SOILS and WATER QUALITY

1. PRELIMINARY COMMENTS

1.1 Soils information required in the EIS

The Department of Planning Director's Requirements (Annexure 1-1 Vol. A) for the EIS acknowledge that all activities will occur in an area of 60,000 ha, identified as least sensitive,

> "...with site-specific information providing the basis for consideration of likely environmental impacts."

With respect to soils, this has not occurred.

The EIS is also required to provide:

> **2. Description of the Existing Environment [cl.57(2)(c)]**
> d) a description of soil landscapes and erosion hazard of the soils which they include. A description of the influence of climate, soil erodibility, topography, land use and past and present management on current soil erosion rates. Advice should be obtained from the Department of Conservation and Land Management in this regard;
>
> **3. Consideration of the Likely Environmental Impact [cl. 57(2)(e)]**
> c) the impact on soils, in particular their physical and chemical properties, likely changes in rates of soil loss and long term sustainability;

1.2 Adequacy of Soils Information in draft EIS

In response to 2d), it is submitted that the soils information in the draft EIS draws on an eclectic mix of theoretical models, which rely on derived, subjective data in the absence of the necessary primary data.

The only information sourced to the Department of CaLM are the schematic drawings of soil landscapes (Attachment 2, Appendix 11) and descriptions of soil landscapes. In regard to the latter, it is worth noting that the relationship between the "soil types" classification system used in the EIS (Table 3 -12, p.3:33) and the "soil landscapes" and accompanying physical analysis of samples developed by CaLM for its Soil Data System and soil mapping program, is at best unclear, if not problematic.

In response to 3c), it is submitted that the draft EIS does not adequately or correctly consider the physical and chemical properties of soils, any changes in rates of soil loss or long term sustainability.

By way of brief example, the Universal Soil Loss Equation, which remains SFNSW only erosion hazard assessment “tool”, assumes that soil is lost only in the first year, then averages this loss over 40 years in order to arrive at an “acceptable” figure of soil loss in terms of tonnes per hectare per year. The EIS does not mention changes in rates of soil loss when integrated harvesting occurs in the same compartments twice within ten years, a situation proposed in the EIS, and already occurring in many LSA.

2. RESPONSES TO SOILS INFORMATION

2.1 Expert commentary has been provided on soils information contained in the draft EIS, by Mr. Jetse Kalma, Senior Principal Research Scientist, CSIRO Division of Water Resources, and his notes are attached in full at Attachment 1.

2.1.2 Expert input to this EIS response has been provided by privately engaged consultants who have worked for many years with CSIRO Division of Soils. This input included a comprehensive physical and chemical analysis of soil samples obtained from compartments in Mumbulla SF, which are regarded as being of “High Conservation Value” in the Department of the Environment, Sport and Territories’ “High Conservation Value Forests” project (1994), but classified as ‘Least Sensitive” of the LSA (red) by SFNSW in the same year.

2.2 We endorse Prof. Kalma’s opinion about the poor organisation of soils information in the EIS. (Attachment 1) The information is poorly arranged, requiring the reader to constantly move between tables, figures, chapters and volumes in order to access the essentially subjective data, which is invariably presented in complex and confusing ways, or worse, is missing.

For instance, research data from SFNSW’s own soil surveys are not included in the draft EIS. This data, referred to as Table 4 in Appendix 22, Volume C, has for some reason, not been included. Having obtained a copy of Table 4, it is apparent that all that is described are the sorts of analyses undertaken, but no information about sites, methodology or results of laboratory soil tests (done in only **one** [Yambulla] of the five survey areas) are given.

As noted by Dr. Hairsine in Appendix 22, there is no stated rationale for these soils surveys, no soil/ land maps have been produced as survey products (p.22:26) and, importantly, these studies relate to only a fraction of the areas defined as “least sensitive”, yet LSA’s are supposed to be the overall focus of the EIS.

2.3 Given the above comments, it is necessary to ask what should be expected of an EIS? Our courts have held that an EIS

> "..should be written in understandable language and should contain material which could alert lay persons...to problems inherent in the carrying out of the activity." (Prineas v Forestry Commission of New South Wales and Others (1982) Land and Environment Court of NSW per Cripps, J.)

It is reasonable to assert that this EIS, by virtue of its ommissions (in particular, important primary data), generalisations, contradictions and dubious use of reference sources, does alert a reader to the existence of many problems inherent in proceeding with the proposal.

