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Expert excuses from "Neurotoxicology" journal

"Academic journals and societies show an auto-immune response to information that should be the life-blood of medicine."
– Prof. David Healy, author of *Pharmageddon*

This chapter consists of just the reviewer reports from this "scientific" journal, with my replies interpolated. The reviewers' words are in **bold** and mine are non-bold. It is presented here in the same order the reviewers made their comments, and so what you see at the beginning here is not the most important or exciting points first.

Reviewer reports from Neurotoxicology journal, with author's replies

Ref.: Ms. No. NEUTOX-D-13-00253 Robin P Clarke
Autism, adult disability, and 'workshy': Major epidemics being caused by non-gamma-2 dental amalgams

Reviewer #1:

1. The Abstract is misleading as to what information this manuscript provides, stating that, "This is the first-ever study of health consequences of non-gamma-2." This is not a "study," as usually defined, as no measurements of non-gamma-2 were made, nor were any health consequences assessed except population-level statistics about prevalence of disability.

Three baseless pseudo-points in one sentence there – I will chop it up for my replies.

> no measurements of non-gamma-2 were made,

As my review stated, indeed, no-one has ever bothered to keep records of the usage or prevalence of non-gamma-2.

That lack of data is not a fault of this work but rather of those authorities who didn't bother to even keep records. However, an indisputable inference can still be made that the overall amount of non-gamma-2 in people's mouths would have progressively increased as more and more of their teeth were fitted with the new materials. This review thus provides the absolute best quantitative information currently (and almost certainly ever) available.

> nor were any health consequences assessed except population-level statistics about prevalence of disability.

Again, that lack of data is not a fault of this work but rather of those authorities who didn't bother to even seek reports on possible adverse events from amalgam, but instead implemented the cover-up measures documented in the paper. Which means those population-level statistics are about as good as it can get. It does not follow that they are worthless, else quite a number of other "not-really-studies" in very prestigious journals would also have to be dismissed.

> This is not a "study", as usually defined, as no measurements of non-gamma-2 were made, nor were any health consequences assessed except population-level statistics about prevalence of disability.

Really? In that case there are numerous other papers which were "not really a study", despite being published in the most prestigious journals and highly promoted as indeed being important "studies". Their authors likewise didn't do any measurements or diagnoses but instead presented existing data as I have. These include for instance:

- **JAMA**. 2003 Oct 1;290(13):1763-6. Association between thimerosal-containing vaccine and autism. Hviid A, Stellfeld M, Wohlfahrt J, Melbye M.
- **Pediatrics**. 2003 Nov;112(5):1039-48. Safety of thimerosal-containing vaccines: a two-phased **study** of computerized health maintenance organization databases. Verstraeten T, Davis RL, DeStefano F, Lieu TA, Rhodes PH, Black SB, Shinefield H, Chen RT; Vaccine Safety Datalink Team.

- **N Engl J Med**. 2002 Nov 7;347(19):1477-82. A population-based **study** of measles, mumps, and rubella vaccination and autism. Madsen KM, Hviid A, Vestergaard M, Schendel D, Wohlfahrt J, Thorsen P, Olsen J, Melbye M.
- **Pediatrics**. 2003 Sep;112(3 Pt 1):604-6.Thimerosal and the occurrence of autism: negative ecological evidence from Danish population-based data. Madsen KM, Lauritsen MB, Pedersen CB, Thorsen P, Plesner AM, Andersen PH, Mortensen PB.
- **J Child Psychol Psychiatry**. 2005 Jun;46(6):572-9. No effect of MMR withdrawal on the incidence of autism: a total population **study.** Honda H, Shimizu Y, Rutter M.
- **Pediatrics**. 2004 Sep;114(3):584-91. Thimerosal exposure in infants and developmental disorders: a retrospective cohort **study** in the United kingdom does not support a causal association. Andrews N, Miller E, Grant A, Stowe J, Osborne V, Taylor B.
- **Pediatrics**. 2006 Jul;118(1):e139-50. Pervasive developmental disorders in Montreal, Quebec, Canada: prevalence and links with immunizations. Fombonne E, Zakarian R, Bennett A, Meng L, McLean-Heywood D.

