no effects" (Joos et al., 1988; Staeger, 1989). A seventh study (Schmutz et al., 1996) showed a questionable effect. The author's conclusion (page 1343) for the above studies was that "it is clear that they do not support the hypothesis that RF radiations from transmitters harm trees and forests." It is obvious that this is another arbitrary "conclusion" not at all supported by the facts presented by the author himself.

(5) The above are only a few examples of arbitrary statements made by the author. Similarly, his general conclusion in the abstract/summary of his review is "Overall, many alarming investigations were found but most are characterised by severe methodological shortcomings. For this reason these studies do not provide any evidence that observed biological effects are associated with exposure to the electromagnetic fields. So far, the studies do not prove that environmental exposures to mobile phone base station radiation (and other environmental RF exposures) are harmful to wildlife." This "conclusion" is not supported by the facts and it is also an arbitrary statement as we detail in the following paragraphs.

3. Real emissions of wireless telecommunications are highly variable and "exact" dosimetry has no meaning. Only representative average levels can be reliably estimated

Most studies criticized by Dr Verschaeve employed real mobile telephony signals emitted by commercially available and approved for the market devices, or corresponding approved base station antennas. Thus, the exposures by such antennas are the real exposures regardless of "accurate" or "inaccurate" dosimetry. Real digital mobile telephony signals emitted by base stations and handsets are highly varying. The parameters of this type of radiation (intensity, frequency, modulation, phase, etc.) change constantly and unpredictably each moment. More specifically, in the case of a mobile phone during a usual phone conversation, the intensity of emitted radiation varies significantly each moment depending on signal reception, number of subscribers sharing the frequency band each moment, air conductivity, location within the wireless infrastructure, presence of objects and metallic surfaces, "speaking" versus "non-speaking" mode, etc. These variations may reach ±100% of the average signal intensity. Moreover, the phase of the carrier signal varies continuously during a phone conversation. The RF carrier frequency constantly changes between different values within the available frequency band. The wave shape is also constantly changing depending on how the changing information transmitted each moment modulates the carrier wave. Thus, the parameters of this radiation change constantly and unpredictably each moment and large, sudden, unpredictable variations in the emitted electromagnetic field (EMF)/radiation take place constantly during a usual phone conversation. The more the amount of carried information is increased (by adding text, speech, pictures, music, video, internet, etc.) in more recent phone types (2G, 3G, 4G, etc.), the more complicated and unpredictably varying become the cell phone signals (Tisal, 1998; Hillebrand, 2002; Curwen and Whalley, 2008). Corresponding changes take place constantly in the signals emitted by base station antennas as well. Thus, the measurements of this type of radiation can only be representative, indicating the order of magnitude and representative values of the average intensity. That is the meaning of the dosimetry in this case.

The above facts account not only for the near-field for which it is known that the intensity of emitted radiation includes large variability for any type of antenna, but in the case of telecommunication antennas, the variability accounts for the far-field as well in a smaller degree due to the above reasons. A GSM (Global System for Mobile Telecommunications) mobile phone antenna's near-field extends to a distance of 5.2 or 2.6 cm, for 900 or 1800 MHz, respectively (most commonly employed carrier frequencies in 2G mobile telephony radiation), according to the relation, $r = \lambda/2\pi$ (r is the distance of near-field far limit from the antenna when the length of the antenna is smaller than the wavelength λ of the emitted radiation) (WHO, 1993). Thus, the author's statement (page 1324) that the reactive near-field extends to a distance up to one-third of a wavelength (i.e., ~11 cm for 900 MHz) is incorrect. It is evident that the billions of mobile phone users are daily exposed for hours within the near-field of their devices when they carry them on their bodies and use them in contact with their heads. The author's reasoning is that once we cannot accurately estimate this exposure, we cannot talk about any biological effects…. But whatever the accurate measurement is each moment, the highly variable emission is what it is, and the observed effects are a fact. There is only an upper limit determined by the maximum output power of the antenna (between 1 and 2 W in GSM handsets). Moreover, as we explained, 100% accurate measurement is not possible in this kind of radiation. According to the author's reasoning, studies showing alarming effects must simply be dismissed because of "inaccurate dosimetry" and there is no reason for cautious use or more stringent limits. In other words, since we cannot measure it accurately, it doesn't exist! This is absurd and – of course – unscientific.

