NZ Community Water
Fluoridation is Safe and Effective

A critique of Fluoride Free NZ's criticisms of the NZ Fluoridation Review

Dr Ken Perrott
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Summary

*Health Effects of Water Fluoridation: a Review of the Scientific Evidence* is an authoritative and up-to-date review on water fluoridation in New Zealand. The anti-fluoridation activist organisation Fluoride Free NZ (FFNZ) attempted to discredit the review with their report *Scientific and Critical Analysis of the 2014 New Zealand Fluoridation Report*.

Here I analyse the FFNZ critique and show it was written, and "peer-reviewed" by well know anti-fluoridation activists.

This report analyses the three main articles in the FFNZ document showing that the critique is based on misinformation commonly promoted by anti-fluoride propagandists.

I point out a small mistake in the executive summary of the Fluoridation review and describe how it arose. The concept was explained correctly in the body of that report. The mistake, little more than a typo, has now been corrected.
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Introduction

In August 2014 the Royal Society of NZ, together with the Office of the Prime Minister’s Chief Science Advisor, published a review on fluoridation - *Health Effects of Water Fluoridation: a Review of the Scientific Evidence*. It was commissioned by Sir Peter Gluckman, the New Zealand Prime Minister’s Chief Science Advisor, and Sir David Skegg, President of the Royal Society of New Zealand - at the request of Auckland City on behalf of several local Councils.

Activist anti-fluoride groups have pressured New Zealand local bodies to prevent, or stop, community water fluoridation. The Fluoridation Review provides councils with an expert summary of the current knowledge on the benefits and possible harms from CWF to counter the misinformation they had been subjected to from the activist groups.

The anti-fluoride activist organisation Fluoride Free NZ (FFNZ) attacked this report in press releases and on internet sites. This was part of a coordinated attack on the review organised by Paul Connett's Fluoride Action Network. In November 2014, FFNZ produced a report critiquing the NZ Fluoridation Review - *International Peer Review of the Royal Society/PM Science Advisor Office Fluoridation Review*. They described their report as "International" and "peer-reviewed" clearly hoping to provide an air of authority to it. However, a simple analysis shows the articles in the report were written by, and "peer-reviewed" by, well-known anti-fluoride activists. These articles misrepresent the NZ Fluoridation Review in many ways and also contained the usual misinformation we have come to expect from such activists.

We should understand that the size and accessibility of the Review is aimed at informing public decision-making on the issue. For this reason it also deals with New Zealand aspects. It is not meant to be as extensive and detailed as the 530 page US National Research Council report, for example.

Hopefully any future consideration of community water fluoridation by local body councils will be better informed because of the Royal Society review. In particular it should help counter the sort of misinformation that has confused some councils in the past.
This current report is based on a series of blog articles in Open Parachute where I analysed each article in the FFNZ report in detail. Wherever possible I have contacted the authors of each section of the Fluoride Free report I commented on offering them a right of reply. So far none of them have taken up my offer.

Readers can access the relevant document on-line, or download them, using the following links.

**Review by Royal Society of NZ and the Office of the Prime Minister’s Chief Science Advisor:**


**Critique by Fluoride Free NZ organisation:**


**Open Parachute articles:**


*Did the Royal Society get it wrong about fluoridation?* - https://openparachute.wordpress.com/2014/12/14/did-the-royal-society-get-it-wrong-about-fluoridation/
The farce of a “sciency” anti-fluoride report

I came up with the image above after a quick glance at a “report” promoted by the local Fluoride Free groups and Paul Connett’s Fluoride Alert organisation. (Scientific and Critical Analysis of the 2014 New Zealand Fluoridation Report). It illustrates the incestuous network of authors and “peer reviewers” involved in the “report.” I have also illustrated connections of these people to a number of anti-fluoride organisations and 2 publications.

The first column lists the authors in red, and their claimed peer reviewers in green. The third column lists the anti-fluoride organisations and several publications these people are connected to.

The middle column lists some other people who are also connected to these organisations and publications.

Let's just consider the connections between these authors, “peer reviewers” and anti-fluoride organisations.

Taking in each other’s laundry

Most of these names are familiar to anyone who has followed the anti-fluoride movement. That fact in itself shows how this report can in no way be seen as “expert,” “independent” or at all credible. Here are some details on the illustrated people, organisations and publications.

NRC Review minority: There were several disagreements on the 12 member panel which produced the 2006 NRC report “Fluoride in drinking water. A scientific review of EPA’s
standards” because 3 members were anti-fluoride. They were Robert Issacson, Hardy Limeback and Kathleen Theissen.

**Hardy Limeback** is involved in several anti-fluoride activist groups.

**Kathleen Theissen** appears not to be organisationally involved but regularly makes anti-fluoridation submissions when the issue is debated. Some idea of her lack of credibility as a "peer-reviewer" and author of this report is displayed in this interview by the freelance reporter Douglas Main. The article was about Hirzy's paper referred to below. The one for which he had to publish a retraction because of the simple arithmetic error - a 760-fold miscalculation.

From the article:

“Experts not involved with Hirzy's study agreed with its findings.”

“I think this is a reasonable study, and that they haven't inflated anything,” said Kathleen Thiessen, a senior scientist at SENES Oak Ridge Inc., a health and environmental risk assessment company.”

Seems Thiessen either didn’t bother to read Hirzy’s study prior to commenting on it, or she overlooked his glaring error, too.”

**Fluoride/ISFR:** The **International Society for Fluoride Research (ISFR)** publishes the journal **Fluoride** and organises regular conferences. They provide an avenue for authors to publish anti-fluoride articles, and generally poor quality research from areas where endemic fluorosis is common which may not be acceptable in the normal scientific journal.

The Society is based in New Zealand and is registered here as a charity. **Bruce Spittle** (one of the "peer-reviewers") is the treasurer and journal managing editor.

**FTRC/Second look:** The anti-fluoride organisation and web site Second Look as set up the Fluoride Toxicity Research Collaborative (FTRC). It appears to be a weak attempt to provide a front “scientific institute” for anti-fluoride activists who want to present themselves as scientific experts.

This reminds me of the creationist Biologic Institute set up by the intelligent design creationists at the Discovery Institute. Actually, the Intelligent Design “pretend” scientific journal **Bio-complexity** also reminds me of the anti-fluoride journal **Fluoride**.

The FTRC lists the following staff:

- Russell Blaylock, M.D., FTRC Medical Director
- Hardy Limeback, Ph.D., D.D.S, FTRC Principle Investigator
- Phyllis J. Mullenix, PhD., FTRC Research Program Director
- Aliss Terpstra, RNCP, FTRC Research Coordinator
So far they claim to have sponsored (financed?) 2 research papers only by Phyllis Mullinex. Have a read of them and make up your own mind about their quality.

**Case Against Fluoride:** This is Paul Connett’s book *The Case against Fluoride: How Hazardous Waste Ended Up in Our Drinking Water and the Bad Science and Powerful Politics That Keep It There*. It is usually treated as a holy scripture in the anti-fluoridation movement. His co-authors were H. S. Micklen (another of the reports authors) and James Beck.

Connett is very proud of this book and relies on it to support his claim to be a “world expert” on fluoridation.

**Fluorine in Medicine:** This is the sole scientific paper related to fluoride that Paul Connett can claim authorship (actually co-authorship) to:


The senior author Anna Strunecká is also part of the anti-fluoride network illustrated above.

I am personally very suspicious of the quality of the journal which published this paper – anti-fluoride people have a history of placing poor quality papers in suspect journals purely to attain some sort of scientific credibility.

**FIND:** The *Fluoride Information Network for Dentists* is one of the local Fluoride Free’s astroturf organisations claiming about 8 members but only Stan Litras is active. Stan uses his FIND hat for his anti-fluoride press releases – such as the one promoting the “report” considered here.