No-one has ever proposed that any of those were not really studies. And that's just a few I've come up with this minute. A more reasonable consideration of the matter is as follows. Journals categorise papers as either "reviews", or "studies", or something else such as commentaries. But that categorisation is rather crude, like categorising people as either "black" or "white". In reality there is a fudging between two notional ideal types, namely "proper reviews", of which the input data consists entirely of pre-existing published studies (of for instance whether walking causes autism), and "proper studies", in which the investigators do some measuring either in a laboratory or out in the wider world. Those seven famous papers listed above fit into neither of those ideal categories, just like this present one. But so what. There has never before been ANY scientific paper about the health consequences of non-gamma-2 amalgams. And no-one has ever compiled any measurements into a published study. It follows that it cannot be either of those ideal types, but it does not follow that it cannot be an excellent scientific paper any more than those seven above are not. In reality it is properly described as both the first ever study of the known data, and as the first ever review of the evidence.

2. P.4: Evidence needs to be provided for the statement that "...Hal Huggings and other dentists were struck off the register of practitioners." for issuing warnings about the

amount of mercury released from non-gamma-2 amalgams.

Firstly, that point is far from a key foundation for any conclusions of this review. I doubt whether it warrants taking up additional space on documentation merely on the basis that some people might wish to not believe it. Secondly here are some evidential details of the matter which I have quickly dragged from the web:
Hal Huggins de-licenced for challenging amalgam:
http://www.quackwatch.org/01QuackeryRelatedTopics/huggins.html
http://connection.ebscohost.com/c/articles/9608142846/huggins-vows-fight-after-license-revocation
"Judges Block Dental Board Gagging Dentists Who Discuss Risks of Mercury Fillings":
http://www.cdchealth.com/judgeblocks.html
"California's compliance with dental amalgam disclosure policies";
"The American Dental Association has a gag rule – yes, a gag rule telling dentists not to give warnings about the toxic effects mercury might have":
http://www.gpo.gov/fdsys/pkg/CHRG-108hhrg93640/html/CHRG-108hhrg93640.htm
Details of several more dentists struck off, and it only takes a handful to scare all the rest into never telling their victims that "silver" fillings are actually mainly mercury:
http://www.mercurypoisoned.com/dentists_disciplined/dentists_gagged.html

3. P.5: The statement that, "Consequently, declining rates of amalgam installation would conceal an increase of prevalence of the amalgams in patients' mouths" is a non-sequitur. If fewer amalgams are being placed, how could their prevalence increase? It might mean that this trend would conceal an ongoing release of mercury vapor in the mouths of individuals with such amalgams, but not the number of individuals with them.

Dear Reader, please go to your kitchen sink, put the plug in firmly and water-tight, and then turn on the tap to flow fairly fast, till an inch or two of water accumulates. Then turn the tap down so there's only a little more coming out per second. And now you can see that the water level in the sink stops rising but instead quickly goes down, as it must because the rate of additional input of the water has decreased, so obviously the total amount in the sink must

decrease correspondingly. Or at least that is presumably what happens in the kitchen of someone with enough scientific expertise to judge such things.

For the educationally-deprived among us I'll go through that paragraph again, with tutorial hints added:

Dear Reader, please go to your kitchen sink [analogous to patients' mouths], put the plug in firmly and water-tight [analogous to the fact that non-gamma-2s stay in those mouths for whole lifetimes], and then turn on the tap [analogous to dentists installing non-gamma-2s] to flow fairly fast, till an inch or two of water accumulates. Then turn the tap down so there's only a little more coming out per second [analogous to "declining rates of amalgam installation"]. And now you can see that the water level [analogous to the prevalence of the amalgams in the mouths] in the sink stops rising [contrary to silly me's expectations] but instead quickly goes DOWN [well, highly-qualified expert Reviewer #1 apparently thinks so, so there], as it must because the rate of additional input of the water has decreased, so ["]obviously["] the total amount in the sink must decrease correspondingly. Or at least that is presumably what happens in the kitchen of someone with enough scientific expertise to judge such things.

I did emphasise in my review that the whole point of non-gamma-2 was that they are <u>far more</u> durable (indeed can easily last a whole lifetime). Just like that water which doesn't suddenly start to rush out of the sink just because you turned the tap down.