4. Wireless communications radiation types include the highest variability than any other type of microwave radiation due to the highest density of varying transmitted information

The highly varying radiation character encountered in mobile telephony radiation accounts for every type of wireless communications radiation as well, such as Internet connection wireless devices and local wireless networks (Wi-Fi), domestic cordless phones (Digitally Enhanced Cordless Technology [DECT]), baby monitors, etc., but not for microwave radiation types that do not transmit information (such as radiation from radars or microwave ovens). A highly varying stress type is reasonably expected to be more bioactive than radiation of the same average intensity but lower variability, since living organisms cannot adapt to a constantly and unpredictably varying stress type. Thus, studies employing wireless communication signals (e.g., mobile telephony antennas) cannot be directly compared to studies investigating effects of invariant signals, for example, from radars, and the author's methodology to draw conclusions from such comparisons (as he does e.g., on page 1334) is a flawed one.

Moreover, digital wireless telecommunication signals are pulsed signals while radio (FM) or television signals are continuous, although they all transmit information. But although a radio or TV antenna emits varying information from one station, a mobile phone base station antenna emits varying information from thousands of users each moment. By pulsing the signal, the density of transmitted information is increased. Pulsed signals are more varying and are thus expected to be more bioactive. Reports that show biological effects on animals appeared after the exposure to GSM antennas while preceding exposure to radio–TV signals had no obvious effects (as the author refers to on page 1336) are thus not very surprising and should not be evaluated with suspicion by Dr Verschaeve.

On the other hand, although radar radiation is also pulsed and not continuous microwave radiation, it does not carry information and thus, it is invariant. Apart from the signal pulsing at a constant repetition rate, there is no modulation or other signal changes as in mobile telephony radiation.

In conclusion, digital wireless communications radiation types, and especially mobile telephony radiation, include the highest degree of variability and are thus expected to be the most bioactive according to the above reasoning. This explains the great number of positive and corroborating results that Dr Verschaeve attempts to invalidate in his review.

5. While the author attempts to discredit studies for "wrong dosimetry," he presents wrong dosimetry himself. Dosimetry included in the studies is correct as representative average intensity

While the author criticizes studies for near-field mobile phone measurements as having "wrong dosimetry," he provides radiation intensity values (Table 1 in Verschaeve 2014) for "against the head of a GSM mobile phone user" (1.7–2.7 mW/cm^2 = 1700–2700 μW/cm^2) "assuming far-field conditions." Why then the author criticizes the studies for reporting intensity values in the near-field? Moreover, as we explained, the user with the phone against the head is exposed in the near-field, not in the far-field. In addition, the above values given by the author are extremely high and incorrect as average values. They do not even account either as instantaneous maximum values. Thus, the author, who requires "accuracy" and criticizes studies for not having that accuracy, presents wrong dosimetry himself.

