Peer review of nitrous oxide reports

Ministry of Agriculture and Forestry
Te Manatū Ahuwhenua, Ngāherehere

BIOSECURITY
NEW ZEALAND

NEW ZEALAND
FOOD SAFETY
AUTHORITY

AGREEMENT MAF POL 0809-11159:

Peer Review of Nitrous Oxide Reports

by

Keith Smith

School of Geosciences, University of Edinburgh, UK

Final Report
25 February, 2009

1. Introduction

1.1 Peer reviews have been conducted on three reports:

- Review of New Zealand Specific $Frac_{GASM}$ and $Frac_{GASF}$ Emissions Factors (Project Code CC MAF POL_0708-72), by R. Sherlock, P. Jewell and T. Clough (October 2008);

- Incorporation of the Nitrification Inhibitor DCD into New Zealand's 2009 National Inventory (Project code CC MAF POL_0809-37), by T.J. Clough, F.M. Kelliher, H. Clark and T.J. van der Weerden, 31 October 2008;

- Review of IPCC 2006 guidelines to determine NZ inventory requirements from 2010, by T.J. van der Weerden, C.A.M.de Klein, F.M.Kelliher, H.Clark and K.R.Lassey, August 2008.

1.2 Interim comments on the first two reports, together with a series of completed "New Zealand Inventory Approval for change to emission factor, parameter or methodology" forms, were sent to Dr Gerald Rys on 19 December 2008. The comments on the Nitrification Inhibitor report led to a follow-up question and a request for an assessment form relating to the nitrification inhibitor methodology being sent by Dr Rys by email in late January, 2009; that email and my response to it (re-sent on 23 February 2009) are included here in this final review, together with all the material sent on 19 Dec.

1.3 My comments on the 3rd of the NZ reports listed above: Review of IPCC 2006 guidelines….", by T.J. van der Weerden et al., August 2008, were sent to Dr Rys on 23 February 2009. That material is also included in the present document.

2. Comments on "Review of New Zealand Specific $Frac_{GASM}$ and $Frac_{GASF}$ Emissions Factors (Project Code CC MAF POL_0708-72)", by R. Sherlock, P. Jewell and T. Clough (October 2008)

2.1 This review begins with an excellent coverage of NH_3 volatilisation from pastures.

2.2 The next section, on NO emissions, is also an informative and well-written section. The only comment I have here is that a potentially useful additional reference on NO emissions (including the impact of applying DCD) is the paper by Skiba et al., in *Soil Biol. Biochem.*, 25, 1527-1536, 1993.

2.3 The section on international data on ammonia volatilisation in pastoral agriculture is good, both in the comprehensiveness of the review, and in the discussion. One point that is well made is that a key difference between the NZ and European typical systems is that the proportion of dung and urine deposited directly onto pasture is much lower in NZ.

2.4 The review of New Zealand data on ammonia volatilisation in pastoral agriculture is very adequate, and I have no additions or changes to suggest.

2.5 The review of international and New Zealand data on ammonia volatilisation from fertiliser-N in grazed pastures, in Section 6, is also equally satisfactory. The evidence that the ammonia emission factor for urea is lower at low N application rates seems very clear, and the consequent judgement that the NZ FracGASF should be of the order of 10% is reasonable.

2.6 The review of international and NZ data on NO_x emissions from pastoral agriculture is, once again, a good and very adequate coverage of the subject.

2.7 General Discussion, Conclusions and Recommendations (Section 8): this section sums up the topics very succinctly. It makes a good case for consideration of adopting a NZ-specific value of 0.1 for $Frac_{GASM}$, but also makes an equally good case for further refinement by means of more whole-system measurements. Similarly, the case for adopting a NZ-specific value of 0.1 for $Frac_{GASF}$ is well made, and the proposal for work on the effect of urease inhibitors is reasonable. Re Scenario 1 (p. 44), however, I feel that neither the use of irrigation nor the timing of N application to coincide with rainfall is sufficiently controllable to be a factor that can be used to further amend EF values.