3. SOILS INFORMATION IN THE EIS

3.1 Sources of soils information

There are three major sources of soils information in the proposal ;

i) Catherine Hird and Associates - acted as consultants for SFNSW with regard to soils information relied on in the draft EIS. Unfortunately, the professional backgrounds and expertise of the consultants in forest soils can only be guessed, since no information of this nature is provided in the draft EIS.

C. Hird & Associates are currently providing a consultancy for Bega Valley Council in relation to the reuse of sewage effluent.

ii) CaLM - Soil Conservation Service. The schematic drawings of soil landscapes attributed to M. Tulau (Attachment 2, Appendix 11) are presented without supporting field data or description. To both informed and lay readers, they are rendered meaningless.

iii) CSIRO Division of Soils: Consultancy - Final Report prepared by Dr. P. Hairsine and Ms. R. Hook: **"Review of erosion hazard prediction methodology and assessment of adequacy of NSW Forestry Commission soil data in the SE Region for use in an appropriate hazard prediction model."** (17th. June 1993)

Although it might seem trivial, it is worth noting that this work is included as Appendix 22 Volume C, but entitled "Review of Erosion Hazard Prediction Methods and Assessment of the Adequacy of Existing Soil Data for Erosion Hazard Prediction in the Study Area for Eden Management Area Environmental Impact Statement."
Dr. Hairsine was somewhat surprised that this paper was included in the EIS (pers.communication).
The use of Dr. Hairsine and Ms. Hook's work in this EIS is addressed in detail later in this response.

3.2 Assessment of Erosion Hazard

3.2.1 The selection process for the LSA's did not include any assessment of Erosion Hazard . Why was this ignored?

3.2.2 Throughout the EIS, reference is made to a number of methods for assessment of the erodibility of various soil-types, and the consequent erosion hazard. The scientific basis for these is not discussed. Instead, 'land units' are assigned an erodibility assessment - low, moderate, high and extreme - in an essentially subjective process, rather than one based on laboratory testing of a large range of physico-chemical properties for each soil sample.

3.2.3 Slopes - the Slope Classes map (Fig. 3.3 p. 3:7) and accompanying information (Table 3:3, p. 3:2) can leave the false impression that erosion hazard bears a linear relationship to slope. In fact, slopes defined as moderate (less than 19.9 degrees) may include areas of low, moderate and high erosion hazard.

Typically, the information used for Fig. 3:3 is derived from broad-scale parameters that take no account of local variations. Of greater concern, however, is the reliance on derived models which are nothing more than approximate guesses about critical primary data, such as topsoil and subsoil erodibility, soil properties etc. In the absence of verified scientific laboratory analysis of soil samples, the EIS presents estimates of soil properties, which are then treated as though they represent confirmed laboratory results.

How can a reader be confident that, for instance, 91% of the Least Sensitive Areas to be logged fall into a 'moderate' erosion hazard? (Table 3-19 p.3-49) What of the 11,686 ha (19.6% of LSA's) classified as high-extreme erosion hazard? Is this suitable for a high intensity land-use like integrated harvesting? According to the EIS, such extreme erosion hazard is not an impediment. Why is this the case?

3.2.4 Dispersion percentages are a critical variable in any assessment of soil erodibility, yet the dispersibility of soils in the EMA/LSA has not been discussed in the EIS. Why ?

3.2.5 K factors: Why does the EIS rely so heavily on imputed values in non-specific "models", "types", "unique areas", and so on, instead of analysis of raw field data?
Why are no examples of the results of laboratory analysis of soil physical properties in the EMA present anywhere in the proposal?
Estimated K factors are heavily relied upon to predict erosion hazard. Why have some schematic drawings developed for the CaLM Soil Landscapes Mapping program been included, but the available field data, on which these drawings are based, excluded?

A plausible conclusion is that for instance, the K factors or the dispersion percentages in the EMA may in fact raise the erosion hazard in many areas.

Evidence that this is the case is not difficult to find. The example Harvest Plan included in the EIS (Compartment 2164 Mumbulla SF; Appendix 8, Vol. C):-identifies two soil types, and states:-

> "Soil has been analysed and K factor determined at 0.025. Local evidence suggest risk of erosion from concentrated flow............Analysis result [for second soil type] gives K factor of 0.020." (p. 42)

The reference to soil analysis gives the impression that this has, in fact, occurred.