**NZ Tour of Don Quixote & Sancho Panza:** Sorry, can’t help thinking of these two when the upcoming NZ tour of Paul Connett and Bill Hirzy is mentioned. They do seem to be charging local fluoridation windmills with meetings in Taupo and Thames.

**William Hirzy:** He is Paul Connett’s wingman on the Don Quixote & Sancho Panza Tour. Unlike Paul’s sole co-authorship he actually has 2 published scientific papers related to fluoridation where he appears as senior author. (See “Comparison of hydrofluorosilicic acid and pharmaceutical sodium fluoride as fluoridating agents—A cost–benefit analysis” and “Corrigendum to “Comparison of hydrofluorosilicic acid and pharmaceutical sodium fluoride as fluoridating agents—A cost–benefit analysis” [Environ. Sci. Policy 29 (2013) 81–86]“)

The “credibility” of his “expertise” on the subject is shown by the fact his second paper was necessary to correct the huge arithmetic mistake he made in the first paper!
Perhaps you can see why the Connett/Hirzy act brings Don Quixote and Sancho Panza to my mind.

**Conclusion**

The “report” is discredited even before addressing the arguments presented – simply because of the well-known anti-fluoride stance of all the authors and “peer-reviewers.” The diagrammatic network shows just how incestuous the “report” is. It is simply an attempt to put a “sciency” face on their political stand and their attack on the NZ Fluoridation Review.

As a scientific presentation it is a farce.
Theissen and Neurath

My analysis in this chapter relates to a draft version of the final report (International Peer Review of the Royal Society/PM Science Advisor Office Fluoridation Review). Kathleen Theissen's article is the only one that made it into the final report where it appears basically unchanged.

General comments

Anti-fluoride activists have busily mentioned all the apparent contributors to this critique but I will only deal with Kathleen Theissen’s and Chris Neurath’s comments as only these have any substance. James Beck declined comment saying only “On the current New Zealand case I don’t think I can do any better than Chris has done.” Similarly Speeding Micklem says only that “Chris’s analysis is excellent.”

Hardy Limeback does not engage at all with the science in the NZ Fluoridation Review, but does express his emotional attitude towards it. He says:

“I can’t be bothered to show step by step where this review does not meet the standards of critical scientific analysis. . . . Chris is right on the ball in critiquing this review. I’m not sure many people will appreciate just how detailed he is. I’m disgusted by how sloppy the NZ reviewers were. They were obviously politically motivated.”

However, Limeback’s comment on the use they will make of their critique is interesting:

“The effort to critiquing every paragraph of the NZ review is taxing but once it is done and posted on the website, it would be most useful for those people who want to take on the promoters of fluoridation who will undoubtedly use this review to support the profluoridation agenda and point to how unscientific, one-sided, politically motivated this review really is. I would be happy to lend my name to the list of scientific reviewers of this critique.”

So, you can get an idea of what their purpose is in their “peer review.”

Now, the specific issues raised in Theissen and Neurath’s comments.

Margin of safety

The NZ Fluoridation Review found some population groups may exceed the prescribed “safe” levels of F intake. So Theissen concludes that “the fluoride concentration in drinking water is too high and should be lowered.” However, she ignores completely the review’s comments on this issue.

“Infants 0-6 months of age who are exclusively fed formula reconstituted with fluoridated water will have intakes at or exceeding the upper end of the recommended
range (UL: 0.7 mg/day). The higher intakes may help strengthen the developing teeth against future decay, but are also associated with a slightly increased risk of very mild or mild dental fluorosis. This risk is considered to be very low, and recommendations from several authoritative groups support the safety of reconstituting infant formula with fluoridated water.”

The review also noted that children from 1 – 4 yrs old do not exceed recommended levels on F intake but intake from ingested toothpaste may increase intake above recommended levels. It goes on to conclude:

“Consumption of fluoridated water is highly recommended for young children, as is the use of fluoride toothpaste (regular strength – at least 1000ppm), but only a smear of toothpaste should be used, and children should be supervised during toothbrushing to ensure that toothpaste is not swallowed/eaten.”

I also discussed the issue of risk for formula-fed infants in my article When politicians and bureaucrats decide the science and in my exchange with Paul Connett. Here I note how “peace of mind” advice to those parents who may be concerned about increased risk of dental fluorosis gets presented by anti-fluoride people as a safety warning.

**Adequacy of standards for fluoride intake**

Theissen discusses the adequacy of standards for F intake at length but her only beef with the NZ Fluoridation Review on this appears to be that it doesn’t challenge existing standards. She herself considers that these standards should be revised to “obtained values much lower than those currently considers desirable by the New Zealand government.” But here she is promoting a personal agenda and not objectively critiquing the Royal Society Review.

**Effects of community water fluoridation (CWF) in NZ**

Theissen considers that the NZ review offered little documentation for the beneficial effects of fluoride.” Strange – has she read the review? Table A2 lists 21 major reviews it considered and 7 New Zealand sources were included – the 2009 NZ Oral Health Survey and regional studies in Otago, Southland, Canterbury, Wellington, Auckland and Northland.

If that was insufficient for Theissen then why is her only counter to mention John Coulquhoun’s reminiscences in his article of 1997? It is not enough for Theissen to use his assertions “that there are virtually no differences in tooth decay rates related to fluoridation” and “25 percent of children had dental fluorosis.” But has she bothered to check out his data at all critically? Why no more citations supporting her claim?

Coulquoun was a committed anti-fluoridationist and a critical check of his claims show them to be unreliable. Here is a sentence from the abstract of his paper Colquhon 1985:

“In the unfluoridated areas all the children, and in the fluoridated areas only selected children, had received regular topical fluoride treatments.” And he concluded “When
the socioeconomic variable is allowed for, child dental health appears to be better in the unfluoridated areas.”

Apart from the wishful thinking displayed in his interpretation of a statistically non-significant difference he has glossed over the fact that both fluoridated and unfluoridated groups were receiving fluoride treatments of one sort or another!

Similarly, Theissen puts more trust in Colquhoun’s brief comment on dental fluorosis than the several pages on this subject in the review. Anti-fluoride propagandists are continually misrepresenting dental fluorosis data to imply any extremely mild forms attributable to fluoridation should be treated like the severe forms which are not caused by fluoridation. The Royal Society review’s comment on the aesthetic effects help bring some context back on this issue:

“It is important to note that the seemingly high prevalence of fluorosis reported in some studies and systematic reviews includes mainly mild and very mild (and sometimes questionable) degrees of fluorosis, with only a small proportion that would be considered to be of aesthetic concern.

Surveys have shown that very mild to mild dental fluorosis is not associated with negative impact on perception of oral health,[142] and that adolescents actually preferred the whiteness associated with mild fluorosis.[143] In a recent study, adolescents answered a questionnaire regarding the impact of enamel fluorosis on dental aesthetics, older adolescents rated photographs of mild fluorosis more favorably than younger ones. A fluorosis score indicative of moderate fluorosis was the level considered to have aesthetic significance. Carious teeth were rated significantly lower than fluorosed teeth.[144]

Carcinogenicity and genotoxicity

Theissen, like almost all anti-fluoride propagandists, relies completely on the Bassin et al., (2006) study for evidence here and ignores later studies which did not confirm Bassin’s work. In my exchange with Connett I criticised him for the same tactic (see Fluoride debate: Final article – Ken Perrott):

“the importance Paul gives to a single study on fluoride and osteosarcoma illustrates his mechanical and selective approach to “weight of evidence.” He has not bothered including either the study by Comber et al., (2011) of this issue in Ireland or the study by Levy & Leclerc (2012) for the US. Possibly because both of these concluded that water fluoridation has no influence on osteosarcoma incidence rates.”