2.8 A final comment: I fully agree with the general approach of this Report, in which a strong body of data is provided to justify Recommendation 1, but there is a recognition that whole-system methodologies are better in principle and therefore that more research is desirable, using such methods to further refine the $Frac_{GASM}$ value (Recommendation 2). Likewise, the proposed adoption of Recommendation 3, a NZ-specific value for $Frac_{GASF}$, is based robustly on the weighted mean of a large body of work (19 studies). Recommendation 4 – to do research on the effect of urease inhibitors and then reconsider whether the $Frac_{GASF}$ value can be further refined – is also a sound one.

3. Comments on "**Incorporation of the Nitrification Inhibitor DCD into New Zealand's 2009 National Inventory (Project code CC MAF POL_0809-37)", by T.J. Clough, F.M. Kelliher, H. Clark and T.J. van der Weerden, 31 October 2008.**

3.1 The Summary at the beginning of this review covers the topics in the main text, but it would be much improved by inclusion of explicit statements of the new EF and FracLEACH values that are being recommended, as is done in the Review of FracGASM and FracGASF (Section 2, above).

3.2 Section 1 (Introduction), Section 2 (Nitrification inhibition: dicyandiamide) and Section 3 ("Good practice" application guidelines) are all clear and succinct summaries of the current state of knowledge. My only detailed comments are very minor ones in Section 1 contained in my annotations to the Review document.

3.3 Section 4 (Incorporating DCD into New Zealand's N_2O emissions inventory), and Section 5 (Incorporation of the mitigation methodology, revised emission factors and parameters into New Zealand's national greenhouse gas inventory):

My principal comments on these key sections are in the following paragraphs; additional minor points are included in my annotations to the Review document, where I have also identified those subsections which are perfectly satisfactory.

3.3.1 Revision of EF$_{3PR\&P}$

It is very clear that there is an impressive – and unique – body of literature on the effect of DCD on N$_2$O emissions from grazed pasture soils in New Zealand, and furthermore that the results clearly show a very substantial decrease in autumn/winter emissions in the presence of DCD. Thus the desirability of considering a modification of the relevant emission factor for inventory purposes, when DCD is used, is obvious. However, most of the studies cited in this report and some of those selected and put forward in support of this objective have involved the co-use of urea fertiliser and urine in lysimeter studies, or the application of urea to field plots that were also grazed. The research reflects the actual dominant agronomic system in operation in dairy systems in New Zealand, i.e. regular applications of synthetic N fertiliser to pastures that are grazed and thus also receive urine and dung throughout the year. However, in this particular context of seeking to modify the EF$_{3PR\&P}$ emission factor for N deposited during grazing, when DCD has been applied, this combined use of fertiliser N and urine N raises the problem of how to differentiate between the emission from the urea fertiliser (which would have an EF$_1$ emission factor) and that from the actual or simulated urine-N deposited on the soil, for which EF$_{3PR\&P}$ is used.

The material in Table 1 on p. 15 of the Report (used to calculate the average reduction in EF$_{3PR\&P}$ arising from DCD use), illustrates the problem. Three papers are cited: Di et al (2007), and Smith et al (2008a and b). In the first of these papers, its Table 2 (p. 3) indicates that the only N treatments used on the Lismore and Templeton soils were urine +/- DCD, but the Horitiu soil also received a total of 120 kg urea-N/ha (in 3 applications) and the Taupo soil 84 kg urea-N/ha (also in 3 applications). Smith et al (2008b) applied 50 kg kg urea-N/ha in one application in 2004, and 3 x 50 kg urea-N/ha in 2005, as well as subjecting the plots to grazing. Finally, Smith et al (2008a) studied a grazed kale crop, to which 30 + 96 kg urea-N were applied, then urine was applied to the ground (+/- DCD) after harvesting and pugging by leading a cow up and down. Thus the Report table combines results for two pasture soils with urine only, three pasture soils also receiving different rates of urea N, and a bare soil where the urea had almost certainly all gone before the urine treatment — in summary, 3 experiments with, and 3 essentially without, urea.