However, in the Soil Conservationist's Field Inspection Report to the Regulatory and Public Information Committee (letter from Mr. Charles Bell to RAPIC, 5.9.94), it is stated in relation to Compartment 2164:

> "The Compartment was not inspected as an adjoining compartment with similar soil type and topography has been inspected."

There is no mention of soil sampling, and had the Soil Conservationist inspected the Compartment, it would have been apparent to him that it does not "adjoin" the compartments referred to (2180 & 2181), which are some ten kilometres to the east, with very different topography.

The Soil Conservationist's Field Inspection Report to RAPIC (Mr. Charles Bell,1/8/94) in regard to these two compartments states:

> "An erosion hazard assessment was calculated...but the k factor has been overestimated. A more accurate figure for k is 0.02, not 0.04." (iii; p.1)

There is no further supporting evidence and no explanation as to how the alleged over-estimation arose. The effect of a reduction in k is to reduce the assessed erosion hazard.

Samples obtained for the CaLM Soil Landscape Mapping program give K factors in the approximate area of Compartment 2164 ranging from 0.025 -0.055. (Interpretation: 1st. March 1994, Soil Conservation Service)

It is of grave concern that information contained in Harvest Plans to guide logging operations purports to be based on laboratory analysis of site-specific soil samples, when this clearly has not occurred. If soils had been analysed, as stated, who undertook the sampling and analysis and where are the results?

Further, given the multifactorial impacts on soils during and after harvesting, any responsible assessment of what can happen once major disturbance occurs must rely on more than K factors. After all, a beach sand dune has a K factor of 0.000, but no-one would argue that sand dunes are not erodible.

3.2.6 **"Soil types"** (p. 3.38) actually tell the reader very little about the nature of the soils or their properties. For instance, average depth of topsoils in the EMA/LSA are not mentioned.

The fact that similarly described soils occur across a range of soil landscapes in the EIS, could lead a reader to believe that the different soil landscapes share similar soils, with homogeneous physical and chemical properties. This is an untruth brought about by poorly designed typographies, and left unchallenged by the complete absence of any scientific discussion of soil chemistry in the EIS.

Simply asserting that

> "..data on the physical properties of...soils in the study area are very limited..." (p.3:39)

is no justification for guesswork, especially when it is considered how many years SFNSW have been maintaining this position (see EIS for EMA 1988 and Supplementary EIS' for 1990, 1991, 1992).

3.3. A Reader's Quandary:

If, as claimed in 1992, a

> "...study is under way (sic) using Commonwealth Soil Conservation funding to study changes to physical soil properties on different sites including areas within the Eden Management Area" (Assessment Report 1992 Supplementary EIS, Forestry Commission of NSW, p. 12)

where is this research discussed in the draft EIS? What has been the product of the Commonwealth funds? How can changes be detected when the original physical and chemical properties of forest soils are not known and poorly understood?

Furthermore, why does SFNSW require additional public subsidies for basic research (which many would consider should be done <u>before</u> a major land use like integrated logging is undertaken), when it is supposedly a sustainable, profit-making enterprise?

According to the NSW Minister for Land and Water Conservation (Mr. George Souris):

> "State Forests had reported a realised operating profit of $27.6 Million for the year ended June 30th [1994]". (Bega District News 9/12/94)

How much of this profit is directed into credible research in the EMA?

3.4 The soils experts engaged by SFNSW have recommended that there is still a need in the EMA to undertake rigorous and extensive research into forest soils. (Hairsine and Hook: Appendix 22; Catherine Hird & Associates, Appendix 11, Volume C).

Where in this EIS is this research need addressed? And what is the point of publicly funded research when it would appear that SFNSW uses this material in a highly selective manner, and ignores any findings that do not easily fit their proposals for forest management?

4. Methods for Predicting Erosion Hazard

In this section, attention will be focussed on Appendices 11 and 22 (Vol. C), since these constitute the major source of soils information used in the draft EIS.
Appendices 11 and 22 are referred to in Table 1.2 List of investigations and technical studies **specifically carried out to support this EIS.** (p. 1:22; emphasis added)

The work of Dr. Hairsine and Ms. Hook (Appendix 22, Vol. C) was **not** "specifically carried out to support this EIS". The research neither supports nor rejects the proposal contained in the EIS, but reviews the available literature both in Australia and overseas, and assesses the adequacy of SFNSW soil data for the purposes of predicting erosion hazard.