So while Theissen is upset the NZ Review “dismisses” Bassin’s work, this is not the “out of hand” rejection she implies. The Review says:

“The few studies that have suggested a cancer link with CWF suffer from poor methodology and/or errors in analysis. Multiple thorough systematic reviews
conducted between 2000 and 2011 all concluded that based on the best available evidence, fluoride (at any level) could not be classified as carcinogenic in humans. More recent studies, including a large and detailed study in the UK in 2014, have not changed this conclusion. “

Neurotoxicity

I partially agree with Neurath’s charge on the inadequacy of the Royal Society’s comment on the standardised weighted mean difference in IQ scores discussed by Choi at al (2012). Some people have made a lot of the confusion around this issue. I would like someone with good statistical skills to comment on the risks involved in making such an analysis in a meta study where there is no conformity of experimental design or treatment in the individual studies. Wikipedia lists a number of pitfalls in statistical meta analysis, two of which seem particularly relevant here – publication bias and agenda-driven bias. In my article Quality and selection counts in fluoride research I described how the studies used by Choi et al., (2012) had been selected and it is hard not to see an agenda behind this. So, I do think Choi et al’s statistical analysis is questionable.

But Neurath and others who have pointed to the mistake in the executive summary of the NZ Fluoridation Review (and ignored the discussion in the body of the review) are making a mountain out of a molehill - or in this case a typo. I discuss the mistake in my penultimate chapter.

However, this issue is irrelevant to CWF because of the generally high drinking water fluoride concentrations used in these studies. Theissen and Neurath resort to the special pleading in their efforts to avoid that problem.

Theissen stressed that in the Choi et al., review “One study had “high” at 0.88 mg/L, quite relevant to CWF.” Neurath says “In fact, one of the Chinese IQ studies had an average water concentration of 0.88 mg/L in the high exposure group.”

At first sight this seems relevant to CWF and Paul Connett, like many anti-fluoride activists, stresses this study in defending the relevance of Choi et al., (2012). Strange, then, that none of them actually discuss the study details. Perhaps we should.

The study is a one and a half pages newsletter report:


It has few of the details we normally expect in scientific papers. For example, I would like to know what the range of fluoride concentrations was in the drinking water, what other dietary intake occurred, how was the “dental fluorosis” observed defined, etc.

Children from low iodine areas were compared with a group from another area that had received iodine supplementation. About 15% of the children suffered mental retardation, 69% of these exhibited subclinical endemic cretinism. The effect of iodine supplementation was
clear, the effect of fluoride not so clear. But anyway, hardly a report to hang any conclusion on about CWF in New Zealand.

They also resorted to special pleading to downplay other problems with these studies:

Theissen:

“the one study . . . that did not show lower IQ still showed a tendency in that direction (just not statistically significant) and it certainly did not show clear absence of any effect”

“While some of the neurotoxicity studies did not address confounders, some did handle them responsibly” [Most of them didn’t]

Neurath:

“most of the studies did consider other sources of exposure such as from food dried over coal fires . . . This in almost all studies, major alternative sources of fluoride exposure were ruled out or controlled for” – [In fact they weren’t as most didn’t consider other inputs]

“several of the studies did consider each of these potentially confounding factors, and at least one group of researchers (lead by Xiang) considered all of them and more.” [yes, one – “all and more” – but why not consider Xiang in detail then? Why try to spread his thoroughness throughout all these meagre studies?]

"simply failing to assess these factors in a study does not mean the study was confounded and produced invalid results.” [well no, but isn’t it best to check known confounders?]

Anti-fluoride people also often single out the study of Xiang, et al., (2003). Effect of fluoride in drinking water on children’s intelligence. Fluoride, 36(2), 84–94, because unlike the others it is more detailed. Xiang’s team has studied areas where fluorosis is endemic. Below is a slide from his presentation to Paul Connett’s recent anti-fluoride “get-together” (Xiang 2014). This is not the very mild dental fluorosis attributed to CWF.

Now I think severe dental fluorosis like this would create learning difficulties for children in the same way dental decay does (Seirawan et al., 2012). I suggested this in Confirmation blindness on the fluoride-IQ issue. Presumably Xiang could have analysed his data to check if the apparent IQ drop was correlated with the prevalence of dental fluorosis. I would think that could be an obvious first step.
Theissen berates the NZ Fluoridation Review for suggesting there is no plausible mechanism for the effect of F on IQ. Instead she resorts to special pleading again – admitting “no mechanism has been established,” but attributing that to lack of research, not the absence of a mechanism. And then speculating on possible mechanism related to thyroid function, etc. The trouble is that this sort of special pleading can soon convert logical possibilities into established proof in the minds of the faithful. And meanwhile an obvious possible cause of the IQ data may be staring her in the face but she is oblivious because it does not involve “brain damage.”

**Note**

Since this was posted Choi et al., (2015) have published more recent work where although they did not report a significant relationship between drinking water fluoride concentration and cognitive defects. However, cognitive defects were significantly related to medium and severe dental fluorosis. This prompted me to send a letter to editor of the journal suggesting that Choi et al., not limit their hypotheses to chemical neurotoxicity but consider other possible causes like the known effect of severe dental fluorosis on quality of life (see Perrott, 2015).

**Animal studies**

Theissen rejects the NZ Review's dismissal of results from animal studies because of the high concentrations used in them. She says baldly “animals require much higher exposures (5-20 times higher, or more; see NRC 2006; 2009). But what does NRC 2006 actually say (The
NRC 2009 simply references NRC 2006)? It discussed the contradictory data used for attempting to show a ratio between humans and rats for blood plasma levels and concluded:

“Dunipace et al. (1995) concluded that rats require about five times greater water concentrations than humans to reach the same plasma concentration. That factor appears uncertain, in part because the ratio can change with age or length of exposure. In addition, this approach compares water concentrations, not dose. Plasma levels can also vary considerably both between people and in the same person over time (Ekstrand 1978).”

Again Theissen resorts to special pleading converting a vague possibility into an established “fact” in an effort to justify the unquestioned use of animal studies using high concentrations.

Mullinex et al., (1995) also attempted to justify use of similar animal studies by comparison of blood plasma F levels. However, there is a huge range and variability in these levels so extremely easy to make subjective justification. I am suspicious of such speculation.

While I am happy to acknowledge that it may be too simple to equate the effects for humans and animals at the same intakes, I think Theissen’s assertion “animals require much higher exposures” is straw-clutching. Mullinex et al., (1995) exhibited the same straw-clutching when she asserted plasma levels in her rats were similar to those in “humans exposed to high levels of fluoride.” Anti-fluoride activists love to quote Mullinex while ignore or downplaying the word “high.” She was quoting plasma F concentrations for children receiving 5 – 10, and 16 mg/L F, 10 or 20 times higher than used in CWF! But the huge effect of treatment time on plasma F concentration in rats must surely warn any objective reader to be very careful about these sort of claims. (Rats receiving 125 ppm F had plasma concentrations of about 0.1 mg/L after 6 weeks exposure but 0.64 ± 0.31 mg/L after 20 weeks).

Endocrine effects

Theissen appears not to have properly read this section of the Royal Society Review.

Contrary to her assertion it does refer to the NRC discussion of these effects and comments:

“Most of the reviewed animal studies were designed to ascertain whether certain effects occurred, and not to determine the lowest exposures at which they occurred. The report concluded that fluoride (at unspecified levels) can affect normal endocrine function or response, and that better characterisation of fluoride exposure in humans in epidemiological studies is needed to investigate the potential endocrine effects of fluoride.”