Moreover, the reported Table 1 electric field values "near mobile-phone base stations where the general population or animals can get access" (<1–2 V/m, corresponding to <0.26–1 μW/cm^2) are unrealistically low (humans can have access at distances up to a few meters, e.g., on roof-tops where antennas are installed, and animals, e.g., birds or insects can have access even at zero distance!). The author claims that "The places where the public can access are always at least at several meters from the radiations source. Therefore, living close to mobile phone base station antennas means that the exposure duration is long, but only at a low intensity. On-site measurements reveal that the electric field in places where the public can stay for a rather long period is nearly always lower than 2 V/m." But he does not provide any specific measurements at specific distances from base station antennas. In contrast, the electric field values given in the second column of Table 1 (1-2 V/m) do not correspond to the (supposedly corresponding according to the author) power density values given in the next two columns (<0.3–1 mW/cm^2 = <300–1000 μW/cm^2) which are realistic for very close (a few meters) proximity to a base station antenna. The electric field values reported by the author (<1–2 V/m) are representative for distances hundreds of meters from a typical base station antenna, and in no case for "several meters from the radiation source." Anyone who has some experience in measuring radiation levels at different distances from base station antennas knows this. Thus, while the author criticizes tens of important studies for "unreliable" and "inaccurate" dosimetry both in the near and the far antenna fields, he himself presents wrong values both for the near-field of a mobile phone, and for "at several meters" (far-field) from a base station antenna. Moreover, the author who requires "accuracy" from others does not specify with what instrument or procedure he got these values. Moreover, he does not provide any measurements in the extremely low frequency (ELF) range of mobile telephony fields, although ELF fields are considered to be very bioactive, and it is known that any mobile phone user is exposed simultaneously to both RF and ELF fields. Nevertheless, he criticizes other studies that provided measurements in the ELF range for unreliable ELF dosimetry. Finally, the author does not explain how exactly the specific absorption rate (*SAR*) values given in his Table 1 were estimated. Thus, it is actually himself that should be blamed for unreliable dosimetry, and since his criticism is mostly based on alleged dosimetry "shortcomings" of other studies, it is actually his "study" that should be dismissed.

The measurements included in many of the studies are very useful as representative of the average exposure levels over a certain period. With this logic they are absolutely correct. The reported average 6-min levels ranging from ~0.2 to ~400 μW/cm^2 (in the case of exposures by GSM handsets) for corresponding distances from 1 m down to 0 from the device (Panagopoulos et al., 2010) are absolutely correct as representative average radiation intensities, even for the near-field. It is true that measurements in the near-field, for example, in contact with a mobile phone, can have a considerable degree of inaccuracy. But when many measurements of average intensity over several minutes period are recorded, the mean average value from many such measurements is definitely representative of the real emission. The instantaneous intensity values may be significantly lower or higher than this value. There is no meaning to characterize every instantaneous change taking place each moment during a real exposure of this type. The use of the measurements in this case is to give a representative dosimetry level as an order of magnitude and average intensity for this type of radiation. The intensity values reported at least in most of the studies criticized by the author are correct as representative average values.

The intensity values given by Khalafallah and Sallam (2009) at 3–12 m from a base station antenna installed 8 m above the ground (0.07–0.15 mW/cm^2 = 70–150 μW/cm^2), criticized by the author as "not consistent with the exposure conditions," are very reasonable and usual for base station antennas at such distances. This is well known to anyone who has performed measurements. These values are in very good agreement with other studies and in no agreement with Dr Verschaeve's Table 1 values.

6. The author neglects important findings, while making unjust criticism on minor issues

In a study examining effects of radiation from mobile telephony base station antennas on storks (Balmori, 2005), it was found that the fertility of the birds was reduced with decreasing distance from the antennas (or increasing radiation intensity). The author (Dr Verschaeve) instead of recognizing the difficulty of conducting field studies in open air conditions and the importance of the findings, he criticizes any methodological approach. Instead of recognizing the distinct similarity of the findings with corresponding findings on sparrows (Balmori and Hallberg, 2007; Everaert and Bauwens, 2007), bees (Sharma and Kumar, 2010), Drosophila (Panagopoulos et al., 2004, 2007a, 2010), rats (Gul et al., 2009) etc., the author compares with studies that found no effects including a study of his own (Verschaeve and Maes, 1998) and concludes that the results of the stork study justify suspicion (!). According to his reasoning, it should then be concluded that the negative studies (including his own) justify suspicion. The author criticizes the dosimetry in the stork study as if this was the major issue, and not the dramatic fertility decline that was recorded. The electromagnetic field meter used in the study had indeed low sensitivity and that was reported in the paper ("device Model LX 1435 with 10% sensitivity") but it was very useful for assessing indicative average radiation intensity levels. By using the same apparatus in both measurements (nests located either less than 200 m or further than 300 m from the antenna), the results were absolutely comparable. Given the impossibility of making measurements up to the nests (which was criticized by the author), measurements were performed at the corresponding locations on the ground, in direct line of sight with the antennas mast. Thus, measured values were slightly lower but proportional to the radiation levels in the nests. In any case, the measurements were recorded in the two groups of nests in the same way, and thus the results were absolutely comparable.