On a point of detail, I am unable to fully understand the calculations of Smith et al (2008a): they give an emission reduction for the urine patch (in their Table 1) of 41% (only significant at the 10% level), and mention a "26% reduction over the whole area", but also have values that give a reduction of 54% -- the figure used in Table 1 of the Report.

It is very evident from Table 1 in the Report that a great deal of consistency has been obtained in the magnitude of the decrease in N_2O emissions when DCD has been added in the way described, whether or not urea was also applied, and the scientific outcome – that emissions are greatly reduced whether or not urea was included – is beyond doubt. The results clearly indicate an effective inhibition of N_2O emission by DCD from urea <u>and</u> from urine, and therefore are a very clear stimulus to wider adoption of DCD treatment in order to reduce national emissions. However, although I am not familiar with the actual formal procedures involved in achieving a change in the value of $EF_{3PR\&P}$ that is acceptable to IPCC and UNFCCC, I think that for the application to go smoothly there is a need to use data that are solely derived from urine experiments <u>without</u> fertiliser N.

One gets the impression that the large body of work carried out with both urea and urine applications was undertaken for the best of reasons, namely to mimic as closely as possible the characteristic N-input conditions of NZ dairy pasture systems – but there is a need to demonstrate their individual contributions.

A way out of this difficulty would be to carry out plot experiments on N_2O emission, on grassland that has not been recently grazed but merely fertilised with urea +/- DCD, to derive the reduction factor for this component. The results could then be used not only for EF_1 calculations, where more data are desirable (see next section) but also for the calculation of the "urea" component of the combined emissions and their DCD-induced reduction, and thus the "urine" component also, in all the combined urea/urine experiments. The N_2O emission study of Luo et al (2007) involved urea N only, but unfortunately did not have a DCD treatment, but it could be a model for a new experiment which did.

Additional urine-only studies, on the soil types where only combined urea/urine studies have been carried out, are also desirable, to add robustness to a NZ-specific $EF_{3PR\&P}$ for inventory purposes. In the meantime, the mean of the two urine-only values of Di et al (2007), i.e. (67 + 73)/2 = 70%, hardly different from the mean of the six in Table 1, 67%, could be used, pending the acquisition of fertiliser-only (and urine-only) data, but this would be much less robust than a new emission factor based on a more representative range of sites, soils and conditions.

3.3.2 Revision of EF_1

This subsection recommends a decrease in the value of EF_1 that is the same as the decrease proposed for $EF_{3PR\&P}$, i.e. 67%, but I have considerable reservations about this recommendation, for different reasons from those expressed above. The proposed decrease has been obtained from averaging one result obtained in New Zealand and one in the UK. My concern about the NZ result is due to the fact that it comes from a brief (5-week) pot experiment, under glass, with urea +/- DCD applied to a horticultural crop (lettuce). In my opinion it will be hard to justify the application of such data to the situation pertaining to pasture grass throughout the year. Why weren't the results of Zaman et al. (2008) used instead? These authors carried out a 3-month experiment in Hamilton on a pasture on which no grazing had been allowed for the previous two months, and compared N_2O emissions from urea with (among others), a urease inhibitor, Agrotain, and a combination of Agrotain and DCD. The Agrotain decreased the emissions by only 5% but the combined inhibitors decreased them by 37%, and it seems reasonable to assume that the difference can be attributed to the DCD. Nonetheless, this would still only provide data for a single site, and so not be generally representative.

The last paragraph on p 25 says: "As noted above, no attempt has been made to calculate DCD mitigation of N fertilizer use in the dairy industry until monthly sector-specific fertilizer N data are available. However, once available it will be a simple matter of applying the DCD weighting factor in conjunction with EF_1 and the land area treated." This seems to conflict with the calculation of the DCD-induced decrease in emissions in Table 2 and the inclusion of the "Revision of EF_1" section.

