5

Expert excuses from other "scientific" journals

"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 mainly of just the "peer reviewer" replies from 18 journals with my rejoinders added in between. I sent the lengthening compilation to each successive journal in turn. You can see that not a single real fault of evidence nor reasoning (nor presentation) has been shown by any of these "scientific" journals.

1. BMC Medicine

In an email reply sent 21 August 2012, Claire Tree-Booker declared on behalf of the editors of *BMC Medicine* that their refusal to consider this present manuscript was because:

> " we did not feel that it was sufficiently different from the 2011 article published in *Journal of Occupational Medicine and Toxicology* (Mutter J. Is dental amalgam safe for humans? ".

And yet only one of Mutter's 160 references overlapped with the 50 cited here. There is no other overlap of evidential basis between Mutter's review and this one. And this one concludes with evidence-based estimates of the huge scale of morbidity being caused, whereas Mutter's makes no such estimates. And this one contains six (highly-original) graphs of data whereas Mutter's contains no graphs nor tables. Plus my predictions and preventive advice none of which are contained in Mutter's review. Above all, my review is explicitly of consequences of *the change to non-gamma-2 amalgams* whereas Mutter's makes no mention whatsoever of non-gamma-2 or any change. *BMC Medicine's* sole ground for rejecting is thus shown to be wholly false.

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2. Journal of Occupational Medicine and Toxicology

The *Journal of Occupational Medicine and Toxicology* then took 14 weeks to refuse the paper without giving any reason at all.

3. Environmental Health

The paper was then sent to *Environmental Health* on 4th December 2012. On 10th December, David Ozonoff sent a reply whose least unsubstantive content consisted of the following:

> "Our journal requires prior approval for review articles. In this case the Editors feel that having a plausible hypothesis is not sufficient. There is an abundance of speculation on what is causing the increase in autism or even if that increase is real and not an artifact. Over the time period at issue many things have changed, not just dental amalgams. Whether any of them, including your hypothesis, are credible or not will require a more fine grained and targeted analysis."

But there is not even one genuine scientific objection shown by those comments, as is explained in the following.

>"Our journal requires prior approval for review articles."

The only proper criterion for a journal that validly claims to be scientific is to publish the best possible (or potentially best) content in whatever form it has to take. The very essence of science is that it is an exploration of the unknown. It follows that unless an editor is content to confine their mind to some pseudic form of "prophetically anticipated science" they must be open to publishing whatever form and origin of content happens to present itself, especially when a very important catastrophic situation is carefully presented with clear practical implications as here. Of all the duties of a medical research community, there can be none greater than facilitating the publication of substantial warnings of harm being done by medical practices themselves.

>"In this case the editors feel that having a plausible hypothesis is not sufficient."

But the paper does not present merely a "plausible hypothesis". If so I wouldn't have bothered writing it let alone sending it to the journal. Instead (even as stated in the abstract) it presents extremely substantial evidence and reasons for ruling out all other possibilities. Furthermore it is very far from mere hypothesis but instead a causal theory. A journal editor ought to know and understand the important fundamental difference between the concepts of theory and hypothesis. [*Hypothesis:* "Sugar tastes sweet".; *Theory: "*Putting sugar on your tongue induces a chemical reaction in the taste-buds (which have evolved to detect nutrients), and this in turn induces action potentials in neurons which thereby transmit signals to other neurons in the brain which register a sensation generally reported to be sweetness; Blaggg (1987) showed pictures of the taste-bud receptors in which the blue dots are the etc....; Freddd (1997) showed that the increased action potentials only occurred when sugar was present etc...."]

>"There is an abundance of speculation on what is causing the increase in autism or even if that increase is real and not an artifact."

Indeed an abundance of *speculation*. By contrast, I present strong *evidence* and *reasoning*. What this editor should be doing is explaining what specific errors or gaps there are in it, not just adding his own rather facile speculations of what errors he reckons he would find in it if he bothered to check.

And indeed, the huge abundance of mere speculation about these matters should properly be recognised as showing that this is an extraordinarily important subject and one in which there is indeed too much mere speculation and not enough actual evidence-based and reason-based coherent theory such as presented by this paper. So he got it exactly the wrong way round there - it's all the more reason that they *should* be publishing it (or at least not casually dismissing it).

And as for the alleged non-increase, please explain to me how anyone credibly accounts for those charts figs 1-4 other than by a real increase (let alone the peculiarly close parallel increase in Fig. 5).

>"Over the time period at issue many things have changed, not just dental amalgams."

That is a statement of the idiotically obvious – as if it would never have occurred to myself to ask if anything else might have changed, before I wasted months producing that review of the possibilities. But this editor's cheap words fail to address the fact that not even one of those other things can account for the clear involvement of mercury in the increase of autism (let alone also account for the timing and other details). I made that point clearly in the paper, but it seems we have here the reply of someone who is so clever that he doesn't need to actually read something before having yet more prophetic revelations of what it will "obviously" have failed to say.

>"Whether any of them, including your hypothesis, are credible or not will require a more fine grained and targeted analysis."

But I challenge anyone to suggest even <u>one</u> alternative to "my" "hypothesis" which I have not already ruled out in that manuscript. You can't. And that's the <u>whole point</u> of it. And there exists no alternative review of the subject – this is the best now and likely best there ever can be (in consequence of the failure to keep records).

Editors of *Environmental Health* failed to respond to these rebuttals, so I proceeded to send to a further journal.

4. BMC Public Health

It was next sent to *BMC Public Health*, on 13th December 2012. On 20th December Natalie Pafitis replied that:

> "We have now looked over your submission and are sorry to inform you that the journals in the BMC series do not generally consider narrative reviews for publication. We are therefore unable to consider your manuscript for peer review and are closing your file."

Again there is no sound ground for non-consideration offered there. Even if it is in some aspects a "narrative review", so what? It still remains the <u>only ever</u> review to date of non-gamma-2 amalgam consequences. It must therefore at this date be the best available

science on this most important question (and probably best-ever given the institutional failures to record data), and the fact that editors can speculate some notional "proper" sort of review as a hypothetical substitute is entirely irrelevant. As already pointed out at top of page 2 here, any journal having pretences to being scientific should be definition be open to the unknown nature of new discoveries and becomes merely pseudoscientific to the extent that it insists on confining itself to its prior presumptions of what the best science should look like, a false "prophetically anticipated science".

