Errors within Errors within Errors

On the Occurrence of Curious Errors in Scientific Research on Possible Adverse Health Effects of Mobile Phone Radiation

by

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1. Introduction

In the past decades the use of mobile phones and other forms of wireless communication technologies has risen exponentially. These technologies make use of low level non-ionizing radio frequency electromagnetic fields (RF-EMF), which are currently deemed safe for modern day use. Because high energy levels of RF-EMF are known to be able to cause adverse health effects by generating heat in organic tissue, strict maximum exposure levels have been set for the use of RF-EMF. Current safety guidelines have been established by the International Commission on Non-Ionizing Radiation Protection (ICNIRP) in the late 1990's, and are based on maximum energy levels of RF-EMF that can be absorbed by organic tissue without resulting in significant increase in temperature. These measurements are known as the Specific Absorption Rate (SAR) of RF-EMF, and are most often given in watts per kilogram (ICNIRP, 1998).

Even though RF-EMF used in mobile phones and other kinds of wireless communication technologies generally stay well below these established safety levels, since the onset of mobile phones concerns have risen among a small portion of the population about possible adverse health effects from these kinds of RF-EMF. Most concerns are based on the believe that exposure to RF-EMF from mobile phone use might increase risks of developing brain tumors, while over the years people have also began to associate other adverse health effects with exposure to low level RF-EMF, including heightened allergic reactions, headaches, cognitive decline, and other subtle health effects. The existence of any kind of adverse health effects from exposure to RF-EMF below current safety levels are generally dismissed as implausible however, as currently there are no scientifically established mechanisms that can sufficiently explain biological effects from exposure to low level RF-EMF. Any directly perceived health effects are instead explained as most likely being caused by other factors, such as stress, or relegated to psychosomatic symptoms.

Nonetheless, some concerns are shared by a number of scientists currently engaged in research on possible adverse health effects from exposure to RF-EMF. Opinions among scientists differ greatly within this research field though. Currently there appears to be no consensus among scientists on the plausibility of the existence of bioelectromagnetic phenomena associated with low level RF-EMF, and scientific views between different leading research groups sometimes even appear diametrically opposed. This seems especially the case among research groups that are studying more serious health effects, such as a possible association between mobile phone radiation and risks for developing brain tumors.

In recent years, the dispute between different research groups studying possible RF-EMF health effects appears to become more and more severe. Given the significance of scientific insights

on these phenomena for a society which increasingly is built upon on RF-EMF technologies, I decided to look into a number of major scientific studies done in the past several years on possible adverse health effects of RF-EMF — focussing mainly on mobile phone radiation and brain tumor risks — and cover the quibbles between scientists that are involved in these studies, in hopes of getting a clear picture of what factors drive the current dispute.

This paper started out as a sociological treatment of the field of knowledge surrounding RF-EMF and health research, and how such knowledge manifests in society. As my research expanded, I started to concentrate more and more on the details of scientific publications on RF-EMF. In the end the main focus of this paper lies on uncovering the many errors and miscommunications between different publications, resulting in a number of unexpected and potentially troublesome revelations I myself had not foreseen. Incidentally, these findings resulted in this paper becoming much longer than usual for a paper of this kind, yet I did not see a way to make it shorter without compromising the quality and outcome of my research.

Ideally I hope this paper will create a better understanding of the difficulties the RF-EMF health research field is currently facing, and show the crucial necessity for critical reflections, from both a scientific and a sociological perspective, of the issues that complicate RF-EMF health research.

2. The Internet

Every now and then an article or news item pops up in the mainstream media covering scientific research on the possible adverse health effects of mobile phone radiation or other forms of RF-EMF used in wireless communication technologies. If one pays a bit more attention to these news articles, one will quickly notice many articles report conflicting findings contradicting each other (Bilton, 2015, Davey, 2016, Demasi, 2016, Sullivan, 2015). This might quickly leave one confused about the scientific views on these matters. If one study shows there is a possible link between, for example, brain cancer and mobile phone use, and another study claims no such link can be shown, what should one believe? Is there a reason to be concerned? Are the researchers that believe there are possible adverse health effects associated with RF-EMF technologies simply hunting for ghosts, suffering from wild imaginations and improper research equipment? Or are we being played by nefarious industries trying to stray our attention from things we'd rather not know anything about? Unfortunately there is no easy answer to this.

The most common sense way of dealing with apparent discrepancies between scientific views, is to dive deeper into their publications and examine possible differences and similarities in order to find possible causes that lie behind the conflicting outcomes. However, scientific publication are often difficult to find and comprehend for the lay person. Therefore, most people rely not so much on first hand scientific publications, but on other sources in order to get insight into a topic of interest, such as newspapers, magazines and books. Nowadays many of these sources can be primarily found on the internet, and the internet itself has given rise to a great amount of additional sources of information. Because for many people these kinds of sources on the internet nowadays more and more often form the first exposure to new information, I will first take a look at what kind of information one might find on the internet about mobile phone radiation and health research, before examining the scientific research itself. This way I hope to give an impression of how readily available information stacks up against harder to find and, especially, more difficult to comprehend scientific publications. Later in this paper it will hopefully become clear why I have chosen this approach.

2.1. Searching

A simple search on the internet on "mobile phone radiation" at first shows a number of results of government and popular science websites that reassure there is currently no established

scientific evidence for adverse health effects associated with RF-EMF used by mobile phones, Wi-Fi routers and other devices. However, one will quickly find more websites, such as www.sarshield.com, www.emfnews.com and www.earthcalm.com, that offer alarming information that purports established scientific evidence exists about severe adverse health effects associated with these kinds of RF-EMF.[1]

If one starts to become concerned by the information given by some of these websites, luckily they often offer a wide range of solutions to reduce exposure to RF-EMF. Besides tips on reducing overall exposure, these solutions come in the form of different kinds of devices and health products that are sold, which can purportedly shield and protect people from exposure to RF-EMF.

2.2. George Carlo

A researcher who's name seems to circulate widely on the aforementioned websites is George Carlo. The website of SAR Shield for example cites Carlo on its product info page as follows:

> "The number of scientists warning manufacturers, the media, and governments of the health dangers caused by electromagnetic pollution is continually increasing. [...] George Carlo, a public health researcher who spearheaded a three-year, $27 million research program for the cellular telephone industry on possible health risks associated with such devices said in his report that the radio frequency radiation from wireless phone antennae 'appears to cause genetic damage in human blood,' while another case study uncovered a 'statistically significant increase' in neuro-epithelial brain tumors among cell phone users." ("Product Info SAR Shield", n.d.)

On the website of LifeExtention, a supplier of nutritional supplements that apparently also publishes a lifestyle magazine, the work of Carlo is covered more extensively in an article dating from August 2007 (Kovach, 2007). The article details on the developments in mobile phone health research in the early 1990's in the US. The picture that is portrayed does not look very pretty. The article covers how George Carlo was selected as an outsider in RF-EMF health research to lead a large multimillion dollar study on the possible health risks of mobile phone radiation. This study was set up by the telecom industry under the umbrella of the Cellular Telephone Industry Association (CTIA) and overseen by the FDA. Carlo himself is cited describing how these different

[1] In order to avoid filter bubbles skewing search results towards my search history and other personal information, I have used the search engine www.duckduckgo.com for these search term results.

interest groups were undermining each other's aims, while he himself tried to turn the project into a viable research program by inviting renowned scientists from the field.

As the study developed, the telecom industry became increasingly discontent with the results of this project, and put the research team of Carlo under increasing pressure to distort the results. While Carlo tried to continue releasing the undesired findings, by the end of the 1990's, according to the article, he started to experience multiple attempts at character assassination directed at him from the industry. At the end of 1998 Carlo's house was burned down, apparently by arson, and since then he decided to take a break and leave the scene.

A few years later Carlo got into contact with a journalist, and published a book in 2001 on the dangers of cell phone radiation. Around the same time Carlo set up different organizations surrounding RF-EMF safety topics directed at consumers — apparently as part of a court settlement of a lawsuit set up by consumers against the research project Carlo had headed, as this project failed to deliver any results in the end. When describing this puzzling court settlement in the interview, Carlo referred to his published book on cell phone dangers in order to make clear that, despite the failings of the research project he led, he himself was in fact on the side of concerned consumers.

Next to this compelling story behind Carlo and his research project, the article addresses some information about health effects from RF-EMF exposure established by Carlo that looks very concerning, such as "intracellular free-radical buildup, leakage in the blood-brain barrier, genetic damage, disruption of intercellular communication" (Kovach, 2007). It furthermore mentions expected skyrocketing brain tumor incidence rates by 2010, some European studies confirming the findings of Carlo, and possible involvements of industry in a number of other European studies that do not seem to find any association between mobile phone radiation and adverse health effects. The article finishes off with a boxed text that mentions a few studies on the protective effects of few specific vitamins and the hormone melatonin against the harmful effects of RF-EMF.

2.2.1. Looking in more detail at Carlo's output

Looking more into Carlo's output, he appears to be a prolific critic of mobile phone radiation, and has written critical articles on the current state of RF-EMF health research which, for example, can also be found on websites such as the one of Powerwatch (www.powerwatch.org.uk) — an organization based in the UK that extensively follows and covers scientific developments on EMF

health research, while at the same time offering different kinds of protective materials and measurement devices in an associated web shop called EMFields (www.emfields-solutions.com).

Throughout his later career, it appears Carlo became an active supporter of a device that could protect against harmful EMF, known as BioPro. However, in 2008 it was uncovered that BioPro was a sham device that had no electromagnetic protection powers whatsoever. When the story got out BioPro was a sham device, Carlo publicly distanced himself from BioPro in a public statement, also shared on the website of Powerwatch, explaining he got tricked into believing it was a proper device by data handed to him by its creators (Carlo, 2008).

When looking further into Carlo on the internet, more strange things start to show up about him. SourceWatch, a website which tracks industry involvement of researchers and scientists, describes a long history of questionable science projects by large industrial corporations in which Carlo apparently was involved. It details Carlo has been associated with chemical companies such as the Dow Chemical Company to sooth the public against fears of Agent Orange (dioxin) problems in the 1960's, and that he was later working for Philip Morris to falsify scientific statements against links between second-hand smoke and lung cancer. It was after these dubious involvements that, in the beginning of the 1990's, Carlo was asked by the CTIA to run an advisory board for the US government on RF-EMF, which was the research project he left a little decade later after unresolved disputes. Millions of dollars of funding from the telephone industry had been spend on sham research by the time the project was cancelled. According to SourceWatch, only after leaving this project Carlo sided with RF-EMF critics, and since then has claimed his split with CTIA was due to his disagreement with malicious science manipulation practices of the industry he witnessed as a researcher ("George L. Carlo", n.d.).

According to Microwave News (a relatively established scientific news outlet covering EMF research) and other sources, Carlo's involvement with BioPro was probably also more than a mere accidental misstep, given his long history with other dubious industries ("Scams Galore, 2010, George Carlo Abandons BioPro!", 2008). Carlo's statements following the demise of BioPro are viewed by these sources as an attempt of hiding his original devious intentions, and his splitting with the CTIA is explained as most likely caused by financial disagreements — not any 'ethical' objections, as Carlo himself has proclaimed since the split.

2.2.2. Final Consequences

To what extend these accusations are indeed true or not seems difficult to trace if one relies solely on the internet and remnants of other documents found on there. Carlo seemed to have been quite a prominent figure in the 2000's in the RF-EMF debate, even making national television appearances in the USA (Emf Less, 2011). Since the BioPro scandal in 2008, it appears he has disappeared from the internet however, with several of his websites closed down. A Wikipedia page that once existed on him has been removed, with only a deletion log notification on Wikipedia remaining, dating from June 2012, stating: "Non-notable scientist. Having looked through the history of this article and done a search for sources, I cannot find enough coverage of him in third-party sources to satisfy the notability guidelines." (Jenks24, 2012)

2.3. Powerwatch

The aforementioned website Powerwatch, on which some articles from Carlo can be found, is run by Alasdair Philips, an electronics engineer who has been involved with Powerwatch since the late 1980's. Powerwatch seems to be a more reliable source than Carlo (despite its support of him when he was still active online). It extensively covers new scientific developments and even has appeared on television shows (Goldacre, 2007a). However, between 2006 and 2008 Powerwatch has had numerous disputes with Ben Goldacre, a popular British science journalist and columnist for The Guardian. Goldacre has been running the weblog *Bad Science* (www.badscience.net), which mostly focuses on debunking pseudoscience, and next to this has written a few popular critical science books on topics such as pseudoscience and associated alternative medicine practices (Goldacre, 2009).

An example of the disputes between Powerwatch and Goldacre is their views on electrosensitivity (ES). ES a term used for a possible sensitivity for EMF which some people believe they are suffering from, and could result in symptoms including headaches, skin problems, fatigue and other related effects. Goldacre has been a strong critic of what he calls the 'electrosensitivity lobby', and published a number of pieces on ES and the lobby behind it on his blog. In a few articles he repeatedly has pointed to the lack of evidence in a number of prominent studies on ES (Goldacre, 2007c). Powerwatch takes a different standpoint however, and pointed to shortcomings in these studies that would render them grossly insufficient in providing conclusive evidence against ES ("EHP publish Powerwatch response to Eltiti "Essex" Study.", 2008).

Besides these on-topic disputes with Powerwatch, Goldacre has reported on the rather suspicious practices of Powerwatch. He points to the fact that the organization behind Powerwatch is selling rather expensive equipment for detection of, and protection against, 'electrosmog' (a term commonly used by EMF critics for artificial EMF believed to be harmful), and their apparent attempts at scaremongering the public, for example on the program BBC Panorama (Goldacre, 2007b). Philips has responded extensively to this criticism of Goldacre. He claims the products they sell help people protect themselves against harmful radiation for as long as governments won't take action, and are 'military tested' and therefore expensive to make. Furthermore, Philips also points to the fact that they have never hidden the their involvement in selling protection materials, leaving the public free to make their own judgments ("Our response to Ben Goldacre's comments re. Panorama, WiFi, etc.", 2007).