Re Table 2, I see no problem in averaging appropriate NZ data with overseas data to produce a more robust average, but I wish to draw attention to the study by Dobbie & Smith (*Nutr. Cycl. Agroecosys.*, 67, 37-46, 2003). This provides another two years' data to add to that of McTaggart et al (1997), which <u>is</u> used in the Report. Dobbie & Smith's results are only in graphical form in their paper and therefore the numerical values are given in Table 1, below; they showed a 71.8% reduction of N_2O from urea over 12 months, with DCD, and a reduction of 46.2% the next year, with a mean over 2 years of 59.0%. It may well be that a single UK figure is preferred, before combining with NZ results; if so then consideration should be given to averaging the Dobbie and Smith data with the McTaggart et al. data.

The comments made in the previous section about the need for fertiliser-only studies would, of course, provide extra data that would make the estimate of DCD-induced changes to EF_1 much more robust, as well as helping to unpick the results from the combined urine/urea studies.

Table 1. Data from Dobbie & Smith (2003)

Fertiliser (Code)	1999-2000		2000-2001	
	kg N_2O-N/ha	SD	kg N_2O-N/ha	SD
Amm. Nitrate (AN)	11.6	3.4	16.0	4.1
Urea (UR)	3.9	2.3	9.1	3.9
Urea + DCD (UR(N))	1.1	0.4	4.9	1.4
Urea + urease inhib. (UR(U))	8.6	4.3	7.7	2.5
Urea + urease inhib. + DCD (SU)	3.6	2.2	13.2	5.6
Controlled-release urea (CR)	0.44	0.18	8.3	2.7
Control	0.37	0.15	0.33	0.16

3.3.3   A supplementary question was posed by Dr Rys, 29 Jan 2009, following receipt of the comments in 3.3.1 and 3.3.2 above: "…. We accept that there are not enough trials that have urine plus DCD alone to look at a urine emission factor that is not confounded by nitrogen fertiliser. The feeling for using this trial methodology is that it more clearly reflects farming conditions, and that the emissions from applied N fertiliser are so much lower that applied urine due to the rates applied. However, most of our trials include treatments: Controls (with N fertilisers); Urine+ fertilizers ; and urine + fertiliser + DCD. The difference between urine+fertiliser and fertiliser only treatments should give the emission factor for the urine. We will check the data and see if this can provide a correction factor. However we may not be able to identify any interactions if they exist. What do you think?"

My response is to suggest the following procedure for modifying estimation of N_2O emissions from grazed pasture when using DCD:

1. Determine the average % decrease in direct N_2O emission from applied synthetic N fertiliser (essentially from urea) (EF_1), when also applying DCD, using plots or lysimeters *which have only received urea* $\pm DCD$.

2. Similarly determine the average % decrease in N_2O emission from deposited urine ($EF_{3\text{-}PR\&P}$), using only urine $\pm$ DCD.

3. Use equation 3 (p. 24) in the CC MAF POL_0809-37 document, replacing EF_x by EF_1, to calculate the DCD weighting factor for EF_1, and

4. Use the equation *separately*, replacing EF_x by $EF_{3\text{-}PR\&P}$, to calculate the DCD weighting factor for $EF_{3\text{-}PR\&P}$.

5. Calculate the N_2O emissions from *the urine* deposited on grazed pasture according to Equation 4 (p. 26), using this DCD weighting factor for $EF_{3\text{-}PR\&P}$.

6. But in addition calculate the direct N_2O emissions from the fertiliser N applied to grazed pasture as Fert-N $\times$ EF_1 $\times$ DCD weighting factor for EF_1 $\times$ (44/28).

This separate calculation would explicitly deal with the separate contributing emissions from the fertiliser N and the urine N, respectively. The NZ country-specific values for EF_1 and $EF_{3\text{-}PR\&P}$ in the absence of DCD happen to be the same (0.01), but it may turn out that the % reduction in emission from the addition of DCD is not the same for fertiliser N as for urine N. Furthermore,

Eq. 4 on p. 26 is not appropriate for the fertiliser component of the total N additions to pastures, because it includes the "95%" correction to allow for that small part of the excreta not deposited on the pasture. So this is another reason for making separate calculations right through the process, and simply summing at the end to get the total emissions from all pastures.