5. Health Research Policy and Systems

On 20th December it was sent to *Health Research Policy and Systems*. On 24th December the HARPS Editorial Team replied that:

> "Pre-peer review of your manuscript is now complete and I am sorry to say that we cannot consider the manuscript for publication given that your article is out of scope for our journal."

And yet that notion that it was "out of scope" for that journal is difficult to square with the following evidence copied from the Covering Letter I had sent to them:

> "
>
> Why this is suitable for *Health Research Policy and Systems*? Please note all the relevant bits of your journal statement which I have **bolded herebelow**:
>
> > *"Health Research Policy and Systems* aims to provide a platform for the global research community to share their findings, insights and views about all aspects of the organisation of health research systems including **agenda setting, building health research capacity**, and **how research as a whole benefits decision makers and practitioners in health and related fields and society at large**."
> >
> > *"Health Research Policy and Systems* considers **manuscripts that investigate the role of evidence-based health policy and health research systems in ensuring the efficient utilization and application of knowledge to improve health and health equity**, especially in developing countries. Research is the foundation for improvements in public health. The problem is that **people** involved in different areas of research,

> together with managers and administrators in charge of research entities, **do not communicate sufficiently with each other. How well informed is the public of the results of their research? How do they make sure that what they do will actually improve health? Do they have good links with the decision makers who can actually influence how their research findings are used? Is the money used to sponsor their activities spent wisely, fairly and efficiently? Are there means to assess the impact and utility of their work? How many of them are leaving the country for greener pastures? How can they be enticed to stay?"**

And this is indeed an article which presents proof of the **agenda setting** avoidance of having dental amalgam toxicity on the agenda, with avoidance of **building research capacity**, and **how research as a whole** is prevented from **benefiting decision makers and practitioners in health and related fields and society at large**.

It is a **manuscript that investigates the** prevention of a **role of evidence-based health policy and health research systems in ensuring the efficient utilization and application of knowledge to improve health and health equity**, about **people** who deliberately set out to prevent others to **communicate sufficiently with each other**.

How well informed is the public of the results of their research?: They are kept in deliberate ignorance by systematic censorship and deceit as documented both in the review and in its shallow blocking by editors of four pretendedly scientific journals.

How do they make sure that what they do will actually improve health?: The review shows how they go out of their way with deceits to prevent such improvement. **Do they have good links with the decision makers who can actually influence how their research findings are used?** Yes, evilly-"good" links as indicated in the review.

Is the money used to sponsor their activities spent wisely, fairly and efficiently? No, it is used criminally in support of cover-up of a gigantic crime. **Are there means to assess the impact and DISutility of their work?** Yes, this review. **How many of them are leaving the country for greener pastures?** This review documents the reasons why just about all the honest researchers have been driven out by a Lysenkoist regime of pseudo-science and persecution of those who try to do honest study of the subjects.

How can they be enticed to ~~stay~~ come back?" By publishing this review.

It should be clear from the above that this review fits very much with your statement of what would be relevant.

6. Emerging Themes in Epidemiology (*Permitted Themes in Epidemiology*?)

It was next sent to *Emerging Themes in Epidemiology* on 25th December 2012.

An email reply from the ETE Editorial Team dated 8th February 2013 stated the following two paragraphs of rationales for not accepting. These rationales have at least an appearance of being much more substantive than those received in earlier responses, but they do fall apart on proper examination as I will now show.

"1. The aim of the paper is to present evidence of a causal relationship between exposure to dental amalgams and autism, as well as other disabilities. We found no evidence to support such a claim in this article. The article presents time trends demonstrating secular increases in autism diagnoses and disability claims. There is, however, no data presented regarding the population level exposure to non-gamma-2 dental amalgams over this period."

Firstly, as regards data of the population exposure to non-gamma-2: In response to my FoI request the UK Department of "Health" stated that they have kept no records of usage or prevalence. And indeed I can myself confirm that my own dental notes from many years under the "care" of a leading Dental Hospital and School give no indication of which types of amalgams were installed or present, even though I was well aware that in earlier decades I frequently had amalgams crumbling in my mouth (indicating they were the crumbly earlier types), whereas I later had a large number of amalgams which never degraded even over decades (indicating they were the non-gamma-2 types). And in the UK more generally, dental notes are only kept for ten years. And with the substantially more complex and fragmented medical system in the USA it is highly unlikely that there would be any better records there. However, despite that callous neglect of documentation, we can still reasonably infer that the prevalence of the highly-durable non-gamma-2 progressively increased from the

time of its introduction in 1975-6 onwards. At first there would be mostly just ones and twos in a few patients, while later there would be many patients accumulating more and more high numbers. The review thus presents about the best evidence as we can ever hope to obtain and yet it is still indicative enough of close relationships to increases of both autism and adult disability such that the precautionary principle should be strongly evoked thereby.

"There is also no evidence presented that those exposed to these products have higher rates of these conditions compared with those not exposed to them, nor that those with these conditions are more likely to have been exposed to these products."

Not so. In respect of autism I cited Holmes et al 2003 and Geier et al 2009, and could have added Majewska et al 2010. In respect of adult disabilities I cited the separate reviews by Mutter 2011 and Hanson 2004, which reached similar causality conclusions to my own via almost entirely different data. And that is despite the gross avoidance of carrying out any studies of these sorts, in line with the pseudo-scientific denialism documented in the Appendix of the paper.

Besides which, this is specifically a review of such evidence as exists of an epidemiological nature. It would take an even much longer paper to re-review all the other data which has already been adequately covered in the cited Hanson 2004 and Mutter 2011 (in respect of adult disability) and Geier et al 2010 (in respect of the clear involvement of mercury in much autism).

"2. There have been at least two large-scale randomised clinical trials with long-term follow-up that have investigated whether use of dental amalgams has adverse neurological or psychosocial effects. Neither has shown evidence of an effect, yet these studies are not mentioned in the manuscript."

Not so. Firstly, the paper's second paragraph stated: "Some relatively large-scale trials have been asserted to show amalgam safety, but they have been substantially flawed and in at least one case in reality showed harmfulness rather than safety (as explained by Mutter [19])."