In defense of Powerwatch, one can note that Powerwatch has only minimal advertisements for its shop counterpart, which only sells a small selection of protection material (such as steel-wired fabrics) and measurement equipment that do seem legitimate, while they do not seem to sell bogus gizmos, such as the aforementioned BioPro, that do not shield or give any real 'protection' against any RF-EMF. Still, as Goldacre suggests, Powerwatch's economic side activities can be a potential source of bias for the views they present.

2.4. Ben Goldacre and Martin J. Walker

When I was diving deeper into this dispute, I discovered Goldacre himself has created a rather large and hostile opposition during his crusade against pseudoscience. In recent years, a book by Martin J. Walker, titled *Cultural Dwarfs and Junk Journalism — Ben Goldacre, Quackbusting and Corporate Science*, started circulating among alternative health websites and affiliated sources, which challenges Goldacre's reputation as a trustworthy science journalist (Walker, 2008).

The book, which is free to download, is accusing Goldacre of having numerous ties with corporate industries, forming the source for his campaign against so called 'bad science' to oppose legitimate alternative medicine practices in favor of mainstream pharmaceutical industries. According to the book, Goldacre's ties even extend directly to the telecom industries, helping them hide their abusive practices — and one of the victims of the campaign of the telecom industries against honest science is the aforementioned aforementioned George Carlo.

The writer of this book appears to have a rather questionable reputation himself however. A number of skeptic bloggers wrote about the work of Walker, including Skepticat (MacLachlan,

2011) and the journalist Brian Deer (Deer, 2011). According to these writers, Walker's work is full with unsubstantiated claims, lacking proper sources while mainly consisting of direct attacks against skeptics and other critics speaking out against dubious alternative health figures, such as the widely discredited vaccine researcher Andrew Wakefield (Deer, n.d.).

In *Cultural Dwarfs*, Walker mainly defends the nutritional therapist Patrick Holford, a well-known supplier of health supplements in the UK. Goldacre has repeatedly attacked Holford's enterprise, describing it as a scam solely set out to make money from susceptible alternative health followers by selling useless products to them (Goldacre, n.d.) In light of Walker's works defending these kinds of questionable individuals, he is described as a "liar for hire" by Deer and Skepticat, and is accused of writing books and articles for whomever offers money in the alternative health sector for a fierce defense of their practices.

If these accusations are correct, it must be noted that the attack on Goldacre in *Cultural Dwars* appears to be quite cleverly set up by Walker. By defending a large number of alternative health practices and fringe scientists — who, next to Holford, have been scrutinized by Goldacre as well — Walker appears to try to tarnish the reputation of Goldacre under an as large following as possible, in order to cleanse the name of Holford and his enterprise among people who are open to alternative medicine, and feel this sector is mistreated by the extensive criticism of Goldacre and the like.

It's probably wise to note at this point that Goldacre, after his extensive scrutiny of the alternative health industry in the 2000's, has since shifted his focus towards dubious practices in the pharmaceutical industries and their manipulation of scientific research, and has become a strong advocate for reform of scientific publishing protocols to minimize selective and manipulative research and publishing practices by pharmaceutical industries and the like (Goldacre, 2012).

2.5 Summary of The Internet

The internet yields many more sources on 'mobile phone radiation', but it appears from these discussed web results that many online sources that proclaim mobile phone radiation is dangerous, are mostly associated with companies interested in selling health products and protection material against proclaimed harmful RF-EMF. This makes these kinds of claims rather unconvincing at best. However, there are exceptions such such as Microwave News, that do seem to give well founded information on scientific developments on these matters. It remains difficult to tell however what to believe, so in the next section I will finally dive directly into the scientific

publications most prominently under dispute, and examine possible differences and similarities between these studies in hopes to get a more clear picture of the causes behind the different views on this matter.

3. Scientific Studies

Within the research field concerned with possible adverse health effects associated with RF-EMF, in recent years several large scale studies have been set up in hopes to settle the debate on whether mobile phone radiation can cause brain tumors and /or other adverse health effects. Some of the largest and most widely cited studies in this regard are the multinational 'Interphone' case-control study, a number of smaller case-control studies by a research group led by the Swedish oncologist Lennart Hardell, and a large Danish cohort study.

3.1. Interphone

The Interphone study was set up by the International Agency for Research on Cancer (IARC), which is part of the World Health Organization (WHO). Over the years several reports where made with intermediate results from different countries participating in this project, and the final results of the study were published in 2010 (The INTERPHONE Study Group, 2010).

The Interphone study was set up to provide a thorough and well balanced analysis of possible health risks of mobile phone radiation, and was anticipated to provide conclusive results about the possibility of carcinogenic effects and other adverse health effects from RF-EMF produced by mobile phones. Despite it's ambitious approach however, the study was plagued by a number of issues, which scientists, the mainstream media, bloggers, and organizations concerned about possible health risks associated with use of RF-EMF, reported extensively on. Eventually the study underwent delays of several years. This was mainly caused by complications in assessment of data, and the involved researchers not being able to reach a consensus over the established results. Disputes among researchers continued until after publication (Sample, 2010, "Interphone Points to Long-Term Brain Tumor Risks; Interpretation Under Dispute", 2010).

To summarize the findings: the Interphone study found a possible weak significant association for increased risks of developing brain tumors in the heaviest category of users of mobile phones. These findings where countered however by an apparent protective effect for risks of developing brain tumors in moderate users of mobile phones. In other words, moderate use of mobile phones results in a lower chance of developing brain tumors compared to no mobile phone use at all, according to the final results published by Interphone. Because of unresolvable differences of opinion among the scientists involved with Interphone about interpretation of these results, the conclusion of the study turned out as follows "Overall, no increase in risk of glioma or meningioma

was observed with use of mobile phones. There were suggestions of an increased risk of glioma at the highest exposure levels, but biases and error prevent a causal interpretation." (The INTERPHONE Study Group, 2010).

On the date of publication the study was already accompanied by an editorial which criticized the study design on several levels, noting a number of issues in the methodology and the results of the study that could indicate a downward bias in the study (Saracci and Samet, 2010). Most notably is the aforementioned fact that the Interphone results show moderate users have a lower risk of developing brain tumors than non-users. As a protective effect of mobile phone radiation is considered highly unlikely, these results are suspected to be caused by some kind of bias of a variety of sources.

A possible cause of the lowered risks in moderate users might be that mobile phone users are generally more healthy people than non-users. Acknowledging this as a potential influence on the outcome of the study, the Interphone group decided to do an additional analysis where non-users were taken out and replaced with the lowest category of users of mobile phones, which were then compared with the higher category users. This analysis shows a higher risk for high category users of mobile phones than the main results of the study. However, it was reasoned that it was not possible to rule out that different forms of participation bias also affected the observed risks in the highest user groups in an *upwards* fashion, and therefore these results where not added to the main conclusion of the study. The additional analysis was published in an appendix of the final report, only available separately (Saracci and Samet, 2010, The INTERPHONE Study group, 2010).

Another issue of the Interphone study noted by Saracci and Samet was the time period over which users had used mobile phones, which as of yet is a common issue in research on mobile phones and brain tumors, given the short history of use of these kind of devices. Most subjects in the Interphone study only used mobile phones for less than 10 years. Many cancers caused by known environmental hazards take much longer to develop, even from known carcinogenic forms of ionizing radiation, thus it could hardly be expected that any results would come out of the study given the limited timeframe which is used (Saracci and Samet, 2010).

Another noteworthy observation on difficulties surrounding the study can be found in an article of The Economist, which explains some more issues in the study design that could lead to overestimation of risks, such as recall bias — which is a well known possible bias in case-control studies of this type. The case-control design of the Interphone study has been set up in such a way that cases were selected on the basis of whether they had developed brain tumors. The mobile phone use of these cases was then compared to the mobile phone use of comparable participants, the controls, who had not developed any tumors. Participants where asked to recall mobile phone

use in the past to assess their exposure to RF-EMF from these devices. It is not hard to imagine an assessment based on memories will turn out skewed. Especially when one has developed a brain tumor, it is reasoned, one might suspect a possible link between ones phone use and the tumor, and could overestimate the amount of use of ones mobile phone as a result. The Economist mentions that the recall data of some of the different Interphone user groups of different countries was compared with data from phone companies on the amount of calls and call time. The recall data did not compare well with the data from the phone companies in this comparison, and pointed towards a possible overestimation of the risk ("Mobile madness", 2008).

More critique on the Interphone study, mainly directed towards possible selection bias of study participants and erroneous definition of users groups, has been expressed by a research group led by the Swedish oncologist Lennart Hardell, who has done similar research on mobile phone use and brain tumors. Hardell et al. pointed out the exclusion of adolescents and young adults up to the age of 30. They argue that, according results from their own studies, users that started to use a mobile or cordless phone before the age of 20 have the highest risks for developing brain tumors associated with mobile phone use. The exclusion of subjects below the age of 30 in the Interphone study might therefore lead to lower overall observed risks. A second issue in the Interphone study which Hardell and his team pointed out, was the exclusion of cordless house phones in its selection criteria. Hardell et al. argue cordless house phones make use of comparable RF-EMF technologies as mobile phones, and exhibit comparable exposure levels. This would be a good reason to include users of cordless phones in the exposure group. However, cordless phone use was not considered in the final report by the Interphone group, and cordless phone users ended up in the non-exposed group. If use of cordless phones can increase the risks for developing brain tumors as well, leaving users of cordless phones in the control group will equal out possible differences between the control group and the exposure group, leading to underestimation of the risks (Hardell et al., 2011).

3.2 The Hardell Studies

The Hardell group has published several papers on a number of case-control studies on mobile phone use and brain tumor risks. Over the years, these studies have started to show increasingly strong indications for increases in risk. The first of their studies on this topic was published already in 1999, though only in later studies statistically significant results came out, with a 2011 pooled analysis of their studies showing a more than twofold increase in risk for malignant brain tumors associated with wireless phone use of more than 10 years (OR = 2.1, 95% CI: 1.6, 2.8)

or more than 2000 hours of cumulative wireless phone use (OR = 2.4, 95% CI: 1.8, 3.3). (Hardell et al. 2011, Hardell et al. 2013a).[2]

The results of the Hardell group seem to be in conflict with the results which came from the Interphone study, which has given far less strong evidence for any association compared to what the Hardell group has consistently shown over the years, though Interphone was much larger in scale.

Unfortunately, in the final report the Interphone group hardly discussed the Hardell studies, nor other relevant studies that had been published before their final report, despite comparable study designs which makes it relevant to discuss differences and similarities between these studies. The final Interphone report does refer to a review by Ahlbom e.a however in this respect, which prominently criticizes the methodology of the Hardell group (Ahlbom e.a, 2009).

The primary argument against the Hardell studies in the review of Alhbom et al. appears to be the fact that the data presented by Hardell et al is inconsistent with most other studies Ahlbom et al. assessed. Where most studies they assessed showed mostly no risks for the development of different types of brain tumors in mobile phone users, only the studies of Hardell and one other study showed increased risks (Ahlbom e.a, 2009). Looking for a possible explanation, the biggest difference Ahlbom et al. were able to observe between the studies of Hardell et al. and other studies they reviewed, was the inclusion of cordless house phones in the Hardell studies. In order to address whether this inclusion could be a possible cause for the different results of the Hardell studies, Ahlbom et al. referred to a few assessments of cordless house phone use by two Interphone groups (one from Germany and one from Sweden), which were published in separate intermediate papers in previous years (Lönn et al., 2005, Schüz et al., 2006a, 2006b). Based on these papers Ahlbom et al. ruled out the inclusion of cordless phones as a possible cause for the difference in results.

Given that the discrepancy with other study results remained unresolved with this analysis, Ahlbom et al. concluded there must be something else wrong with the studies of Hardell et al. (Ahlbom e.a, 2009), although they could not specify what. However, one thing to note is that these intermediate Interphone reports indicated similar results for cordless phones as the overall results for mobile phone use in the Interphone study, namely: a tendency towards *decreased* risks of brain tumors for moderate to regular users of cordless phones. These numbers were only based on crude exposure assessments (especially compared to the detailed assessments of Hardell et al. in their publications). Also, the highest category of exposure for cordless phones in the intermediate reports of the Interphone groups was 5 years of use or more (compared to 10 years or more in the same

[2] The OR (odds ratio) gives the estimated relative risk of exposed subjects compared to non-exposed subjects. 1 indicates no increase, anything higher than 1 indicates an increase in risk, and anything lower than 1 indicates a lowered risk. CI (confidence interval) shows the amount of uncertainty associated with the estimated odds ratio. When the confidence interval does not cross 1, the calculated odds ratio is statistically significant. (Hardell et al., 2013a).

paper for mobile phone use, which, also interestingly, was the only exposure category of mobile phone users based on time since first use that, although not statistically significant, showed an indication for an increased risk (OR=2.20, 95% CI: 0.94, 5.11) for glioma type brain tumors) (Schüz et al., 2006a). Why these details are omitted by Ahlbom et al. does not become clear from their paper.

3.2.1. Hardell's response to criticism

When Microwave News covered the final Interphone publication, they asked Hardell about the criticism from the Interphone group on his studies, to which Hardell replied "I cannot understand their statements, either they do not understand or have not read my papers". Microwave News added that Hardell considered the Interphone results to support his own findings ("Interphone Points to Long-Term Brain Tumor Risks; Interpretation Under Dispute", 2010). In contrast however, after the final Interphone results were published, the overall results of the different studies of Hardell et al. received further criticism due to their perceived dissonance with the Interphone results, which were deemed more reliable due to Interphone's larger budget and scope.