4. P.5: The author states that information is not available on "usage or total prevalence of non-gamma-2 in people's mouths." Given this, any statements made about the health consequences must remain purely conjecture.

Firstly Reviewer #1 here misrepresents what I wrote. I did not state that "information is not available…". My words were:

"I have been unable to obtain any **<u>numerical data</u>** on usage or total prevalence of non-gamma-2 in people's mouths. The DH have told me they have no such records. And NHS dental records have not recorded the types of amalgam used. It is unlikely that any better information is available in other countries. But we can very reasonably assume that the overall prevalence of non-gamma-2 will have gradually, progressively increased in the decades following its introduction."

And you can see there that I had already pre-answered this half-baked objection. It is in the nature of reality that the prevalence of something *must* inevitably increase for some period after its

introduction as the new standard product. And it is common knowledge that people usually have their further tooth fillings put in in dribs and drabs over the years so their prevalence will correspondingly increase over a period of years rather than of minutes or millennia. Which is very much in line with those increase curves of autism, adult disability, and later age of onset, which also occur over years following the change to non-gamma-2.

5. P.6: The author states that he or she did not "cherry-pick.. selected data to prove any point," yet that is done in the last paragraph on this page, when reviews supporting the hypothesis that mercury is etiologically involved in autism are cited, but reviews that conclude that it is not are not cited.

But again, Reviewer #1's assertions are multiply untrue. Firstly, reviews are not data. Secondly, *in that very section supposedly at fault here*, *I did indeed explicitly cite the entire (supposed) counter-data*, namely the three studies which have been claimed to disprove the mercury-autism link, namely Ip et al, Soden et al, and Hertz-Picciotto et al. So that instance asserted by Reviewer #1 shows the exact opposite of what Reviewer #1 asserts. And thirdly, my statement about not cherry-picking was only in my section headed "My epidemiological investigations", and specifically a comment about my own presentation of data of the time-trends of autism, adult disability, and amalgams. What great contrary data have I omitted there? In reality there has been not the slightest cherry-picking and this is merely yet more nonsense from this so-called peer reviewer.

6. P.7: The fact that mercury excretion is increased following administration of DMSA in individuals with autism does not prove much, as the action of DMSA is nonspecific. Excretion of other metals (lead, antimony) is also increased.

Yet more cheap muddle from Reviewer #1. The finding in Bradstreet et al was not "The fact that mercury excretion is increased following administration of DMSA in individuals with autism". Rather it was the finding of a major difference between autistics and non-autistics, with the autistics outputting three times as much mercury as the non-autistics (with fluke probability of 1 in 5000). AS ALREADY CLEARLY STATED RIGHT THERE.

7. P.7: The conclusions of the Holmes et al. (2003) study are weak, not because of whatever biases the investigators might or might not have but because the findings are not credible. In this study, the mean mercury level in the hair of controls was 3.63 ppm, which is much higher than would be expected in a representative sample of infants. By comparison, measurements of mercury in children's hair in an NHANES survey conducted about the same time (1999-2000) (McDowell et al., Environ Health Perspect 2004;112(11):1165-1171) reported a mean of 0.12 ppm (and 0.16 among fish-consuming children). This suggests that the controls included in the Holmes et al. study were biased with regard to their mercury status and that an 8-fold reduction reported in the hair mercury level of "autistic cases" is likely an artifact.

Here Reviewer #1 shows a bit less incompetence, and stumbles only in terms of a rather more subtle fallacy. We could call it "the fallacy of the assumed all other things being equal". A good other example of it is found in various comments about the Hallmayer et al 2011 twin study finding of autism being mainly environmental. Commenters on Hallmayer et al have concluded that it shows that the earlier twin studies were "wrong". But well, they "must be wrong" mustn't they?, because Hallmayer et al is a big powerful new study and so it must trump those little old ones into the wastebin of "wrong" results.