The author criticizes another open-field study, investigating effects of mobile telephony base station antennas radiation on sparrows (Balmori and Hallberg, 2007) for similar reasons (mainly dosimetry). To date and to the best of our knowledge, there are only three studies published on the topic performed in three different countries (Spain, Belgium, and India) by three different research teams (Balmori and Hallberg, 2007; Everaert and Bauwens, 2007; Bhattacharya and Roy, 2014). All three studies reached the same conclusion: There are fewer sparrows where radiation levels are higher. Nevertheless, the author does not even mention the important fact that results from three different groups on the same bird revealed the same effect. Similarly, he does not mention the distinct and important similarity with results on other organisms (bees, fruit flies, storks, etc.).

In another study investigating the effects of mobile telephony base station antennas radiation on frogs (Balmori, 2010a), the author (Dr Verschaeve) criticizes again dosimetry issues. He criticizes the fact that the radiation measurements were performed outside the two water tanks containing the tadpoles and not inside the water. It is very difficult to find a submersible meter for measurements in the water where the tadpoles are submerged. The radiation levels in the water were certainly lower but proportional to the radiation levels outside, and they were certainly very different in the electromagnetically shielded aquarium (used for the control groups) compared to the exposed aquarium in which increased mortality and other adverse effects were found. The measurements were performed under the same conditions in both aquariums and the results on mortality and the other effects in tadpoles were very different between the exposed and the control groups. Since the radiation levels within the water were proportional to the measured levels outside the water, the study shows, undeniably, that the recorded effects found only in the exposed tadpoles were due to the radiation.

The above findings on storks, sparrows, and frogs were recently reviewed and published again in a high impact journal (Balmori, 2014, 2015).

The author (Verschaeve) criticizes the Drosophila studies performed by Panagopoulos et al. (Chavdoula et al., 2010; Panagopoulos, 2012; Panagopoulos et al., 2004, 2007a, 2010) for "unreliable dosimetry" because of the radiation measurements in contact (or very close) with an active cell phone reported in these studies. He attempted to discredit these studies that have shown a dramatic decrease in reproductive capacity after only few minutes of daily exposure for a few days to mobile phone radiation during usual "talk" phone emission. The decrease in reproductive capacity was found to be due to DNA damage induction in the gonads and consequent cell death in the gametes resulting in dramatic decrease (up to 60%) in reproductive capacity (number of F_1 pupae) (Panagopoulos et al., 2007b, 2010). The DNA damage was found to be accompanied by actin cytoskeleton damage (Chavdoula et al., 2010). Instead of recognizing the importance of these findings, the author tried to discredit them because of radiation measurements close to the mobile phone handset included in the studies! Whatever the accurate radiation intensity is from a commercially available and approved for the market device used in the experiments, the effect of this radiation is a fact. Not recognizing this is unscientific and intentionally misleading for the readers of his review. As for the measurements, it is known that, especially within an antenna's near-field, they include a considerable degree of variability, but actually, mobile phone users are exposed within the near-field of their devices and there is no way to perform 100% accurate measurements. This is partly due to possible capacitive coupling of the probe with the antenna. But capacitive coupling results in higher root mean square measured values than the "real" ones. With increased number of average intensity measurements and comparing with the theoretically expected slope of radiation intensity with respect to the distance from the antenna, an experienced scientist can reliably reduce imprecision and obtain realistic measured values and even dismiss unrealistically high values resulting from capacitive coupling when occurring. After all, this is the only way for measuring radiation very close to the antenna where the users are actually exposed. Thus, the included near-field measurements are very useful as indicative average values and are – of course – in no way a reason to decrease the value of the biological findings which are shown to be related with the real exposures from mobile phones to which the billions of users are daily subjected.