And on the contrary the Childrens' Amalgam Trials showed significantly decreasing urine mercury despite increasing intake, which is evidence of developing toxicity. And there were severe

limitations in those studies, such that an accompanying editorial stated they were not capable of showing amalgam to be safe. And they certainly did not have "long-term follow-up", indeed, if I myself had been included in those studies I would have been registered as evidence of harmlessness because I became chronically disabled only after the age at which the trials ceased. By remarkable fluke of PhD-qualified professional design and peer-review those studies started too late to detect autism and ended too early to properly detect adult pathology. (And despite their very poor quality they had no difficulty getting promptly published (and in prominent journals) in contrast to this present paper)

And again, these defective studies were not mentioned because they had already been demolished in the cited Mutter 2011 (and by others such as Boyd Haley) and it is not reasonably to be demanded that this present review should completely re-review every defective propaganda study anew, else it would need to be even longer.

Finally, even if the editors of ETE indeed did not find the presented evidence compelling, their proper response should still have been to act in accordance with the precautionary principle, publishing the review while stating alongside it their notions about the unsoundness of inferring causality from it.

Annals of General Psychiatry (not fully submitted to)

It was next sent to *Annals of General Psychiatry* on 15th February 2013. But they were unwilling to allow a waiver to below their discounted fee of £1,180 / $1,880 / €1,480. That would be beyond my means as a chronically mercury-disabled benefits-dependent with no earning prospects, so I decided to seek another journal which would provide open-access without a high publishing fee. [I haven't added this journal to the counting here.]

7. Chinese Medical Journal

It was next sent to *Chinese Medical Journal* on 2nd March 2013 (CMJ20130601). The editor replied on 25th March, stating as follow:

> We provide a list of the most common reasons why we reject your article instead of a detailed description of comments about the article from our reviewers.
>
> **First reviewer's comments:**
>
> To the Author
>
> In this long review article, the author described in

> detail the potential toxic effects of non gamma-2 dental amalgam, a most commonly used dental filling material used in the last decades world wide. The consequences including autism, adult disability, and 'workshy' seem astonishing, but dental amalgam is seldom used in dentistry nowadays, and the data the author cited was mostly from online with lower grade and published years ago.
>
> Conclusion: Reject
>
> **Second reviewer's comments:**
>
> 汞合金作为补牙材料已经完全被树脂材料所替代，尽管汞合金作为重金属可能对中枢神经系统造成影响，但作者应用综述的方式来作出相关的论述已有较多文献发表，而本文并无相关的实证数据，故论文缺乏现实性、科学性。
> 不建议在本刊发表。
>
> Conclusion: Reject

My rough translation of the above: "Amalgam as a dental filling material has been completely replaced by resin material, the amalgam's heavy metals may affect the central nervous system. But reviews of health effects of amalgam have already been widely published in the literature. In this article there is no empirical data, so the paper is not realistic or scientific."

On 31st March I sent a reply which included the following replies to quoted points.

> >"The consequences including autism, adult disability, and 'workshy' seem astonishing,"

But huge increases in these outcomes are evidenced in reality as shown in various references cited in the article (and shown in the graphs). And they are not so astonishing given that mercury is well-known to have various such neurotoxic effects and a huge increase of mercury was introduced with no attempt at monitoring.

> >"but dental amalgam is seldom used in dentistry nowadays,"

Maybe that is true in China (of which it is difficult for me to get information from due to my limited language capability). But certainly not elsewhere.

Indeed on the contrary, in the UK (and US and many other countries too) amalgam use is still being taught to the dental students (as entirely harmless)(I just now phoned 0121 466 5000 to obtain confirmation of this), and it is the only treatment approved for molar teeth in the UK's NHS and in the various health insurance schemes in the USA.

"Immediate phase-down of dental amalgam use in the UK unlikely":
http://www.dental-tribune.com/articles/news/europe/7333_immediate_phase-down_of_dental_amalgam_use_in_the_uk_unlikely.html

There continues to be a huge international industry of installation of new amalgams. That is the reason why there has just this year been a call from the UN for worldwide "phase-down" of amalgam usage as detailed at

http://www.prnewswire.com/news-releases/scientific-dental-academy-to-aid-un-global-phase-down-of-mercury-fillings-188091681.html

[….]

And also of crucial importance, as my review states, the causal factor is not the amount of new installation, but rather the amount already existing in people's mouths, and that is with these non-gamma-2 amalgams being extremely long-lasting.

>"and the data the author cited was mostly from online with lower grade and published years ago."

But as the article states, that data is the best that is available on the matter and none of the opponents of this article have shown any proper scientific objection to that data.

(The second reviewer wrote in Chinese which I will try to translate here to English.)

>汞合金作为补牙材料已经完全被树脂材料所替代,
>"Amalgam as a dental filling material has been completely substituted by resin material,"

Not so (outside China), as detailed above in reply to the first reviewer.

>但作者应用综述的方式来作出相关的论述已有较多文献发表,
>"But review of applications to make the exposition has been widely published in the literature,"

(I guess a correct translation here is more like:
>"But reviews of health effects of amalgam have already been widely published in the literature,")

But again, the article explains that the other reviews have

never examined any epidemiological data, and there have never before been any reviews of the change to non-gamma-2 (which is the whole point of the article).

> >而本文并无相关的实证数据，故论文缺乏现实性、科学性。
> >"this article there is no empirical data, so the papers to the lack of realistic, scientific."

(Again, I guess a more correct translation would be:

> >"In this article there is no empirical data, so the paper is not realistic or scientific.")

But again that is not true. The article presents all the empirical data that is available on the subject. Most of its content is such presentation. There are many references, almost none of them previously cited by for instance J Mutter, and which include the various graphical data.

8. Journal of Psychiatry and Neuroscience

There was no response to the above replies and so after some revision (minor improvements plus rearranging to produce a new section titled "Is mercury involved in causation of autism") it was next sent to Journal of Psychiatry and Neuroscience on 14th May 2013.

The Co-Editor in Chief, Dr Joober, replied on 27th May, saying that it was *"not suitable for publication in the journal. Because of increasing space constraints, we have to be extremely selective about the manuscripts that we ultimately publish."* And yet no faults or inadequacies or other evidence were adduced to support that assertion.

9. Iranian Biomedical Journal

Then after some adjustment between the different journal formatting requirements, it was sent to Iranian Biomedical Journal on 30th May.

The Executive Manager replied on 17th June that:

"Our referees have carefully reviewed your manuscript and suggested that this paper is more suitable for other journals than Iranian Biomedical Journal. We hope that you can publish this valuable manuscript in the above mentioned journals."