The fallacy here is the unfounded assumption that all other things are equal (constant). In respect of those twin studies, please have a look at my still-unchallenged paper "A theory of general impairment of gene-expression manifesting as autism", which appeared in print years ago and is still essential reading for anyone who wants to have a clue about the subject. Therein I specified the conditions under which autism would change from a mainly genetic condition to mainly environmental: *"If a rare perinatal adversity were to become somewhat more common, then obviously, autism of the environmental category would become more prevalent."* And now with the huge impact of non-gamma-2 in parents' and carers' mouths, exactly such a condition has indeed occurred, and so hardly surprisingly the causation of autism has indeed CHANGED from mainly genetic to mainly environmental. There is no real conflict between Hallmayer and the earlier twin studies, merely differences of the underlying and unexamined variables. Likewise, in respect of mercury and autism we know that there is a lot we do not know. You can see in my own review section there how the various studies of autistic hair give divergent results and that there is nevertheless

good reason to find them all valid and true. Likewise, to dismiss the Holmes et al result as "not credible" just because of those non-standard levels entails an unwarranted gross presumption that there are no important unknowns going on between the different studies. And so the finding of Holmes et al should not be dismissed unless there is a more substantial basis for doing so. And on the contrary, later studies have supported their 'perverse' data of lower hair mercury levels in autism. This Reviewer #1 is here categorising the careful work of Holmes et al as {either grossly incompetent or grossly fraudulent}, on a basis of no real evidence but merely because he/she does not find their results in accordance with the required commercially/professionally-convenient dogma.

> I don't think it is appropriate to state that a pattern of findings provides any evidence as to whether an investigator was "acting competently and honestly."

Whereas I do think it appropriate. And that is because fraudsters or incompetents are extremely unlikely to come out with a whopping strong result that is:

(1) markedly contrary to what they would have expected;

(2) markedly contrary to what they would have found convenient to report; and

(3) only subsequently supported by the collection of results of later other-people's studies of autistic hair mercury.

And in the context that many have presumed to shallowly discredit Holmes et al as either incompetent or fraudulent (as Reviewer #1 here does him/herself), that consideration is outstandingly eminently appropriate to be stated.

8. P.7: The author multiplies the P-values from 6 studies to calculate the probability that the findings are due to chance. This is a meaningless calculation. First, the studies included reached different conclusions about the hair mercury levels of children with and without autism (although the author argues that age needs to be taken into account). Second, given that all P-values are less than 1, multiplying them necessarily results in a smaller and smaller number the more studies one includes. If each of the 6 studies yielded a P-value of 0.5 (indicating no statistically significant relationship), then using the author's method, the combined P-value would be 0.0156, which would suggest that, in aggregate, the studies provide significant evidence of an association. Third, even if the author's method was valid, it

would be necessary to include in the calculation all of the studies ever conducted of a particular hypothesis, not just those selected because they purport to show an association (just as it is necessary, in a meta-analysis, to include all available evidence).

Again I shall have to chop the above into shorter bits for reply, as follows.

> The author multiplies the P-values from 6 studies to calculate the probability that the findings are due to chance. This is a meaningless calculation.

(It is absolutely standard probability maths to multiply together probabilities to get the compound probability of them all happening merely by fluke, as any betting shop can confirm, but we must continue here with Reviewer #1's further exposition on this point……)

> Firstly, the studies included reached different conclusions about the hair mercury levels of children with and without autism (although the author argues that age needs to be taken into account).

This misrepresents the situation. I don't "argue" that age needs to be taken into account, rather I *observe* that age needs to be taken into account, in that the earlier ages always give lower mercury in autistics, while the later ages always give higher mercury. Thus none of those studies are in any conflict with the reasonable hypothesis mentioned by Majewska et al that the adrenarche plays a role in the hair mercury levels. There is therefore not any real conflict between these studies but rather voices declaring in common that mercury is involved in autism in some way. (And Reviewer #1 is here again employing that fallacy of the presumed all other things being equal – age in this case.) And so there is no valid ground there for not multiplying together those probabilities.

> Secondly, given that all P-values are less than 1, multiplying them necessarily results in a smaller and smaller number the more studies one includes.

That is of course true. [Note for non-expert readers: smaller P-values indicate the results are less likely to be mere flukes and so are more "significant".]