The fact that this work is referred to only twice in the Main Report, and then in a misleading fashion, and the fact that the major recommendations receive no comment in the EIS, makes it highly misleading to refer to it in the manner of Table 1.2.

4.1 Soil Classification Models and Methods for Assessment of Erosion Hazard.

In 3.2.6.8 (pp.3:46-3:47), a system of land classification is proposed that denotes 'land units', assumed to be homogeneous across a range of environmental parameters.

> "Within these relatively uniform land units it is then assumed that erosion processes...will also be relatively uniform."

This assumption sounds more like a dangerous conceptual leap, about which strong reservations have been expressed (see Attachment 1, pp.2-3; Appendix 22:28) .

The following is a description of the methodology used for erosion hazard assessment, resulting in Tables 3.16 and 3.17, and Figure 3.12.

> "The relative susceptibility of land units to different processes of erosion was determined subjectively by considering the properties of a land unit in relation to their importance in erosion processes.........the uncertainty associated with local erosion hazard predictions makes them unsuitable for planning at the local scale. Furthermore....no method can predict all forms of erosion." (p.3:47)

In Appendix 22, Dr. Hairsine et al note, in regard to regional methods of erosion hazard prediction:-

> "The process-based models reviewed are generally too complex for application at a regional scale....there appear to be too few data linking land/forest type with hydrologic regime for a map of relative risk zones to be produced...". (22:27)

Therefore, what credibility and accuracy can a reader attach to Table 3.16 & 3.17, let alone Figure 3.12, which purports to be a pictorial representation of erosion hazard across the EMA?

Hairsine et al continue:-

> "The alternate approach of assessing inherent susceptibility of pre-defined land units, to erosional processes is also not immediately implementable due to the lack of a systematic definition of land types, even at a broad level, throughout the region......... This lack of a complete system of classification of the land within the SE forests, on the basis of all major land characteristics, is regarded as a major deficiency impeding the implementation of an appropriate regional method."(22:27-28)

Although it may be thought that the land-unit classification system developed by Catherine Hird and Associates (see Appendix 11 Vol. C and Section 3.2.6 Vol. A) goes some way towards meeting the above criteria, there are major flaws in the approach described in the EIS (see Attachment 1).

Hairsine points out that

> "...soil mapping is not the entire solution for most of the quantitative methods because **soil attributes, such as permeability and erodibility are not always well correlated with soil classificatory units."** (22:28; emphasis added)

He recommends direct in-field assessment of soil attributes, and contrary to the implication in the EIS (p.3:47), that since no one method can be universally applied, the USLE is 'good enough', Dr.Hairsine actually proposes

> "...that separate approaches for assessing erosion hazard be developed for the three forms of erosion.." (.22:28);

namely sheet/rill, gully and mass movement erosion.

The need to know local subsoil erodibilities is emphasised, and it is stressed that information about soil attributes <u>cannot be derived</u> in the absence of appropriate sampling and testing. (22:31; emphasis added)

4.2 Universal Soil Loss Equation (USLE)

One of the most alarming features of the soils discussion is that soils are considered primarily in terms of 'soil loss'. Soils are far less renewable than this EIS assumes. The intense activity of harvesting and regular burning means that soils cannot have time to be created, especially in dry sclerophyll forests (p. 6:25).

Is this the reason for the focus on soil loss, as opposed to the soil creation required by the <u>Forestry Act</u> (1916), which directs SFNSW to "..maintain and improve soils..."?

Nowhere in the Forestry Act (1916) does it state that SFNSW are permitted to cause the loss of 40-400 tonnes per hectare per year - the “acceptable” soil loss as calculated by the USLE.

4.2.1 Adequacy and Application of the USLE

The EIS states in relation to Appendix 22:

> “At the local scale, they noted that no method could predict all forms of erosion, but that the Universal Soil Loss Equation (USLE) as adapted to forested lands in the United States, is the most readily applicable quantitative method for local erosion hazard prediction. CaLM and SFNSW, when developing the Standard Erosion Mitigation Guidelines (SEMGL), considered that adoption of the USLE was the most practical method of estimating soil erosion hazard.” (p.3:44)

It is surprising that the only statement drawn from Dr. Hairsine’s paper for inclusion in the EIS should be this highly abridged version of the suitability of the USLE.