10. Acta Medica Iranica

After some further adjustment for journal formatting requirements, it was sent to Acta Medica Iranica on 22nd June 2013. On 17th September a reply stated that it "has been evaluated by referee(s) and I am sorry to inform you that we have therefore decided that this manuscript cannot be accepted for publication."

Again these two Iranian journals did not raise any actual criticisms of the paper.

11. Neurotoxicology

At this point I became aware that the publishers of *Neurotoxicology* had introduced a possibility of a waiver of the fee (for not only designated countries), which had not previously been the case. It might otherwise have been my first choice of journal before all those listed above.

After further adjustment for journal formatting requirements, it was received by Neurotoxicology on 24th September 2013.

On 12 October a reply from assigned editor Pamela Lein stated it had been declined on a basis of three reviewers' reports. And yet, just as with the previous responses documented above here, those three reports contained no reasonable basis for refusing publication. More importantly, they contained a spectacular compilation of half-baked pseudo-expert pseudo-faults, as is made clear in my rejoinders which I have put in a separate document.

12. Molecular Autism

Following a further reasonless reply from Ms Lein, and further adjustment for differing journal formatting requirements, it was sent to *Molecular Autism* on 7th November 2013 with a request for a waiver (which was granted on 8th November. The editors replied on 29th November 2013 as follows:

> Dear Mr Clarke,
>
> We very much appreciated reading your manuscript on dental amalgams as an autism risk factor. We think you have done a good job reviewing the literature and the question is of considerable interest.
>
> The advice we have received is that the methodology would not get through critical peer review from our journal,

> so we think that it is better for you if you submit this to the other journal that has expressed an interest in this. This will also save you time.
>
> We wish you success with your research and thank you for considering our journal.
>
> Best wishes,
>
> Profs Joseph Buxbaum and Simon Baron-Cohen.

The references in that reply to "the other journal" mis-characterise the general-readership magazine I had mentioned (What Doctors Don't Tell You) as a (perfectly reasonable alternative) "journal" when that would incorrectly suggest it functions as a primary science journal publishing scientific papers (indexed in for instance Index Copernicus, DOAJ, or PubMed), which is of course far from the case – WDDTY has to date only published popular journalism articles reviewing or commenting on the primary journals' papers. That reply furthermore notably fails to specify any actual *reasons* why my paper would "not get through the critical peer review". And that is in the context of all those previous "peer review" critiques being shown to be vacuous as above.

Indeed it is those journals' own "peer reviews" that glaringly fail any honest test of "critical" examination, not my own work.

And this reply appears to be outrightly deceitful, because it is the editors themselves who decide whether or not it does indeed "get through critical peer review from our journal". (That's exactly what being a journal editor is about.) This reply is thus very much like an executioner saying "I really wish you the best in your hopes of staying alive, but I'm terribly sorry that my arm isn't pulling hard enough to prevent this axe falling on your neck. Anyway, I wish you survive in future executions". Note that Chapter 12 here contains further discussion of whether Dr Baron-Cohen tells the truth or not.

Before [, in the event, not] finally sending to WDDTY I decided to first send to just two more journals, namely the *Russian Open Medical Journal*, and the new journal *eLife*.

13. Russian Open Medical Journal.

After further re-formatting it was sent to ROMJ on 9th December 2013. The editor sent an email reply on 19th March 2014, of which the here-relevant content was as follows:

> "….We received several conflicting reviews on your article. Editorial board members carefully studied and reviewed the text of article. The views of members were different. In sum of debates, all members concluded unanimously that style of presented article not suited to the format and scope of our journal and our readers. ….
>
> We recommend you submit your article to another journal (mass media), specializing in the acute medico-social problems and not having narrowly specialized readership…."

So yet again, no actual faults of the paper were identified. As for the notion that there might be some unsuitability of style, this is evidently not a real problem given that no such comments on style were made by any of the other journals, including the abundant false criticisms from Neurotoxicology journal.

14. eLife

I then noticed there was the journal eLife recently founded by the 2003 Nobel laureate, which claims to have a novel approach to publishing and does not cause much delay in its decisionmaking anyway. So I sent it to eLife on 24th March 2014.

The next day the editors of eLife sent the following reply.

> Dear Dr. Clarke,
>
> Thank you for choosing to send your work entitled "Autism, adult disability, and 'workshy': Major epidemics being caused by non-gamma-2 dental amalgams" for consideration at eLife. Your initial submission has been assessed by Prabhat Jha in consultation with a member of the Board of Reviewing Editors. Although the work is of interest, we are not convinced that the findings presented have the potential significance that we require for publication in eLife.
>
> Specifically, the theories about possible sources of reported increases in autism need much better justification than provided here, and also need to be reviewed in the context of other putative risk factors. As such, this paper

> might be more suitable for a specialized journal.
>
> eLife rejects a high proportion of articles without passing them on for in-depth peer review, so that they can be promptly submitted elsewhere. This is not meant as a criticism of the quality of the data or the rigor of the science, but merely reflects our desire to publish only the most influential research. We wish you good luck with your work and we hope you will consider eLife for future submissions.
>
> Best wishes,
> Randy Schekman, Editor-in-Chief, eLife
> Fiona Watt, Deputy Editor, eLife
> Detlef Weigel, Deputy Editor, eLife

In considering that reply from eLife, it should be borne in mind that I had sent them the replies from the previous thirteen journals, including the failure of any of them to find any genuine fault. In that context, eLife put forward two objections.

Firstly the notion that the theories "also need to be reviewed in the context of other putative risk factors". And yet in the paper I had already pointed out that any proper explanation of the autism increase had to account for the now substantial involvement of mercury. And having ruled out mercury from vaccines, that leaves only dental amalgam as the one remaining source of that mercury. In respect of the adult disabilities, one could of course speculate about a great many potential causes which have increased in recent decades, and yet we see here (a) a major increase of mercury, clearly resulting in the autism increase; (b) adult disabilities which are very much characteristic of mercury vapor poisoning; (c) a peculiarly close coincidence of timing of that adult disability increase with the autism increase; and (d) that peculiar system of official falsehoods about the subject. I consider that to be an adequate review of the other putative risk factors. The only alternative would appear to be an endless list of speculations about the many other things that have changed over the decades, and might be supposed to have somehow caused all those disability claims.