In response to the criticism from critics citing the Interphone study, Hardell and his team decided to include a reassessment of their data on glioma risks for the highest exposure groups in their own critique of the Interphone study, using the same selection criteria as the Interphone group had used in their analysis. For this reanalysis, Hardell et al. narrowed their age range of 20-80 years down to the 30-59 age range used in the final Interphone report. Compared to their original results, this *decreased* brain tumor risks in the exposed groups in their own data. Furthermore the Hardell group showed what happened when leaving out cordless phone use in the exposed group, and adding this data to the control group in their reanalysis. This showed another decrease in brain tumor risks for mobile phone use when compared to the original data analysis.

Next to using the primary results of the Interphone study, for their comparison the Hardell group also took into account the additional analysis found in the appendix of the Interphone report. Using these adjusted selection criteria based on the criteria of the Interphone study, the final comparison made by the Hardell group showed surprisingly similar risks for glioma brain tumor development associated with heavy use of mobile phones in the datasets of the Hardell and Interphone studies (Hardell et al., 2011).

Carefully considered, with their re-analysis the Hardell group countered most criticism directed at their results, and have given a compelling argument for the validity of their own results,

and the results found in the appendix of the Interphone report that indicate increased risks for brain tumor development associated with heavy use of wireless phones.

3.3. WHO and IARC

Critics will maintain case-control studies such as those of Hardell and Interphone are subject to methodological shortcomings such as selection and recall bias, as mentioned in section 3.1. Nonetheless, the outcomes of the Interphone study and the studies of Hardell et al. are substantial. Together these studies turned the balance for the WHO and IARC when they reassessed the possibility of carcinogenic effects of RF-EMF in 2011. In May 2011 the WHO and IARC issued a statement that they had classified RF-EMF as "possibly carcinogenic to humans (Group 2B), based on an increased risk for glioma, a malignant type of brain cancer, associated with wireless phone use." (World Health Organization, 2011).

The 2B classification indicates there is a possibility of increased risk for development of cancer from exposure to a substance, but without conclusive evidence. To put the 2B classification into perspective, other substances that are categorized under this category include coffee, DDT and dry cleaning fumes ("Agents classified by the IARC monographs, volumes 1-114", n.d.).

3.4. The Danish Cohort Study

Already shortly after IARC announced their reclassification for RF-EMF, new results from other studies were published which countered the findings of both the Hardell group and Interphone. The nationwide Danish cohort study (Frei et al., 2011) has been the most cited study in this respect, especially within mainstream media. Several reports of this study have been published before as well (Johansen et al., 2001, Schüz et al., 2006).

In the most recent update of the Danish cohort study, published in 2011, the incidence rates of brain tumors in more than 350.000 Danish mobile phone subscription holders was compared with brain tumor incidence rates in 3.21 million subjects of the Danish population. Earlier reports of this study were already included in the IARC assessment, but this last report was published after the IARC decided on its 2B classification. The study was solely based on record linkage — meaning no personal contact was needed with participants for data collection. Data on brain tumor incidence rates was taken from the Danish Cancer Registry, and data about mobile phone subscriptions was collected from telephone companies, so no recall bias could appear. The researchers concluded in

2011 that there was no indication for increased risk of brain tumors whatsoever amongst mobile phone users as of yet (Frei et al., 2011).

Given the scale and setup, this study has been presented as a major indicator for the non-existence of any link between RF-EMF and brain tumors. The 2011 update was accompanied by an editorial by Ahlbom and Feychting, hailing it as a solid indication of the non-existence of an increase in brain tumor risks for mobile phone users (Ahlbom and Feychting, 2011). Ahlbom and Feychting supported the results of the Danish cohort study with data from the Swedish Cancer Registry, showing no increase in brain tumor incidence rates in recent decades. They stated that the explosive increase in mobile phone use in the past decades without any increase in brain tumor rates in Sweden provided a powerful additional argument for no association between increased risks of brain tumors and mobile phone use. Furthermore, they mentioned these statistics were a strong argument against other epidemiological studies showing increased risks, such as the studies of Hardell et al. — if increased risks would indeed have been present, as these studies appear to show, then it would be expected these effects had began to to show up in national brain cancer statistics already.

However, the Danish Cohort study has been criticized extensively for a number of possible flaws and errors by several different experts and RF-EMF researchers ("The Danish Cohort Study: The Politics and Economics of Bias", 2011, "Use of mobile phones and risk of brain tumours: update of Danish cohort study — All rapid responses", n.d.). One of the most emphasized issues of the study design is that a group of potentially heaviest users were *left out* of the mobile phone user group. As described in the publications of the Danish Cohort study itself, a substantial amount (more than 200.000) of subscriptions in the time period covered by the study were corporate phone subscriptions, and could therefore not be included in the exposure group because no personal data was available of these users. These users therefore instead ended up mostly in the control group. Another major reported flaw was the lack of any data about usage. No data about the amount of mobile phone use of subscribers was used in the study. Only information on when subscriptions were started was available. Adding to this, only users who started a phone subscription before 1996 were part of the exposure group. This group was compared to the general population of Denmark that had no subscription before 1996. This means people who took a subscription after 1996 were included as *non* users in the study. Also, again no data on cordless phone use was used in the study.

The researchers themselves addressed all of these issues in their reports, and according to their own analysis most of these elements were unlikely to be significant confounders. (Frei et al., 2011). However, summarizing the biggest issues of the Danish Cohort study in an opinion piece for the scientific magazine *The Scientist* in 2013, the Polish radiation researcher Dariusz Leszczynski

went as far as stating that the errors made in the Danish Cohort study were so severe that it should not even have been accepted for publication (Leszczynski, 2013, February 25).[3]

Another more thorough critiques on the Danish Cohort study came from the research group of Hardell, in an extensive review of four reports of the Danish Cohort study (Söderqvist et al., 2012). It addresses most of the issues raised above in this section. Notably however, this review also addressed the editorial of Ahlbom and Feychting and its discussion of reported brain tumor incidence rates in Sweden. Söderqvist et al. found it rather peculiar that the piece did not use data from the Danish Cancer Registry instead, given the Danish Cohort study solely covered the Danish population. Söderqvist et al. subsequently pointed to relevant data on brain tumor incidence rates in Denmark, which showed a significant increase between 2000 and 2009. Concerning the data coming from Sweden, Söderqvist et al. noted that the data collection methods of the Swedish Cancer Registry have been questioned in other studies, having found underreporting in relation to nervous system tumors and a number of other cancers (Barlow et al., 2009).

3.5. Brain Tumor Incidence Trends

The Nordic countries have attracted special interest for statistics on brain tumor incidence and mobile phone subscriptions, as these countries are amongst the earliest adapters of mobile phone technologies worldwide.

Looking at other publications on cancer incidence rates among the Nordic countries, a paper by Bray et al. (2010) found overall patterns of increasing incidence of brain tumors in the past few decades. One exception among the Nordic countries was again Sweden, showing a downward trend in brain cancer incidence trends. The paper points to known issues in data collection in Sweden as a cause for this deviation however. The researchers furthermore mention that the increases observed in the other Nordic countries were difficult to explain (without mentioning anything about mobile phones). They note these increases might, at least in part, be explained by changes in diagnostic techniques (Bray et al., 2010).

Data from other countries have also been used to assess whether brain tumor incidence rates have changed in relation to the onset of mobile phone use. According to one study, statistics on

[3] The fierce attack of Leszczynski in *The Scientist* was heavily criticized by other researchers however, such as the epidemiologist Geoffrey Kebat in the financial magazine *Forbes*, which spurred a heated argument between the two (Kebat, 2012, March 3). In reply to this critique, Leszcynsky claimed Kebat had a dubious research background, having worked for the tobacco industry (Leszczynski, 2013, March 18). Kebat in response claimed these accusations were false ad hominem attacks, and stated Leszcynsky failed to deliver proper scientific arguments, falling back onto conspiracy theories about dubious involvements of industries in the RF-EMF health debate to support his claims (Kebat, 2013, March 20).

glioma incidence trends in the US do not show a significant increase as of yet. The incidence rates in this study were however compared somewhat favorably with the slightly increased risks associated with heavy mobile phone use found in the Interphone results, but unfavorably with the higher risks found in the results from the Hardell group (Little et al., 2012). Another study found an increase of brain tumor incidence in Australia, primarily of malignant tumors (Dobes et al., 2011).

The papers of Little et al. and Dobes et al. have also been discussed by Hardell et al. in a paper in 2013, with a detailed breakdown of a number of issues they found in the methods of data collection and analysis of Little et al. (Hardell et al., 2013b). Among some other notable points, they point out that Little et al. paid little attention to the fact that high grade gliomas *do* show an increase in incidence in the US in recent years. Interestingly, these types of tumors have shown the highest risk associated with mobile phone use in the studies of Hardell et al.

Concerning brain cancer incidence trends in Denmark, in 2012 Microwave News took note of a press release coming from the Danish Cancer Society which reported on a substantial increase of malignant brain tumors in recent years, especially among men ("Spike in "Aggressive" Brain Cancer in Denmark", 2012). In December 2013 Microwave News reported again on their attempt to get more clarity on these developments. However, any communication between the researchers connected to the Danish Cancer Society and Microwave News seems to have been silenced since. Microwave News was informed by others that the statistics from the Danish Statens Serum Institut released in 2013 confirmed an increase in overall brain tumor incidence rates. In a 2015 update, Microwave News added that observed incidence rates have seen a decline again in Denmark in the last few years ("Something is Rotten in Denmark", 2013).

The above observations can be easily confirmed with the NORDCAN database (www-dep.iarc.fr/NORDCAN). Here, Denmark shows a peak in the last decade, but with recent declines. Norway shows a similar pattern, and Finland shows a comparable temporary peak as well, but far less pronounced. There is no overall increase visible in brain tumor incidence rates of Sweden in recent decades, as can be expected from the discussion above. What is interesting to note in this regard though, is that Hardell et al. already mentioned the following in a 2013 paper, concerning different possible effects of tumor promotion and tumor initiation mechanisms on incidence trends: "Initiation and promotion have different effects on the incidence of brain tumors. An initiating effect would have the most direct effect on the incidence. Our results indicate that such an effect would be apparent after more than a 20-year use of mobile phones, and thus be too early to be found in cancer registries. On the other hand, if the exposure acts as a promoter, this would decrease latency time for already existing tumors, giving a temporary, but not a continuous, increase in incidence." (Hardell et al., 2013c) The Hardell group furthermore notes in this paper that their

current findings support the hypothesis that wireless phone radiation can act both as a promoter and as an initiator of brain tumors.

Nonetheless, Hardell et al. do warn for brain tumor statistics being used too eagerly to counter or compliment the findings of epidemiological studies such as their own, pointing to issues that can arise when attempting to correlate brain cancer statistics with trends in mobile phone use (Hardell et al., 2013b). Changes in diagnostic techniques and other factors that can possibly influence observations of incidence rates, are in fact often discussed in publications on incidence data. Such factors should always be considered when turning to these datasets for analysis of phenomena of interest. Especially when applied on statistical data covering longer time periods, attempts to correlate different data sets can turn out problematic due to confounding factors such as new developments in tumor diagnostic techniques over the years, and possible changes in exposure to other (unknown) carcinogenic substances.

3.6. Summary of Scientific Studies

On the surface, conflicting results from the Interphone study, the studies of the Hardell group and the Danish Cohort study appear difficult to explain, leading to the impression that these studies at best contradict each others findings. However, an in-depth analysis of the differences and similarities in study design and research outcomes gives room for a less troublesome interpretation hidden within the results, indicating that especially the Hardell studies and Interphone study results are actually quite compatible. It should be no surprise by now that therefore the Hardell and Interphone studies have contributed to the 2011 IARC classification of RF-EMF as possibly carcinogenic, while, based on the already available publications, the Danish cohort study was considered less informative due to it's "considerable misclassification in exposure assessment" (IARC, 2013).

Additionally, a careful assessment of recent trends in brain tumor incidence rates in the Nordic countries shows these trends do not contradict the hypothesis mobile phone radiation might increases risks of development of brain tumors, contrary to claims by some researchers.

Still, the increases in risks seen in the studies of Interphone and the Hardell group that indicate tumor promotion and initiation effects of RF-EMF, could be caused by selection and recall bias in these studies, and the observed bumps and peaks in brain tumor incidence trends of some of the Nordic countries might be no more than temporary flukes. Therefore, studies such the Danish cohort study showing no increase in brain tumor risks cannot be entirely discarded yet.

However, when carefully scrutinized, one has to come to the conclusion that the severe flaws of the Danish cohort study are difficult to ignore. They should not be dismissed too easily, irrespective of wether RF-EMF turn out to be carcinogenic. If the issues of the Danish cohort study are as severe as a number of researchers and critics have indicated, the dataset of this study would be entirely insufficient for any kind of analyses of possible health risks associated with mobile phone radiation. Attention should therefore be raised to the fact that this potentially fallacious dataset might even turn out to be malignant, and has already started to spread to some other epidemiological studies in Denmark. A number of studies based on the Danish cohort dataset have been published on different health risks and diseases, such as skin cancer, multiple sclerosis and other neurological diseases, in order to assess whether there is a possible association with mobile phone use. These studies seem to have found some indications for increased risks among mobile phone users for development of a few of the studied diseases, but have shown a number of 'statistically significant' decreased risks among mobile phone users for several other diseases as well, which remain difficult to explain (Schüz et al., 2009, Harbo Poulsen et al., 2012, Harbo Poulsen et al., 2013).

