

Response to COST281's 'Scientific Comment on Individuals Statements of Concern About Health Hazards of Weak EMF' (<http://www.cost281.org/activities.php>)

I. Introductory remarks

On 23rd November 2001, the Steering Committee of COST Action 281, allegedly at the request of the Irish Department of Public Enterprise, released a discussion paper – ‘Scientific Comment on Individual Statements of Concern About Health Hazards of Weak EMF’ (www.cost281.org/activities.php) - purportedly ‘*to provide a balanced view of the debate*’ initiated by my Report (‘The Physiological and Environmental Effects of Non-ionising Electromagnetic Radiation’ - http://www.europarl.eu.int/stoa/publi/default_en.htm), which had been commissioned by the Scientific and Technological Options Assessment (STOA) Programme of the Directorate-General for Research of the European Parliament towards the end of the year 2000. Far from fulfilling its proclaimed aim, however, the COST paper is an ill-considered, deliberate attempt to misrepresent and distort the sense of my text by means of many untruths and inaccuracies – often attributing to me erroneous statements that appear nowhere in my text – their intention clearly being to undermine the integrity of my work and minimise its impact, in the hope that it will either be dismissed out of hand or left unread. The whole tenor of their response betrays the hallmark of a panic, rear-guard action to attempt to maintain the (industry-beneficial) *status quo*. Their tactic is to frame their criticisms in such a way that in many cases they appear eminently reasonable to anyone who has not read my original STOA Report; in reality, however, their remarks are littered with *non sequiters*, and they repeatedly ‘go off at a tangent’ from what I actually did say, clutching at any straws they can in often quite pathetic attempts to cast my deliberations in the most negative light possible.

Before embarking on their diatribe, the authors of the COST Action 281 document would have done well to have informed themselves of the origin of my Report, as set out in the Restricted Invitation to Tender No. EP/IV/A/STOA/2000/07/03 contained in document No.25 325 issued in Luxembourg on 13th November 2000. For in Annex I, under section A.2, entitled ‘Project objectives’, it states that...‘*In particular, the study should look critically at studies and experimental results concerning such effects (of non-ionising electromagnetic radiation on the environment and on human or animal health), which for various reasons have not been taken into account by the scientific mainstream consensus*’. Again later on, in Section A.3, referring to the results of scientific experiments worldwide suggesting that electromagnetic fields exert effects on animal tissue, although perhaps subtle and as yet unexplained, it states...‘*Such results have not yet necessarily entered the realms of peer-reviewed, commonly accepted scientific wisdom*’, whilst Section A.3 concludes with the following statement: ‘*We therefore need a critical and detailed look at “alternative” theories regarding the effects of electromagnetic fields if only to disprove or eliminate them. It is recognised that the subject is controversial and that powerful commercial considerations are involved, hence the need to look closely and in a detached way at all the scientific evidence available from whatever source.*’

It is further necessary to be aware that the above restricted Invitation to Tender itself arose from dissatisfaction with an earlier Working Document of the same title as mine, which had been prepared by *Essor Europe* for the STOA Panel in May 2000 – an inherently unbalanced

document that had simply regurgitated the conventional wisdom, as promulgated by international institutions such as the WHO, ICNIRP and COST, and by other national bodies such as NIEHS and the Royal Society of Canada; this report was never published. The whole *raison d'être* of my Report was to redress this lack of balance and provide STOA with an alternative, less conventional perspective: *this is precisely what my report did!* This, however, was clearly not to the liking of the industry-infested Steering Committee of COST281, whose cage I had evidently rattled! The ferocity of their attack suggests that my Report contained things that they would have preferred not to have seen published.

The COST document will now be considered section by section.

II. Detailed analysis of COST 281 paper

Section 1. Introduction: Here, it is stated that my work is '*supported by a company, which markets EMF protective devices*'. If *financial* support is here implied, the statement is totally untrue and malicious. My general views on the importance of ensuring electromagnetic biocompatibility by taking due regard of the existence of non-thermal influences and their potential to undermine homeostasis is, of course, known to a number of such companies who occasionally seek my opinion on the likely efficacy of their products; but it is quite wrong to infer that they financially support or influence my work.

