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Suppression by Medical Journals of a Warning about Overdosing Formula-Fed Infants with Fluoride

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In January 1990, a short letter was sent to the editor of the international medical journal, *Pediatrics*, to alert its readers that the standard, highly quoted paper by Singer and Ophaug on fluoride intake by infants, published in 1979 in the same journal, required revision/correction in order to protect one group of infants from receiving substantial overdoses of fluoride. This group comprises infants who are fed almost entirely on powdered formula which is reconstituted with fluoridated water.

The letter was based on the well-established pediatric guidelines of water intake by infants and the fundamental toxicological principle of protecting groups at highest risk. It did not question the fluoridation of public water supplies. Nevertheless, the letter, together with a response to it by Ophaug, was rejected by the editor of *Pediatrics*, "due to a large backlog of articles." Following a protest, the letter was reviewed by three referees, two of whom conceded its main point, but was still not published.

In the present paper, the original, previously unpublished letter on fluoride intake by infants is first reproduced verbatim, and then the comments of the referees and editors are reported and examined. It is concluded that the most plausible explanation for the rejection of the letter is that it might assist the anti-fluoridation movement. Another possible contributing explanation is that publication of the letter might reduce the status of the scholars who had defended the previous position and might be perceived to diminish the status of the journal.

Keywords: fluoridation; fluoride intake; infant formula; overdose; intellectual suppression.

INTRODUCTION

The work reported in this paper may be considered to be an unplanned experiment to investigate the power structure of medicine in the area of fluoride science in general and water fluoridation in particular. This paper also seeks to alert scientists and medical practitioners to the existence of a particular subgroup of the population in fluoridated areas which ingests much higher doses of fluoride than average.

In 1990, the authors of the present paper sent a short letter to the editor of the international medical journal, *Pediatrics*, to alert its readers that the highly quoted paper by Singer and Ophaug (1979) on fluoride intake by infants aged 0–6 months, published in the same journal, required correction in order to protect one group of

infants from receiving substantial overdoses of fluoride. This group comprises infants who are fed almost entirely on powdered formula which is reconstituted with fluoridated water. This group has a fluoride intake 2–3 times higher than the so-called 'optimum,' and considerably higher than that of the 'maximum' intake group considered by Singer and Ophaug (1979). They had also underestimated the water intake of infants.

The letter also pointed out that related work by Ophaug, Singer and Harland (1980), calculating the average fluoride intake of infants, was irrelevant, because, according to the principles of toxicology, it is the high-risk groups which must be protected, not just the average (Klaassen, 1986).

Our concern was and is that the parents of children using powdered infant formula should be warned by their medical practitioners to use unfluoridated or defluoridated water to reconstitute the formula. Thus, our letter did not question fluoridation of water as such, although other papers by one of us (M.D.) have done so. So, in effect, the letter was also an unplanned test of how objective the largely pro-fluoridation pediatric establishment could be in protecting high-risk groups of infants from overdoses to fluoride without opposing fluoridation.

In the course of rejecting the letter, the editor and reviewers made some revealing statements which are reproduced here.

The Materials and Methods section of this paper takes the form of the letter-to-the-editor which was submitted unsuccessfully to *Pediatrics* and then to another medical journal. The Results section gives the responses of the editors and their referees and some specific discussion of the points they make. Under General Discussion, the broader implications of the rejection are considered.

MATERIALS AND METHODS

The following letter was submitted to the journal *Pediatrics*. After rejection by that journal, it was submitted to *New Zealand Medical Journal*. (The references to this letter have been changed into the format of *Accountability in Research*.)

Fluoride intake by infants
To the Editor—

The papers by Singer and Ophaug (1979) and Ophaug, Singer and Harland (1980) appear to be the principal sources for estimates of the fluoride intakes by infants (Committee on Nutrition, 1986). But, we suggest that Singer and Ophaug's (1979) 'maximum' estimate requires revision and their 'average' (Ophaug, Singer and Harland, 1980) values are irrelevant, because there is a significant group of infants whose daily fluoride dose is considerably greater than their 'maximum' value. This group comprises infants who are fed mostly on powdered formula which is reconstituted with fluoridated water.