It acknowledges potential effects (at unspecified levels) despite Theissen’s claim it “failed to mention” them. However, at this time no such effects have been observed in humans at the concentrations used in CWF. So the Review summarises its findings this way:
“A number of other alleged effects of CWF on health outcomes have been reviewed, including effects on reproduction, endocrine function, cardiovascular and renal effects, and effects on the immune system. The most reliable and valid evidence to date for all of these effects indicates that fluoride in levels used for CWF does not pose appreciable risks of harm to human health.”
Stan Litras’s cherry-picking and misinformation

There is a lot in Stan Litras’s article to criticise – there is a lot which is misleading or outright wrong. I hope Stan will seriously consider my criticisms and respond to them, especially where he thinks I am wrong.

My criticisms should also be considered by Bruce Spittle and Hardy Limeback who Fluoride Free NZ listed as “peer reviewers” of Stan’s article. They must bear some responsibility for allowing the article to go ahead without the necessary corrections.

I offered Litras, Spittle and Limeback the right of reply to this post - they all refused.

Litras makes many of his criticisms of community water fluoridation (CWF) in passing – without argument or evidence. But he declares:

“My comments will focus on the gross over statement of the purported benefits of fluoridation in our society, New Zealand, 2014.”

So, I will start with the claims he makes on this.

“Overseas studies” – The WHO data

Central to this are Stan’s assertions:

“The ‘elephant in the room’ is that while decay rates fell in areas where fluoridation was implemented, it also fell in areas that weren’t, often at a faster rate. (8)”

And

“Globally, fluoridation is seen to make no difference to reduced decay rates, there being no difference between the few countries which use artificial fluoridation, and those that don’t. (8,7)”

His only evidence for this is this figure prepared by Chris Neurath from the Fluoride Action Network – using data from the World Health Organisation (WHO). This figure is in a slightly simpler version to the one used by Stan.

I am amazed that anti-fluoride propagandists keep using this graphic as “proof” that fluoride is ineffective. But they do – which can only mean they haven’t thought it
through.

While the plots do show improvements in oral health for countries independent of fluoridation they say nothing about the effect of fluoride. Simple comparison of countries obscures all sorts of effects such as differences in culture, history, social and political policies, etc. Such plots are also influenced by changes and differences in dental treatment and measurement techniques.

Robyn Whyman in his report “Does delayed tooth eruption negate the effect of water fluoridation?” exposes the little trick Stan is trying to pull with the WHO data:

“Studies that appropriately compare the effectiveness of water fluoridation do not compare poorly controlled inter-country population samples. They generally compare age, sex, and where possible ethnicity matched groups from similar areas. Inter-country comparisons of health status, including oral health status, are notoriously difficult to interpret for cause and effect, because there are so many environmental, social and contextual differences that need to be considered.”

There are some within country data within the WHO data set Neurath used which can give a better idea of the beneficial effects of fluoridation. This plot shows the results for the WHO data for Ireland. A clear sign that fluoridation plays a beneficial role.

Neurath covered up evidence for the benefits of CWF by simply using the mean of fluoridated and unfluoridated areas for countries like Ireland and New Zealand. Also, the straight lines in Chris Neurath’s plots are a real give away to the poor quality of the data used. Two data points for each country!

New Zealand – Cherry-picking the MoH data

I have criticised Stan’s misrepresentation the Ministry of Health (MoH) data before. At the time he was using and misrepresenting some of my own graphics on his business website. He has since removed the offending article but now he returns with a vengeance – with tables and figures of his own.

This has given him free hand to cherry-pick and misrepresent to his heart’s content.

He claims:

“Ministry of Health figures recorded every year in 5 year olds and year 8s (12-13 year olds) consistently show minimal or no differences between fluoridated and nonfluoridated areas of NZ.”
And he backs this up with a graph.

That looks about right. The data for 2011 shows 59.9% of 5 year olds in fluoridated areas were caries-free while 59.2% were carries free in non-fluoridated areas. No real difference.

![Graph showing % Caries free - Total](image)

But come on! A single data point, one year, one of the age groups for the fluoridated and unfluoridated areas! That is blatantly cherry-picking – as I mentioned in my article *Cherry picking fluoridation data*. In that I presented all the data for 5 year olds and year 8s, and for the total population and Maori, and for % caries free and decayed, missing and filled teeth (DMFT).

I have reproduced this data here in a simpler form using several figures.

Considering the % caries free data there are several points:

1: These do not “consistently show minimal or no differences between fluoridated and nonfluoridated areas” as Stan claims.

2: They do show a decline in differences between fluoridated and non-fluoridated areas in recent years.

3: This trend is less obvious for Maori but still present.

4: Stan has blatantly cherry-picked the data points for 5 year-olds in 2011 to give him the least possible difference (see red circle in figure).

The data for decayed, missing and filled teeth (DMFT) shows similar trends (see below).

Presumably both measures (% caries free and DMFT) are useful indicators of oral health but they probably convey complementary and not exactly the same information.
I discussed features of the graphs and their trends in my articles *Cherry picking fluoridation data and Fluoride debate: Response to Paul’s 5th article* where I also discussed limitations in the data.

We need to appreciate this is just normal school clinic data, without technique standardisation for those making the measurements or proper recording of place of residence. The decline in differences between fluoridated and unfluoridated areas latter probably shows up more strongly after 2004 when a “hub and spoke” dental clinics system was introduced further confusing proper records of likely fluoride intake. One school dental clinic could serve a number of areas – both fluoridated and non-fluoridated. This mixing is a likely explanation for the apparent decline of the effectiveness of fluoridation after 2006.

So, yes, the MoH data is not straightforward. But this means it should be considered sensibly, taking into account its limitations and the social factors involved. Instead, Stan has leapt in – found the data points which best fit his own biases and then tried to claim those data are representative when they aren’t.

Stan presented another self-prepared graphic using data for the 4 different regions for 5 year olds (see his page 27). He appears not to have used the correct data – at least for the Northern and Southern regions. My own graphic (see left) for this shows differences to his. (Of course, the mistake may be mine – if Stan can show I am wrong I will happily delete this part from my critique).

Again, that data should also not just be considered at face value – or selected to confirm a bias. It has limitations. For example in this case there were only 55 children in the fluoridated Southern region compared with 7568 in the non-fluoridated area. A footnote on the data sheet says:
“2. Excludes Southern DHB because data were not reported for 1 Jan-20 Feb 2012, and fluoridation status was not captured for most children throughout 2012, due to transition to a new data system.”

Proper consideration of such data must take these sorts of limitations into account. But of course all Stan Litras did was select data to support his assertions and ignore the rest. Any limitations in the data did not concern him.

**Lifetime benefit**

Stan has a thing about the “lifetime benefits,” or lack of benefits, of CWF. Most studies of CWF have used data for children – data for adults is less common but there is still research literature on this available.

But all Stan did on this was cherry-pick a graphic (Figure 53) from the [NZ Oral Health Survey](#) showing no significant change in DMFT for 65-74 year olds between the years 1976, 1988 and 2009. He then claims:

> “Data from the NZOHS 2010 do not support statements of a lifetime benefit, indicating that the action of fluoride is simply to delay the decay. (13)”

But he has had to work hard to avoid other data like that in Figure 49 below which do show a significant improvement in the number of retained teeth of that age group. The Oral Health Survey report itself says:

> “In dentate adults aged 65–74 years, the mean number fell from 17.1 to 12.1 missing teeth per person on average from 1976 to 2009.”

**Figure 49:** Mean number of teeth missing due to pathology, among New Zealand dentate adults in 1976, 1988 and 2009, by age group (unadjusted prevalence)

Again, instead of cherry-picking, searching for an image to fit his story, Stan should have considered the data and figures critically and intelligently. Perhaps the DMFT data does not show what he claims because more teeth have been retained by the elderly in recent years. The decline in missing teeth could have been balanced by increases in fillings due to increase in remaining teeth. The lack of a significant difference in DMFT actually suggests the opposite to what he claims.