The other objection from eLife consists basically of the "skepticism" which I have commented on already. Some of the greatest discoveries in science were dismissed for decades with such "skepticism", so I do not regard it as a meritable objection here.

15. Biometals

Having at this point now given fourteen putatively scientific journals the opportunity to publish this paper, and met only with false objections, I did not see much merit in allowing any further journals to obstruct the publication any further. But before finally turning to other options or none, I noted that the journal Biometals had recently published two papers relating to amalgams, so decided to give them the final chance. After further changing to journal formatting requirements I also added references to Taylor 2013 and Homme 2014 and carefully revised the presentation of the section on mercury causing autism and of the conclusions section. On 26th April I received a reply which contained no other grounds for refusing the publication other than the following:

> Although this is an interesting topic, the manuscript is restricted to statistical data containing no experimental results. Therefore, this manuscript is not suitable material for the journal BioMetals which has an audience of experimentally working scientists. We therefore suggest to submit this manuscript to a journal on environmental health or to a journal with a focus on toxicology which may have the interested readership.

This is of course in the context that they knew I had already sent it to such other journals, and again, no good reason for non-publication was given. Perhaps you can work out for yourself whether they were being honest there.

16. Toxicology Reports

At that time I received notification of the new journal Toxicology Reports which had obvious appropriateness to this paper. So after further formatting adjustment I sent it on 4th May 2014. The editor Dr Lash sent a reply on the 7th May, the essential content of which was as follows:

> "Most of the discussion of published findings provided a conclusion without showing the data that support the conclusion. The final sentence of the Abstract to me illustrates the lack of balance in the presentation. The statement on page 3 that the evidence to support a causative role for mercury in autism is "beyond a reasonable doubt" provides another example of the lack

> of balance in the presentation. Accordingly, I am afraid that I must agree with the previous reviews and reject your manuscript for publication in Toxicology Reports."

But yet again, it can be shown that there is nothing there that justifies a refusal to publish this scandalous important precautionary information.

>"Most of the discussion of published findings provided a conclusion without showing the data that support the conclusion."

We see here yet again the familiar objection to many great discoveries, along the lines of "I can't (or prefer not to) see your credible evidential case, therefore it doesn't exist". But numerous other readers have had no difficulty seeing that case. One such stated that "Your paper is important", and "Your work is fine". And when one group of people claim *not* to see something that another group claim they *do* see, the "non-see-ers" have to have some very special grounds to be justified in prevailing in suppressing the evidence which the "see-ers" endorse. And they don't.

>"The final sentence of the Abstract to me illustrates the lack of balance in the presentation. The statement on page 3 that the evidence to support a causative role for mercury in autism is "beyond a reasonable doubt" provides another example of the lack of balance in the presentation."

But both of those statements are firmly grounded in facts presented in the paper. Where is the evidential case that those statements are wrong? There is no such.

Of course certain interest-conflicted readers would prefer those conclusions to not be true but that is not a proper basis for how scientific papers are selected or not.

>"Accordingly, I am afraid that I must agree with the previous reviews and reject your manuscript for publication in Toxicology Reports."

But he was not "agreeing with the previous reviews", because they had came up with entirely different sets of cheap excuses for rationalising the same predetermined decision that they didn't want to put their names to publishing this embarrassing information.

xx. Social Science and Medicine

I decided to next send to Social Science and Medicine in view of there being as much implications about the social context as about the disabilities. The editors replied on 20th June including the following comments:

> "At Social Science & Medicine we have to prioritise papers which contribute substantially to one of the major health social sciences and are of particular interest to a wide international readership. I am therefore not forwarding your manuscript for review, as we feel it has limited social science content. [....] This is not a reflection of the quality of your paper, but rather concerns the topic and likely audience."

In view that that is not clearly a false excuse I will not count this journal in the numbering here, hence the "xx" above.

17. Medical Hypotheses

After some reformatting for different journal requirements, and reading of other documents before deciding not to change the text anyway, I sent it to Medical Hypotheses (a journal with controversial recent history of publisher interference).

On 7th October 2014, the editor Dr Manku replied as follows.

> Dear Mr Clarke,
>
> Reviewers's comments on your work have now been received. You will see that they are advising against publication of your work. Therefore I must reject it.
>
> I admire the author's efforts, however, I need to mention these points:
>
> The whole text seems like a newspaper article in terms of writing and I doubt whether this is format of your journal or not. For eg. note the statement on page 7 line 38 "cherry-picked selected data" instead of randomly chosen
>
> Page 3 does not include the "introduction/background" title. In fact the whole manuscript does not follow the structure mentioned in "author guides"
>
> Page 4 line 44--> abbreviations such as NHS and DH should be fully introduced at their first appearance
>
> Page 5 line 30--> as the item above for GPs
>
> Page 6 line 21--> I personally don't admire the statement

"The famous US dentist Hal Huggins states that ....." which seems more like a TV/Radio report than a scientific citation

Page 6 line 36--> this sentence "Dispersalloy is the most widely used amalgam with over 25 years of proven performance, i.e., since before 1979, but perhaps after their 1974 patent no. 3841860" seems like a commercial copy from the manufacturer which is not scientific

Page 7 lines 2, 13, 23, etc--> using pronouns such as I, My, We, etc is not appropriate

Thank you for your submission, I am sorry to inform you that it has been rejected.

Thank you for giving us the opportunity to consider your work.

Yours sincerely

Dr. M. Manku PhD

Yet again, there is from Dr Mankku zero indication of any actual fault of the science, or even of the content other than some new notion that it "seems like a newspaper article in terms of writing", an observation which curiously was not made by any of the previous readers indicated above. Basically just yet more shallow excuses.

18. Life

After further reformatting for different journal requirements, and delay due to continuing to have to be my own medical consultant and practitioner in absence of a half-decent healthcare system here, I sent it to the relatively new journal *Life*, on 30th November 2014. On the 2nd of December 2014 the Assistant Editor replied with an email indicating that:

"Your manuscript was not given a high priority rating during the initial screening process. ..... Therefore, our decision is not necessarily a reflection of the quality of your research but rather of our stringent resource limitations."

To which I replied:

"Thanks for your prompt reply.

I appreciate that all your other papers relate to origins of life etc rather than medical matters."

The Assistant Editor then forwarded a substantial text of the external editor's comments, as follows (again indicated in bold with my replies non-bold).