Singer and Ophaug (1979) have taken care to use a realistic calorie intake, but have underestimated the water intakes of 4-month and 6-month infants. It is well known that, in the first 6 months after birth, infants have an average daily water requirement of about 150 ml/kg body mass (Francis, 1986; Murnane, 1986; Green, 1988), with a typical range of variation of 120-180 ml/kg body mass (Green, 1988). If

this water requirement is obtained from 1 ppm fluoridated water, it corresponds to a fluoride intake of 0.12–0.18 mg/kg body mass. Nowadays the 'maximum' fluoride intake group comes from a diet comprising almost entirely reconstituted powdered formula. It cannot be assumed that the fluoride content will necessarily be 'diluted' by the presence of solid food which is low in fluoride and, in particular, by the formula powder itself. At least in Australia, instructions on formula containers first ensure that the water requirements are met, by asking the parent to measure out the equivalent of more than 150 ml water/kg body mass/day and then to add the formula to the water. Thus, we submit that the correct method of calculating the fluoride intake from powdered formula is to work out the tapwater intake first, and to allow 1 mg fluoride per liter of tapwater. [An incorrect method is to simply allow 0.67 mg/liter of reconstituted formula (Barness, 1981)]. To the range of fluoride intakes of 0.12–0.18 mg/kg body mass/day must be added the fluoride content of the formula powder itself.

Thus, for infants of typical mass 8.1 kg at age 6 months (Singer and Ophaug, 1979), the range of fluoride intakes by the 'maximum' group exceeds 0.97–1.46 mg/day. More generally, the fluoride intake of infants aged 0–6 months who drink almost entirely powdered formula reconstituted with 1 ppm fluoridated water is at least 4–6 times the 0.25 mg/day intake recommended by the American Academy of Pediatrics (Committee on Nutrition, 1986) for fluoride supplementation in unfluoridated areas.

Some people might take the stance that the safety of groups such as infants who drink powdered formula in fluoridated areas should be ignored because they are a minority group and, after all, the *average* fluoride intakes by infants were [at least in 1980 (Ophaug, Singer and Harland, 1980)] much lower than those of a 'maximum' group. But, in toxicology and radiation protection, the responsible and cautious approach is to identify the high-risk groups in the community and to set safety standards to protect these groups with a high degree of certainty. We see no good reason why fluoride should be granted special dispensation.

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RESULTS

The above letter-to-the-editor was first mailed to the international journal *Pediatrics* on 29 January 1990. On 27 March 1990 the Editor in Chief, Jerold F. Lucey, M.D., replied, enclosing a response by Ophaug, also in the form of a letter-to-the-editor. But Dr. Lucey wrote that

.... We will not be able to publish either of these letters due to a lack of space plus a large backlog of articles. However, feel free to correspond directly with Dr Ophaug if you wish.

Dr. Ophaug's letter of reply, dated-stamped 26 March 1990 by *Pediatrics*, was concerned with defending the scientific validity of his original paper (Singer and Ophaug, 1979):

.... The 'maximum' and 'minimum' fluoride intakes we calculated for 2, 4 and 6 month-old infants reflected specific assumptions which were explicitly stated in the article.... Obviously, if one bases the

calculations on other assumptions, higher (or lower) fluoride intakes could be calculated. The fluoride intakes of infants consuming only powdered formulas diluted with fluoridated water is an example....

Thus, Dr. Ophaug conceded in general terms the main point of our letter, without mentioning the large amount by which the doses received by some members of the powdered formula group exceed that of his so-called 'maximum' group. Incidentally, although Ophaug places quotes around 'maximum' in his letter, to indicate that it is only the largest intake subject to the assumptions and constraints he lists above, the quotes are omitted in Singer and Ophaug (1979). In the abstract to this paper, the bald statement is made that "The maximum total daily fluoride intake for infants up to 6 months of age is 0.127 mg of fluoride per kilogram of body weight." Readers who did not read the materials and methods section of this paper very closely could easily misinterpret Singer and Ophaug's constrained maximum to be the true maximum intake.