4. Corporate Agenda's

When looking at the above developments, one can imagine that suspicions about conflicts of interests among scientists might not always be entirely unsubstantiated. Historical studies seem to support the observation that such conflicts of interests play a substantial role in scientific research in the past century. The incredible efforts industries go through in manipulating public perception, government policies and scientific research itself in the past and present has been covered extensively in for example *Doubt Is Their Product* by David Michaels (2008), praised by science journals such as New Scientists (Goozner, 2008) and Science (Cranor, 2008). This book covers how the tobacco industry and other industries, such as those related to the production of asbestos and DDT, have managed to obstruct acknowledgement of scientific findings, delaying changes in public health policies for these substances often for decades. The book also shows manipulation of science has become increasingly professional and refined over the years, as corporate and industry interests have gotten more and more entangled with scientific research and its role in shaping public opinion and government policies. Similar practices within the pharmaceutical industry are also discussed by Goldacre in his book *Bad Pharma* (Goldacre, 2012).

In light of these practices, critics of studies on mobile phone radiation and health that often do not show any adverse health effects while exhibiting poor study design, understandably express concern these studies are compromised by parties associated with the telecom industry. However, suspicions of industry involvement are generally refuted by parties defending these studies. In the case of Interphone for example, supporters point out that, despite substantial funding coming from industry, the IARC has stated in its publications that great effort had been put into keeping active industry involvement at a distance (World Health Organization, International Agency for Research on Cancer, 2010, "Funding", n.d.). On the other hand, Hardell et al., for example, point out that, according to the IARC protocol, industry representatives *were* allowed to contribute to the Interphone studies, as the following passage makes clear: "Other parties may also be involved in the Study Group as observers or consultants. These may include representatives of industry, other concerned organisations..." (IARC, 2001, Hardell et al., 2013a).

Careful analysis shows dat indeed, possibilities of industry influences should not be dismissed too easily. In 2011 it was uncovered that Ahlbom, considered one of the most prominent researchers in the field and an outspoken critic of the studies of the Hardell group (discussed at length in previous sections), co-founded a consulting company specialized in telecommunications together with his brother in 2010. Due to these possible conflicts of interests, the IARC subsequently removed Ahlbom from the 2011 evaluation meeting on mobile phone radiation, which he was

originally set to chair ("IARC Drops Anders Ahlbom from RF–Cancer Panel", 2011). To spice things up even more: Ahbom himself has been a long time member and chairman of ICNIRP, and contributed to the original SAR guidelines of 1998. Meanwhile, his brother, with whom he later cofounded the consulting company, had been a lobbyist for the telecom industry in Brussels since the early nineties, according to the Swedish investigative journalist Mona Nilsson (Nilsson, 2011). Ahlbom has hardly left the scene since these questionably close ties with the telecom industry have been revealed, and still plays a prominent role in the field, for example as researcher involved with the forthcoming Swedish prospective cohort study COSMOS (Schüz, 2011, Cosmos, n.d.).

Hardell et al. have published articles about industry bias in different research fields on possible carcinogenic substances themselves as well, for example a scientific article from 2006 on a number of notable scientists in the telecom, tobacco and chemical industries that were paid by industry, including the well known epidemiologist Richard Doll (Hardell et al., 2007). The paper stirred up quite some debate about its content in the responsible journal, but the main findings about financial ties between researchers and industries do not seem to be contested. However, in the responses, suspicion of undisclosed conflicts of interest of Hardell was expressed as well, as he had been a paid expert witness in an early court case on mobile phone radiation and brain tumor risks. These claims were rebutted by Hardell, noting he did report his involvement in this case to the present journal (McLaughlin et al., 2007, Hardell et al., 2007b, Wakeford, 2007, Paustenbach, 2008, Hardell et al., 2008).

These well-documented back and forth accusations shed an interesting light on the dynamics between scientists in research fields that might be affected by industry. Finding out whether or not conflicts of interests play a role in these fields turns out to be a tedious task, and when researchers themselves express concerns about conflicts of interests of other researchers, exchanges between scientists predictably can turn sour, potentially hampering further discourse between scientists.

My eyes fell on something else as well though. To my own surprise, the full author list of this 2006 paper by Hardell et al. includes non other than the aforementioned Martin J. Walker, who, as described in section 2.4, has been fiercely disputed by a number of journalists and bloggers. Besides this paper, it appears Hardell and Walker have not published anything else together before or since, but, curious about this collaboration, I decided to ask Hardell in an email about the collaboration. In his reply, Hardell mentioned he did not know Walker's other work well, and that the collaboration consisted primarily of Walker providing some of the crucial sources on Richard Doll. Perhaps somewhat ironically (considering the journalists who fiercely criticized Walker), the findings of this paper were even covered at the time by The Guardian, in a news article which primarily focussed on the revealed industry ties of Richard Doll (Boseley, 2006).

More serious accusations of possible scientific misconduct by the Hardell group have been expressed by other researchers. In November 2011, the radiobiologist Eric van Rongen, a long term member of the ICNIRP and secretary of the Comity Electromagnetic Fields of the Health Council of the Netherlands, was interviewed by the Dutch magazine *Vrij Nederland* in an extensive article on possible health risks of mobile phone radiation. In this article, Van Rongen expressed his skepticism about the possibility of carcinogenic effects of mobile phone radiation, and specifically questioned the reliability of the studies of the Hardell group. To substantiate his claims, Van Rongen pointed out there have been suspicions that the Hardell group has been manipulating data, although he did add there is no hard evidence of this, and such manipulation remain difficult to prove (Vanheste, 2011) A few years later, Van Rongen's criticism towards Hardell's studies fully materialized in a report on mobile phone radiation and cancer research, released by the Health Council of the Netherlands in 2013 (Health Council of the Netherlands, 2013). Due to the potential impact on the conclusions of my own paper, I decided to cover this report and its extensive criticism on the Hardell studies in detail in section 6.

5. National and International Policy

In the shadow of the scientific disputes surrounding research on mobile phone radiation and brain tumor risks, national and international health regulation institutes have remained reluctant to change policies in relation to RF-EMF usage and exposure — despite the 2011 categorization of RF-EMF as a possible carcinogen by the IARC and WHO. In light of the recent scientific developments, some governments have begun to promote a more precautionary approach however, such as France (Gitlin, 2008, ANSES, 2013), Italy ("Italy's Health Council Recommends Precaution", 2011), and Belgium ("New regulation for the sale of mobile phones as of 2014", 2014). These countries give public advice promoting a more restrained use of mobile phones to reduce unnecessary risks to possible harmful exposure to RF-EMF. Belgium even introduced a ban on the sale of wireless devices aimed at children under the age of 7 in 2014, next to actively advising people on reducing exposure to mobile phone radiation.

The approaches of these countries are in stark contrast with health regulations of other countries however, such as the US ("Cell phones", n.d.), the UK ("Mobile phone safety", n.d.), and The Netherlands ("Mobiele telefoons en zendmasten", n.d.). These countries currently base their regulations on the notion that there is no scientific consensus yet on whether there are any health risks associated with current uses of RF-EMF, and that there is therefore no reason to adjust policies for the use of RF-EMF.

The main reason for the differences in approach across different countries, despite the precautionary warning on the possible carcinogenic effects of RF-EMF by the IARC and the WHO, appears to be that countries generally base their policies on their own additional assessments of the scientific literature. The Health Council of The Netherlands (the main scientific health advisory organ for the Dutch government), for example, released an extensive report in 2013 (the first part of a three part series of reports) on the state of scientific research on mobile phones and brain tumor risks. This report deviates somewhat from the IARC findings, and states the evidence for an increase in brain tumor risks is at best inconclusive, and concludes that therefore there is no need for any precautionary measures (Health Council of the Netherlands, 2013). The report bases this conclusion mainly on the Interphone study and the latest publication of the Danish cohort study, and downplays the findings of the studies of Hardell et al., which played a more important role in the IARC classification.

The report given out by the Health Council of the Netherlands has gotten criticism from RF-EMF critic organizations in the Netherlands such as StopUMTS, which expressed discontent with its main conclusion ("Gezondheidsraad: Geen duidelijk bewijs voor verband tussen

hersentumoren en langjarig mobiel bellen", 2013). The aforementioned radiation researcher Leszczynski also commented on the report on his personal blog. Leszczynski complimented the clear quality assessment of the studies discussed in the report. He did not agree with the final conclusions however, and criticized the weight the Health Council of the Netherlands gave to the Danish cohort study (Leszczynski, 2013, June 14).

6. Assessment of The Health Council of the Netherlands

There is definitely something to be said for the 2013 report of the Health Council of the Netherlands (from here on referred to as "The Council"). Despite its conclusions wavering away from the precautionary stance of the WHO and the IARC in their 2011 evaluation, the report of The Council appears to be a thorough and adequate assessment of the existing scientific literature on this topic. It covers all the relevant studies on brain tumor risks and mobile phone use up till 2012, and primarily focuses on the Interphone study, the Danish Cohort study and the studies of the Hardell group. In its assessment, the The Council acknowledges most of the shortcomings of these studies that have been discussed in the previous sections of this paper, but it puts more emphasis on different issues it has come across, leading to an overall conclusion that mainly focuses on findings that show inconclusive results. Most prominently in this regard, after seemingly careful analysis, The Council puts less weight on the findings of the Hardell group, pointing throughout its report to a number of inconsistencies and apparent errors it has found in these studies. The main issues The Council has come across are summarized on page 108 of the report:

> "In summary, there is doubt on the internal and external consistency of the Hardell data on account of (1) the increased risk observed already with very short usage times; (2) the unusually high response rates in the controls; and (3) the increased risks observed for cordless phone use, again in some cases for very short usage time. For these reasons, in combination with the lower numbers of subjects, the Committee has given the Hardell et al. studies less weight than the INTERPHONE studies in the overall analysis and conclusions." (Health Council of the Netherlands, 2013, p. 108).

The Council discusses these issues in great detail, putting the research of the Hardell group in an entirely new perspective. Therefore it is worthwhile to take a closer look at this assessment. I will therefore assess the three main issues raised by The Council, and will finish this section with some additional remarks and observations.

6.1 Latency Times in the Hardell Studies

The first point in the summary of issues by The Council of the Hardell studies was the short latency times for tumor development associated with mobile phone use. According to The Council this is an unlikely effect, as brain tumors generally have latency times of 10 years or more, and therefore The Council implies these short latency times must be the result of some kind of bias in the analysis of the Hardell studies. Additionally, there is a lack of increase in incidence rates of brain tumors in the general population, that should be visible if these short latency times were indeed true, according to The Council (Health Council of the Netherlands, 2013, p. 33–34).

If RF-EMF only act as a tumor *initiator*, the short latencies are indeed an unlikely finding, given the long latency times for brain tumor development associated with known carcinogenic agents such as ionizing radiation. However, there might be other possible mechanisms at play that can explain the findings of Hardell et al. Specifically, an observed increased risk for tumors with unusually short latency times in relation with first use of wireless phones, can be the result of tumor *promotion* effects of RF-EMF. Promotion effects accelerate the growth of existing tumors, and thus can lead to an earlier onset of symptoms and, subsequently, detection of existing tumors in exposed subjects. Promotion effects as a possible explanation for apparent short latency times of observed tumors in relation with wireless phone use have been discussed by the Hardell group as early as 2003 (Hardell et al., 2003), and have even been discussed in the ICNIRP guidelines of 1998 (ICNIRP, 1998), but there is no mention of this in the report of The Council at all. This is somewhat strange, especially since a mechanism for an association between mobile phone use and tumor risks has not yet been established. When one kind of *assumed* mechanisms is used as an argument against observational results, should this mechanism not be weighted against other possible mechanisms that could explain the observed results, especially when other possible mechanisms for findings under scrutiny have been discussed in relevant and related publications? The negligence of The Council in this regard is remarkable, to say the least. A proper discussion of different kinds of possible mechanisms should not be left out in a report that is meant to present an overview of the available scientific findings and insights in a research field.

The interpretation of brain tumor incidence rates by The Council will be discussed in detail in section 6.5 (also see section 3.5 for a general overview and discussion of brain tumor incidence rates).

6.2 Response Rates in the Hardell Studies

The second main point addressed by The Council are the "unusual high response rates" among controls in the Hardell studies.

Response rates in case-control studies give the percentage of participants of a study, calculated from the total amount of considered eligible cases and controls and the final amount of cases and controls that participate. Given that unknown factors contributing to non-participation can bias the outcome of a study, studies with high response rates are considered more reliable than studies with low response rates.

The main study of the Hardell group used by The Council had a response rate of 85% for cases and 84% for controls. Other studies of Hardell which The Council refers to in its report have response rates of up to 91% among cases, and 92% among controls. The total response rates for Interphone was 64% for cases with glioma brain tumors, 78% for cases with mengingioma brain tumors, and 53% for controls (Health Council of the Netherlands, 2013, p. 65). Noting the high response rates of the Hardell group, The Council refers to other investigators that have noted the response rates of the Hardell group studies are "virtually impossible" to attain.[4]

In order to support its observation, The Council picks out four other Swedish studies with comparable designs from the same time period as the Hardell studies, showing lower response rates between 59% and 83% among cases and 53% and 82% among controls. (Health Council of the Netherlands, 2013, p 65). However, these are four hand-picked studies, and might not give a clear overall picture of these trends in Swedish case-control studies. Therefore I did a short search myself, and quickly found one review paper discussing international trends in response rates. For its analysis this study focused on labor force surveys, and found overall response rates for Sweden from 94% in 1983 to 87% in 1996 (de Heer, 1999). These rates seem not too far off from the response rates Hardell et al. achieved in their studies among controls, although they might not be directly comparable due to possible differences in different types of studies. However, another study cites response rates of 95–97% for studies of the National Health Interview Survey (NHIS) in the US, from the 1960s till the 1980s. Since then the NHIS has seen response rates declining, with 91.8% reported in 1997 and 86.9% in in 2004 (Galea et al., 2007). These rates seem very well in accord with the rates the Hardell group has achieved, thus the concerns of The Council seem difficult to uphold.