"The paper is a review and hypothesis paper (the claim in the abstract that "This is the first-ever study of health consequences of non-gamma-2." is rather misleading) stating the argument that mercury from a specific type of dental filler is the primary cause of autism. The argument is basically that diagnoses of autism have increased, it is alleged that nongamma- 2 mercury amalgam use has increased on a timescale matching the rise in autism diagnoses, and because mercury is neurotoxic the two are therefore linked.

I will chop this up for replies as follows.

(the claim in the abstract that "This is the first-ever study of health consequences of non-gamma-2." is rather misleading)

I had already fully answered that point at the start of my earlier replies to Neurotoxicology. This reviewer makes no advance on my replies here.

"The paper is a review and hypothesis paper (the claim in the abstract that "This is the first-ever study of health consequences of non-gamma-2." is rather misleading) **stating the argument that mercury from a specific type of dental filler is the primary cause of autism.**

The reviewer here misrepresents the essence of the paper, which makes clear even in its title that it is about a lot more disability than just autism.

The argument is basically that diagnoses of autism have increased, it is alleged that nongamma- 2 mercury amalgam use has increased on a timescale matching the rise in autism diagnoses, and because mercury is neurotoxic the two are therefore linked.

The reviewer here grossly misrepresents the case presented. The timing of the autism diagnoses increase is one part of the evidence, but only one part.

The argument is basically flawed in two ways. Firstly, as the author says himself (page 13) that he has no numbers for the use of amalgam in any territory. So the data presented seeks to correlate changes in incidence of autism with the *introduction* of a specific type of mercury amalgam. It is assumed that the amalgam use increased steadily after that. It may do, but there is no evidence of this at all.

Again, this objection had already been fully dismissed in the replies to Neurotoxicology.

but there is no evidence of this [that the amalgam use increased] **at all.**

No "evidence" is needed here. It should go without saying that when a new type of dental restorative is introduced as the new standard then it is going to become more prevalent in mouths over a period of years thereafter. In recent decades it has been the now universal standard "ordinary" amalgam. All this was addressed in the previous replies to Neuropseudotoxicology.

The second flaw is that amalgam use has actually declined substantially in the last decade,

Again I fully answered this point in the Neurotox replies. Again this reviewer merely repeats rather than advancing the discussion here.

especially in Europe as US-style concerns for dental cosmetics mean that patients are no longer willing to have metallic lumps in their teeth. To an extent this has also been driven by consumer concern (whether justified or not) over the health effects of mercury. Thus since the 1990s mercury amalgam use has declined substantially in the UK, even to the point of drilling out old amalgam fillings and replacing them with newer material, largely for cosmetic reasons. This can be readily verified by the obvious lack of metal in the mouths of most young people.

But amalgams are only used in the pre/molar teeth where they are not "obvious".

However autism incidence rates have not come down, even in the under- 10s. In Sweden they have been phased out almost entirely in the time 2000 - 2005 (see http://www.kemi.se/Documents/Publikationer/Trycksaker/PM/PM9_05.pdf) - has the incidence of autism gone down?

Good question. An equally pertinent question is whether (or how much) there has been a decline of the prevalence of non-gamma-2s already in the mouths of the parents of those children (as it is the parents' amalgams that cause the autism). Readily accessible autism incidence data for recent years is a bit patchy (and subject to recency bias) but such as I have seen so far suggests that incidences have generally leveled off at a high level (with prevalence consequently increasing as a lot more autistics are added while only

few are dropped out by end of life).

Dentistry today http://www.dentistrytoday.com/ has about 40 articles on materials for fillings, listing hundreds of composites that are preferred over amalgam, again mostly for aesthetic reasons but also functional ones. If this reflects dental practice, autism should also have plummeted.

Firstly, in the UK the NHS provides only amalgams for molars and premolars, and consequently they are still widely used. Similar applies in the US. Of course the fancy pricey new things get a lot more attention, so what?

Secondly, what causes the autism is not installation of the amalgams, but their presence in mouths. Many millions of the things are still in the mouths of the parents, let alone children still having them put in by the NHS and by equivalent organisations in other countries. So it is wrong to predict that "autism should also have plummeted". Though my expectation is that we are going to see such plummeting within the next decade or so, at least in Sweden.

The author makes the equivalent argument very forcefully in the supplementary material with regard to mercury in vaccines as *not* being relevant to autism - autism has continued to rise despite the decline in thimerosal in vaccines. Yes, good argument. So why does the same not apply to autism in Sweden with regard to amalgam fillings?

Because as explained above.

Without some actual measure of amalgam use, therefore, this is a post hoc ergo propter hoc argument that is not convincing, and is just a specific example of the 'amalgam causes autism' argument that has been done to death in the literature and the blogosphere.

Far from it, this is the first ever study of these epidemiological questions, the first ever study of health consequences of non-gamma-2, and the first ever presentation of that evidence. And the comment above also fails to give any recognition to my **confirmed prediction** that the earlier amalgams would have caused an increase of their own, as very starkly confirmed in the Update section and Figure 7. Again this is all entirely new evidence on the subject.

The cheap stereotyping of being "just another" "amalgam causes autism" argument is also noted there.

The paper is also seriously flawed in its presentation of the case, and should be rejected for a complete rewrite even if the basic argument was sound.

Curiously this alleged fault was not remarked upon by numerous previous reviewers. And Dr med. Mutter (author of a notable review) on the contrary commented in emails that it was "important and should be only a little bit corrected" (29 March 2013), and "Your work is fine" (8 November 2013). I am reminded of the comment I made in a letter to Nature (*Nature386;319 (1997))* that: "The paper involved was described by a reviewer for *Personality and Individual Differences* as well-written, well-argued, and well-documented, whereas a *British Journal of Psychiatry* reviewer reckoned it was of lowest grade in all three respects". In light of this, plus this reviewer's evident difficulty in distinguishing sense from nonsense, I find more credibility in the words of Prof HJ Eysenck (most cited-ever author): "Well-written", and Prof D Horrobin: "You obviously write well".

In the introductory section the author makes a number of highly charged statements, (page 2, lines 21-27) without a single reference or attribution.

Here are the first three of these "highly charged" statements:

> "[1] No safety testing was undertaken before or after it was introduced. [2] Patients and the public in general have still not been informed of the change, let alone of the increased levels of mercury involved. [3] No informed consent has been sought, and no warnings have been given of any possible harmfulness."