In fact, Dr. Hairsine stated:

> “The USLE as adapted to forestry application in the United States is identified as **currently** being the most readily applicable quantitative method for local erosion hazard prediction. **However, being based on Hortonian overland flow, its use is severely limited as it is only capable of reliably predicting sheet and rill erosion where the run-off is clearly defined.”** (22:3; emphasis added)

In this context, Dr.Hairsine states importantly that

> “...the USLE as adapted to **roads** will be a useful tool for predicting soil loss from them, **through sheet and rill erosion**.” (22:28; emphasis added)

The USLE is of no benefit in predicting gully erosion and this is identified as a major limitation.

> “Gully erosion is a complex phenomena which depends on landscape-scale hydrological processes as well as subsoil erodibility. Models and methods which have been developed for its prediction have generally been empirical and thus site-specific. For this reason, the authors consider it inappropriate to use such methods without extensive local validation.” (22:29)

The Executive Summary in Appendix 22 concludes:-

> **“It is the opinion of the authors that the currently available soil data are inadequate to support any reliable erosion hazard prediction either at a local or a regional scale.”** (22:3)

In the face of such expert evidence and opinion, how is it that SFNSW and CaLM should consider the USLE as "the most practical method of estimating soil erosion hazard" ? Even if it is 'practical', what scope is there for reliability, accuracy, application and the precautionary principle supposedly guiding this EIS?

Have all SFNSW personnel involved in preparing Harvest Plans actually read and understood the original research article, which detailed the complex interactions between the factors considered in the soil loss equation? (see: A Guide For Predicting Sheet and Rill Erosion On Forest Land Dissmeyer, G.E. and Foster, G.R USDA - Forest Service, Southeastern Area, Atlanta, Georgia. Technical Publication SA -TP 11)

If the EIS is correct, a reader would have to assume that the answer is no.

> "At the present time, the USLE relies on a number of **estimates** for some factors pending further development of the equation for use in forested areas.
> Overall, the use of the USLE remains limited to determining long-term average erosion levels, and it is therefore of limited use at the field management level."
> (Appendix 24.3 Vol. C; emphasis added)

It must be asked whether it is negligent to allow the continuation of integrated harvesting of a further 60,000 ha, "..pending further development" of a model that is acknowledged to have little application where it is most needed - in the coupes being harvested.

Yet, this EIS proposes that

> "...under the SEMGL, the current version of the USLE is used and will be applicable during the currency of this EIS." (24.3)

SFNSW have stated in the EIS (24.4) that the USLE is only applied to roads and snig tracks. How are calculations for soil loss from other parts of the harvesting coupe determined? After harvesting and the post-logging burn, close to 100% of the coupe has experienced major disturbance. As inspection of any site of active harvesting would confirm, the claim that only 14% of the coupe (Table 6-2, p. 6.15) is disturbed is nonsense. Post-logging burns do not usually occur for at least a year after completion of harvesting, which means that sites and soils are subject to prolonged disturbance and exposure.

4.2.2 Rainfall Intensity and Rainfall Erosivity

Calculations for the rainfall erosivity factor in the USLE are based on the work of Rosewell, C.J. and Turner, J.B.: Rainfall Erosivity in NSW (Technical Report No. 20. Department of CaLM. NSW 1992)

This Report is apparently out of print and even the CaLM officer seconded to SFNSW

> "..to assist with and advise on the implementation of planning initiatives related to soil conservation...." (p. 3:28),

is trying to obtain a copy . (pers. commun. Mr. Charles Bell, 11-1-95).

For reasons unknown, it has been impossible to obtain a copy of Technical Report No.20. However, rainfall erosivity is a factor taken into account in calculating the USLE. The minimal information provided in the draft EIS indicates that the rainfall erosivity factor applied in the USLE is derived by reference to a 1:1,000,000 map of rainfall intensities, and manipulating one (1) hour rainfall intensities, averaged over thirty (30) years (p.3:19).

Note that the SEMGL (Appendix 7, p. 7-17) 'Glossary of Terms' defines rainfall erosivity as

> "A measure of the ability of rainfall to cause erosion. It is the product of rainfall energy and maximum 30 minute intensity for each storm (refer Fig. 2, Rosewell and Turnover (SIC), 1992)."

The discrepancies between the duration of rainfall intensities applied to calculations for rainfall erosivity are neither explained nor acknowledged. There is similar confusion as to whether or not rainfall defined as 'storm events' is included in the calculation.