Litras also misrepresents the York review on the question of benefits from CWF for adults. He says:

“The York Review found there was no weight of evidence to support benefit in adults or in low SES groups, or increase of decay in cessation studies. (7)”

Just not true. The York report says:

“One study (Pot, 1974) found the proportion of adults with false teeth to be statistically significantly greater in the control (low-fluoride) area compared with the fluoridated area.”

Sheiham and James (2014) stressed that a proper assessment of oral health problems should include data for adults as well as children. Recent research is starting to take up this issue. For example O’Sullivan and O’Connell (2014) recently showed that water fluoridation provides a net health gain for older Irish adults.

**Systemic vs topical**

Stan promotes the common mythology of the anti-fluoridation propagandist that any mechanism for a beneficial effect of fluoride in restricting tooth decay is purely “topical.” He claims:

“It has been widely accepted since the 1990s that any effect on tooth decay from swallowing fluoride is insignificant or non-existent. To quote: CDC 1999: “the effect of Fluoride is topical “ (5); J Featherstone 1999: “the systemic effect is, unfortunately, insignificant” (6).”

Let’s consider what the sources Stan cites actually do say. I will quote from the 2001 edition of Stan’s citation 5 which he (partly) cites on page 36:

“Fluoride works to control early dental caries in several ways. Fluoride concentrated in plaque and saliva inhibits the demineralization of sound enamel and enhances the remineralization (i.e., recovery) of demineralized enamel (12,13 ). As cariogenic bacteria metabolize carbohydrates and produce acid, fluoride is released from dental plaque in response to lowered pH at the tooth-plaque interface (14 ). The released fluoride and the fluoride present in saliva are then taken up, along with calcium and phosphate, by demineralized enamel to establish an improved enamel crystal structure. This improved structure is more acid resistant and contains more fluoride and less carbonate (12,15–19 ) (Figure 1). Fluoride is more readily taken up by
demineralized enamel than by sound enamel (20). Cycles of demineralization and remineralization continue throughout the lifetime of the tooth.”

And

“Saliva is a major carrier of topical fluoride. The concentration of fluoride in ductal saliva, as it is secreted from salivary glands, is low — approximately 0.016 parts per million (ppm) in areas where drinking water is fluoridated and 0.006 ppm in nonfluoridated areas (27). This concentration of fluoride is not likely to affect cariogenic activity. However, drinking fluoridated water, brushing with fluoride toothpaste, or using other fluoride dental products can raise the concentration of fluoride in saliva present in the mouth 100-to 1,000-fold. The concentration returns to previous levels within 1–2 hours but, during this time, saliva serves as an important source of fluoride for concentration in plaque and for tooth remineralization (28).”

(Note: Stan simply quotes the first part of this statement (in red) in his article (page 36) and completely omits the second part (in black) – presumably because he wants to deny a role for fluoridated water in influencing the saliva fluoride concentrations. This cherry-picking of the CDC statement is typical for anti-fluoride propagandists – see Fluoridation – topical confusion).

There is an attempt to confuse a “topical” or “surface” mechanism with a “topical” application (eg. toothpaste or dental treatments). However, fluoride is transferred to saliva from food and drink during ingestion so that ingested fluoride also contributes to the “topical” or “surface” mechanism.

However Stan wants to deny a “topical” role for ingested fluoride and claims (page 36):

“The required elevation of baseline levels only occurs after using fluoridated toothpaste or mouth rinse, a concentration of 1,000 ppm or more instead of 1 ppm from water.(24)”
His citation 24 is to Bruun (1984) and he misrepresents that paper which actually said:

“\It was concluded that direct contact of the oral cavity with F in the drinking water is the most likely source of the elevated whole saliva fluoride and that the increased availability of fluoride in the oral fluids has an important relationship to the reduced caries progression observed in fluoridated areas.”

**Systemic role.**

Featherstone does say:

“Fluoride works primarily via topical mechanisms which include (1) inhibition of demineralization at the crystal surfaces inside the tooth, (2) enhancement of remineralization at the crystal surfaces (the resulting remineralized layer is very resistant to acid attack), and (3) inhibition of bacterial enzymes. Fluoride in drinking water and in fluoride-containing products reduces tooth decay via these mechanisms. Low but slightly elevated levels of fluoride in saliva and plaque provided from these sources help prevent and reverse caries by inhibiting demineralization and enhancing remineralization. The level of fluoride incorporated into dental mineral by systemic ingestion is insufficient to play a significant role in caries prevention. The effect of systemically ingested fluoride on caries is minimal.”

There is some debate over the role of systemic fluoride exuded by salivary glands. Many feel the concentration is too low – but because its effect is also determined by the presence of calcium, phosphate, organic species and pH it is best not to be dogmatic about this. It is, anyway, difficult to separate salivary fluoride derived from transfer from food and beverage in the oral cavity from that exuded by the salivary glands from systemic sources.

Stan is determined to deny a role for systemic fluoride during tooth development asserting:

“the erroneous theory that fluoride incorporated into children’s developing tooth enamel would make teeth more resistant to decay.”

While often neglected because of the concentration on surface mechanisms with existing teeth the theory that fluoride is incorporated into the developing teeth of children and confers a degree of protection is far from erroneous.

Newbrun (2004), for example, stressed in a review of the systemic role of fluoride and fluoridation on oral health:

“The role of systemic fluoride in caries prevention is neither “minimal” nor “of borderline significance.” On the contrary, it is a major factor in preventing pit and fissure caries, the most common site of tooth decay. Maximal caries-preventive effects of water fluoridation are achieved by exposure to optimal fluoride levels both pre- and posteruptively.”
Cho et al., (2014) presented data showing that children exposed to CWF during teeth development retained an advantage over those never exposed to it even after fluoridation ceased.

Let’s stop confusing the issue. Systemic fluoride may not play a role with existing teeth but it does during tooth development – even if the relative contributions of systemic fluoride and “topical” or surface fluoride to lasting oral health are difficult to determine.

**Tooth eruption delays**

Stan resorts to special pleading when he claims with reference to NZ MoH data:

“Small apparent differences could be accounted for by other factors such as delayed eruption of teeth in fluoridated communities, therefore less time in the mouth exposed to plaque acids, ethnic distribution and urban/rural differences.”

He relies on the “York review” (McDonagh et al., 2000) to back up his “delayed tooth eruption” excuse:

“Importantly, the York Review noted that the variation of tooth eruption times between fluoridated and unfluoridated areas was not taken into account. (7)”

But that review actually said on this subject:

“It has been suggested that fluoridation may delay the eruption of teeth and thus caries incidence could be delayed as teeth would be exposed to decay for a shorter period of time. Only one study compared the number of erupted teeth per child. The difference was very small and in opposite directions in the two age groups examined, however no measure of the statistical significance of these differences was provided. Only one of the studies attempted to control for confounding factors using multivariate analysis (Maupomé 2000).”

Robyn Whyman has gone into this claim in more detail in his report “Does delayed tooth eruption negate the effect of water fluoridation?”. Here he critiques Paul Connett’s reliance on this excuse and concludes from his review of the literature:

“The studies and reports cited by Professor Connett to try and validate an argument for delayed tooth eruption either do not make the claims he suggests, or do not have direct relevance to trying to assess the issue. The claimed association is at odds with the published literature which indicates minimal variation in eruption time of permanent teeth by exposure to fluoride. A rational explanation exists for the minimal variations that have been reported based on the relationship between fluoride exposure, caries experience in the primary teeth and emergence timing for the permanent teeth.”