One has to wonder quite what sorts of references I should be expected to put there. I have now for a decade been challenging so-called experts to provide evidence of safety of amalgams, and am well-aware of what a vacuum of evidence there is for amalgams in totality, let alone in respect of non-gamma-2. Rather obviously I can't cite studies which have never existed. Again, in respect of informing and consent and lack of warnings it is starkly obvious to a UK resident that rather than being informed they are still being disinformed about the change to non-gamma-2. And ditto in the US. Again, quite how do I cite such non-events? The onus has to be on others such as this so-called reviewer to point out evidence that these things did indeed happen. But they never have.

And a better characterisation of those statements is not "highly charged" but rather "stupendously criminally outrageous in their implications".

The author then goes on to list 'untruths', listing a tinyurl address as evidence. Looking through the relevant URL, it seems to be mostly assertions by the author and not responses from the target of his sometimes quite aggressive questions. If I received a list of questions starting with an accusation that I was an unethical, lying weasel who was suppressing data, I would also tend not to try to be helpful.

What a load of shameful rubbish. None of the questions started off with any "accusation of being unethical lying weasels or suppressing data", and anyway they were all addressed to organisations rather than individuals. And even if it were true that any of the questions had been "aggressive", that would still be irrelevant, as many FoI questions are far from what the receivers wish to be reading. More to the point those were all extremely pertinent and important questions, to which answers rightly demanded to be given. And easily could have been given if the recipients were indeed honest non-charlatans. The only reason why answers were not forthcoming is the very simple one that those questions exposed extremely criminal charlatanism which could not find any honest answers with which to defend its untruths. (Note I did not call them "lies", though there's a strong bet that's what they should be called.)

Furthermore the reviewer's contention is undermined by the pseudo-responses themselves which do not cite the wording of the questions as reasons for refusing to answer, but instead find other shallow rationales or none.

If this reviewer had the slightest bit of impartiality he or she should at that point be remarking about the outrageousness of those humungous non-answers to massively important questions rather than drivelling about the supposed manner of asking.

The review of mercury an autism is very polemically stated,

Translation: This reviewer is very strongly biased against the conclusions reached and wouldn't recognise a neutral exposition even if it was clearly printed out and highlighted in front of them for ten hours.

but is an OK review of the case for mercury being causal in triggering autism. The statistical argument (page 4, lines 24-26) is not valid, as it assumes independence of data sets, sources of error and bias.

Again, this objection had already been thoroughly discussed and debunked in the reply to Neurotoxicology. This reviewer does

nothing to advance from that here.

The statistics of positive *and negative* studies should probably be combined using a Baysian approach if the author wants to do this.

Again, nonsense which was fully discussed in the Neurotox. There were no negative studies.

The statistical nature of this argument also invalidates the logic of the author's next statement that a negative finding cannot invalidate a positive one - epidemiological statistics arguments are not existence proofs, they are evidence proofs, and failure to find evidence *if it is looked for rigorously* is evidence for absence.

Again fully discussed in the Neurotox, with reference to "never having seen the Queen".

The issue is that the author believes that the evidence against mercury is not well done. The case against mercury is not really stated (simply dismissed), and

On the contrary, I debunked all three studies purported to make a case against mercury involvement in autism. And I don't see any coherent grounds for refusing publication there.

there is no discussion of the (many) other ideas about the causality of autism - the author should at least acknowledge that there are other, well argued cases.

On the contrary, I cited my published theory paper which explained that there are many factors in autism causation. And pointed out that because mercury was shown to be a major factor in modern autism, then that ruled out just about everything else as a potential main cause of the increase, so we were left with looking for the source of that mercury. Either vaccines (which I also debunked in an appendix) or amalgams.

The author dismisses reference 38 because they did not prove that the measure of autism in adults was not comparable to that in children. But that was not the point. If incidence goes up in adults and in children in parallel, then some change common to adults and children is most likely to be the cause of the increase. If the numbers are also similar, that suggests (but does not prove) that the measures *are* comparable, but as the author is claiming that the *increase* in autism is evidence for the role of amalgam, then

comparison of rates of increase is valid and relevant.

Again I fail to see any coherent objection to my critique of the Brugha study at this point. In the absence of a means of determining the equivalence of the measuring for the two different age groups, no meaningful data could be derived from the study, and hence it could only be a load of wishwash.

The discussion of UK disability benefits claims is naive in the extreme.

As we will see....?

DB claims are massively changed by changes in policy about what 'disability' means, what specific schemes are available, what the thresholds of duration and severity are, and (critically) what other benefits are available instead of DB.

Any evidence on this point? In the absence of such evidence it is reasonable to guess that no such things have been going on. In fact I'm well aware as a UK resident throughout all that time that there were no such changes of specific schemes or of thresholds, until more recently as indicated by the quote of "caseload growth now controlled". And there were no relevant other benefits available either. More remarkably this reviewer does a great job of ignoring how this curve of the disability claims is a remarkable exponential that remarkably "just happens" to so closely coincide with the autism increase which also "just happens" to begin tellingly just after the change to non-gamma-2. This reviewer would have us believe that some (non-existent anyway) procedural changes "just happened" to produce that increase just such as to "just happen" to have those abovementioned characteristics.

One could equally correlate the number of people on higher band tax with mercury amalgam use, and claim that mercury makes you wealthy.

But I didn't because I was testing a boring rational strongly-suspectable hypothesis rather than an igNobel-prize-winning stupendous discovery silly one.

The political statements that the author quotes (with evident disapproval) illustrate that this is a political posture, not a scientifically testable statistic.

On the contrary, the political statements reflect the strong *opposition* to these increases rather than anything that could be politically causing them.

The arguments about whether British workers are 'workshy' compared to foreign ones is also a) a political posture, not a referencable fact and b) not relevant anyway as by definition foreign workers who come here to work must be fit enough to work.

Many employers have stated that they find UK native workers to be too workshy in contrast to the immigrants. I doubt if this can be adequately explained away in terms of the definitional concept given that the same criteria of "fit enough to work" apply to both sources anyway. And no answer has been made to the point about the ancestors of this nation of workshy somehow having built all those medieval cathedrals in a harsh rainy land and then gone on to create the largest empire in history.

Insomnia, fatigue, memory loss and consequent depression are all plausible results of increased consumption of processed food, increased TV watching disrupting sleep patterns (40+ free channels 24 hours/day) or any one of dozens of other changes over the last 30 years.