Calculations could have been based on durations of 6, 12 and 24 hour periods, which arguably would have been more relevant to the EMA, and in particular the higher rainfall coastal areas of the LSA. Further, no account is taken of drought periods in calculating either rainfall intensity or rainfall erosivity. It is common knowledge that significant rainfall intensity is a notable feature of hydrologic regimes in the EMA, and rainfalls of 300 mm in 24 hours are not an uncommon occurrence.

As Doeg and Koehn (1990) have demonstrated, average rainfalls are of little assistance in predicting erosion hazard when it is known that a substantial portion of the annual rainfall occurs in an extremely short period of time, such as 12-24 hours (p. 40).

The authors also argue that it is unreasonable to assume that factors such as geology, soils, slope, vegetation, climate, operations (e.g. adherence to prescriptions) and chance events (e.g. fire, cyclones),

> "...will be the same in any two catchments, even if they are contiguous, let alone over a large geographical area." (p.40)

They conclude that;-

> "...the actual results (in terms of absolute values) obtained from any forestry study, and particularly those from traditional paired-catchment studies, are probably restricted to the specific catchment." (p. 40)

With regard to SFNSW soil studies, and as noted previously, only the Yambulla research has involved any physical measurements directed at obtaining information on soil erosion, the EIS notes:

> "Many of the tributary streams within the granitic catchments of Yambulla SF have channels embedded in bedrock, with a low potential for lateral stream migration." (p.3.44)

The EIS states that soil disturbance was assessed in a total of 41 coupes in the EMA, 37 of which are in Yambulla SF. (6.3.2.2., p. 6.15) This area cannot be considered representative of the entire EMA, nor should any research data generated from this area be applied to the entire EMA.

Yet, the EIS maintains that:-

> "Catchments located in Yambulla and Tantawangalo State Forests, and applications of the resulting data are pertinent to most of the catchments of the EMA that contain dry sclerophyll and moist sclerophyll forests, respectively." (3.50)

This is a reckless statement.

4.2.3 Vegetation and Groundcover

> "The natural regeneration process quickly restores vegetation and litter cover, which stabilise the site following disturbance." (3:44)

This statement can only be described as wishful thinking, since no scientific studies are discussed which support this assertion, and field evidence would suggest that this is not the case. The impact of broad area prescribed burns and post-logging burns, the drying effect of increased wind velocity in logged coupes and the effect of soil compaction can combine to make it extremely difficult for groundcovers to regenerate, as well as new trees. This is especially the case on former snigging tracks and access roads, which are still identifiable as such and remain relatively unvegetated after 30 or more years (6:9)

Yet the EIS goes on to optimistically state that soil

> "..exposed on snig tracks provides a receptive seed bed for eucalypt seedlings to become established." (6:17)

Elsewhere in the EIS, a time-span of five to ten years is mentioned for forest floor rehabilitation (6.25).

The USLE relies on an estimated value for Cover Factor (C). In Harvest Plans in the EMA, this factor is always given as a standard 0.45. The lack of variability applied to the C variable is an inherent limitation. How is the factor 0.45 derived? What values are

given to the five sub-factors that are the basis for C? Should these be standardised? Is this an example of an "estimated factor" in the USLE?
The answers cannot be found in the EIS.

4.2.4 Nutrient Loss

The EIS asserts that

> "...nutrient losses caused by harvesting, burning and erosion are not expected to affect site quality, especially given the management intent of extending the rotation length of future cutting cycles". (ES:9)

This is an extraordinary statement in view of the fact that alternate coupe-cutting cycles are occurring in LSA's within ten, and sometimes less than four years of integrated harvesting of the first coupes. What is the accumulative effect of integrated logging in the same compartments over a very shortened period of time?

It should also be obvious that future cutting cycles will have to be greatly extended for the practical reason that the timber resource will have been exhausted.

Nutrient loss from forest soils, and accompanying soil erosion, is a documented problem (see Attachment 1). The concerns do not only relate to tree removal during harvesting, but also the impact of regular burning on groundcovers (including leaf litter) and the required species composition necessary for nutrient-replacement in the regenerating of forests.