> If each of the 6 studies yielded a P-value of 0.5 (indicating no statistically significant relationship), then using the author's method, the combined P-value would be 0.0156, which would suggest that, in aggregate, the studies provide significant evidence of an association.

And that is also indeed true. But so what. It is indeed the reality that several bits of weak evidence can add up to strong evidence. Indeed that is the whole point of making a (for instance clinical) study large enough to give a significant result. Any such study can be conceived of as being a combining together of lots of smaller sub-studies, any one of which could give non-significant results, but when all put together would enable a highly significant result. And that high significance is not some specious false result, rather it is the entirely sound statistical inference. And that's what I've done there, **except that my p values were all highly significant already**. And the fact that the evidence there is of diverse types adds all the more to its methodological robustness, as it is not wholly founded on any one premise.

> Thirdly, even if the author's method were valid, it would be necessary to include in the calculation all of the studies ever conducted of a particular hypothesis, not just those selected because they purport to show an association (just as it is necessary, in a meta-analysis, to include all available evidence).

Again, not so. Firstly, there IS no contrary evidence on the mercury-autism question such as could make any meaningful reduction of my combined calculation. I've pointed out that even the three supposedly counter results were actually pro in reality. Secondly, I made the point that that is the probability only from those few studies combined. It logically follows that if there were more studies, and continuing on the same 100% positive connection trend, then that would simply make my big fluke number even bigger (smaller). So there is still no sound objection to my probability calculation.

9. P.8: The argument about the evidentiary value of never having seen the Queen is a little ridiculous and, in my view, has things completely backwards. It is by means of the falsification of hypotheses that science advances. A single negative result is enough to call into question a positive result that has repeatedly been observed and might be the

result of bias (all it takes is the observation of one black swan to refute the statement that, "all swans are white"), but no number of positive observations is sufficient to demonstrate the universality of a statement.

Again I will need to chop this up for my replies.

> 9. P.8: The argument about the evidentiary value of never having seen the Queen is a little ridiculous and, in my view, has things completely backwards.

As we'll see in the next few lines…(?)

> It is by means of the falsification of hypotheses that science advances.

Partly so, but also there cannot be any advance at all if hypotheses are prevented from being properly raised in the first place. And Reviewer #1 is doing a great job of preventing some very important hypotheses being raised, via these unflattering would-be-critiques right here.

> A single negative result is enough to call into question a positive result that has repeatedly been observed and might be the result of bias (all it takes is the observation of one black swan to refute the statement that, "all swans are white"), but no number of positive observations is sufficient to demonstrate the universality of a statement.

Reviewer #1 here uses some extremely incompetent language to confuse the matter. Namely the notion of a "negative result". For example an investigation of whether or not the Queen actually exists could come up with two very different types of results, both of which Reviewer #1 would have us class as "negative results". On the one hand, there could be a failure to see the Queen on peeping over the palace wall; on the other hand there could be a finding of the absence of the Queen anywhere in the UK following an insanely detailed mega-search from South to North and back. The difference between a "negative" failure to find something and a (positive) finding that that something is actually absent, is complete and absolute, and not to be confused by conflating into a false notion of "negative results".

> A single negative result is enough to call into question a positive result that has repeatedly been observed and might be the result of bias

I shall here now correct Reviewer #1's grossly incompetent language.

"A single FINDING OF POSITIVELY CONTRARY evidence is enough to call into question THE UNIVERSALITY OF [an earlier] result that has repeatedly been observed and might be the result of bias."

"A billion mere FAILURE-TO-FIND results CAN BE STILL NOT enough to call into question [an earlier] result that has repeatedly been observed and might be the result of bias."

When I used the words "negative results" it was self-evident from the context that I could only mean the latter, more common meaning of the term, and not the "positively contrary" meaning. But Reviewer #1 still managed to muddle it as ever.

> a little ridiculous and, in my view, has things completely backwards.

Indeed.