Ophaug's letter went on to point out that

.... we,[2,3] as well as others,[4] have repeatedly emphasized that infants consuming powdered formula diluted with fluoridated water constitute a special group of children whose dietary fluoride intakes may exceed that associated with the development of dental fluorosis....

[Refs. 2 and 3 of this letter from Ophaug correspond to our references Ophaug, Singer and Harland (1985) and Ophaug and Singer (1988) respectively].
Ophaug's letter did not acknowledge the point we made that Singer and Ophaug and others such as Barness (1981) had underestimated the water intake of infants and hence the fluoride intake from fluoridated water used for reconstituting powdered formula. Nevertheless, we would have been partially satisfied if our letter and Ophaug's reply had been published together, at least drawing the attention of pediatricians to the issue.

In case the editor of *Pediatrics* did not fully appreciate the implications of his rejection, one of us (M.D.) wrote back to him on 1 May 1990 in a letter not intended for publication at that time:

... Dr Ophaug's response concedes that the main point of our letter is correct, but states that the point has been made elsewhere. Unfortunately the papers† in *Pediatrics* by Singer and Ophaug are still being used incorrectly in several countries as the principal estimates for fluoride intakes by infants. The presence, in journals not normally read by pediatricians, of guarded qualifications to Singer and Ophaug's original papers does not obviate the need to issue a frank and clear correction in *Pediatrics*.

That was the purpose of our original letter-to-the-editor. It will not be served by private correspondence with Dr Ophaug or anyone else. Surely it is the responsibility of your journal to ensure that your readers around the world are properly informed of the current situation. I would not expect *Pediatrics* to evade or suppress the publication of such important information by claiming lack of space. Such a course of inaction could ultimately leave yourself and your journal open to legal action.

The intention of M.D.'s last sentence was simply to suggest that we would make no secret of the rejection of our letter and that this may leave the journal liable to legal action by parents of children who drank powdered formula reconstituted with fluoridated water and were therefore overdosed with fluoride. It galvanized

†Note that our use of the plural here was a typographic error. Only one of Singer and Ophaug's papers on fluoride intake by infants was actually published in *Pediatrics*; others appeared in the *American Journal of Clinical Nutrition* (Ophaug, Singer and Harland, 1985) and an obscure dental education journal (Ophaug and Singer, 1988).

the editor into further action, but not publication of our letter. Although he had already rejected our letter, he sent copies of the unpublished correspondence to Dr. Ophaug, to Dr. Lewis A. Barness and to a third reviewer who was anonymous. Dr. Ophaug's second letter was date-stamped 9 July 1990 by *Pediatrics* and forwarded to us by Dr. Lucey. It stated correctly that Singer and Ophaug (1979) made no attempt to deal with powdered formula. He did not come to grips with our concern that other authors and institutions preparing guidelines on fluoride intake could easily misinterpret his constrained 'maximum' fluoride intakes as if they were true maxima (see above). He then drew attention again to papers published by himself and his colleagues in other journals which mentioned the issue in passing, for example:

> The ingestion of fluoride-containing dentifrice or milk formula diluted with fluoridated water may result in intake levels exceeding that associated with the development of dental fluorosis (0.1 mg F/kg body wt). (Ophaug, Singer and Harland, 1985) and
>
> .... In such instances, dietary fluoride intakes greater than 0.10 mg/kg body weight may be obtained. (Ophaug and Singer, 1988).