The “delayed tooth eruption” excuse is nothing more than special pleading and straw clutching.

**Socio-economic factors**

Stan again misrepresented the York review regarding socio-economic effects on oral health and the effectiveness of CWF when he claimed “there was no weight of evidence to support benefit in adults or in low SES groups.” The York review actually said:
“Studies should also consider changes in social class structure over time. Only one included study addressed the positive effects of fluoridation in the adult population. Assessment of the long-term benefits of water fluoridation is needed.”

And

“Within the UK there is a strong social gradient associated with the prevalence of dental caries. This is found both in adults and in children. Those who are more deprived have significantly greater levels of disease. There is also geographical variation with the northwest of England, Scotland and Northern Ireland most severely affected. (Pitts, 1998; Kelly, 2000)”

There have been a range of studies internationally showing that fluoridation can aid in reducing differences in oral health due to socio-economic effects. See for example Cho, et al., (2014).

**What happens when fluoridation is stopped**

Stan briefly refers to this issue, citing (as anti-fluoridation activists always do) Künzel and Fischer (2000). I will simply refer him and interested readers to my article *What happens when fluoridation is stopped?* This boils down to the need to read the scientific literature properly as usually the anti-fluoridation activists ignore the details referring to fluoride treatments and procedures which replaced CWF. For example, regular fluoride mouth rinsing programme, fluoride varnish applications and improved provision of dental facilities.

**Conclusions**

Stan Litras has simply indulged in blatant cherry-picking of data, and misrepresentation of the literature, in his critique of the recent review *Health Effects of Water Fluoridation: a Review of the Scientific Evidence* produced by the Royal Society of NZ together with the Office of the Prime Minister’s Chief Science Advisor. Perhaps we shouldn’t expect better from a political activist in the anti-fluoride movement but he, and Fluoride Free NZ, attempt to present this, and other articles in the collection, as objective and scientifically credible. It is neither – such cherry-picking and misrepresentation violates any scientific ethics and needs to be exposed for what it is. The Fluoride Free NZ claimed “peer reviewers,” Bruce Spittle and Hardy Limeback, must share responsibility because, by their endorsement, they signal their approval of such behaviour.
Micklen distorts the science

H. S. Micklen, who wrote the second article in the Fluoride Free NZ report, is one of the co-authors, together with Paul Connett, of the book *The Case against Fluoride* which anti-fluoride activists treat as gospel. His article was “peer-reviewed” by James Beck, the other co-author of the book.

In my comments I use the section headings used by H. S. Micklem.

Dental fluorosis

I think Micklen’s comments on dental fluorosis are quite muddled. He confuses the relevance of the different grades of dental fluorosis and unfairly attributes the more severe forms to community water fluoridation (CWF). Consequently he calculates a cost of dental treatment which is wrong.

Recently I put dental fluorosis; its different grades and its contribution to oral health satisfaction into context with the image below (see *Water fluoridation and dental fluorosis – debunking some myths*):

![Dental fluorosis grades (%)](image)

Dental fluorosis of grades none, questionable, very mild and mild are common in countries suitable for CWF – in both fluoridated and unfluoridated areas. Fluoridation may cause a small increase in mild grades. But **these first 4 grades (none – mild) are judged purely “cosmetic.”** In fact children and parents often judge the grades “questionable – mild” more highly than “none.” Research finds these milder forms of dental fluorosis often improve dental health related quality of life (Do and Spencer, 2007; Chankanka et al., 2010; Peres et al., 2009; Biazevic et al., 2008; Büchel et al., 2011; Michel-Crosato et al., 2005).
Micklem is straw-clutching to take one reference used by the NZ Fluoridation Review out of context to imply that these studies are wrong because “subjects liked the appearance of a complete set of artificially white teeth.” He says “they did not like the whiteness associated with fluorosis.” But the authors actually say:

“The ranking of images of teeth with a fluorosis score of TF 1 may lead to the inference this sample of 11 to 13 year olds do not consider milder presentations of fluorosis to be aesthetically objectionable. The very white teeth represented an unnatural presentation that could only be achieved by cosmetic procedures. . . . This is consistent with previous work related to dental aesthetics [18,19] whereby teeth with mild forms of fluorosis (TF 1, TF2) were rated similarly.”

Micklem raises the bogey of the cost of veneers (up to $1750 per tooth) but this is just scaremongering as veneers would not be used for teeth with these mild grades of fluorosis.

Moderate and severe grades of dental fluorosis are common in areas where fluorosis is endemic, but relatively rare where CWF is used. Occurrences in the latter case, despite the low concentrations of fluoride in treated drinking water, will have other causes – high natural levels in well water, industrial pollution, excessive consumption of toothpaste, etc.

Treatment of moderate and severe cases of dental fluorosis using veneers may well be appropriate for a very few young people in countries like New Zealand and the US but it is misleading to attribute this to CWF. Interestingly, Micklem’s misattribution mirrors that of Ko and Thiessen (2014). They also assumed all moderate and severe dental fluorosis was caused by CWF thereby enabling them to declare no cost benefit to CWF because of the required dental treatments.

Micklem has simply continued the anti-fluoride propagandist tradition of confusing data for the relative amounts of different grades of dental fluorosis and attributing problems with the rare moderate and severe forms to the more common questionable and mild forms.

**Neurotoxicity and IQ**

In this section Micklem attempts to contrast the Choi et al., (2012) metareview with the Broadbent et al., (2014) study. He erroneously refers to both as being relevant to CWF and “the case that water fluoridation poses a development risk to human intelligence.”

Let’s make this clear. The Choi et al., (2012) review did not include studies of CWF. The authors made clear that their results should not be seen as relevant to CWF. Most of the brief reports they reviewed studied areas of endemic fluorosis and Xiang (2014) (one of the authors of an included study) gives some idea of how this is manifested in a title slide to a recent talk.
The only study Micklem comments on that involved CWF was that of Broadbent at al (2014). Micklem describes this as “inconclusive” but does not say why. Do I detect some bias there?

Given the available studies I think the NZ Fluoridation Review was justified in concluding “that on the available evidence there is no appreciable effect on cognition arising from CWF.”

Passing on to the question of the Choi et al., (2012) metareview, which is not relevant to CWF. Micklen concedes that included studies were individually “not strong” but argues “the existence of so many studies almost all saying the same (important) thing” should be treated with attention and respect. I agree – but let's not allow that attention and respect to be blind. Let’s be aware of the limitations and attempt to understand what the results might mean.

The authors of that metareview have extended their work to making their own measurements in a pilot study (Choi et al., 2014). In this new paper they did not find a significant relationship between cognitive deficit measurements and drinking water fluoride. We need to accommodate this finding in our assessment of the metareview.

Choi et al., (2014) did find a significant association of cognitive deficits with severe dental fluorosis. Perhaps we need to respect that finding and give it some attention. Rather than assuming the mechanism of such cognitive deficits is the speculated but unproven neurotoxic activity of fluoride we should be open to other possible mechanisms (Perrott 2015).

I have done so with my article Severe dental fluorosis the real cause of IQ deficits? and would welcome any feedback Micklem could give on this. I feel that the effects of a physical deformity like severe dental fluorosis on learning is a more realistic mechanism (for which
there is a lot of published evidence) than some sort of vague chemical toxicity which has
never been noted at these low concentrations. See also Perrott (2015).

Incidentally, Micklem attempts to discredit the Royal Society’s understanding of Choi et al.,
(2012) saying it suggested that the measured IQ reduction was “arguably negligible.” The
NZ Fluoridation Review actually said:

“Setting aside the methodological failings of these studies, Choi et al. determined that
the standardised weighted mean difference in IQ scores between “exposed” and
reference populations was only -0.45. The authors themselves note that this
difference is so small that it “may be within the measurement error of IQ
testing”.[172]”

Choi et al., (2012) said:

“The estimated decrease in average IQ associated with fluoride exposure based on
our analysis may seem small and may be within the measurement error of IQ testing.”