However, The Council also mentions the Hardell group calculated the response rates differently compared to Interphone, by excluding for their calculations deceased cases and cases

[4] It would be worthwhile to look deeper into these criticisms from other investigators as well, together with the responses of the Hardell group, however for brevity I will leave that for another time.

denied by their physician for inclusion in the study. The Council recalculated the response rates, where possible, using data from the Hardell studies, for comparison with Interphone, and came to lower numbers ranging from 59–72% for cases and 79–90% for controls (Health Council of the Netherlands, 2013, p 66).

The Council seems to additionally imply the Hardell group calculated their response rates not according to general standards, by noting their method of excluding cases is "incorrect", however without further reference. I therefore started to look further into the concept of response rates myself, and found out it turns out to be a rather sensitive topic in epidemiology. For example, the aforementioned study of Galea et al. notes:

> "The term ''response rate'' has become freighted with conflicting meaning, much of which is frequently incomprehensible to any but the most careful reader of a particular epidemiologic paper. Unfortunately, there is no such thing as a simple ''response rate,'' with different modalities of data collection embedding particular, but important, elements, each of which may contribute to the calculation of several ''response rates'' that may give us an indication about participation in a particular study." (Galea et al., 2007).

If these observations are correct, then, instead of implying the Hardell group did something wrong or suspicious in this respect, The Council should have noted there is no common consensus among epidemiologists about the best way to calculate response rates. Furthermore, given the Hardell group is very clear about their method of calculating response rates, there is not that much room to doubt the feasibility of the response rates in the Hardell studies. Let alone that the calculated response rates of the Hardell studies could be a reason to lower the weight given to their studies when comparing these to other studies.

In light of different modalities, it does make sense to recalculate the response rates of different studies to better compare them. However, if one takes a closer look at the comparison The Council makes between the recalculated response rates of the Hardell studies and the Interphone studies, the considerations of The Council fail to follow through to the end. In its comparison of response rates, The Council does not take into account the differences between the assessment methods of The Interphone study and the Hardell studies. Hardell et al. used questionnaires sent through mail, and additional telephone interviews if necessary, while the Interphone study used face-to-face interviews for their assessment. It should be easy to imagine a study based on a questionnaire sent through the mail yields a different response rate. Given it is easier to complete a

questionnaire in one's spare time, than to make time for a face-to-face interview, it might easily yield a higher response rate. A possible negative impact of face-to-face interviews on the response rate of studies is mentioned by de Heer in order to explain observed downward trends in response rates, citing a number of publications in this regard:

> "Several researchers have attempted to explain these trends. Goyder (1987) used meta-analyses to evaluate American and Canadian surveys with respect to nonresponse. His conclusion was that nonresponse was increasing for face-to-face surveys, but that the nonresponse trend for mail survey nonresponse appeared stable. Similar patterns have been noted by De Leeuw (1992) in the Netherlands, Lyberg and Lyberg (1990) in Sweden, and Bretschneider and Schumacher (1996) in Germany." (de Heer, 1999).

Regarding the method of calculating response rates of cases by the Hardell group, there is an additional argument which I believe can be raised in support of their method. Hardell et al. might have achieved higher response rates compared to the Interphone studies in part by leaving out deceased cases and cases deemed too ill by their physician beforehand, but the manner in which these cases are excluded are clearly given in the relevant papers. Consequently, the careful selection criteria of the Hardell group lower the possibility of *unknown* factors leading to selection bias, and, as explained by Hardell et al. in numerous papers, reduces recall bias due to exclusion of too ill cases suffering from memory impairment and other confounding factors related to brain tumor diseases. Taking into account the possibility of unknown factors leading to less reliable results is the main reason for calculating response rates. Consider the alternative if too ill cases would have been included. This would at best deliver unreliable data reported from these cases, compromising the overall reliability of data collected in these studies. Taking these things into consideration, the difficult to interpret findings of the Interphone studies, which did not take such factors into account for their selection criteria, actually gives a solid additional argument supporting the methodology of the Hardell group.

It might however still be possible that excluded cases somehow have had different usage patterns, leading to lower overall calculated risks. Some critics of the Hardell studies, including the Swedish Radiation Protection Agency, implied this might be the case. Although the Hardell group found such speculative effects unlikely, they specifically took note of this kind of criticism, and in response expanded their research with diseased cases from the same time period in a later study to see if this would lead to a different outcome — which overall it did not (Hardell et al., 2010).

Again, The Council did not mention any of these discussions and nuances in its critique on the Hardell studies regarding response rates.

6.3 Inclusion of Cordless Phones in the Hardell Studies

The inclusion of cordless phones by the Hardell group is the third point summarized in the conclusion of the report of The Council, and is discussed in great detail. According to the council, the results the Hardell group found in relation to cordless phones should be a strong indication their overall findings are flawed. Specifically, The Council points out the Hardell group misinterpreted an assessment of the power output levels of cordless phones. According to The Council, contrary to the interpretation of the Hardell group, the power output levels of cordless phones are at least a magnitude lower than those of mobile phones. This would make the findings of the Hardell group inconsistent, as the amount of increased brain tumor risk they found with cordless phone use is almost similar to the increased risk they found with mobile phone use.

The Council bases its criticism primarily on a study by Vrijheid et al. (Vrijheid et al., 2009) that was apparently misunderstood in a paper by Redmayne et al. (Redmayne et al., 2010) which was cited by the Hardell group for their assessments (Hardell et al. 2011a, 2011b). It is worthwhile to quote the entire passage in which this issue is raised by The Council:

> "An issue that needs to be discussed before going into detail on the strengths and weaknesses of the different studies, is the exposure from cordless phones versus that of mobile phones. Hardell claims in his studies that the RF EMF exposures from both types of phones are of comparable magnitude, and that the observed increased risks associated with cordless phone use he observed in his studies are consistent with this. But is this claim valid? Vrijheid et al. (2009) used software modified mobile phones used by over 500 volunteers in 12 countries to measure the output power of mobile phones. The 900 MHz phones transmitted with an average power of 133.3 mW (maximum 250 mW, based on 46994 calls), and the 1800 MHz phones with an average of 64.2 mW (maximum 125 mW, based on 29505 calls). The maximum power of a cordless DECT phone is 10 mW and during a call transmission is always at this maximum. There is no transmission in standby mode. This means that exposure to radiofrequency electromagnetic fields from DECT phones is considerably lower than exposure from mobile phones. Some authors, however, have concluded otherwise. Redmayne et al. (2010) discussed the exposure by cordless phones and compared that with

> the data for mobile phones as assessed by Vrijheid et al. (2009). Vrijheid et al. state that "Analyses included data recorded during speech communication only." This means: not during texting, but for the entire duration of a call, both during speaking and listening. However, Redmayne et al. (2010) erroneously interpreted this statement that power was only registered during speaking and not during listening. They conclude from this that the average exposure from mobile phones is likely to be much lower than the levels given by Vrijheid et al. (2009) and that the exposure from cordless phones during a conversation might be considerably higher than that from a mobile phone. This incorrect conclusion is adopted by Hardell et al. (2011) to explain the increased risks observed with cordless phone use." (Health Council of the Netherlands, 2013, p. 102–103)[5]

The Council probably couldn't have expressed its rebuttal of the assessment of the power output of cordless phones by Hardell et al. and Redmayne et al. more thoroughly. But is this rebuttal valid? First, it should be noted The Council itself actually also notes a study in their report which found lower output levels of on average 25.76 mW for GSM phones operating on 1900 MHz (Health Council of the Netherlands, 2013, p. 103, Kelsh et al., 2011). Here, it should be mentioned Kelsh et al. used a different measurement technique. Where Vrijheid et al. used software modified phones to measure exposure levels, Kelsh et al. used an external measurement system, which can explain some of the differences between measured output levels. Still, other studies have shown even lower output levels (see below).

The Council however specifically points out Hardell et al. took over a wrong conclusion of Redmayne et al. concerning the power levels of mobile phones. The Council omits here though that Hardell et al. mention that, next to their reference to Redmayne et al., they discussed such findings themselves as well in their earlier publications: "Radiofrequency emissions from a cordless phone are in the same magnitude as from a digital mobile phone, as discussed in our publications and recently shown also by Redmayne et al." (Hardell et al., 2011a)

However, Hardell et al. do not specify in which publications they have discussed this, so perhaps The Council missed this discussion (although this shouldn't have happened if The Council would have fully read the papers they have included in their reference list). Hardell et al. go into a detailed assessment of the exposure values of different phones in a paper in 2006:

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[5] One might have noted The Council states cordless phones do not transmit during standby mode. This is a very curious statement however when taken into context. Although it is true according to the publication of Kramer et al. that The Council cites, in this publication Kramer et al. also state DECT base stations transmit a continuous signal throughout the day, making it a prime source of RF-EMF in homes: "It should be noted that the continuous transmission of the base station is a unique property of the DECT system (CT devices did not continuously radiate) which potentially makes DECT the dominant RF source in homes and offices." (Kramer et al., 2005)

"It has been argued that use of cordless phones should not be assessed since they have lower power output than GSM phones. However, as discussed elsewhere (Hansson Mild et al. 2003), the GSM phone regulates the output power depending on the quality of transmission. Measurements show that, for instance, in Stockholm city the GSM 900 phones only use 4% of the maximum output power as a median value (Persson et al. 2002). Furthermore, the DTX function which makes the phone transmit with 217 pulses per second when one is talking, but only with 2 pulses per second when listening, in principle causes a further reduction with a factor of up to two. Most GSM phones have less than 1 watt peak output power instead of the allowed 2 watt in the standard. Thus, the GSM phones have a median power of 10–20 mwatt, i.e., the same order of magnitude as the cordless phones." (Hardell et al., 2006).

Note that the DTX function which Hardell et al. here refer to is the function which Redmayne et al. (erroneously, according to The Council) noted was not taken into account by Vrijheid et al. in their assessment of the power output of mobile phones. It only constitutes a part of the argument of Hardell et al. however, and when reading the paper of Redmayne et al. one will find it was only a part of their assessment of the power output of different wireless phones as well:

"During calls, DECT handsets have a time averaged 10 mW output power delivered in bursts at the maximum transmit power of 250 mW. DSS phones in the US are permitted 100 mW output power, operating at a transmit power of up to 1 W. This is the same transmit power as for 900 MHz mobile phones. For most portable telephone models, output power does not vary with distance from the base. On the other hand, mobile phones adjust their power output according to the clarity of signal by using adaptive power control (APC). This means the output power varies considerably according to phone type, the network provider, and a variety of conditions including network user-load, obstacles, handover between cells, and proximity to a base station. While the phone is establishing a connection and sending text messages (SMS) it functions on or near full power. At other times, APC may scale the time-averaged maximum output power from 250 mW at 1800 MHz or 125 mW at 900 MHz down to as low as 1–2 mW according to conditions.

Other sources have measured time-averaged output power of mobile calls variously at below 1 mW for 3 min in suburban areas, and, most recently, at 128 mW (900 MHz) or 63 mW (1800 MHz) for calls longer than 1 min averaged across all locations. This

multicentre study found that output power decreased with increasing call duration. However, it only accounted for exposure during speech; as APC reduces power output when the caller is listening, this study almost certainly overestimated actual mean exposure." (Redmayne et al., 2010).

Only the final sentence notes the overestimation of power output levels by Vrijheid et al. Redmayne et al. also refer to other sources which found lower power output levels of mobile phones, while The Council only took issue with the interpretation by Redmayne et al. of the power measurements of Vrijheid et al. during speech time. But did Redmayne et al. really *misinterpret* the measurements of Vrijheid et al.? There seems to be hardly any reason to doubt this finding of The Council, given it clearly illustrated this point with a quote they gave from the paper of Vrijheid et al. However, it appears that, if The Council would have taken into account the sentence which follows *directly* after the sentence they quoted from Vrijheid et al., it should have come to different conclusions concerning the assessment of Redmayne et al. Given in full, Vrijheid et al. have stated: "Analyses included data recorded during speech communication only. *The SMPs did not record information about DTX (discontinuous transmission mode)*." (Vrijheid et al. 2009, emphasis added). According to Vrijheid et al. the SMPs (software modified phones) did not record information about DTX (discontinued transmission mode). This is precisely the mode Hardell et al. explicitly (and Redmayne et al. implicitly) referred to for addressing that the measurements of Vrijheid et al. are up to twice as high as as they would have been if this mode was taken into account. When the effect of DTX would have been taken into consideration, the results of Vrijheid et al. would have been closer to half the reported power output levels. The claim by The Council is simply incorrect, and is supported by a quote that is severely taken out of context.

It should also be noted maximum recommended power output levels are inversely proportional to frequency range used by cordless phones (ICNIRP, 1998), and cordless phones most commonly operate at 1800 MHz (Kramer et al., 2005), thus comparison with mobile phones operating at these higher frequencies is most appropriate. When the average power output of these types of mobile phones in the study of Vrijheid et al. is divided by half to reflect the effect of DTX, it is still somewhat higher than the estimations the Hardell group uses, but not as far off from the levels produced by cordless phones as The Council implies.

Additionally, it should be noted the effect of DTX also applies to the measured average of 25.76 mW for GSM phones found by Kelsh et al. cited by The Council. Kelsh et al. addressed the role of DTX in their assessment as well, and in more detail than Vrijheid et al.:

> "To address the effect of voice activated discontinuous transmission (DTX), which reduces output power when there is no speech, all phones were subjected to continuous rock music to generate a high proportion of sound to the phones. This could have caused a relatively higher average output power than what would be observed with normal conversation on a mobile phone." (Kelsh et al., 2011).

Furthermore, as mentioned above, the studies of Vrijheid et. al. and Kelsh et al. are not the only studies on which Hardel et. al. base their comparison of cordless and mobile phone radiation levels, with the other studies showing even lower output levels for mobile phones in certain areas — possibly due to differences in environmental factors and the period of time in which measurements took place (mobile phones of 10 years ago are different from those of 20 years ago). Factors like these should, ideally, also be considered in a proper assessment.