These published statements, of which we were unaware at the time, show commendable social responsibility, but they still do not reveal the full magnitude of fluoride intakes from the group of infants ingesting powdered formula reconstituted with fluoridated water. As pointed out in our unpublished letter, some 6-month-old infants in this group are ingesting 0.12–0.18 mg F/kg body mass per day, i.e. approximately 2–3 times the so-called 'optimum' dose of 0.05–0.07 mg F/kg mentioned in Singer and Ophaug's papers and 4–6 times the intake recommended by the American Academy of Pediatrics (Committee on Nutrition, 1986) for fluoride supplementation in unfluoridated areas.

Strictly speaking there is little if any scientific evidence that there is any dietary requirement for fluoride at these levels. Most of the evidence on for the reduction of tooth decay by fluoride is consistent with a topical mechanism (i.e. acting at the surface of teeth) rather than systemic—see review in Diesendorf (1990a).

Moreover, the first of the above quotations from Ophaug's second letter, which comes from the abstract to Ophaug, Singer and Harland (1985), may inadvertently create the misleading impression in readers unfamiliar with infants or dentistry that fluoride-containing dentifrice (i.e. toothpaste) is generally a comparable source of ingested fluoride to that from reconstituted formula. But the use of toothpaste is negligible in infants aged 0–6 months, because for most infants teething only begins around age 6 months (Green, 1988). Thus Ophaug's quotation obscures the point that, for infants aged 0–6 months, reconstituted milk formula in fluoridated areas is the principal source of overdosing with fluoride.

The colorful response by Dr. Barness was dated 29 May, date-stamped 5 June 1990 by *Pediatrics*, and forwarded to us by Dr. Lucey. It does not recommend explicitly for or against publication, but its tone is unmistakable:

> Flushed water goes down the toilet counter-clockwise in the southern hemisphere. Perhaps other perspectives also change. The data of the Diesendorfs are approximately correct.... Approximately 15% of the formula distributed in this country is powder. For the preparer to get fluoride-free formula in a fluoridated area requires considerable expense and inconvenience. For these preparers to be told to use unfluoridated water may be a surreptitious attempt to eliminate fluoridation—a subject beyond the consideration of infant formula preparation.

The latter sentence, which is very revealing, will be discussed in the General Discussion section.

Neither Barness' response to us, nor Committee on Nutrition (1986) (which Barness chaired in 1979), nor Barness (1981) comes to grips with our main point that infants drinking powdered formula reconstituted with fluoridated water ingest several times the fluoride dose recommended by Committee on Nutrition (1986) as a dietary supplement for infants of the same age living in unfluoridated areas. This is a large inconsistency in dosage, with significant health implications, yet Barness and his Committee simply ignore it.

The third and anonymous referee (undated report in 1990) asserted, without any substantiation, that the papers by Singer and Ophaug did not require revision and that (s)he believed that their 'average values' in their market basket studies (Ophaug, Singer and Harland, 1980 and 1988) were relevant:

.... As Ophaug reports in his reply to the Diesendorf letter, his data on intakes is carefully derived from an analysis of the FDA method of 'market basket' food collections; a very relevant method of measuring actual intakes. It is, in fact, more relevant than estimating that a 6 month old infant will be drinking 1.6 liters of formula which, in my experience, is roughly double the intake of an average 6 month old infant.

It appears that this referee, unlike Ophaug, does not understand the fundamental principle of toxicology: namely that safety standards are not set to protect the average member of the community, but rather those receiving the highest doses and those at greatest risk (Klaasen, 1986). Nor does this referee appear to understand that, although the market basket method can be a useful method of obtaining average intakes of certain food groups, it is a poor method for obtaining maximum intakes; furthermore, it is completely inadequate for measuring water intakes, because ingested fluoridated tapwater is not normally purchased in a form which can be put into a 'market basket'.

The third referee has also made an error of arithmetic. For a 6-month-old infant of mass 8.1 kg ingesting the recommended level of water of 150 ml/kg body weight (range 120–180 ml/kg), the total water intake is 1.2 liters/day (range 0.97–1.46 liters/day), as pointed out in our letter, not 1.6 liters/day. For our 'maximum' group, we assumed that essentially all ingested water is fluoridated at 1 ppm.