And their abstract reported the:

“standardized weighted mean difference in IQ score between exposed and reference
populations was −0.45 (95% confidence interval: −0.56, −0.35).”

There has been some confusion because Choi et al., (2012) used a standardised weighted
mean difference to accommodate the different IQ scales used in the studies they reviewed.
But their warning about the small size of the calculated difference and its relationship to
measurement errors in IQ testing is relevant.

The NZ Fluoridation Review did indeed make a mistake in the executive summary where it
referred to a claimed IQ shift of “less than one IQ point” when it should have said “less than
one standard deviation.” I discussed this in Did the Royal Society get it wrong about
fluoridation? and noted that even Harvard University made the same mistake in its initial
press release of the Choi et al., (2012) work. I have also discussed this in the penultimate
chapter of this report.

The authors of the NZ Fluoridation Review have now corrected that mistake, as Harvard
University did. There was no mistake in the review’s evaluation of the Choi et al., (2012)
paper.

**Lead**

In this section Micklem attempts to cast doubt on the NZ Fluoridation Review’s comments on
the form of fluoride in drinking water and a possible role of fluoride in releasing lead from
pipe fittings.

The hydrolysis of fluorosilicic acid when diluted during water treatment may not be
completely understood (nothing ever is) but recent high quality research (Urbansky & Schock
has confirmed the review’s statement it is “effectively 100% dissociated to form fluoride ion under water treatment conditions.” Despite acknowledging the need for more and better research Urbansky (2002) concluded “all the rate data suggest that equilibrium should have been achieved by the time the water reaches the consumer’s tap if not by the time it leaves the waterworks plant.”

This debate only exists among anti-fluoride propagandists because of selective and motivated reliance on old and poor quality research, together with confirmation bias. For example, the report by Crosby (1969) that “evidence from specific-ion electrode and conductivity measurements at 25° confirms that sodium fluorosilicate, at the concentration normally present in public water supplies, is dissociated to at least 95%” is interpreted by Coplan et al., (2007) as “proof” the fluorosilicate is 5% unhydrolysed!

Micklem relies on the papers of Master et al., (2000), Copelan et al., 2007 and Mass et al., (2007) to argue that fluoride treatment chemicals enhance lead release from pipes. However, I think an objective assessment of these papers would conclude the authors argue determinedly for a preconceived hypothesis and that many of their arguments are irrelevant and faulty. This is not to dismiss their finding on lead levels in drinking water – but as Masters et al., (2000) themselves point out – “statistical association should not be confused with causation.”

Similarly, I suggest that Micklen’s reliance on Sawan et al., (2010) to support Copelan’s hypothesis amounts to special pleading as those workers used drinking water concentration of 100 mg/L of fluoride and 30 mg/L of lead.

**Osteosarcoma**

Micklem uses the old anti-fluoride activist trick of fixating on a cherry-picked paper which fits his agenda and downplaying or attempting to discredit papers which don’t. He concentrates on Bassin et al., (2006), despite its description by its authors as “an explanatory study” requiring “further research to “confirm or refute” its conclusions. That paper fits Micklem’s agenda because it found a statistically increased risk of osteosarcomas in male boys exposed to water fluoridated at 1.2 mg/L F.

In such a complex area, for a cancer with such a low incidence, a balanced overall consideration of research reports is necessary. All papers have their advantages and drawbacks so conclusions should be derived from proper consideration of the total research findings – as the NZ Fluoridation Review appears to have done.

The Bassin (2006) findings have not been confirmed by any later work – despite a range of such studies (Kim et al., 2011; Comber et al., 2011; Levy and Leclerc 2012; Blakey et al., 2014). The NZ Fluoridation Review pointed out previous reviews had all concluded that “based on the best available evidence, fluoride could not be classified as carcinogenic in humans.” And that “more recent studies have not changed this conclusion.”

Micklem has produced nothing to counter that conclusion.
Cardiovascular and renal effects

Micklen attempts to use the paper, Martín-Pardillos et al., (2014), cited by the Royal Society Review, against the Review’s conclusions. The Review presents the paper this way:

“A number of studies indicate that fluoride may reduce aortic calcification in experimental animals and humans.[199] This preventive effect was recently confirmed by in vitro experiments, but in vivo findings from the same studies showed the opposite result – that phosphate-induced aortic calcification was accelerated following exposure of uremic rats to fluoride in water at around 1.5 mg/L.[200] The authors suggested that chronic kidney disease could be aggravated by relatively low concentrations of fluoride, which (in turn) accelerates vascular calcification. However, further studies are required to test this hypothesis.”

Martín-Pardillos et al., (2014) proceeded from the hypothesis that fluoride did not initiate calcification but because it is attracted to calcified deposits it may influence subsequent crystallisation of the calcified material.

Their in vitro results indicated a protective effect against calcification. While the opposite was observed with the 5/6 nephrectomised rats with induced calcification they still concluded:

“The direct inhibition of ectopic calcification could still occur in vivo when the renal function is correct, such as during aging or even the initial stages of diabetes, and this possibility deserves further research.”

This is relevant to healthy humans without chronic kidney disease (CKD).

The acceleration of induced vascular calcification with the 5/6 nephrectomised rats does raise the need for further studies, and monitoring the situation with humans suffering CKD. But let’s not forget the rat model was extreme. Rats had all of one kidney and 2/3 of the other kidney removed. They were also fed a phosphate enriched diet and the induced CKD was clearly indicated by urea and creatine blood concentration.

Of course these findings are relevant when considering ongoing research and monitory the situation of CKD human patients. As the authors say “the effects of fluoride on renal function and vascular health are more complicated than expected.”

However, the current advice of the National Kidney Foundation is that:

“Dietary advice for patients with Chronic Kidney Disease (CKD) should primarily focus on established recommendations for sodium, calcium, phosphorus, energy/calorie, protein, fat, and carbohydrate intake. Fluoride intake is a secondary concern.”

Given that such patients are already monitory their diet and more advanced cases also probably regularly monitory blood indicators any possible effect of fluoride for individuals
should be detected. It is likely that by the time any problem with fluoride in drinking water is indicated other problems will also have occurred and patients will be taking steps such as water filtering and careful dietary management to handle their situation.

In fact, individually directed management of food and drinking water appears to be a sensible way of handling problems if they do occur with a few people.

Micklem’s “call for CKD sufferers to be warned to avoid tap water” is too extreme and alarmist. Already the advice is that persons with CKD should be notified of the potential risk of fluoride exposure and be kept up to date with new research. Any patients who are particularly worried can then take steps like using filtered water for their own peace of mind. This seems more appropriate than denying the rest of the population access to a simple, effective and safe (for them at least) social health policy like CWF.
Was the NZ Fluoridation Review mistaken?

Did the Royal Society of NZ and the Office of the NZ Prime Minister’s Chief Science Advisor make a big mistake in their report *Health Effects of Water Fluoridation: a Review of the Scientific Evidence*? Did they misrepresent a scientific paper which reported an effect of fluoride on the IQ of children?

This is what “Connett’s Crowd,” anti-fluoridation activists and propagandists, are saying in their attempts to discredit the review. So, did this review make the mistake its critics claim?