10. P.8: The discussion of the validity of the three studies sometimes described as refuting an autism-mercury link requires fleshing out. It is necessary to tell the reader the arithmetic error Ip et al. made and to demonstrate the extent to which it altered the study conclusions. The reader is told that DeSoto and Hitlan (2010) concluded that Soden's study "actually proved the opposite," but no information is provided that would enable the reader to evaluate this statement. The conclusions of Hertz-Piccioto et al. are misstated. The second-to-last sentence of this paper actually states, "This report did not address the role of prenatal or early-life Hg exposure in the etiology of autism." The major finding was that total Hg in blood was not elevated or reduced in preschool children with autism/ASD compared with unaffected controls and resembled those of a nationally representative sample. The reason for the authors' qualification is that only concurrent measures of blood Hg were available, meaning that they could draw no conclusions from their data about the role of prenatal or early-life mercury exposure. To say that the authors concluded that their data, ".constituted no evidence whatsoever against causation of autism by mercury" is simply wrong.

Again, I need to chop this up for my replies.

> 10. P.8: The discussion of the validity of the three studies sometimes described as refuting an autism-mercury link requires fleshing out.

….because…..

> It is necessary to tell the reader the arithmetic error Ip et al. made and to demonstrate the extent to which it altered the study conclusions.

Really? I cited the conclusion of DeSoto and Hitlan (2010) that the study actually proved the opposite. (Ip et al was retracted due to their major but elementary errors.) On this question this reviewer should either explain why D&H were wrong or else shut up. Here's what they said:

> "The author of record has publicly acknowledged that these numbers and the statistical calculation were in error in an erratum (Ip et al. 2007) and the journal editor notes the reason given was a series of typographical errors (Brumback 2007). Furthermore, a careful and correct analysis of the full data set results in a statistically significant difference (Brumback 2007, DeSoto and Hitlan 2007, DeSoto 2008) with autistic children having higher mean levels of mercury. As can be seen by comparing the erratum to the original article, the standard deviations were wrong for both groups, the stated statistical significance in 2004 was not even close: their original stated level of statistical probability was off by almost 10 fold."

> The reader is told that DeSoto and Hitlan (2010) concluded that Soden's study "actually proved the opposite," but no information is provided that would enable the reader to evaluate this statement.

Not so. I provided the citation of D&H along with the citation of the original Soden, which is all the information that is needed for that evaluation. If Reviewer #1 reckons there is something wrong with D&H's conclusions then he/she should state what it is, or else shut up. Here's what D&H said:

> "In the end, the statistical test conducted by Soden and coworkers is meaningless and distracting from the essentials of what was done. The authors measured metal levels, then (based on the lab definition of toxicity) all values were defined as zero, then – they tested this actual zero statistically and found that one could not rule out zero. "
>
> "But let readers be clear about this central point: if one is willing to consider the actual numbers reported and test those numbers, the results are clear - a larger proportion of autistics had heavy metals excreted as the result of chelation."

It is not the business of authors of papers to have to recite the details of all the prior papers they cite in support; if they did there would be even more that everyone had to read. Any half-proper peer reviewer would check out the background references themselves (where required), and indeed in this case ought to be an expert familiar with these important key papers (on ***Neurotoxicology*** of autism) already anyway. What a timewasting pseudo-expert charlatan.

>The conclusions of Hertz-Piccioto et al. are misstated.

Not so. They are not in the slightest mis-stated in my report.

> The second-to-last sentence of [their] paper actually states, "This report did not address the role of prenatal or early-life Hg exposure in the etiology of autism."

Indeed that is the case. But so what? That is exactly my point about it. [Note to non-expert readers: "Hg" means mercury and "etiology" means causation.]

> The major finding was that total Hg in blood was not elevated or reduced in preschool children with autism/ASD compared with unaffected controls and resembled those of a nationally representative sample.

Indeed that is the case. But so what? I never said otherwise.

> The reason for the authors' qualification is that only concurrent measures of blood Hg were available, meaning that they could draw no conclusions from their data about the role of prenatal or early-life mercury exposure.

Indeed that is the case. But so what? That is exactly my point about it.

> To say that the authors concluded that their data, ".constituted no evidence whatsoever against causation of autism by mercury" is simply wrong.

No it isn't. Their words quoted above indicate PRECISELY that. As Reviewer #1 appears to be having some peculiar difficulty with either language or logic I will try to parse this for them as follows. (I apologise that I have to assume the reader is an idiot here.)