In his attempt to limit the importance of the above studies in Drosophila, the author compares their results with the results of another group's study on Drosophila (Weisbrot et al., 2003) which found an increase in reproduction after mobile phone exposure. All studies performed in a variety of animals find that this radiation causes decrease in reproduction. We do not know why this particular study found an increase, which is indeed a peculiar result, although several parameters were different (intensity of radiation, carrier frequency). In any case, the fact that a particular study found a different result than the vast majority of studies by no means affects their importance. But the author concludes: "Above investigations thus showed either a decreased or increased reproductive capacity following RF mobile phone exposure. These results are therefore to a certain extent contradictory." Making such a statement just because a single study found an opposite result than all the other studies is arbitrary and unjust. The author continues that "It may also be stressed that older studies on genetic changes which may cause reproductive effects…were all invariably negative," and refers to older studies investigating the induction of specific mutations or loss of chromosomes after exposure to microwaves. Induction of mutations or chromosome loss is very unlikely after short-term exposures to nonionizing radiation, in contrast to effects on reproduction which is a widely known sensitive biological index. Thus, such a comparison is again totally invalid and unscientific (see also Section 8).

The author criticizes a study which reported a significant decline in colony strength and in the egg-laying rate of the queen in bees exposed to mobile phone radiation for 30 min daily twice a week for a total of 3 months by two handsets placed on the two side walls of the bee hives in call mode compared to unexposed and sham-exposed control hives (Sharma and Kumar, 2010). The results of this study are in complete agreement with the decreased reproduction found in fruit flies due to mobile phone exposure (Panagopoulos et al., 2004, 2007a, 2010) which was found to be due to DNA damage in the gametes (Panagopoulos et al., 2007b, 2010). The result of the above study (Sharma and Kumar, 2010) explains the recently reported phenomenon of sudden disappearance of bees with little or no sign of disease or infection, as reported during the last 10–15 years especially in Europe and North America (Hamzelou, 2007). Yet, Dr Verschaeve sees nothing of these, and criticizes the study for employing only four bee colonies (two exposed, one sham-exposed, and one unexposed control). In his own words: "It does not allow drawing any conclusion due to small sample and many shortcomings." Moreover, he criticizes the study because its authors reported that the bees became "quiet and still or confused during exposure, as if unable to decide what to do," that (in his own words) "this is not a scientific observation." We comment on this in the next paragraph.

In another study with bees (Favre, 2011) in which again two active mobile phones were used for exposure, Dr Verschaeve claimed that "experimental conditions did not coincide with 'normal' exposure conditions, for example, where hives are nearby a base station antenna," which is a very strange statement because (a) radiation from base stations is very similar with that of corresponding mobile phones with an intensity depending on distance (since base station antennas are about 100 times more powerful than corresponding mobile phones, an active handset at 20–30 cm distance corresponds approximately to a base station antenna at 20–30 m distance, which is realistic for base stations located in the country); (b) bees can also be exposed by people carrying their mobile phones at close proximity to the hives. (c) It is usual in experiments to employ an intensity higher than "normal" in order to observe more clearly an effect. As reported in the paper, the bees emitted an alarming piping sound after the exposure to mobile phone radiation. The piping signal was observed 25–40 min after the onset (not the offset of the mobile phone communication as reported by Dr Verschaeve) which makes the response time shorter. According to Dr Verschaeve, "the rather long response time could indicate that the piping signal was caused by another phenomenon than the RF field." This is another arbitrary conclusion of the author in his attempt to discredit studies. There can be many reasons for a delayed response to an annoying stress. An organism naturally takes some time until it recognizes an unknown mild stress factor as detrimental. But according to Dr Verschaeve's logic, it is "all or nothing." Either the bees respond immediately to an unknown stress, or there must be some other reason…. But all other factors were well-controlled and there were sham-exposed hives during the experiments in which the bees did not produce the alarming sounds produced by the exposed bees. The only difference between exposed and sham-exposed bees was the exposure to the mobile phone radiation. Why then Dr Verschaeve should claim that "the rather long response time could indicate that the piping signal was caused by another phenomenon than the RF field," or that it was due to temperature increases within the hives by the operating mobile phones? It is well known that mobile phones – especially at distances 20–40 cm – do not cause any detectable temperature increases (IARC, 2013; Panagopoulos et al., 2010, 2013). Moreover, anybody would immediately note that the 25–40-min latency in the bees response is in complete agreement with the observation of the previous study, that the bees became "quiet and still or confused during exposure, as if unable to decide what to do" which Dr Verschaeve disregarded as "non-scientific." It simply verifies that the bees may not respond immediately to electromagnetic stress.