After our letter had been rejected by *Pediatrics*, we submitted it to *New Zealand Medical Journal*, edited by Prof. Richard G. Robinson. This journal was hardly ideal for gaining widespread debate about the issue (it is not indexed in *Current Contents*), but it was chosen because it had previously published letters on fluoridation, and so it seemed possible that the present letter might not be ruled out automatically. The editor's reply was:

I would rather suspect that this will be over the heads of most of our readers. It starts off criticizing some North American papers, which probably most of my readers are not familiar with and moves into some Australian experience. I suppose there are meant to be lessons for New Zealand in this concerning.(sic) I think if you wanted to communicate about this topic to a New Zealand audience, you would have to more specifically orientate your contribution that way There are considerable difficulties in this sort of transcultural exercise.

This view runs counter to the principle that science is international. We support the principle and, contrary to Prof. Robinson's view, do not believe that the readers of his journal could be generally provincial and uninterested in results which are

relevant to infants in all developed countries which are highly fluoridated, including New Zealand.

Rightly or wrongly, the papers by Singer and Ophaug are an important input to the American Academy of Pediatrics Committee on Nutrition's recommendations on fluoride supplementation, and this in turn influences the estimates of fluoride intakes and recommendations on fluoride supplementation in the USA and other developed countries where fluoridation is implemented. For instance, that committee's reports are cited in reports by the Australian National Health and Medical Research Council (1985), e.g.:

> The Working Party examined in detail the fluoride supplements schedule now recommended by the American Academy of Pediatrics and regarded it as being relevant to the remaining non-fluoridated areas in Australia....

In another report by the Council, the papers of Singer and Ophaug are cited directly (National Health and Medical Research Council, 1991).

GENERAL DISCUSSION

As pointed out in our unpublished letter to *Pediatrics*, there are three important shortcomings in the papers on fluoride intake of infants by Singer and Ophaug:

a. Singer and Ophaug (1979) only consider a 'maximum' fluoride intake group with ready-to-feed formula with fluoride content 0.68 ppm, instead of powdered formula with fluoride content about 1 ppm in a fluoridated area.

As we learned in the course of the above correspondence with the editor of *Pediatrics*, Singer and Ophaug had mentioned the existence of the high fluoride intake of the powdered formula group in other journals (Ophaug, Singer and Harland, 1985; Ophaug and Singer, 1988). However, the latter reference seems to be very obscure. As far as we can determine, Singer and Ophaug did not publish quantitative estimates of these intakes in any journal. In the former reference they merely stated that fluoride intakes were greater than 0.1 mg/kg body weight and were similar to those of the maximum intakes estimated for ready-to-feed group studied in Singer and Ophaug (1979). Therefore, it is our view that the warning by Singer and Ophaug and colleagues was at best a muted one.

b. Even the estimates of 'maximum' fluoride intake in the ready-to-feed formula group in Singer and Ophaug (1979) are underestimates, because their hypothetical children at ages 4 months and 6 months receive much less than the recommended water intake of 150 mg/kg body mass. Indeed, if they were real children, they would have suffered from dehydration.

c. The studies based on market basket food collections by Ophaug, Singer, and Harland (1980 and 1985) offer useful comparisons of the *average* fluoride intakes in different regions of the USA, but are irrelevant to the problem of determining *maximum* fluoride intakes. Market basket studies cannot measure tapwater intake.

For these reasons, it was important to publish a letter or paper to draw attention to the overdosing of the powdered formula group and to correct, or at least debate, the other shortcomings of the previous papers on this subject. The journal *Pediatrics* was chosen initially, because of the "long-standing scientific etiquette which says

that papers pointing out errors should be published in the same journal in which the original paper appeared." [The source of this quotation is an unnamed Nobel laureate quoted in Menger and Haim, 1992]. *Pediatrics* was also chosen because the American Academy of Pediatrics has a strong influence through its recommendations on fluoride supplementation (Committee on Nutrition, 1986). At the time we made the decision to write a letter (which has low academic status), rather than a paper, in order to increase its chances of publication and widespread dissemination, thus alerting medical practitioners to the risk to a particular group of infants.

