Problems with Laboratory EMF Experiments

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When the lives of so many people are being affected by microwave transmitters, mainly as mobile phone and private mobile radio masts, including O2 Airwave TETRA, it would seem self-evident that investigation should be thorough and urgent.

However, whilst there is ongoing research into hand-held mobile phone use, there is virtually nothing on base station epidemiology, on follow-up causation research, or on valid evaluations of symptoms attributed to what is described as some form of hypersensitivity. Too often, self-reported experiences are treated with disdain, as irrelevant, or presumed to be psychosomatically induced.

Apart from the striking unfairness in these presumptions, it is short-sighted, since these observations, treated properly, may reveal aspects of chronic exposure to low-level radiation that carries *both* the characteristics of microwave-frequency signals *and* extremely low frequency coherent time-variance, through amplitude and pulse modulation.

It would be disastrous to generate research results that fail to address the full nature of this exposure whilst purporting to prove that the experiences are false, especially if those experiences are precursors to more serious ill health. It is then improper to treat the 'hypersensitive' person as being the problem, for having a sensitivity that must be treated in some way (*eg* through psychiatry, for being self-induced), rather than treating the cause as a complex phenomenon likely to affect a wide range of people. As a parallel, we have been treating millions of people worldwide for decades for 'tobacco sensitivity', rather than removing a cause suspected throughout this period, and now as near proven as possible.

We therefore need to derive robust research approaches that avoid simplification whilst revealing the true nature of exposure environments.

1. Epidemiology and complexity

Epidemiology is a weak form of research. This is true, since it is a mapping and a modelling of what can be measured, and by implication omits what cannot. At best, it presents a picture for comparison with other factors in order to suggest possible correlations. From correlation we move on to hypotheses of causation, which we go on to test experimentally. If this can be confidently replicated we accept that the causation is probably a sufficient explanation of the initial epidemiological result. We can, sometimes, then reverse the

situation: find where there is a likely cause in a similar environment and locate previously unattributed effects. In many circumstances a model emerges that shows degrees of effects according to strength of cause, and this may or may not be linear.

Epidemiology is especially difficult in complex scenarios, where multiple correlations may be at play, with their own interactions, separate models of cause and effect, and where the cause/response relationship is very individual. Investigation by epidemiology is a bit like creating artificial flavours: you can represent the strongest flavour elements with a limited chemical repertoire, but it won't taste as authentic as the real thing to most people. The model you create from epidemiology will be a simplified form of reality, that you can deal with.

Nevertheless, the first test of clustered anecdotal reporting is observation, not working solely from known causes. The latter gives rise to assertions that nothing is happening simply because we do not have a mechanism for how it might. Clearly clustered anecdotes represent something; it may be surprising or unexpected, or it might have an alternative rational explanation. Exposure to low-level non-ionising radiation is very much in this area. There exists a substantial body of anecdotal evidence of effects on people from such sources.

2. Anecdote and validity testing

Anecdotal, by definition, means that reports by people, first or second-hand, remain untested for validity. Here again, validity is a difficult construct. A first hand report may be deemed valid because, at face value, this is what a person reports as an experience, for whatever cause, and unless there is specific reason not to, we have to assume they are not deliberately misrepresenting their experience.

However, the test of validity may be pressed further. If they say they have the results of an injury, this is easily tested. If they say they have headache, it is not. Further, if the observer is mistrustful of a reporter's representation, whilst the result of injury may be contested on grounds of cause (I don't believe you walked into a door), an injury itself cannot (I can see you have a black eye). However, even without mistrust, a headache can be contested (this is psychosomatic, there is no headache at all) and the reporter cannot prove the contrary.

Clustered anecdotes (many similar reports, co-located) are also complex. Where the anecdotes are of commonly experienced symptoms, such as headaches, there will almost certainly be a mix of causes. This by itself does not mean that a number of the people do not a share a new and common cause. Similarly, where there is a possible psychological element, and such an element may of itself give rise to a real experience, this too may be a cause. But again this by itself does not mean that a number of the people do not share a new and common cause. It is impossible to quantify regular and psychological cause elements, even by using control samples, because local causes for headaches, for example, are not a tabulated standard and populations tend to be mobile in the course of their days, experiencing other physical and social environments.

Despite the complexity, there is no reason to reject clustered anecdotes. There is no way of telling whether 90 percent are representing a new and singular cause, whether 50 percent are due to a new but non-singular cause (*ie* some are secondary to the cause), or only one per cent have anything at all to do with a new cause. There is no *prima facie* argument for any such stance. Reasons for psychological causes, or for consistent misrepresentation or bias can be noted, but these cannot be automatically attributed to any proportion of the sample.