Sorry but I don't think this comment warrants any reply. Where are all the studies you have published about these "plausible" explanations of those disability statistics? Where are your answers to the questions on the last page of the text? My own review goes far beyond mere "plausible", it goes to confirming of a massive theoretical prediction (in Figure 7), which is the very essence of hard, competent science rather than "plausible" speculation.

I have no idea on what evidence the author says that fibromyalgia is 'often cured' by amalgam removal, given that a) there are no consistent diagnostic criteria for fibromyalgia and b) amalgam removal is stated to release more mercury into the patient than just leaving it there. There is no reference for this statement.

My own suspicion is that a number of vaguish labels, including "fibromyalgia", "MS", "CFS", and "ME", are all actually just unknowing clinical perceptions of amalgam mercury poisoning which has not been recognised as such (in the context of NHS denialism). Anyway, Andrew Hall Cutler's book mentions fibromyalgia ten times in its index, and here's a ref for some studies which have been done. http://www.fms-sas.co.uk/fmsmercury.html

"The foundation for Toxic-Free Dentistry has compiled statistics from 6 studies on a total of 1,569 patients. The patients reported on their symptoms before and after their mercury

amalgams were removed. These included everything from vision problems to depression. Most saw dramatic improvement once the fillings were gone. There are many people who have recovered from chronic illnesses after having their fillings removed; for example, recoveries realized from diseases such as fibromyalgia and CFS. (762 patients used FTFD Patient Adverse Reaction Report to send changes in their health directly to the FDA and FTFD. Dr. Mats Hanson reported on 519 Swedish patients. Henrik Lichtenberg, S.D.S of Denmark reported on 100 patients. Pierre LaRose, D.D.S. of Canada reported on 80 patients; Robert L. Siblerud, D.D., M.S. reported on 86 patients in Colorado.)"

and b) amalgam removal is stated to release more mercury into the patient than just leaving it there.

Yes if done by NHS incompetents. No if done by using competent protocols specifying high suction and separate air supply among other things, in which case a spike of intake is prevented.

Stock was poisoned by *huge* amounts of mercury. The case of acute, massive, severe mercury poisoning is scarcely relevant.

As a person who "just happened" to become chronically severely disabled myself, when I encountered Stock's account it was just like (after so many years) I was reading my own autobiography written by someone else. On what basis was Stock's intake so much more "huge" than that of someone with grams of mercury constantly stored in their mouth for years? Even if you wash your hands in the stuff and drink it it doesn't amount to that much more intake because it enters very inefficiently by those routes compared to breathing and implanting. So Stock's account, collaborated by my own and by Cutler's comments, is eminently relevant there.

The cover letter is an ill-judged and innaccurate rant,

Yes, I can see you'd have some expertise about that.

but I can sympathise with the author's frustration. The list of comments on other rejections shows that the author really does not understand the difference between research and review, and is completely unwilling to take guidance as to how to get his ideas taken seriously.

Which could be why various notable people (Eysenck, Rimland, Horrobin, among others) have so greatly enthused about my ideas.

These documents are not part of the paper and so are not reasons for rejecting the paper, but they do suggest that more detailed comments that I have provided here would be a complete waste of time, and would only result in the author ranting about us to someone else.

Well, that's one prediction this reviewer has got right.

It is a shame that the author has chosen to write what is a polemic instead of a paper.

On the contrary, it is a shame that the reviewer has chosen to write what is a polemic instead of a review.

I think that he may have a good point. Amalgams do emit mercury, mercury is not good for you, dental mercury can be converted to methyl mercury by oral fauna (this has been published), and methyl and dimethyl mercury is *severely* toxic. Phasing them out seems like a good idea. But with no new evidence,

On the contrary, major new evidence is presented in the paper.

highly biased arguments, flawed logic and statistics,

As demonstrated above? Or rather de-debunked above.

and text full of comments accusing opponents of bias, data suppression and radically unethical conduct without any evidence, this is a really poor way to make that point."

On the contrary I do show the evidence, such as it is, for those who have eyes sufficiently unblinkered to see it. My text doesn't make "accusations" so much as state facts which speak for themselves of the bias and suppression and unethicality. And no evidence has been raised in rebuttal.

Preprint servers to the non-rescue

Not being known for my patience or persistence, I thereafter sent the thing to some "preprint servers", which claim to publish scientific papers without first subjecting them to a "peer review" process. Sure, I did have some, ooh, ~slight~ scepticism about how free from "peer review" rejectionism these sites would prove to be.

One option was the F1000 website. But for this article longer than 15,000 words their fee would be at least $2000, and they could well decline it even if I robbed enough banks first.

So I sent it firstly to PeerJ Preprints on 23rd February 2016. I uploaded in both their preferred format (double-spaced single-column with line numbers) and a format with double columns as is typical of most published papers. Three days later, not having had any reply notification, I checked their website which said:

"This manuscript has been rejected as unsuitable for publication. I apologize that we cannot consider your submission. You may find that it is better suited to submission at bioRxiv (http://biorxiv.org/) or F1000 research (http://f1000research.com/)."

Having already ruled out the F1000 option I then sent it to bioRxiv (after adding "?" to the title) and got this reply:

MS ID#: BIORXIV/2016/041517
MS TITLE: Autism, adult disability, and 'workshy':
Major epidemics being caused by non-gamma-2 dental amalgams?

Dear Robin P Clarke;
We regret to inform you that your manuscript is inappropriate for bioRxiv as it is not a research paper being prepared for submission to a journal.
Thank you for your interest in using the bioRxiv service.
The bioRxiv team

And yet this rationale for rejection reads oddly in the context that their website states that:

"authors are able to make their findings immediately available to the scientific community and receive feedback on draft manuscripts before they are submitted to journals." And their Submission Guide states that:

"An article may be deposited in bioRxiv in draft or final form, provided that it concerns a relevant scientific field, the content is unpublished at the time of submission, and all its authors have consented to its deposition."

"All articles uploaded to bioRxiv undergo a basic screening process for offensive and/or non-scientific content. Articles are **not** peer-reviewed before being posted online."

And need I remind you that on sending the same content to numerous "peer-reviewed" journals, they had critiqued it as though it was indeed a *"research paper being prepared for submission to a journal"*, rather than declared that it was not. So who's telling the truth here?

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