Although questioning the medical establishment has its difficulties, the letter is not controversial in the scientific sense, since it does not challenge fluoridation in general. In theory, a proponent of fluoridation could have written the letter, but in practice none did. The letter is based on two widely accepted results cited above:

- the high water intake of infants, around 150 mg per kg body weight, during their fist 6 months after birth; and
- the fundamental principle of toxicology, namely that dosage guidelines must be set to protect high-risk groups rather than the average.

The 'optimum' fluoride intake level with which our 'maximum' intake group is compared, is obviously one set by proponents of fluoridation, not opponents.

So, an approach fostering scientific discussion and social responsibility would have been to publish our letter together with Ophaug's reply. Since the letter and reply are very short compared with a typical paper, the explanation offered by the editor of *Pediatrics*, in terms of shortage of space, can be dismissed.

Four possible explanations for the rejection are now considered. The first two are mutually exclusive. It should be noted that we are not criticizing anyone's motivations, but rather are proposing social explanations for the editor's decision.

i. The points made in the letter are wrong.

This does not seem to be likely, since two of the three reviewers, Ophaug and Barness, conceded the main thrust of the letter. The third referee contradicted several points in our letter, including the standard textbook result on water intake of infants, without offering any arguments or substantiation. So [s]he lacks scientific credibility.

ii. The points made in the letter are so well-known that the letter is redundant.

Ophaug was the only reviewer who appeared to take this position, but naturally he is an interested party. His muted warning about fluoride intake from powdered formula, published in other journals, did not offer a quantitative estimate, nor did he give the correct water intake of infants. So, there was justification for publishing our letter or a short paper. The third reviewer did not seem to understand any of our letter, which confirms that there is a need to publish the letter widely.

iii. Publication of a correction by outsiders might reduce the status of the scholars who had defended the previous position and might be perceived to diminish the status of the journal.

This appears to be a possible explanation for a set of rejections of attempts by Menger and Haim (1992) independently to correct errors in published papers in the field of chemistry. In this case, the topic of the papers under question, the kinetics of

cleavage and isomerization of dinucleotides, was not controversial in a broader social or political sense, and yet even then there was strong resistance by the editor of the *Journal of the American Chemical Society* to publication of Menger and Haim's separate submitted corrections to errors they had identified in two papers published in that journal.

It seems likely that elements of this explanation were also operating in the rejection of our letter. But, in our case resistance to publication would have been even stronger, because, while Menger and Haim are chemistry academics, we are not pediatricians or even medical doctors. M.D. is a human ecologist and a health researcher, who was originally an applied mathematician and physicist. A.D. is a health educator and a registered nurse. Moreover, the topic of our letter has implications for an issue of considerable public controversy. This suggests that the following stronger explanation was also operating:

iv. Publication of the letter might assist the anti-fluoridation movement.

This explanation is supported by Barness' comments, notably his last sentence quoted in the Results section suggesting that our letter might be an attempt to undermine existing fluoridation policy. It is also consistent with the fact that the editor of *Pediatrics* did not use arguments (i) or (ii) to support rejection, but rather an obvious excuse. To the present authors, Explanation (iv) appears to be the principal reason for rejection of the letter, although we accept that there may also have been elements of Explanation (iii). One of us (M.D.) had similar experience in attempting to publish an article on fluoridation in the Australian medical magazine *New Doctor* (see Diesendorf, 1981 and 1982).

Explanation (iv) appears to violate both proper scientific procedure and widely accepted medical ethical principles that people should not be exposed to risks without receiving benefits which exceed that risk, and that people should give informed consent to any medical procedure involving risk. Of course, these principles could be applied to the whole fluoridation issue. But, the present case of fluoride intake by infants is particularly worrying because the medical establishment accepts that infants ingesting more than its 'optimum' fluoride dose of 0.05–0.07 mg/kg/day are overdosed, and yet part of that establishment, as exemplified by the attitude of the editor of *Pediatrics* and the comments of Dr. Bamess, has acted to stop parents from receiving the information necessary for informed consent.

v. Publication was denied because the editor thought the letter was boring.