We begin with their paper's second-last sentence that:
"This report did not address the role of prenatal or early-life Hg exposure in the etiology of autism."

That means effectively the same as:
"This report *was not capable of providing any information about* the role of prenatal or early-life Hg exposure in the etiology of autism."

Which means that it is also the case that:
"This report *did not provide* any information about the role of prenatal or early-life Hg exposure in the etiology of autism."

And hence:
"This report did not provide any *evidence* about the role of prenatal or early-life Hg exposure in the etiology of autism."

And hence:
"This report did not provide any evidence about the role of prenatal or early-life Hg exposure in the *causation* of autism."

And hence:
"This report did not provide any evidence about the role of prenatal or early-life *mercury* exposure in the causation of autism."

And hence:
"This report did not provide any evidence about the causation of autism by prenatal or early-life mercury exposure."

And hence:
"This report did not provide any evidence *against* the causation of autism by prenatal or early-life mercury exposure."

And hence:
"This report *constituted no* evidence against the causation of autism by prenatal or early-life mercury exposure."

And hence on merely removing a redundant word:
"This report constituted no evidence against *[~~the~~]* causation of autism by prenatal or early-life mercury."

….which would be identical to my own statement except that there is that extra bit about "prenatal or early-life".

So I was wrong there. I overlooked that autism could still be not caused by exposure to mercury later in life, after that person has already become autistic. So we'd best not publish my non-gamma-2 rubbish after all.

And whatever it takes to become a reviewer for *Neurotoxicology*, it's all too clear I don't have it myself.

Reviewer #2:

Dental amalgams are a continual source of controversy. The current review attempts to survey the adverse health consequences of the amalgam formulation known as non-gamma-2. It asserts that these restorations "are currently by far the main cause of chronic disability in the UK, US, and other such countries, with about 10% of the UK working-age population disabled thereby." It also claims that its introduction led to a 10-fold increase in the incidence of autism.

Indeed. But no faults are there to provide basis for non-publication so far.

As a contribution to this specialized journal, the manuscript lacks any clear connection. It offers no neuro-mechanistic foundation for such a correlation, especially for autism, which is a product of disordered early development.

Not so. In respect of autism, my review(/study/rant/) ties in the newer mercury factual data with the prior unchallenged theory and the related fact of how the mercury binds with DNA to reduce gene-expression and hence [as my antiinnatia theory had predicted] cause autism. And meanwhile in respect of adult mercury poisoning there is quite a developed understanding of how the symptoms are caused. The details of that causality are in the cited literature or secondarily-cited.

It doesn't attempt to demonstrate any kind of dose-response relationship.

No data is available that would enable that. But it doesn't follow that there is no other useful evidence presented.

Its definition of autism lacks specificity.

It doesn't need to have any "specificity". It just uses the definitions that are used as standard by others. As is usual practice.

In addition, the claim that this amalgam formulation accounts for 10% of chronic disability requires advanced statistical modeling of exposure-consequence relationships in which other kinds of exposures are concurrently evaluated.

Firstly, I did not claim that it accounts for 10% of chronic disability. I reckoned from the data of Figures 5 and 7 that it now actually accounts for MOST chronic disability (something like 70-95%). That 10% figure was my estimate that wholly 10% of the UK workforce has been disabled by non-gamma-2 (about 4 million victims out of a 40 million workforce).

That 10% is not a "claim" but rather was expressly only a rough estimate, from looking at figure 5 (plus the further context of Figure 7). You can see that it shows an increase of about 2 million accepted claimants, easily all attributable to non-gamma-2. And you can see that it peculiarly levels off about year 2000 as would be expected from the stated political agenda of "claimant count now controlled". And you can see that otherwise it would most likely have continued upward to something like 4 million – hence 10% of the working-age population.

And no fancy statistical modelling is required to understand what these graphs are showing us. Of course they are not absolute proof, but neither are they any lack of evidence, else we'd have to retract an amazing lot of highly-acclaimed "studies" from the most prestigious journals.

In the absence of these kinds of information, it is difficult to see how this manuscript is compatible with the aims and audience of this journal. Perhaps the author should consider another kind of journal and audience.