Thus, in conclusion, the argument from The Council to discredit the interpretations of Hardell et al. and Redmayne et al. in this matter, appears to be based on omission and misrepresentation of data of the paper of Vrijheid et al. other relevant publications. In fact, it looks like the assessment of Hardell et al., is — contrary to the statements of The Council itself — quite well in accord with the findings of the studies cited by The Council itself on this matter. It thus looks like it can be concluded the findings of Hardell et al. showing carcinogenic risks for both cordless phones and mobile phones are consistent with the published data on output power of the different types of wireless phones studied by Hardell et al.

6.4. Other Discussed Issues

Those who have fully read the report of the Dutch Health Council, might at this point like to point out that, even if all the main issues summed up by The Council have been sufficiently rebutted, throughout the report The Council notes several other purported issues in the Hardell studies. It will be too exhaustive for this paper to discuss all the noted issues here, but I will take out one more illuminating example to illustrate the apparent thoroughness of these claims.

The Council took into account the 2011 reanalysis of the Hardell group (Hardell et al., 2011a) including comparisons with the Interphone results, but noted inconsistencies in the numbers given by Hardell et al. in this reanalysis when compared to their other publications. This would cast doubt on the reliability of the results of the Hardell studies. To quote:

> "The recalculation for the limited age range was done only for the highest category (≥10 years) and resulted in a relative risk that was lower than for the full age range, but still increased. It is puzzling that the OR of 2.26 for the full age range given in the reanalysis paper (95% Confidence Interval 1.60–3.39) differs from that in the pooled analysis paper: 2.6 (CI 1.7–4.1), while also the numbers of cases and controls differ: 88 / 99 in the reanalysis paper and 50 / 42 in the pooled analysis paper. Hardell et al. noted in the reanalysis paper that in their original analysis they used >10 years instead of the ≥10 years in the reanalysis, but then it would expected that the numbers of cases and controls would be lower in the reanalysis, while they are in fact higher. This is one of the inconsistencies of the Hardell papers" (Health Council of the Netherlands, 2013, p. 78).

However, there are a number of issues with this argument. First, The Council compares the numbers from the reanalysis with results presented in another 2011 paper from Hardell et al. (Hardell et al., 2011b). It is important to note however this paper does not consist solely of the dataset on which the Hardell group did the re-analysis. It contains an analysis of both living and deceased cases, while the reanalysis was done on the dataset of only living cases in their earlier studies (which makes sense, given the Interphone also only used data of living cases in their study), which have been presented in a 2006 paper of the Hardell group (Hardell et al., 2006) referred to in their reanalysis. Unfortunately it is difficult to compare the numbers with the 2006 paper, as it does not contain results of gliomas overall separately. Perhaps this was the reason for The Council to use the 2011 pooled analysis including deceased cases, as this paper does contain data on gliomas separately. If so, The Council should have noted their choice in this to make clear that different numbers can be expected.

Things appear to become more curious now though, as one will note that now, the numbers would turn out even more skewed according to the reasoning of The Council, given the dataset used in this earlier paper should be even smaller. This has puzzled me quite a bit I must admit, but giving it some thought and a few careful observations, it appears The Council has, first, not properly interpreted some basic mathematical symbols from this reanalysis, and, second, it neither appears to have sufficiently comprehended the tables given by Hardell et al. in its assessment.

I should warn mathematics is not my strongest point, but I will break this down as far as I can. Hardell set the highest latency group for tumor diagnosis at >10 years (more than ten years) since first use of a mobile phone. Interphone used latencies of ≥10 years (ten years or more) since first use of a mobile phone for the highest latency group. This means that, if Hardell et al. would readjust their data to the selection criteria of Interphone, this would result in the inclusion of more

subjects in their reanalysis for the highest latencies, given a latency of ≥10 years gives a year extra from the pool of data from which cases can be picked, not less.

The Council additionally erroneously assumed the Hardell group used users of exclusively mobile phones in their reanalysis:

> "[...] Hardell et al. make a distinction between the use of mobile phones (such as GSMs) and cordless phones (the wireless phones for indoor use, such as DECT). The reanalysis they performed for the limited age range was done only for mobile phone users, and not separately for cordless phone users." (Health Council of the Netherlands, 2013, p. 77).

It is however quite clear in the table from Hardell et al. (2011a) that they used data of all wireless phones users (mobile and cordless) combined, and only reorganized use of cordless phones in a sub-analysis (to show the diminishing effect of putting cordless phones users among non-users on calculated tumor risks), as clearly indicated in both tables in the reanalysis paper.

Now, the table from the 2011 pooled analysis paper The Council cites for the "unexplainable" differences, shows cases and controls for *all* phone types combined as well, giving 91 cases and 92 controls (Hardell et al., 2011b, Table IV). This seems quite well in accord with the numbers of the reanalysis when taking into account this pooled analysis includes deceased cases (thus a larger dataset when compared with the dataset used for the reanalysis), but with latencies of >10 years (reducing the dataset when compared with the reanalysis). It should be clear these numbers will lead to slightly different ratios in the risk assessment. With these considerations in mind, the associated odds ratio (2.2) and confidence intervals (95%, 1.6–3.1) found in the original paper of Hardell et al. are as close to the numbers given in their reanalysis as could be reasonably expected, and again, the claims of The Council turn out to be unsubstantiated.

6.5. Analysis of The Council's Analysis of Brain Tumor Incidence Trends

Like other publications trying to show there is no solid evidence for increased brain tumor risks associated with mobile phone use, the report of The Council also notes the lack of increase in brain tumor incidence rates, in this case in The Netherlands in 'relevant' age groups, arguing this is a clear indication that, given the rapid increase in mobile phone use over the past few decades, there is currently no reason to suspect any real increase in risk is associated with mobile phone use.

To support this argument, a graph (see figure 1), showing glioma incidence trends in The Netherlands, is given in the report of The Council. The Council's interpretation of this graph is as follows:

> "It is clear from this data that there is no increase in gliomas in the Netherlands during the period of rapid increase in mobile phone use in the age groups that use them most: 20–29 and 30–59 years. There is an [sic] continuous increase in the highest age group of 60–79 years, but this started already before mobile phones started to be used." (Health Council of the Netherlands, 2013, p. 85)

Perhaps this analysis seems to make sense at first, but it already starts to makes less sense when taking into account that (heavy) mobile phone use of 10 years or more shows the strongest associations with increased brain tumor risks in both the Hardell and Interphone studies (Hardell et al., 2011a). Wouldn't this mean that, if the heaviest users can be found among 20–29 and 30–59 age groups, early increases in incidence should be expected to become visible among 30–59 and 60–79 age groups due to latency effects?

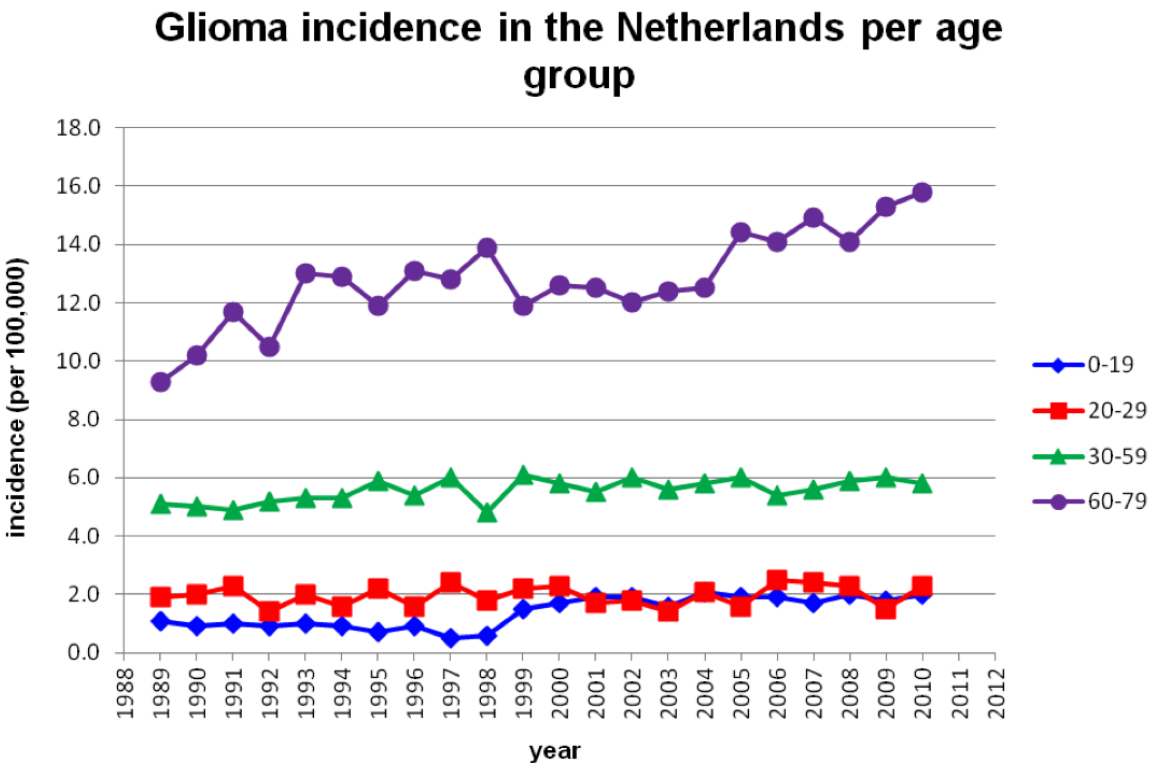

Figure 1: "Glioma incidence in the Netherlands for different age groups. Source: Netherlands Cancer Registry managed by CCCNL." (Health Council of the Netherlands, 2013, p. 85).

When cordless phone use is taken into account, things get more complicated. Unfortunately I have not been able to find data on cordless phone use in the Netherlands, but Hardell et al. note that cordless phones have been introduced on the market in Sweden in 1988, with DECT cordless phones being introduced in 1991 (Hardell et al., 2006). It is noted the increase in incidence in the oldest age group started before mobile phones came in use. It is not clear exactly when this is according to The Council, but the first substantial increases in figure 1 can be seen between 1993 and 1998. Contrary to mobile phones, early adoption and use of cordless phones is likely far less restricted to specific age groups as soon as sales of this technology took off, as no separate subscription is required, prices of cordless phones have quickly become relatively low compared to many types of mobile phones, and the many additional conveniences of cordless phones compared to wired phones make them in principle highly desirable for both younger and older age groups.

Taking note of these factors, observe that all age groups in the above graph, except one, show slight to steady increases in glioma incidence rates over the time period covered, with a few temporary drops here and there. The only exception is the age group of 20–29, which does not show a visible increase, while ages below 20 seem to show a latency in the trend of increasing incidence when compared to older age groups. This is somewhat awkward, but might still be explainable if a number of other potential factors are considered.

First, the increase in higher age groups can be the result of early adoption of mobile and cordless phones among relevant age groups (most notably successful middle-aged business men being early adopters of mobile phones). Second, the delay in the onset of incidence increase in the youngest age groups compared to the older age groups can be explained by earlier adoption of mobile and cordless phones in offices, shops and other businesses compared to family homes. Third, the age group between 20–29 consists for a significant part of students and other people that will leave their family household to live on their own or with their spouse. Combined with the advent of mobile phones, this can be expected to lead to less *cordless* phone use among this age group, while many young people have a lower than average income, thus, despite most of them being users of mobile phones, only *some* might actually be heavy callers. In other words, users between the ages of 20–29 might turn out to be low to moderate callers overall when compared to users in higher and lower age groups.

One might however remark that, if the increase in brain tumors among children and adolescents visible in the graph in Figure 1 is associated with the use of cordless phones, as noted above, this increase should spread out into the subsequent age group of 20–29 as well, even if the latter age group makes less use of wireless phones overall. This should become even more the case in more recent years, given that this age group will have used cordless phones more extensively at a

younger age, especially in more recent years. Because no similar increase in brain tumor incidence in the age group between 20–29 appears to show up, one might reasonably assume there are other causes behind the visible increase in the lowest age group, and other age groups as well, such as changes in diagnostic techniques. This might very well be the case. However, it might just as well speculate that these patterns show subtle signs of the possible role of *promotion effects* on tumor growth associated with wireless phone use. If promotion effects play a significant role in tumor development, this might mean that when wireless phone usage drops, tumor growth slows down quickly as well again. This might in turn lead to a postponement of (severe) tumor symptoms among users that have reduced their use, leading to an *apparent* stagnation of tumor incidence trends among young adults that use wireless phones less than their younger counterparts which are still hooked to cordless house phones — until wireless phone use increases again at a later age or cumulative time of exposure slowly reaches more critical amounts.

In this respect, there is at least one more possible major caveat in The Council's analysis of observed brain tumor trends in the Netherlands that might still need to be addressed. The careful reader will have noticed The Council does not back up its claim about phone use among different age groups with any data. I decided to look around for this, as I figured it wouldn't be too difficult to get some relevant data, but trying to find studies breaking down mobile phone use among different age groups turned out more difficult than I anticipated. I found three recent studies examining mobile phone use among different age groups using either operator data or data collected through software (of both incoming and outgoing calls), though using small sample sizes and done in more recent years, thus not directly applicable to the brain cancer statistics shown here. However, due to apparent lack of other relevant studies, these studies will have to do for the current analysis. One study by Vanden Abeele et al. studied mobile phone use among Flemish subjects, showing weekly call time of users up till ages of 25 years being nearly half (mean 30 minutes per week) compared to users of over 25 (mean 55 minutes per week) (Vanden Abeele et al., 2013). Two studies by Goedhart on Dutch mobile phone users show roughly comparable results, with highest use between ages of 45–54 years (mean 64 minutes per week) (Goedhart et al., 2015a, Goedhart et al., 2015b). Taking into account again most significant increases in risks have been found for latency times of 10 years or more (or highest cumulative amount of call time), this means that, if similar user patters can be considered for the past few decades, and an increase could be expected to show up in brain tumor statistics, the results of Goedhart and Vanden Abeele support the observation discussed above that the earliest increase would be visible at the lowest in the category of ages of 30 and up, with the greatest increase visible in ages of 60 years and up — even when disregarding cordless phone use.