Concerning the ideas raised in Gerald Rys's email of 29 January, I agree that in the trials results the difference between urine + fert and fert-only treatments should give the EF for the urine, but there would still need to be data on urine-only + DCD to derive the % reduction in emissions from the urine. Similarly, I can't see how the % reduction in emissions from the fertiliser on its own can be estimated without measurements based on trials which have fertiliser N as the sole N source, with and without DCD. If there are more data like those provided for the Lismore and Templeton soils in Di et al. (2007) (first two entries in Table 1 of the document), sufficient to allow a robust average % reduction in emission to be obtained for the urine component, that would deal with that aspect of the problem; however, there seems to be a shortage of data from experiments with fertiliser N only.

I think I appreciate the circumstances in which many studies have been done, reflecting the actual practice being adopted in NZ, but the outcome doesn't obviously fit in with the rather pernickety requirements of IPCC/UNFCCC procedures. Ideally, one would like to have a country-specific "grazed pasture EF" that in fact was the overall emission factor for the typical pasture receiving urine AND urea, in the ratio commonly employed. Then, the results obtained with/without DCD, on the pastures receiving this 2-component mixture, would allow a mitigation factor to be calculated, along the lines of what has been proposed. However, I believe that such an outcome would be much harder to achieve than actually acquiring the experimental data in the minimum necessary number of disaggregated experiments, with fertiliser N and urine N used separately.

3.4   Section 6 (Overcoming the potential barriers to mitigation technology impacts) and Section 7 (Future improvements in inventory incorporation methodology):

I fully agree with the interpretation of the science pertaining to the behaviour of DCD in the soil (p. 28).

It says on p. 29: "There may however be a limit to its [i.e. the DCD technology] adoption due to a lack of perceived pasture response compared to alternative pasture enhancing products such as nitrogen fertilizer." [But the cited research, e.g. Menneer et al (2008), contains clear evidence of a positive pasture response – more growth in the presence of DCD, thus achieving the equivalent effect to that arising from using more N – and in fact this sentence is counteracted by the conclusions contained in the next paragraph.

The last two paragraphs of the section constitute a very good discussion.

4.   Comments on "Review of IPCC 2006 guidelines to determine NZ inventory requirements from 2010", Report prepared for MAF August 2008", by T J van der Weerden, C A M de Klein, F M Kelliher, H Clark & K R Lassey.

4.1 In general, the Review is well-written, by established experts in the fields covered, and is very adequately referenced. It explains very clearly the changes made in the *IPCC 2006 Guidelines*, compared with the previous *Revised 1996 Guidelines*, calculates the impacts of the changes on the New Zealand GHG inventory in 2006, and makes predictions of the likely future changes in emissions by 2010 and 2020, respectively.

4.2 The findings in the Review have been used as the basis for arguing for additional research programmes, with the objectives of :

- better quantification of EF_5,
- better quantification of $EF_{3PRP\ SHEEP}$,
- the study of on-farm practices relating to pasture renewal and temporary land use changes such as supplementary feed production (literature review),
- better understanding and quantification of N transformations and subsequent N_2O emission factors for residues of lucerne, forage brassicas and renewed pasture residues and from soil organic matter due to land use change.

The arguments for these proposed projects are well made, and I fully endorse them.