The question then is whether it is deemed sufficient, on suspicion, to regard a clustered anecdotal occurrence as serious enough to warrant investigation. Here the test must include:

- consequence (if it is ignored, is the potential result damaging?)
- responsibility (would ignoring the situation be a dereliction of duty?)
- cost (is the cost disproportionate to the benefit, and to whom?) and
- capability (do we have the resources, or should additional resources be sought?).

In the case of anecdotal reporting from exposure to EMFs from transmission masts, the consequences are potentially very serious, since even chronic loss of sleep represents a danger to health.

The responsibility should be compared to that covering food, drugs and appliances. However, as for other environmental hazards, since the people affected have not elected to be exposed, there is the added responsibility of regional and national protection authorities. In terms of cost, local authorities will, by and large, claim limited finance and no further destination for epidemiological results to be tested for causation through experimentation.

Nationally, however, the consequences of widespread anecdotal clustering can be addressed and the cost justified on grounds of the potential consequences. With singular bodies such as the NRPB being the sole source of national expertise and advice, their responsibility and duty are so much the greater. Similarly, the capability issue is likely to exceed the resources of local authorities.

This tends to suggest that attention to complex issues, such as transmission base stations, needs to be addressed nationally, rather than locally, though regional initiatives could contribute greatly to the overall picture. What is difficult here, is that what represents a major local issue, is merely a dot on the landscape for the national bodies. In the case of EMF exposure, precisely because the experiences include many common symptoms, if there is a causation to be found, then there is likely to be a vast population with caused symptoms who have not correlated them to a largely unrecognised source. The dot may not be so small after all.

3. Epidemiology and causation testing

Let us suppose that an epidemiological survey or surveys is/are conducted in response to anecdote clusters. The raw data can be statistically analysed for some degree of correlation between experiences and can be compared with control areas where the supposed cause is completely absent. However, this is now largely impossible for EMF exposure in the current environment, with mobile technology of many varieties undergoing exponential growth, and with a target of 100 per cent coverage of the population.

Nevertheless, hypotheses can be constructed that require testing. The primary question at this stage is how this can be done and where. 'Ideally' a controlled laboratory situation is chosen, since variables can be introduced or withdrawn at will and, by degrees, under double blind conditions that are consistent for all subjects. But is this functionally correct in practice?

How might laboratory models be better constructed?

There is a problem devising laboratory conditions for EMF exposure other than, for example, direct head contact with mobile phones and heating effects. Typically, laboratory conditions will exclude all but the single condition under test, and then simplify it. (So even here, where inauthentic equipment is used to produce 'GSM-like' or 'UMTS-like' or 'TETRA-like' signals additional caution should be observed, since authentic equipment may produce field patterns or signal complexities falsely understood to be trivial.) Thus, when testing for electrical hypersensitivity or sleep disorders, it may not be good enough to select a pure GSM carrier wave in an anechoic chamber and see if, in short exposure times, anything happens. This does not represent conditions in the field, and whilst giving the impression that EHS cannot be replicated in the laboratory, fails both to account for additional factors (such as signal reflection in a different building) and also for the actual experience out of the laboratory. Just because the laboratory does not replicate the experience does not devalue the experience: perhaps instead it devalues the laboratory conditions.

There are two options that are not represented in typical experimental environments. First, there is no complete modelling of the environments within which experiences are reported. Second, there is no attempt to take the laboratory into the field. Further, there is the issue of how the experiences have come about and how time plays a part.

1. Real environment modelling

The EMF environment in a typical home, from where most anecdotal evidence ensues, is highly complex, and any home will vary from any other. Typically there are several proximate EMF sources outside the home and several inside. Factors such as variable penetrative characteristics of building materials, window size, ground conditions outside, metal structures *etc.*, combine with reflective conditions inside, house-wiring characteristics for picking up and/or retransmitting external frequencies and all the complexities of frequency interactions, constructive and destructive interference between all the various sources. Additionally, there is a very significant difference in EMF levels between Saturday club night and Sunday lie-in morning. So what do you model?

One thing that could be done is to undertake a full 3D mapping exercise of a few houses in circumstances where anecdotal experience of sensitivity to transmissions is reported. Just believe the people for a moment and undertake the mapping, by power level, frequency and magnetic field. On a computer this 3D modelling could be given for each aspect as a colour gradient/isometric representation.

It would first of all be interesting to compare the home models for similarities and differences and to see if any possible extrapolation from source characteristics would be possible. *i.e.* is there an underlying theoretical model that would substitute satisfactorily for the real thing?

It would then be possible to ask the people in the houses to describe any locational characteristics of their experiences (*eg* I always get a headache when leaning over the kitchen sink, sitting in a particular chair *etc.*). These locations could be compared with the 3D model to see if there is any correlation by power level, frequency or magnetic field. If a correlation exists, this could be compared between locations. To test any emerging hypothesis of correlation, the model could be used to first of all predict anecdotal comment

(do you experience anything in particular in this place?) and also to create those conditions in the house, artificially, and return to see if, in a reasonable space of time, it generates a new experience similar to those felt in other homes.