Section 2. Commentary: Exception is taken to my use (solely at the beginning of the qualitative *Executive Summary*) of the expression 'electrosmog', although nowhere did I write anything that conflicts with their remarks. They go on to say that I ignored the fact that the EC has already issued a recommendation to limit public exposure to non-ionising EMF to protect the health and well-being of the population. I ignored it, not only because it was not relevant to my case but also, because it is simply not true that existing EC recommendations 'properly' take into account the frequency of the electromagnetic fields to which humans can be exposed; frequency is taken into account only *via* considerations *not* contingent on the aliveness of the organism.

Section 2.1. Arbitrary postulates: Under this heading much effort is spent in attempting to misrepresent my remarks concerning the influence of GSM radiation on the human EEG (*e.g.* contrary to what they state, I never claimed that the EEG contains well-defined frequencies, but rather that it is the GSM radiation that does, some of which lie in the range of the human EEG), and completely ignore the well-established fact that the human EEG can be affected and entrained by weak external electromagnetic fields. Elsewhere in this section a rather fundamental misunderstanding of the differences between the effects of incoherent noise fields and coherent signals on homeostasis is revealed. Furthermore, nowhere do I claim that there is no threshold for the detection of a signal above noise; quite the contrary, as indicated in the final paragraph of page 22 of my text, which the authors of the COST document evidently did not read, or chose to studiously ignore. **Their tactic is to frame their criticisms in such a way as to give those who have not read my STOA Report the impression that I had not addressed this or that issue, whereas in reality I had, in fact, done so.** Turning to my comments concerning difficulties sometimes experienced in attempts to replicate certain effects, again my

remarks are distorted and misrepresented: nowhere do I argue that replication is unnecessary, but simply rather that difficulties must be anticipated.

Section 2.2. Inconsistencies in argument: Their fulmination against my attack on the sufficiency of existing exposure limits, on account of their neglect of any informational influences of electromagnetic fields of sub-thermal intensity, conveniently ignores the empirical fact that, irrespective of whether or not we understand how such influences can be possible, such influences do actually exist!

Later in this section is found one of their most pathetic statements, concerning the highly frequency-dependent character of non-thermal effects: they argue that the existence of (different) non-thermal effects each characterised by *different* frequencies is actually in contradiction with my '*hypothesis of frequency-selective effects, which by its nature does not allow extrapolation to other frequencies*' (such as those used in GSM)! My Tables I and II are, however, clearly prefaced with caution that not all inclusions are at GSM frequencies – *but some certainly are!* My reason for including non-GSM frequencies was simply to illustrate the fact that non-thermal effects are actually quite common at a variety of frequencies within the microwave band; accordingly, those found at GSM frequencies should not be viewed in isolation. It is, however, essential to appreciate – as they evidently failed so to do - that the existence of different non-thermal effects at different frequencies is certainly **not** incompatible with the frequency specificity of *individual* effects! *Nowhere* do I claim that those found at frequencies other than those currently used in GSM can be extrapolated to GSM – quite the contrary; in drawing attention to the empirical consistency between certain non-thermal effects and the kinds of health problems reported by some people exposed to GSM radiation, I consider **only** effects found under GSM (or surrogate GSM) conditions. Finally, their use of the word 'hypothesis' is mischievous, given that the frequency-selectivity of non-thermal effects is actually an established experimental fact, and **not** a hypothesis invented by me!

They go on to attempt to (theoretically) demolish my claim that exposure to microwave radiation pulsed at certain frequencies can trigger seizures in epileptics - *despite* the fact that there is empirical evidence, both anecdotal (humans) and published (animals), that this is indeed the case, references to which I gave.

Section 2.3. Wrong facts: Their claim that isolation from the influence of the Schumann resonance has (after 30 years of investigation) not been proved to entail adverse health consequences is again at variance with reality, as evidenced, for example, by the fact that NASA space-craft are actually equipped with Schumann simulators, *precisely to compensate for its natural absence in regions beyond the ionosphere.*

In arguing why, in their view, certain electronic instruments are more sensitive to electromagnetic radiation than are humans, and so should be better protected, they conveniently choose to ignore the fact (given in my Report) that the human EEG is sensitive to microwave radiation at an intensity as low as 10^{-15}W/cm^2 , a value well below sensitivities currently realisable technologically!

2.4. Misunderstanding and misrepresentation: Here, I am accused of referring to ‘two arbitrarily selected unpublished findings.’ The references that I gave are not, in fact, arbitrary, but are the *only ones that exist*; furthermore, one *is* published, whilst the other was contained in written evidence to the IEGMP.