Readers will have to judge for themselves whether this explanation, suggested by an anonymous referee, is credible. Since the letter and this paper deals raises questions about the potential health impacts of overdosing infants with fluoride, it seems unlikely that this explanation could be relevant.

Explanation (iv), and to a lesser degree Explanation (iii), are examples of what Martin et al. (1986) call 'suppression of intellectual dissent' or, more briefly, 'intellectual suppression,' which he describes in the following manner:

> First, a person or group, by their public statements, research, teaching or other activities, threatens the vested interests of elites in corporations, government, professions or some other area. Typically this is by threatening profits, bureaucratic power, prestige or public image, for example by providing support to alternative views or by exposing the less attractive sides of the powerful group ... The second feature of suppression cases is an attempt by a powerful individual or group to stop or to penalise the person or

activity found objectionable. This may involve denying funds or work opportunities, blocking appointments, blocking tenure, blocking promotion, blocking courses, blocking publication, preventing free speech, dismissal, harassment, blacklisting and smearing of reputations.

In the social studies of science, intellectual suppression in general, and denial of publication in an appropriate journal in particular, can be seen as a means of exercising professional power to reduce the credibility of those who question the results or policies of elite groups. For this reason, Martin (1991) argues that we should not be surprised that proper scientific processes and procedures and medical ethics are violated. What is at stake for elite pediatricians is their professional power (e.g. to determine recommended doses of fluoride to infants) and its possible erosion by the publication of dissenting views or the correction of work that the elite has already endorsed.

On the other hand, what is at stake from our viewpoint in this debate is the protection of infants from overdosing with fluoride, which entails several health hazards, most of which become manifest in later years. The only one which is admitted by most proponents of water fluoridation is dental fluorosis (fluoride-induced mottling of teeth), which first appears when the permanent teeth erupt. Dental fluorosis on early forming enamel surfaces is strongly associated with *inter alia* infant formula use (Pendrys et al., 1994).

The official line for public consumption is that, at the so-called 'optimum' fluoride intake 0.05–0.07 mg/kg F body mass/day, dental fluorosis at the so-called 'very mild' level of severity first appears in a small proportion of children, and that its severity increases to the 'mild' level around 0.1 mg/kg/day (Singer and Ophaug, 1979; Committee on Nutrition, 1986). However, some experts in dental fluorosis state in the dental literature that:

> ... a daily intake of fluoride as low as 0.04 mg/kg body weight can result in dental fluorosis of the permanent dentition. This amount is considerably below that which is usually referred to in the literature (0.1 mg/kg body weight). This is hardly surprising since a 'magic borderline' below which the signs of dental fluorosis are totally absent from all people does not in reality exist. (Fejerskov et al., 1988)

Incidentally, the same experts also reject Dean's classification of the severity of dental fluorosis—running from 'questionable' to 'very mild,' to 'mild,' 'moderate' and 'severe'—as being subjective. They do not accept the public relations claims that dental fluorosis is merely a 'cosmetic' matter, but recognize it as an indicator of physiological damage (Fejerskov et al., 1988).

In naturally fluoridated areas of several countries, skeletal fluorosis is observed. It is a disease involving changes in the structure of bones and calcification of ligaments, resulting from the ingestion and accumulation of fluoride over many years. Although there is little data on the daily fluoride doses of those suffering from this disease, skeletal fluorosis is observed in older people in some villages in India where the fluoride concentration in drinking water is as low as 0.7 ppm (Jolly et al., 1973; Diesendorf, 1990b). These patients would have been breastfed as infants and hence would have ingested then about one-hundredth the fluoride intake of infants fed on formula in fluoridated parts of developed countries (Ekstrand et al., 1981; Esala et al., 1982). The Indian patients would not have used fluoride tooth-

paste which is swallowed by many children in developed countries. So the fluoride doses of some of the Indian patients with skeletal fluorosis, at least during infancy and childhood, would have been equal to or less than those of many people living in artificially fluoridated areas of developed countries.