When I was examining this graph with glioma incidence rates, I also became curious about incidence rates for different types of glioma tumors. It is easy to dismiss any further breakdown on the basis that it becomes easier to find seemingly significant but irrelevant results. However, the Hardell group found highest increases in risks associated with high grade gliomas (primarily glioblastoma), the most common and aggressive types of brain tumors with the lowest life expectancy rates (Hardell et al. 2006, Hardell et al. 2013b). As noted in the first part of this paper and by Hardell et al. as well (Hardell et al. 2013b), a number of population studies on brain tumor incidence have found recent increases in incidence rates of high grade gliomas, even when overall observed glioma rates are stable.

I came across a study by Ho et al. published a year after the publication of the discussed report of The Council. This study covers glioma incidence trends for The Netherlands for the same time period used by The Council. They found an overall slight, but statistically significant, increase in age standardized glioma incidence rates, from 4.9 to 5.9 per 100.000 between 1989 and 2010 (EAPC 0.7%, $p < 0.001$). Ho et al. noted the biggest increase was observed for glioblastomas. They suggest this increase might be caused by a shift in WHO classifications between grade III and IV gliomas, most notably in 2000 between glioblastoma and anaplastic astrocytoma (Ho e.a, 2014). Indeed, observed incidence rates have decreased for anaplastic astrocytoma during the same time that glioblastoma incidence rates rose, however, the anaplastic astrocytoma and glioblastoma combined still show a visible increase in incidence rates between 1989 and 2010, as can be seen in the graph in Figure 2 taken from the paper of Ho et al.

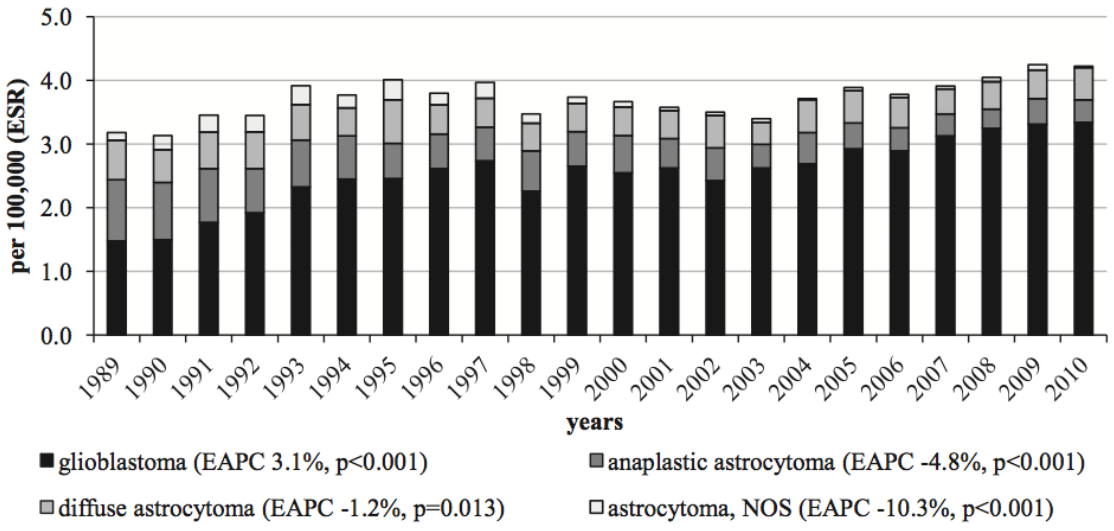

Figure 2: "Age-standardised incidence rates for astrocytic tumours in the Netherlands from 1989 to 2010." (Ho et al., 2014)

It should be stressed any interpretation of observed incidence trends have to be considered with extreme care in light of developments in diagnostic techniques, shifts in classification, and other unknown factors. However, to show how easy it is to come to different conclusions from observed brain tumor incidence data than The Council, I believe it still is worthwhile to address some more other considerations in the analysis of brain tumor trends as well — keeping in mind the speculative nature of any analysis of this kind. For this it will be useful to take into account a few more publications that have been published after the 2013 report of The Council.

As discussed before in this paper, the Hardell group found indications for both *tumor initiation* and *tumor promotion* effects playing a role in their results. In addition, a recent replication study has confirmed earlier findings of tumor promotion effects of mobile phone radiation in mice, further supporting the observation of Hardell et al. and other researchers that tumor promotion effects might play a substantial role in the possible carcinogenic effects of RF-EMF (Lerchl et al., 2015).

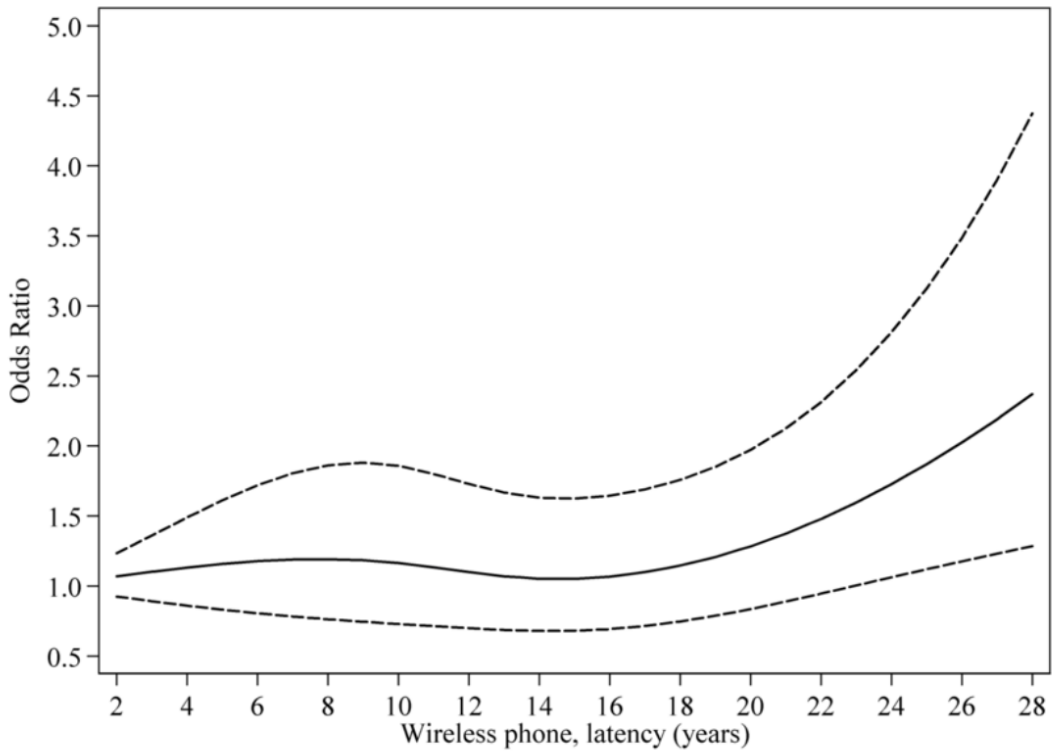

Figure 3: "Restricted cubic spline plot of the relationship between latency of wireless phones and malignant brain tumours. The solid line indicates the OR estimate and the broken lines represent the 95% CI. Adjustment was made for age at diagnosis, gender, SEI-code and year of diagnosis. Population based controls were used." (Hardell et al., 2013b)

Any early increase seen in brain tumor incidence rates that would be associated with mobile phone use, would most likely be related to tumor promotion effects. If visible, an increase in brain

tumor incidence rates caused by tumor promotion effects can be expected to be of temporary nature. It would be followed by a decline again in incidence rates, until tumor initiation effects (which are expected to have a longer latency period) start to show up and take over in observed incidence rates. These kind of trends seem to be visible in the graph given by The Council, especially in the oldest age group, and are perhaps even visible in the graph of Ho et al. Compare these trends with the graph in Figure 3 on malignant brain tumor risks in relation to time since first use of a wireless phone, taken from one of the most recent case-control studies of the Hardell group, published in 2013 (Hardell et al., 2013c).

6.6. Summary of Errors

Concerning the overall assessment of the Hardell studies by The Council, if The Council would apply the criteria it uses to question the results of the Hardell studies on its own report, The Council would have to dismiss its *own* conclusions regarding the reliability of the Hardell studies first. This means furthermore that, if The Council would have properly assessed the studies of Hardell et al., it should have come to the conclusion that the assessed scientific literature suggest there is at least some plausible evidence of an association between mobile and cordless phone use and increased brain tumor risks.

Summarizing the assessment of brain tumor incidence trends, the errors that have contributed to the questionable analysis by The Council are: misclassification of exposure from cordless phones, misclassification of different user groups, and grossly insufficient assessment of possible mechanisms at play. These errors primarily contribute to ignoring relevant sub analyses (different phone types and a more careful breakdown of tumor types) in favor of questionable sub analyses (different age groups with *assumed* overall mobile phone use) for analyzing these trends.

In addition to this, I have discussed more recent scientific publications on brain tumor incidence trends and health research that strengthen the available evidence pointing towards the existence of carcinogenic effects of RF-EMF.

I would still like to emphasize the speculative nature of my analysis of brain tumor incidence trends in the Netherlands in the latter part of this section. A more careful breakdown is necessary before drawing any definitive conclusions. But when speculating there are no effects, it seems little heed is given by The Council, and other research groups, to approach these kinds of data with proper care and cautiousness, let alone willingness to consider different interpretations before drawing conclusions.

Given how eager industry-sponsored critics of unwanted scientific findings appear to be to use the smallest possible missteps to dismiss well developed hypotheses and observations worthy of consideration — no matter how well researched more relevant findings are in a study — it might even be best to omit some parts of my analysis. However, my analysis mainly should make clear that, even at the time The Council released its discussed report in 2013, a careful assessment of incidence trends could not have given much room, if any, for reassuring that current trends cannot support any evidence for an association between brain tumor risks and wireless phone use.

In other words, if The Council would have properly assessed the available scientific literature and data, it should have acknowledged the more critical findings of the IARC and the WHO on possible tumor risks associated with low level RF-EMF. Most importantly however, even if there is only a very weak indication that RF-EMF tumor promotion effects are becoming visible in brain tumor incidence trends in the general population, this observation is strengthened by findings of scientific studies on tumor promotion effects discussed in this paper. If tumor promotion effects play a substantial role in the possible carcinogenic effects of RF-EMF, decreases in exposure will lower tumor growth, no matter in what stage tumor development is, meaning the sooner adequate precautionary measures are taken, the more effective these measures will be to reduce the manifestation of adverse health effects associated with RF-EMF in society, if such effects turn out to be real.

All data considered, instead of reinforcing outdated scientific views on this matter and, perhaps most worrying of all, misrepresenting scientific publications on RF-EMF and cancer research, already in 2013 The Council should have concluded there could be increased health risks associated with exposure to mobile phone radiation, and should have advised implementing precautionary measures until more scientific results become available.

7. Error Theory

Errors in any field of research are inevitable. However, it should be quite clear by now that research into the health effects of RF-EMF is in a dreadful state. The research field appears to be suffering from multiple sorts of errors on different levels of research and scientific discourse. Meanwhile, differences in opinion on scientific findings between different research groups seems to grow. These struggles in turn affect the way in which society as a whole is capable of dealing with the scientific uncertainties that arise from the current rapid adoption of RF-EMF technologies. Some groups become more and more suspicious of RF-EMF, while the rest of society keeps on using these technologies without wanting to give the possibility of adverse health effects any considerate thought. Due to the limited amount of quality research, all sides have a tendency to cherry pick whatever scientific opinion on these matters is closest to what they believe themselves, without consulting the underlying arguments.

Of the analyzed studies in this paper, the Interphone study and Danish cohort study seem to have failed quite badly in forming clear and consistent study designs to offer reliable results, and lack a sufficient discussion of other studies. The studies of the Hardell group seem to have been more consistent — contrary to the impression that opposing groups try to paint of the Hardell studies. Attacks on their results have often been limited to general criticism on the case-control study design, sometimes even on a fundamental level, discrediting the case-control study design as a whole and casting doubt on the reliability of any study of this kind. However, it becomes clear from careful analysis that thorough measures have been applied by the Hardell group to minimize any possible known flaws in the study design that could substantially affect results of their studies. Whether these measures are sufficient could remain a topic of debate, but, as discussed in this paper at length, somehow the most detailed claims by other prominent researchers and experts about inconsistencies found in results of the Hardell group, fall apart when properly scrutinized. As shown in section 6, claims against the Hardell studies in some cases even turn out to be based on questionable misrepresentations of analyses and data published by the Hardell group. Ironically, the studies of the Hardell group in return turn out to be even more reliable than one might initially expect after being thoroughly reassessed in order to deconstruct these claims. Additionally, whenever the results of the Hardell group have been questioned in comparison with other data, for example incidence trends, it seems to have been based on a limited assessment, in contrast with their own extensive analyses of relevant publications in response.

However, besides more commonly known errors such as those on the level of methodology and interpretation of results, struggles between scientists can also be the result of errors on the levels

of scientific discourse. Douglas Allchin categorizes different types of errors within science very thoroughly in this regard (Allchin, 2001). Of special note for the current discussion, is his observations that errors on the level of discourse can lead to misunderstanding between scientists and between scientists and society. In the worst case, they stem from fraudulent intentions, but it might as well be the case scientists erroneously accuse one another of fraud while actually pursuing an honest research agenda. The Hardell group might have been making errors on this level, for example by repeatedly expressing suspicions of conflict of interest of researchers that scrutinize their results or seem to omit critical publications and analyses in RF-EMF health research (Hardell, 2013, December 29, Hardell, 2014, February 13).