They go on to say that many of the effects I cite were ‘found at exposure conditions above existing exposure limits.’ Out of the 7 examples given, this is specifically true in one case, and also holds in the case of *some* of the examples given in another two references. Rather than being an indictment, however, the occurrence of a non-thermal, frequency-specific effect under almost thermal conditions is strong evidence *in support of the reality of the effect*; for at thermal levels, such effects are usually obliterated!

They claim that my concern over the increased vulnerability of pre-adolescents is not new and has already been addressed by in the Stewart Report. This is quite true, but apparently they were unaware that it actually appeared in my written evidence to the IEGMP in December 1999.

Section 2.5. Unbalanced voluntary selection of scientific papers: Here, they claim that in my Tables I and II – which are described as ‘*evidence of health relevant EMF bio-effects*’ – I do not refer to papers containing negative results. Firstly, nowhere do I say that Tables I and II are ‘evidence of health-relevant bio-effects’, but rather that they contain indications of non-thermal effects of microwave radiation, which is quite different; only much later in my Report do I consider the possible connection between some of these non-thermal effects and adverse health effects. Secondly, I devoted an entire section to the difficulties that must be anticipated in attempts at replication, not only discussing in some detail two particular instances where negative results were obtained, but (unlike them) actually identifying possible reasons for failure; notwithstanding this, I am accused of bias!

They, on the other hand, are quick to reject any positive results that have proved difficult to replicate, without appreciating that the reason can invariably be traced to an essential difference in experimental protocol, which *effectively undermines the fidelity of the intended replication*.

Overall, their criticism reveals a not uncommon bias for accepting the veracity of negative results – which are, of course, consistent with the innocuousness of exposure to GSM radiation: for them there is no such thing as a false negative, only false positives! The reliability of any *positive* results is invariably called into question, some excuses being given as to why they should be rejected. Further, if there are roughly equal numbers of positive and negative outcomes in purported ‘replications’ of the same experiment - as they claim is so in the case of the influence of EMFs on calcium ion flux across cell membranes - it is invariably concluded that there is *no* effect – a feature that, incidentally, characterises also the report of the IEGMP (the Stewart Report).

Section 2.6. Use of non-scientific information: Here, I am criticised for saying that ‘anecdotal reports are an indispensable source of information.’ Contrary to what they say, nowhere do I claim that such reports are ‘accepted’. My original comments simply reflected those of (i) the UK Commons’ Select Committee, that such reports are an indispensable source of information that can ‘usefully serve to target further research’, and (ii) the conclusions of the Royal Society

of Canada that there is already sufficient anecdotal evidence (of problems of a neurological nature) to justify further research.

Conclusion: The criticism that my STOA Report is '*unbalanced, uncritical and suffers from a narrow selection of partly outdated literature and arbitrary postulates*' betrays an ignorance of conditions surrounding its commissioning and, in particular, of the function it was intended to serve, both of which have already been addressed in the Introduction. Essentially, the purpose of my Report was itself to balance an earlier one of the same title, the contents of which COST 281 would have not taken any exception, simply regurgitating, as it did, the conventional industry-friendly wisdom.

Far from being uncritical, my Report is highly critical not only of existing safety guidelines, but also of so-called replication attempts on whose negative outcome the tenability of these guidelines so crucially depends.

Furthermore, it is quite untrue to assert that I invoke non-thermal effects at frequencies different from those used in GSM telephony in consideration of health problems reported by some people exposed to GSM radiation; I always clearly identify those results that are obtained under GSM (or surrogate GSM) conditions and those that are not.

Finally, is claimed that I have '*challenged the scientific requirement of replication and of independent confirmation of effects for being considered as established*'. Again, this is a deliberate misrepresentation of what I had actually said, and is quite untrue; rather, I had simply drawn attention to the fact that difficulties in replication must be anticipated, in consequence of the non-linear nature of the effects being investigated.

III. Postscript

Far from providing the public with proper, balanced information, as was their asserted aim, the authors of the COST 281 Paper have completely misunderstood the purpose of my STOA Report and have deliberately misrepresented and distorted the sense of much of my text. Overall, the whole tenor of the COST 281 Paper reflects anger and resentment that an attempt has been made to disturb the industry-comfortable *status quo*: given the industrial connections of certain influential members of COST 281, however, perhaps this is only to be expected.

G.J. Hyland, February 2002