In developed countries, where most fluoridation has only existed for 2–3 decades, it is still too early to expect skeletal fluorosis to be a public health problem. But, there is already one indicator of fluoride damage to bones in developed countries: since 1990, there has been a growing body of literature indicating a higher rate of bone fractures in older people in artificially fluoridated than non-fluoridated areas Jacobsen et al., 1990; Cooper et al., 1991; Danielson et al., 1992; Jacobsen et al., 1992; Jacqmin et al., 1995). [There is also one study which observes no correlation (Suarez-Almazor et al., 1993).] In addition, a prospective study by Sowers et al., (1991) finds a higher rate of bone fractures in older people in a high natural fluoride region compared with a lower fluoride region.

The relationship of these data to the present work is that the uptake of fluoride into bone is greatest in infants and young children (Savchuck, 1951). Thus, infants who drink mainly powdered formula reconstituted with fluoridated water are likely to be a high-risk group for developing both skeletal fluorosis and hip fractures in old age. The fraction of infants falling into this category depends upon the degree of fluoridation in the country of interest, the age of infants and the relative popularity of powdered versus premixed liquid formula. This fraction will not be negligible in the USA (50 percent fluoridated) and Australia (66 percent fluoridated) where large fractions of mothers do not breastfeed infants aged over 3 months and powdered formula is widely available.

As far as allergic and intolerance reactions to fluoride are concerned (Waldbott et al., 1978), these could already be manifest in infancy in this high-risk group. But, there are no data, because most medical practitioners are taught incorrectly that these reactions do not exist.

Finally, we draw attention to a similar case of suppression of information about a high-dose and high-risk group in fluoridated areas: namely, people with kidney damage (Sauerbrunn et al., 1965; Juncos and Donadio, 1972) and in particular patients undergoing kidney dialysis. By the mid 1970s it was well established in the medical literature, on the basis of observations at artificial kidney units at several hospitals, that dialysis patients may experience severe bone diseases unless the water is defluoridated before dialysis (Posen et al., 1971; Johnson and Taves, 1974; Cordy et al., 1974). A few profluoridation reports and books acknowledge this hazard openly (Royal College of Physicians, 1976), but most ignore the issue (e.g. National Health and Medical Research Council, 1991; Murray and Rugg-Gunn, 1982). As an extreme example, the death was reported in Rochester NY of a patient who had been undergoing long-term hemodialysis with fluoridated water and whose bones were found to contain a high level of fluoride (Taves et al., 1965). But an earlier report of this death by a different set of authors did not mention fluoride (Kretchmar et al., 1963). When occasionally the hazard of using fluoridated water in hemodialysis becomes a public issue, some medical authorities even deny the hazard, in the face of all the evidence: for instance, a public statement by the then chairman of the New South Wales Renal Physicians' Committee (George, 1979).

CONCLUSION AND RECOMMENDATIONS

It appears to us that the most plausible reason for rejecting our letter is that it might assist the anti-fluoridation movement. Even if this is not correct, the editorial obligation to allow scientific debate failed in this instance. We therefore recommend that professional societies and editorial boards of scholarly journals examine the ethical principles and guidelines given to their editors. In the light of our experience, the experience of Menger and Haim (1992) and the rejection by the *Journal of Medical Ethics* of a paper proposing an iatrogenic mechanism for the origin of AIDS (Pascal, 1991; Gillon, 1992), it appears that existing guidelines may require revision. Such revisions should state strongly the obligation of editors to encourage alternative scientific theories and hypotheses to be debated and to correct erroneous, incomplete, or misleading results and recommendations in previous publications in their journals, especially when they have potential public health, social equity or environmental impacts.

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