4.3 There are only a couple of areas where I have a problem with the content. The first is in the 3rd paragraph on p. 13, where it says: **"It should be noted that the pasture renewal component of F_{CR} only accounts for additional N to soils from grass/clover stubble residue. Any additional N mineralisation of organic matter and subsequent N_2O emissions following soil cultivation and seedbed preparation would be accounted for under the new F_{SOM} category."** I do not see how the mineralisation of SOM during the pasture renewal can be distinguished from the N coming from mineralisation of the stubble/roots of the grass/clover – this would require isotopic labelling experiments, and I am not aware of any such work having been done to differentiate between these N pools. The 2006 IPCC Guidelines procedures are less than perfect in many respects, but on this particular aspect they offer what I consider to be an improvement on the 1996 version, by making a first attempt to cover a category that was previously missing altogether, and this is a step forward. My understanding of what is intended in the 2006 document is that any N released during pasture renewal should be categorised as F_{CR}, and not as F_{SOM}; there may well be some release from decomposing soil OM accompanying the release from the decomposing plant remains, but that is inevitably included in the F_{CR}.

4.4   The second area that I find unconvincing is the inclusion of the scenario in which the short period of cropping with forage brassica or cereal silage crops (for 6 months – 2 years), employed by some farmers during pasture renewal, before returning the land to pasture, is designated as temporary land use change. In this scenario, the release of N is calculated according to the IPCC procedures relevant to land use change, and thus included in F_{SOM}. The large additional N_2O emission calculated to result from this inclusion is 8.6 – (-2.0) Gg (Table 7), = 10.6 Gg. This emission is deemed to result from the annual average renewal of 300,000 ha of pasture.

Thus the emission per ha = $[(10.6 \times 10^6) / (300 \times 10^3)] \times 28/44$ kg N_2O-N

$$= 22.4 \text{ kg } N_2O\text{-N.}$$

If the emission factor (EF_1) = 0.01, then this implies that the amount of N released per ha = 2240 kg. If the C:N ratio used = 15:1, then the C release = 33,600 kg = 33.6 t. This figure is of the

same order as the C loss of 40 tonnes that "represents the total loss over a 20-year period" (Review, p. 19), so although I fail to reconcile these numbers exactly it seems clear that the calculations that have led to the values inserted in Table 7 apply to a period of 20 years following the disturbance.

I do not understand the reasoning underlying this scenario. Even if forage crops are grown before grass is resown, the period out of grass is brief : 0.5-2 years. Once the land is back into pasture there is generally a buildup of organic matter, involving a renewed accumulation of organic nitrogen. Yet the 3rd scenario calculation implies a continued release of C and N over 20 years, even though the land has long since returned to pasture. However, the literature indicates that the size of the mineralisation pulse is of the order of low hundreds of kg N per ha (see below), and its duration is relatively brief. The Guidelines are not particularly explicit on the time interval involved, but it is reasonable to assume, for inventory purposes, that all the N is mineralised in one year – when the area of land renewed remains constant, the calculated annual N_2O emission will be the same, no matter whether, in any one year, it is considered to be a complete pulse relating to that year's renewal, or a composite with part relating to the current year and part to previous year(s). Whatever the period used, the quantity of N mineralised, and therefore the estimated ensuing N_2O emission, is relatively very small compared with that calculated using the LUC criteria.

References on N_2O mineralisation during pasture renewal, and key values therein:

Francis, G.S. et al. (*J. Agric Sci*, 124, 1-9,1995): New Zealand: 107-131 kg N ha^{-1} mineralised after 4-year grass/clover;

Webb, T.H. et al. (*Aust. J Soil Res.*, 39, 1015-1025, 2001): New Zealand: leached N after ploughing grass/clover: 14-104 kg N ha^{-1}, so total N mineralised ≥ this range;

Davies, M.G. et al. (*Biol. Fertil. Soils*, 33, 423-434, 2001): Scotland: 244-449 kg N ha^{-1} mineralised over 18 months;

Hansen, E.M. et al. (*Soil Use Manage.*, 23, 348-358, 2007): Denmark: 171-256 kg N ha^{-1} leached after ploughing 3- and 5-yr grass/clover leys, so total N mineralised ≥ this range;

Ericksen, J. et al. (*Soil Use Manage*., 24, 373-382, 2008): Denmark: N fertiliser replacement values, when cereals followed ploughed swards (8-yr leys), (indicating amounts of mineral N made available to crop) = 121-130 kg N ha^{-1}.