This is a complicated issue though. Although it is understandable researchers would want to point out possible conflicts of interests of other researchers when becoming aware of such issues, attacking fellow researchers on their possible lack of scientific integrity, inevitably won't be very constructive for the development of a dispute, no matter what the real intentions are.

To properly deal with possible conflicts of interests, and to prevent direct confrontations between research groups that doubt each others integrity, perhaps on a trans-institutional level of organization of research fields, concerns about conflicts of interests should be handled by a third, independent body where researchers and other interested parties can express their concerns anonymously. Issues involving possible conflicts of interest could then be addressed without further affecting scientific discourse among research groups on other levels.

Conflicts of interest are a legitimate topic of concern. The prevalence of dubious parties who appear to act out of corporate or financial interests and get involved with scientific research cannot be ignored in modern times. Falling victim to such individuals and parties could be considered the result of errors on the level of the organization of research groups, and on the level of interactions of research groups with society at large. The collaboration of the Hardell group with the, according to some, questionable journalist Walker (see section 2.4 and 4) might be categorized as such to some extend, but manifestations of these tendencies in RF-EMF health research can perhaps most prominently be found within the research groups of the Danish Cohort study and Interphone, allowing industry collaborations through people such as ICNIRP member Ahlbom — whose close ties with the telecom industry have been discussed in section 4.

A more thorough discussion of these discussed errors and how different kinds of errors can be best dealt with, should be an important topic for future research within and outside of RF-EMF research.

8. Sociology of Scientific Knowledge

Most of the debates in the field of error definition still build their scrutiny of scientific error on some kind of implicit notion of a perfect functioning scientific ethos among scientists, only hampered in their scientific practice by unforeseen and unintended mishaps in their research, development and refinement of scientific theory. Questions about the definition of error, how errors arise and how they can be utilized for gaining scientific knowledge are however an upcoming topic of interest in the current philosophical discourse on the scientific notion of error.

More critical views on the withering influences which the scientific ethos can be subject to only came to prominence in historical and sociological studies of science midway in the twentieth century. One of the pioneers of the sociology of science was Michael Mulkay, who approached the process of scientific discovery as primarily a form of social interactions. He and fellow sociologists analyzed the ways scientists compete with each other in fields of research, as they were questioning each others expertise off the record while tending to present their own views as the most appropriate and scientifically sound approach in attempt to convince their peers (and themselves) of the significance of their own contributions and findings (Mulkay et al., 1982).

These social interactions are part of processes which historians and sociologists of science in recent decades have began to see as vital elements that lead to the acceptance of new scientific discoveries. From these insights, the sociology of science has began to question the pursuit of scientific knowledge in society as an objective faculty of truth, and, in the most extreme cases, science as a whole is treated as fully subject to states and desires of society and social processes (with no such thing as 'pure' science). This relativistic approach became mostly represented by the 'strong programme' of the Sociology of Scientific Knowledge (SSK) introduced by David Bloor, and has become a widely adopted methodology in the sociology of science. Among introducing other principles, Bloor established the 'symmetry principle' for studying scientific developments, where both successful and unsuccessful scientific discoveries are treated with the same regard to their relation to 'truth', resulting in a relativistic approach to science and it's view on reality (Bloor, 1976).

Although it appears to lead to absolute relativism, this approach is primarily used for gaining a better understanding in how scientific knowledge claims manifest and become accepted in society. In his definition of the goals of the strong program, Bloor states that more traditional oriented perceptions of science argue, often implicitly, from the notion that deviation into error is the only applicable domain of study for the sociologist of science, effectively rendering the scientific search of truth dubiously to a teleological process. In this 'erroneous' view, the practice of science encompasses everything that is true, and what is rejected by science are solely delusions that are

destined to be rejected through scientific enquiry. From this reasoning, it appears that there is no possibility of going amiss for practitioners of science, whenever the logical foundations of science are followed properly. Such a teleological perception of the pursuit of scientific knowledge could be problematic for the understanding of how science manifests itself in society. According to Bloor, the acceptance of scientific knowledge in society is to such an extend subject to social influences, that it is more appropriate to study science and the generation of knowledge as purely social constructions. This is not to argue for the lack of an objective reality which science can reveal, but simply for the impossibly of objective reality to be fully comprehended by any single human being, including the sociologist of science. Treating scientific knowledge as objective knowledge would come down to severe self-deception.

In my own research for this paper I have attempted to stay close to this principle, but have found it difficult to maintain this stance at all times. Especially when logical arguments seem to go in favor of one outcome, it becomes tempting to see this as an argument for a somewhat 'weaker' approach of sociological analysis of scientific developments. Bloor's argument of the impossibility of achieving objective knowledge for a human being seems quite fair, especially in light of the Kantian paradigm of the split between subject and object, but the more I am getting familiar with scientific argumentation in any specific field, the more this argument of Bloor seems to be a major error in his reasoning. Although from a purely philosophical point of view it seems to hold true, as soon as one engages with reality, it becomes difficult to uphold.

Especially when discussing publications that contain severe errors according to scientific standards, such as the discussed report of the Health Council of the Netherlands, I think it becomes clear that errors, at least to an important extend, turn out to be more than the result of conflicts in social norms and standards within scientific communities. By discussing the scientific literature in detail that The Council itself has used, I have tried to show that errors resulting from attempts to maintain a specific paradigm, for whatever aims, might in turn result in catastrophic failure that can negatively affect society at large when actual underlying physical and biological mechanisms are undermined. The consequences of such failures become more severe, the more a dominant scientific paradigm fails to sufficiently take into account adverse mechanisms of scientific artifacts. At the same time, these harmful artifacts can become more engrained in the fabric of society, the more an insufficient scientific paradigm responsible for such artifacts is embraced by society. Furthermore, the society in which a scientific community is embedded, can act as a magnifier of ongoing disputes residing within this scientific community whenever artifacts of these disputes, for example in the form of insufficiently developed technologies, leak out into society at large before

these disputes have been resolved. This in turn can have great influence on the development of scientific paradigms that feed back again on the manifestation of harmful artifacts in society.

From this, one can argue that the forces that contribute to the development of paradigms around the notion of possible health effects of RF-EMF, are not solely dependent on social factors, but will in fact arise from an interaction between social and physical elements and mechanisms. If it turns out that exposure to low level RF-EMF can result in adverse health effects, but only a tiny number of the population is affected and the effects remain minor or vague enough, chances that such effects will be acknowledged by society as a whole will remain low. However, the greater the number of people that are affected or the severity of effects, the greater the chance this will influence the way society will accept new scientific insights on these matters. Without any physical effects, there won't be any factor that will steer public opinion on these matters except for social influences, but as soon as physical factors start to play a role, a different dynamic arises where social factors are influenced by physical factors.

Within the strong programme, Bloor wants to play down the role of physical factors in the development of scientific knowledge, in order to come to a full sociological understanding of scientific knowledge that does not depend on external factors and would reduce the sociological study of science to a mere error theory. From the above discussion it should be clear that I suspect these attempts will remain largely in vain. Perhaps in some cases Bloor's view is quite adequate for understanding the acceptance of new scientific discoveries, but if so, I believe this primarily accounts for scientific knowledge that does not involve deeper layers of interaction with other phenomena that influence society. To give a concrete example of this: for the application of Newton's laws of motion to have an effect on a society, a greater amount of social factors are required than, say, for the adverse health effects of a toxic agent such as lead to manifest. This might seem counter intuitive, but to explain as shortly as possible: in the first case, an *active* participation of human subjects is required in order for Newton's laws of motion to be successfully utilized, and only then will such a discovery have an influence society. It requires active participation from subjects to experience and understand the laws of motion, and finally accept them in the body of knowledge and practice of a society. In contrast, lead exerts an influence on human health through *passive* participation. People do not need to do anything with lead in order for it to have adverse effects on health, except come in contact with it. This passive influence requires from a society to *react* more strongly in order to prevent unwanted consequences, but the effect of such a passive agent is already present without the need for active social participation.

Any element of scientific knowledge in society has active and passive aspects, but, depending on different conditions, more active or more passive aspects of an element will dominate and exert influence on a society.

Carefully considered, the possible adverse health effects of RF-EMF fall into the passive category. This means that, for new scientific insights on these phenomena to become accepted in society, it primarily depends on the extend to which such effects become apparent. However, if substantial unwanted health effects do occur, these phenomena will likely become subject to a number of social factors which can distort or accelerate scientific research and influence the development of new scientific insights on these phenomena. In order to support the development of necessary scientific knowledge that will minimize the possibility of adverse health effects of RF-EMF becoming a substantial threat to public health, we need to understand how these social factors influence scientific research, and with this the ways in which relevant scientific insights become accepted in society.

It appears Bruno Latour's actor-network theory and more recent work of Andrew Pickering take a similar stance on the role of non-human entities in the development of scientific knowledge in society (Golinski, 2005). Therefore it might be worthwhile to dive deeper into these theories in the future and connect them to developments in RF-EMF research.

9. Conclusion

Despite the unprecedented rapid adoption of mobile phones and related technologies throughout the world, the scientific research field studying possible adverse health effects of RF-EMF appears a mess, and scientific insight of the possible health risks remains mostly insufficient. Given the widespread use of RF-EMF technologies, this is an alarming conclusion. As I have shown in this paper, thorough analyses of major studies can reveal many flaws in the used methodology and the way these studies have been executed. These flaws, in turn, have caused much debate within and outside the scientific community about the interpretations of the results. Nonetheless, according to the most recent evaluation of the IARC and WHO, the current state of research gives enough reason for implementing precautionary measures to reduce exposure to RF-EMF whenever possible, until more is known about the possible health effects of RF-EMF. However, due to the remaining uncertainties, the new classification of RF-EMF as a possible carcinogenic by the IARC seems to have had hardly any effect on national health policies around the world. Different countries have developed policies that vary a lot. The reasoning behind these different interpretations of scientific results are not always clear or well-founded, as argued throughout this paper. When more elaborate motivation is given for a conservative government stance, the risk assessments seems to be based on limited or even compromised interpretations of the current state of science in this field.

Looking at recent history, curious and conflicting results in scientific research on potentially hazardous substances and technologies have repeatedly turned out to be, to more or less extend, caused by corporate influences on scientific studies and evaluations. On the surface, a conservative government stance towards new scientific insights is understandable when economic stakes are high, as it always remains possible early scientific discoveries turn out invalid in later, more thorough research. However, when large corporations are involved with potentially hazardous substances and practices, malicious industry involvement in scientific research on these substances also becomes increasingly likely, making it from a government point of view more reasonable to take a precautionary stance.

The discussions and disputes that plague the research field of RF-EMF and health also have a considerable impact on the understanding of the uncertainties surrounding this topic in society in general. Due to the widespread and rapid adoption of mobile phones, ongoing scientific disputes fuel debates between different interest groups within the public sphere, online and in the mainstream media. Although it is common to dismiss such general public debates as irrelevant for greater scientific developments, following these discussions can sometimes illuminate the mechanisms at play between disputes within research fields, revealing industry ties and questionable

collaborations, while offering more speculative views on possible mechanisms behind observed phenomena and factors complicating research. However, sources outside of the scientific literature are difficult to verify, and often lead to dead ends, leaving evaluation of the given information at one's own peril. Despite this, many people — likely more than one might suspect — rely on unreliable sources, especially when sources claim to know the truth where science cannot offer readily available and clear answers to pressing concerns about the possible risks of these new technologies.

I believe the many difficulties plaguing the current state of scientific research and knowledge can be dissolved through rigorously applied analyses from error theory, combined with insights from the fields of history and sociology of scientific knowledge. The many flaws and peculiarities occurring within the EMF research field can be categorized into different kinds of errors. Doing so can offer some light on what goes wrong, how scientists can deal with these issues and how governments, interests groups and the public in general can better understand what elements are contributing to the uncertainties in this field of study and how to interpret results. Sociological studies of scientific knowledge can offer further illumination on the factors that contribute to the issues arising around this field of research. Social factors which can affect research on different levels become more apparent when properly examining complex social dynamics, which require more thorough categorization and careful scrutiny in order to minimize negative factors from hampering scientific research in any field like those discussed in this paper.

At the same time however, I believe a proper sociological analysis requires a thorough scientific understanding of the research field under scrutiny. I have tried to show this by breaking down the way in which claims about numerous errors found in scientific research can turn out to be a cascade of errors themselves. Without such a precise breakdown of scientific knowledge, quibbles between scientists often can appear no more than that, quibbles, on which the public can judge for themselves who holds the greatest truth according to their preference. In contrast however, I believe an adequate understanding of a research field reveals the agency of the phenomena themselves that are being studied, which should be fully considered as well if one wishes to get a comprehensive understanding of the many different factors that can contribute to a scientific dispute.

Overall, within Western societies there is a tendency towards funding more fundamental science and its technical applications, while moving away from sociological research and humanities in general. Perhaps the most cynical side effect of this increasing focus of economic resources on technological advancements from both industry and governments, is that it becomes a magnet for scientifically savvy people, who will seek to secure their future income through a technical education. At the same time, this focus will result in people who take a more cautious, critical stance towards

new technologies, lose income securities in an ever increasing technologically fuelled global market, while fundamental research is more and more left at the peril of industry sponsored sectors, relegating scientific progress to those who will think twice before expressing unwelcome scientific views and discoveries whenever it can threaten their research positions.

If anything, I believe the factors that trouble research surrounding RF-EMF reveal that an increasing focus on scientific and technological development requires an equal increase of independent sociological research around these areas, not to mention an adequate and proportional increase in extensive, wide ranging health research. The current state of science surrounding RF-EMF shows there is a dire need among the public and among policy makers and advisers for proper understanding of the relationships between scientific knowledge, technological progress and society as a whole. Especially when the stakes are as high as they have become with the ubiquitous use of wireless communication technologies.

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