The author criticizes another study showing adverse effects of 900 MHz, 1 V/m radiation on ants' conditioning ability and memory (Cammaerts et al., 2012). The criticism concerns the use of an RF generator for the exposure as well as for supposedly inaccurate intensity calculations. But adverse effects on ants as well as on protozoa were also shown in other studies of the same group which employed real exposures from a variety of commercially available wireless RF devices such as mobile phones, DECT phone, Wi-Fi, etc. (Cammaerts and Johansson, 2013; Cammaerts et al., 2011). Why did the author omit those studies in his review?

7. The main point of the studies is the biological findings due to the undeniable exposure at representative average levels presented in the studies, not the "exact" dosimetry of this exposure

The important information provided by the studies and the usefulness of this type of studies is not any "exact" measurement of a highly variable EMF (something which is anyway not possible, especially in the case of mobile telephony radiations), but the investigation of important biological/health effects, regardless of the more or less accurate dosimetry (which in the case of this radiation type can only be representative as explained).

It is as if we are observing a huge tidal wave coming upon a city on a shore and just because we are not able to measure by our instruments its exact height (e.g. whether it is 80 or 90 m), we claim that once we cannot measure it exactly, we cannot draw conclusions for any adverse effects that it may cause!… That – of course – would be absolutely absurd, unscientific, and catastrophic. Although the example with the tidal wave is an extreme one, phenomena such as the observed disappearance of bees (which is explained by induced cell death in the gonads as found in Drosophila studies) or birds may have tremendous adverse effects on our societies.

In this case, we have already hundreds of studies performed on a variety of organisms in many different laboratories around the world, all pointing at the same direction: This radiation at many different exposure levels is responsible for a variety of adverse biological effects ranging from simple alterations in different biological rates, loss of orientation, or retardation of growth, to DNA damage, protein damage, or cell death, transient or permanent infertility, or even the organisms' death in extreme cases. But according to Dr Verschaeve, it doesn't matter…. Since there are other studies that do not show effects and since we cannot estimate accurately the radiation level, there is "no overall evidence," and thus no precaution should be taken! In other words, according to Dr Verschaeve's reasoning, the effects do not exist or they are totally negligible!

8. Studies showing important biological effects should not be weighted equally with studies showing no effects

The author's position that the positive results should not be taken into account and no precautions are needed to be taken by authorities since other studies had negative results is totally unscientific and dangerous for public health. The generally accepted *precautionary principle* states that, as long as it is not proven that an agent is NOT detrimental, we should avoid exposure to this. It does not say that "only when 100% of the studies show effects, then we should avoid exposure." Unfortunately, this is obviously what Dr Verschaeve supports.