Well, no. It’s just a beat up. But there was a small mistake in the review’s executive summary which the anti-fluoridationists are pouncing on. That mistake has since been corrected

The issue

Most critics of community water fluoridation rely heavily on this paper:


This was a metareview of mainly obscure and brief reports (see *Quality and selection counts in fluoride research*) indicating the possibility the fluoride intake by children living in high fluoride areas of China and Iran may suffer IQ deficits. Choi et al., (2012) used a statistical analysis to determine the possible size of the IQ drop averaged over all the studies. They found a small drop and said:

“The estimated decrease in average IQ associated with fluoride exposure based on our analysis may seem small and may be within the measurement error of IQ testing.”

Their abstract reported the:

“standardized weighted mean difference in IQ score between exposed and reference populations was –0.45 (95% confidence interval: –0.56, –0.35).”

(Their use of “standardised weighted mean difference” was poorly explained and has caused confusion with many readers. See below for a brief explanation of the term).

What did the NZ Fluoridation Review say about this?

The review discusses the question of possible neurotoxic effects on page 49-50. Their comment relevant to Choi et al., (2014) appears below:
And this is what was in the executive summary (click below to enlarge). It made a very small mistake by referring to “less than one IQ point” when it should have said “less than one standard deviation.”

from CWF.

Effects on IQ
Recently there have been a number of reports from China and other areas where fluoride levels in groundwater are naturally very high, that have claimed an association between high water fluoride levels and minimally reduced intelligence (measured as IQ) in children. In addition to the fact that the fluoride exposures in these studies were many (up to 20) times higher than any that are experienced in New Zealand or other CWF communities, the studies also mostly failed to consider other factors that might influence IQ, including exposures to arsenic, iodine deficiency, socioeconomic status, or the nutritional status of the children. Further, the claimed shift of less than one IQ point suggests that this is likely to be a measurement or statistical artefact of no functional significance. A recently published study in New Zealand followed a group of people born in the early 1970s and measured childhood IQ at the ages of 7, 9, 11 and 13 years, and adult IQ at the age of 38 years. Early-life exposure to fluoride from a variety of sources was recorded, and adjustments were made for factors potentially influencing IQ. This extensive study revealed no evidence that exposure to water fluoridation in New Zealand affects neurological development or IQ.

We conclude that on the available evidence there is no appreciable effect on cognition arising from CWF.

So, the review reported the Choi et al., (2012) findings accurately but made a small mistake in the executive summary. This is really of no consequence because the overall message of
the small size of the estimated IQ drop (described by the authors as “small and may be within the measurement error of IQ testing”) is not really altered.

What do the anti-fluoride critics say?

Such mistakes are inevitable and authors will universally say they usually find them only after publication when no correction is possible. I remember picking up 5 mistakes in one of my papers – mainly incorrect spelling of my own name several times and a mistake in the address of my institution – those were the early days of word processing! Of course no one used my mistakes to cast doubts on the scientific content of the paper.

Still, “Connett’s crowd” have been merciless in their criticism. Here is an example from the big man himself (see Water Fluoridation: The “Healthy” Practice That Has Deceived the World):

Gluckman and Skegg (sic)* mistakenly claim “a shift of less than one IQ point” in the 27 studies reviewed by Choi et al. (2012). What they have done here is to confuse the drop of half of one standard deviation reported by the authors with the actual drop in IQ, which was 6.9 points. Such an elementary mistake would not have been made by Gluckman and Skegg (sic)* if they had actually read the report, instead of relying on what fluoridation propagandists were saying about it.

* Of course Gluckman and Skegg – who Connett calls The ‘Hollow Men’ of New Zealand – did not author this review - they commissioned it.

H.S. Micklem, in the Fluoride Free NZ report on the Royal Society review, snipes:

“It is hard to imagine how this mistake could have been made by anyone who had actually read the papers that are disparaged so casually.”

I guess critics should read carefully before indulging in such snaky comments. All they have demonstrated is that they did not read past the executive summary of the review (and certainly did not read the relevant section in the review). Or, more seriously, that they wish to misrepresent the review by highlighting the mistake and ignoring what the review actually says.

(At Least Kathleen Thiessen was more honest in her comments in the FFNZ report because she did refer to page 49 as well as the mistake. However she still concluded:

"The RSNZ report is not accurate in its characterization of the Choi et al. (2012) article on effects of fluoride on children’s IQ.”)

It is ironic that the anti-fluoridation authors of this report (and those writing in internet commentaries on the NZ Fluoride Review) have been so scathing as the same mistake was made in the Harvard University press release reporting the Choi et al., (2012) work See http://www.hsph.harvard.edu/news/features/features/fluoride-childrens-health-grandjean-
choi.html where the current web page reports the updating of the press release to remove the mistake. This does put this mistake into context.

Is Choi et al., (2012) relevant to fluoridation?

Not really.

The only study specific to community water fluoridation (CWF) the NZ Fluoridation Review mentions is Broadbent, et al., (2014). *Community Water Fluoridation and Intelligence: Prospective Study in New Zealand.*

The Choi et al., (2012) paper reviews reports mainly from areas of endemic fluorosis where fluoride intake is much higher than areas using CWF. Subsequently the same authors made their own measurements in a similar area of China and did not find a significant relationship of drinking water fluoride to IQ (see Choi et al., 2015. *Association of lifetime exposure to fluoride and cognitive functions in Chinese children: A pilot study*).

However, they did find a relationship of IQ to severe dental fluorosis. I discuss their findings in my article *Severe dental fluorosis the real cause of IQ deficits*, and in my peer-reviewed letter to the editor of the journal which published their 2015 paper (Perrott 2015)

What is this “standardised weighted mean difference”

This term caused a lot of confusion with readers and critics. Choi et al., (2012) used this statistical device because they were attempting to estimate the average decrease in IQ associated with fluoride exposure based on the difference in IQ between children from high fluoride villages and low fluoride villages in a large number of studies. Further, different IQ scales and measurement methods were used in the different studies which had different levels of variation in the data.

They therefore standardised the differences by expressing them as a fraction of the standard deviation for each study. A mean value over all the studies was determined, weighting the contribution from each study according to the precision of the IQ measurements.

The standardised weighted mean difference value of 0.45 has meaning because we know it represents less than half of one standard deviation so it gives us an indication of how it compares with measurement error. But a value of 6.9 as used by Paul Connett is meaningless – until we are told the standard deviation. Choi et al. (2012) did not report a difference of 6.9 implied by Paul Connett who appears to have obtained that value from a response to a letter to the editor where they use a hypothetical example to explain the meaning:

“For commonly used IQ scores with a mean of 100 and an SD of 15, 0.45 SDs is equivalent to 6.75 points (rounded to 7 points).”
Conclusion - another example of misinformation and distortion of the science by anti-fluoride activists

The original NZ Fluoridation Review, *Health Effects of Water Fluoridation: a Review of the Scientific Evidence*, was prepared in response to a request from councils for a summary of the current science on CWF. This is because over the last few years activist political groups, like Fluoride Free NZ (and its international associate Fluoride Action Network) have bombarded New Zealand councils with misinformation and distortion of the science in campaigns to prevent CWF or get it removed.

Councils do not have the expertise to critical consider claims made by such activist groups and have recently adopted a policy of requesting central government take over their responsibilities on the issue. Until that happens, however, councils will continue to have such decisions forced upon them.

The NZ Fluoridation Review provides a timely and authoritative source of information for councils. Understandably Fluoride Free NZ feels somewhat trumped by the review. So it is unsurprising this activist group, and their international associate, will use their media influence to try to discredit it.

This report is an attempt to fool councils by pretending to be objective and international. Yet, as this report shows, it is simply a put-up job. It is not objective – all the authors and “peer reviewers” are working for or associated with the Fluoride Action Network or its associates. The articles follow the typical cherry-picking and confirmation bias of such activist organisations.

The Fluoride Free report is simply disingenuous – a sham aimed at fooling councils.
References