2. Field laboratories

Secondly, there is the opportunity of taking the laboratory to the field, in order to do the opposite to traditional methods. The real environment could be simplified, stage by stage, remodelled and retested. For example, a DECT phone in a home could be removed, the ring main switched off at night (for testing sleep effects) and so on down to Faraday protection of a bed to remove all EMF at night.

As each 3D model is revised, comparison could be made between experiences reported and features of the model.

The advantage of such a study would be that if the EMF as a cause were to be borne out, the subjects would progressively benefit. In current circumstances the subject stands to progressively disbenefit. Many so-called EHS people regard the condition to be progressive or degenerative and are understandably reluctant to have further signals passed into them in a laboratory. Further, since a fair amount of laboratory EHS study is being done by psychology departments, they leave with the impression that they are not being taken seriously and that if the experiment 'doesn't work' they will be branded as having a psychosomatic disorder, not that the experimental method might have been deficient. Researchers do not often describe their method as being deficient, if the results tend to disprove what they did not believe would be the outcome anyway.

3. The role of time and exposure

Time plays a part in dose-response studies, but not solely in terms of exposure times. There is a not unreasonable possibility that delay and lag occur, such that a subject exposed to EMF on the way to a laboratory experiment may experience the outcome of that exposure during the experiment and experience the out come of the experimental exposure after they have left.

Experiments should be as steady state as possible, under all the circumstances described above, for alternative investigation styles. But it is more likely that the researcher will experience the subject under steady state in the environment where the experience is reported, than after travelling to a completely different environment. It would be advantageous to know if one frequency or source, for example, sensitises a person, whilst another produces the experience.

Some people complain of headaches, for example, when using a mobile phone. But for some this occurs within five minutes, for others only after, say, twenty minutes. Some experience the effect after several long calls in a day, others after just one. Is one a valid experience, another not? Is this allowed for equally in laboratory experiments?

Of equal interest might be the question of how a person first came to have these experiences. Was it gradual, is it attributed to a single cause? Or was the onset abrupt? Is there a particular date, but no known reason why? Has the condition developed with an increase in transmission sources? Or of a particular kind? Or at a particular distance?

Finally there is the issue of mid- to long-term effects. EHS is generally thought of as near-instantaneous. The question is put: can we test for hypersensitivity by subjecting a self-reported 'EHS person' to a quick exposure of a GSM-like signal? The supposition is that if, in the laboratory, the result is negative or neutral, then EHS is unattributable to mobile phones. But it also follows that some people may well be sensitive as a result of chronic exposure and that their sensitivity may be more affected by one frequency than another. So this, too, must be tested, and again, a pure signal at a short exposure will reveal nothing.

Why should EHS (a) be an adequate description of the condition we are looking at in the case of mobile phones or base stations? and (b) be a simple phenomenon? Moreover, is it adequate to presuppose that short-term effects are unrelated to longer-term possibilities? And are some longer-term effects possible with as-present, but more weakly-felt symptoms? No laboratory studies featuring subjects under test for just one hour will begin to elucidate this.

4. Understanding the status quo

The assurances of safety currently given to the public typically state that whilst safety of EMF sources can never be proven (the spurious 'you can never prove a negative': no-one seems to notice that you can't prove a positive either, only give a probability), *there is no evidence that it is unsafe*. What this omits to convey is that experimental method, actual experimentation, and mapping of real environments simply *has not been done*.

So the message being given, that requires remediation by proper method, is that the experiment for causality is more powerful than the experience; that laboratory outcomes are more to be believed than what it is that people say. As a result, we have communities of people continuing to report a repertoire of symptoms that are being denied, not because of proper investigation determining that they are both not real and assuredly unrelated to EMF sources, but because of deficient research method and non-existent investigation.

This is detrimental to those communities, represents a threat to people's well-being, but also fails to inform researchers of factors that could contribute to better understanding. Without this knowledge we will continue to have safety standards that only address specific features and completely ignore others.

Addressing experimental method does not imply that we are looking for life threatening conditions. We may be. But it does imply that since anecdotal evidence is currently given no credence, the anecdotal clusters and actual experience (for whatever cause) are simply not being adequately addressed. In this day and age the social fallout of such an attitude is unacceptable.

To close, Sir William Stewart, Chairman of the Health Protection Agency and of the National Radiological Protection Board expressed this, in September 2004:

> 'The general public ... demand clear, understandable information, often so that informed individual choices can be made. There is a need to seek to ensure that advice given, and decisions made, are based on sound science, that they are independently confirmed and defensible against criticism. I have a niggling doubt, nevertheless, that when positive results of an adverse effect are eschewed, the cry goes up that they have to be independently confirmed, but perhaps too often, the same attention is not paid to the need to confirm a negative result.