Alarming positive results should be given more attention than studies showing no effects for at least two reasons: (a) The worst case must always be taken more into account when it concerns human/animal health and the integrity of our natural environment, as this is in line with the *precautionary principle*. (b) It is much easier for any experiment in biology and in physics to find "no effect" (absence of consistent and statistically significant effect) than to find a consistent and statistically significant effect. Actually a statistically significant positive effect is found only after accurate design by experts, and extreme care of every little detail, except for cases in which the effect is at extremely intense levels and thus always easily detected – in spite of small methodological errors – which is rarely the case in general, and it is not the case with non-ionizing electromagnetic radiation (EMR)/EMFs. In contrast, a negative result comes naturally in any case that even a single parameter is not carefully controlled. Randomness never gives systematically positive results, except when there is some serious systematic error. Does the author believe that the hundreds of studies with corroborating results performed by so many different labs, in a wide variety of organisms and under a variety of experimental conditions and exposure levels include similar serious systematic errors and for this reason they come to corroborating results? Finally, the statistical analysis included in almost all peer review scientific studies shows whether any observed effect is "real" or is due to randomness. This is exactly the role of statistical analysis. Statistically significant results cannot be dismissed because of alleged "inaccurate dosimetry," or because other studies did not show effects.

Many studies showing "no effects" may simply look at a wrong end point. A wrong end point may be to look at a resistant biological parameter than to look at a sensitive one. For example, while reproduction, cell proliferation, ionic concentrations, etc. are known to be sensitive indexes in almost all living organisms, studies investigating the induction of specific mutations by non-ionizing radiation will most likely show no effects. Still the author equally weights studies on reproduction that show effects with mutation studies that show no effects (e.g., old studies in Drosophila).

9. It is a common sense among experts that the majority of observed effects are nonthermal. Still the author denies the existence of nonthermal effects

The author supports that nonthermal biological effects such as alterations in intracellular ionic concentrations "…are not sufficiently established to provide a basis for restricting human exposure. Most scientists therefore still consider that such effects have no or at least no proven impact on health" and cites two papers to support this, one of which is his own (Verschaeve, 2012) (page 1315). But it is basic knowledge in cell biology that alteration in intracellular concentrations of different ion types can initiate a variety of biochemical cascades that may disrupt cell function (Alberts et al., 1994; Pall, 2013, 2015).

Moreover, it is known that the vast majority of reported effects at environmentally accounted exposure levels are nonthermal. The author actually dismisses studies showing important effects at nonthermal exposure levels, such as oxidative stress and eye cataract (Haessig et al., 2009, 2012), simply because an older study – the full reference of which is missing in the paper (Ashton, 1977) – did not show effects, or because another older study concluded that "radiation-induced temperature elevation appears to be essential for the cataractogenic effect of microwaves" (Kramar et al., 1975). Thus, any possible cataractogenic effect due to nonthermal oxidative stress in the eyes is a-priori excluded by Dr Verschaeve!

The author dismisses well-established nonthermal effects such as alteration in intracellular calcium concentration (Bawin and Adey, 1976; Goodman et al., 1995; Pall, 2013, 2015) as "not sufficiently established to provide a basis for restricting human exposure" (page 1315), with no further explanation. This is also an arbitrary "conclusion" and a flawed scientific methodology.

The author concludes (page 1357): "Alarming results were often ascribed to thermal effects, and in many cases where non-thermal effects were assumed critical evaluations do show that this is probably not the case." This "conclusion" is again totally arbitrary. The alarming results of most studies criticized in Dr Verschaeve's paper were due to nonthermal exposure levels. In Panagopoulos et al. (2013), it is explained in detail why exposures to man-made EMFs at environmentally accounted levels are necessarily nonthermal, and even IARC explains that temperature increases higher than 0.1°C in human brain due to exposure to mobile phone radiation in contact with the user's head is unlikely (IARC, 2013).

10. The author criticizes studies for "absence of *SAR* evaluation"

The author criticizes studies for "absence of *SAR* evaluation," although electric field values were reported (pages 1343–1344). These studies showed alterations in the synthesis rates of RNA transcripts corresponding to different genes after a few minute exposure of plants (tomatoes) to GSM-like radiation. The intensity levels corresponded to the exposure on the head of a mobile phone user during a phone conversation (40 V/m $\cong$ 420 μW/cm^2), and to lower levels (5 V/m $\cong$ 6.6 μW/cm^2), corresponding to exposure approximately 30–40 cm from a mobile phone handset, or approximately 30–40 m from a base station antenna (Roux et al., 2006, 2008; Vian et al., 2006, 2007).