Or perhaps instead the so-called *Neurotoxicology* journal should consider changing those aims and audience, or change its name to reflect its restricted nature, for instance to *Pedantic Neurotoxicology* or *Pseudoneurotoxicology*.

<u>Reviewer #3:</u> This is an opinion piece on the possible role of mercury exposure in the causation of autism.

Not so. It is not "an opinion piece". Like all scientific papers it does include proposed conclusions which are necessarily of an opinion nature. But for the most part it consists of presentation of data and reasoning thereon, which is entirely in line with any normal scientific paper and not "an opinion piece".

The author makes a very impassioned case for non-gamma-2 amalgam fillings being the major cause for the rise in incidence of autism using ecological data from UK, US and few other countries.

Not so. It is not at all "very impassioned" but rather "very filled with as much useful factual evidence as can be found".

No primary research has been undertaken by the author to test this hypothesis.

So what. Exactly the same could be said about all those seven highly-rated studies listed on the first page here. Has anyone ever called for their retraction yet?

My main concern with this work is that it is not an objective assessment of the evidence available at present.

….because / for instance…..

Key statements that form the basis for the author's argument are unsupported by high-quality evidence.

…such as….

For example, the exposure of children to mercury from their parents' amalgam restorations needs to be confirmed before the author can make such a far-reaching conclusion.

Indeed, no one has bothered to do any measurement studies of this question to date. But that is not the fault of this author or this review. Rather it highlights the urgent need to make a start by publishing this first study of the subject, which can be then followed up by testing studies. But I did already explain why we can be confident that there is enhanced exposure. That is because there is

very low background atmospheric mercury vapor, and it is known to constantly emit from parents' and carers' amalgams, and they commonly spend much time together with babies in enclosed spaces, even talking at them through their amalgam-filled mouths, and so it logically follows that many babies are going to breathe in an increased amount of mercury vapor at least on average. [Studies have also shown prenatal transmission.]

Randomized clinical trials of dental amalgam (Bellinger et al 2006; DeRouen et al. 2006) showed no significant neurodevelopmental deficits in the children receiving amalgam restorations compared to non-mercury fillings.

Those two studies have already been solidly debunked as evidence, as I pointed out via my first page citation of Mutter 2010 (and others). Not least they started too old to relate to causation of autism, and they stopped too young to relate to causation of adult disability. In fact (as in my earlier journal replies) if I myself had been in those studies I would have been recorded as evidence of *harmlessness*, because I only became chronically disabled (by the amalgam scam) *after* the age at which those studies stopped. And an editorial in the very same issue of the journal stated that those two studies did not constitute evidence of amalgam safety. Why didn't Reviewer #3 mention that counter-point in their "unbiased" commentary here?

[Update: The Bellinger, DeRouen and Maserejian studies have now been further demolished by Homme et al. (2014). See also IAOMT (2008).]

In fact, Maserejian et al. 2012 have reported that compared to amalgam restorations, children receiving composite (non-mercury) fillings showed impaired psychosocial function. There are several other such instances in the manuscript where important data have been ignored.

Maserejian et al had not been published when I first sent this review to a journal in July 2012, else I might have mentioned it. But exactly the same methodological problems arise as with the two others cited above. I myself was doing fantastically well at school before the effects of the amalgam scam imposed themselves so heavily on my life. And perhaps bisphenol-A might well have injurious effects but that is a separate matter out of the range of my own documents.

In my opinion, this manuscript does not add unbiased scientific knowledge to the topic of mercury and autism, and I cannot support it being published.

But rather it is this Reviewer who is biased, and has raised only bogus reasons for suppressing the publication of this outstandingly important cautionary information.

IN CONCLUSION:

These three reviewers have failed to raise even a single sound reason for preventing the publication of this very important information. And they have meanwhile deployed a whole load of shallow pseudo-objections, which raises considerable questions about both their competence and their honesty.

And that comes in the context of ten previous journals likewise raising only specious excuses for refusing publication.

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Further reply from "Neurotoxicology" (23rd October 2013):

"…. Thank you for your email. I forwarded it to the editor of the journal. After review it was concluded that your manuscript was handled appropriately and the original decision stands….."

Notably there was an absence of any rebuttal of any of my rejoinders.

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