Also, it is not always sensible to routinely dismiss out of hand non peer-reviewed material. Such results, of course, have to be independently confirmed, and may well be right, and can sometimes give pointers to areas where further high quality research is needed. In any case, confirmed findings, whether positive, negative or equivocal, should also be made available to the non-specialist and the general public in a way that the implications are capable of clear understanding.'

Andy Davidson, BA, MA, MBA, DIp M; November 2004

Additional comments, December 2006, in the light of completion of the Essex University MTHR provocation study, to the research team.

5. What is the sham environment (electromagnetically)?

This is surely more than a situation in which nothing specific has been added. I remarked at the time about arriving to take part, with a headache, having passed some 120 or more roadside base stations during my journey. **The sham environment should be compared with the subject's 'normal' environment, not with the GSM and UMTS exposures.**

The analogy is living in a windy city. One leans, and carries on otherwise normally. For someone who is outside a lot, leaning is normal, and sudden retreat into shelter feels very odd, because leaning becomes inappropriate. (Another analogy is 'sea legs'; sailors coming ashore from a small boat after a long time at sea find firm land disorienting.)

Similarly, anyone with a response to E / M / EM* fields (*because I don't know what part of signal is most bioactive) will naturally compensate. Mildly EHS people compensate fairly well, by this hypothesis, whereas severely EHS people cannot compensate fully at all, and so some of your subjects really fell over. Some 'arrived leaning' as it were. Into your shelter they came, and of course it felt wrong. Misinterpretations of exposure therefore should be expected, not regarded as mistaken. This will be why you probably find that mistaken perceptions are nevertheless detected more accurately in the monitored response/non-response. It may be that you find a statistical significance **against** the interpretation of expectation-anxiety (ie that the more EHS people should anticipate they will have symptoms, and as a consequence exhibit more).

Summary: a sham exposure in a field-free environment is a situation of sensory-deprivation.

6. Crossover confidence in signal identification

For some people I know there is great certainty in signal identification. For example I would still be 100% certain about TETRA. However, between GSM and UMTS the experience can be fairly subtle: is the GSM sensory response the one in the corner of the eyes, whilst UMTS is closer to the temples (or was it the other way round?) The EMF environment is so mixed now that identifying the source can be confusing, even if the sensations are distinct.

How many subjects were correct that something was on, but got the identification wrong **both times**?

In other words they were not mistaken that there is a difference between GSM and UMTS, just that they have always got it the wrong way round.

Alternatively, it may be worth following up the carrier frequencies they are most accustomed to, and compare these with your test signals. For some people, GSM is at 1800MHz, others have it locally at 900MHz, depending on the operator. Similarly some have

UMTS at 1800MHz whilst others have it at 2400MHz. If your lab GSM is at 1800MHz this could either identify or in fact misidentify UMTS.

Summary: if sensory identification of signal type is carrier frequency derived, this could explain misidentification of both signals, whilst correctly identifying that they were on.

7. Fatigue ratings

There are two reasons for fatigue in the case of EHS. One is that it is stimulated, the second that it is consequential.

First, It has in some research appeared that phone-type signals speed up brain activity. I found this in real life with TETRA to begin with.

Second, as consequence of sleep deprivation, EHS people become chronically fatigued. I found this severely from TETRA. But one does get a bit used to constant tiredness, almost as a new normality, but nevertheless you cannot sleep.

Take someone in the first class, arouse them with UMTS and fatigue may either follow, or keep them high. **This effect is not chronic.**

Take someone in the second class and deprive them of the effect that prevents sleep, and you might well find that 'nothing' (sham) brings on a wave of sleepiness. This is exactly what I found arriving at my workplace after a weekend at home after TETRA was added to the site.

Therefore it may be worth seeing if fatigue in particular also correlates to sham exposure.

Summary: take away a chronic cause of wakefulness and you may as effectively cause fatigue, as by providing a direct stimulant of fatigue.

8. Magnetic factors? Air ions?

It does appear to be the case that magnetic fields, including magnetic vectors, may have more role in biosystems than combined EM fields. Similarly electric fields, and especially air ion balance (small / large and positive / negative), affect well-being. On this latter it would be really useful to talk to experts in air ion balance and health.

Unfortunately, these factors are not controlled for in your EM lab. Indeed the air ion balance would be worth measuring, and even as an afterthought to the current experiment, expert opinion sought.

The author invites constructive comment on this paper from researchers prepared to consider and develop robust approaches that will engage people who suffer adverse effects from ELF/EMF exposure from base stations, in an atmosphere of genuine trust and open enquiry. In campaigning for people in this situation, the author wishes to engage in practical dialogue for more representative research, rather than defence of the status quo, or diatribes about the politics of the use of mobile technology.

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