But it is shown that *SAR* includes very large errors when it is estimated by the specific conductivity of the tissue absorbing the radiation, and its credibility as a dosimetric quantity is seriously questioned, especially for nonthermal effects which constitute the vast majority of effects at environmentally accounted radiation levels (Panagopoulos et al., 2013). The author writes the formula for the $SAR = \sigma E^2/\rho$, but neglects to mention that this formula is not valid unless we assume that the specific conductivity σ of the tissue, the internal electric field E within the tissue, and the tissue density ρ are constant within an organ (e.g., eye) or a group of organs (e.g., head) of a living body in which we want to calculate *SAR*. This, of course, is an oversimplification since every organ or group of organs consists of many different types of biological tissue and all the above quantities vary significantly between different tissue types and even within a single type of tissue and within a single cell. For these reasons, only the thermal formula for $SAR = c\delta T/\delta t$ (for an homogeneous medium with specific heat c – thus neglecting again local density variations plus local variations in the specific heat – for a time interval δt and a corresponding measurable temperature increase δT) can be considered credible inasmuch the local variations in specific heat are significantly smaller than corresponding variations in specific conductivity within tissues (Panagopoulos et al., 2013). But this is applicable only for measurable temperature increases in the exposed tissue (thermal effects), which is questionable for 420 $\mu W/cm^2$ power density and totally out of question for 6.6 $\mu W/cm^2$.

The above are not considered at all by the author (Dr Verschaeve). Moreover, although he criticizes important studies for "absence of *SAR* evaluation," he does not explain how he got the *SAR* values that he presents in Table 1 of his paper. Since, as explained, *SAR* is a highly questionable quantity especially for nonthermal effects, simply providing a formula without reporting its limitations, and simply providing some values without showing how they were estimated, is scientifically flawed.

11. The author omits the fact that mobile telephony signals combine both RF and ELF frequencies

All types of digital mobile telephony radiation, except for their RF carrier signal, employ ELFs necessary for the modulation and for increasing the capacity of transmitted information by pulsing the RF signal. The combination of the RF carrier and the ELF pulsing frequencies has been found to be more bioactive than the RF carrier alone (Lin-Liu and Adey, 1982; Penafiel et al., 1997). Moreover, according to theoretical calculations, the ELF frequencies included in any pulsed or modulated RF signal, are those more responsible for the biological effects, and the pulsing of the signal makes it twice more bioactive (Panagopoulos et al., 2002). The increased biological activity due to the combination of RF and ELF in mobile telephony emissions that further explains the great number of effects found in the studies is not taken into consideration by Dr Verschaeve.

12. Conclusion

As we analyzed in detail, the review paper authored by Verschaeve (2014), in which he attempted to dismiss practically every study that shows positive and alarming effects of microwave radiation types on living organisms, is full of arbitrary statements and methodological flaws. His "conclusions" are not supported by scientific data and are mostly based on his claims for "inaccurate" dosimetry in the reviewed studies. This issue as explained is not the case, especially in studies employing real and not simulated exposures by mobile telephony (and related technologies) antennas, since this type of radiation is of highly varying nature by itself, and its levels – regardless of any dosimetry – are simply the same with those exposing daily billions of users.

It is most strange to us that such a reasoning as that of Dr Verschaeve which is evident throughout his review paper is considered scientific and is published in a peer review scientific journal. It is also most strange that a scientist with such a logic is a member of decision-making health organizations such as the International Agency for Research on Cancer (IARC) (IARC, 2013).

The practice of not recognizing the "tidal wave" because of lack of "accurate dosimetry" is not only unscientific but in addition catastrophic for public health in case that those who support and promote it are members of decision-making health organizations.

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