<u>5. SOIL PHYSICO-CHEMICAL PROPERTIES</u>

5.1 Soil physico-chemical properties are addressed in Section 3.2.6.5 (p. 3:40 - 3: 42). The information presented is precisely that given in previous EIS' for EMA, based on research by Kelly and Turner [1978]: "Soil nutrient - vegetation relationships in the Eden area, NSW." (1: Soil Nutrient Survey, Aust. For. 41: pp 127-134) .

It is claimed in the draft EIS that this paper

> "...provide(s) information on the physico-chemical properties of soils in the study area." (p.3:40)

Yet, in 1992 , SFNSW claimed:

> "Detailed soil data are not available for the region or indidual compartments, but **observed** correlations between soil characteristics and geology (Kelly and Turner 1978...) provide a working knowledge of the forest soils and their distribution. This guides the prescriptions to be used. The classification is revised if field inspection shows that a classification is inappropriate." (Assessment Report - Supplementary EIS 1992, p.12; emphasis added)

It is seventeen (17) years since this research was published. The fact that it is re-presented in the draft 1994 EIS suggests that no further research has been done that involves

analysis of soil physical/chemical properties. Kelly and Turner's paper has a specific focus on vegetation and soil nutrients, and the chemical description is very basic (see Table 3-15 p. 3:41 **adapted** (emphasis added) from Kelly and Turner [1978]).
This research does not address the interactions between soil physical properties/soil chemistry, and disturbance leading to sub-soil exposures, such as occurs during integrated harvesting, nor was it intended to.

It must be concluded that SFNSW have no research that could provide an indication of what happens to forest soils in the EMA /LSA as a result of logging. Further, there is no comprehensive research which directly assesses soil erodibility using appropriate laboratory analysis, or which tests correlations between soil chemistry and soil erodibility. Since this is explicitly required of the EIS, it is unacceptable that SFNSW responds by yet again, presenting the same irrelevant research data.

5.2 Chemical Analysis of Soil Samples in a LSA

Due to the lack of soils information provided in Harvest Plans, a soil chemist was privately engaged to provide soil chemical analyses for samples obtained from a number of sites in a LSA within Mumbulla SF. Mr. Ian Little worked with the CSIRO Division of Soils for many years and is a highly respected soil scientist.

More than 80 samples taken at depths of 0 -1600mm, over a 5 kilometre sampling zone, highlighted the fact that soil physical properties and soil chemistry are not variables that can be guessed or derived, either though broad-scale soil typing, geological assessment , or by the field 'bolus test', wherein a handful of dirt is wetted and squeezed in the palm. Soil chemistry is far more complex and variable than this.

The chemical analyses of soils sampled from sites within this LSA showed an extremely high level of sodium. Sodium, when coupled with low calcium typical of forest soils, acts as a dispersive agent. Not surprisingly, the dispersion percentages -an important measure of soil erodibility that is not detailed in the EIS - were correspondingly high.

The analysis showed that:-

> "...there is no relationship, either general or partial, between dispersibility and sample depth. Hence it cannot be said that only the subsoils will be dispersible and that these will be protected by a stable surface cover." (pers. communication Ian Little 1/12/94)

The conclusion was that the sampled soils would be very easily eroded if disturbed. In terms of site-specific information, this data is the best available on soils in this LSA.

During the sampling process, evidence of past soil erosion was evidenced in coupes logged between 1979-81. In many places, it was extremely difficult to obtain samples, even from mid-slope, since it appeared that most of the soil was gone. What was left consisted mostly of gravel and rock.

6. IMPACTS on WATER QUALITY:

Section 6.5 (pp. 6.37 -6.46) ostensibly addresses impacts of forestry activities on water quality, but the reader is alerted to the fact that very little is known. However, this has not limited the assertions made in the EIS about the

> "....little measurable impact on water quality or sediment delivery" (p. 6.12),

expected after logging.

It has been pointed out elsewhere that SFNSW's approach to water quality monitoring is dominated by

> "..grab - sample data [which] generally misses the major storms entirely." (EPA: Memo 14/3/94)

Experience that could be appplied to water quality monitoring in forests is ignored;

> "Many of the lessons of urban stormwater pollution seem not to have transferred across to forestry, and old battles are being re-fought -as though the physical principles involved did not apply to State Forests." (EPA Memo 14/3/94)

6.1 Water Quality Monitoring

The assertion in the EIS about high variability of water quality in forest streams should not excuse the lack of appropriate, catchment-specific monitoring. Yet no water quality monitoring sites are proposed for much of the LSA during the period of this EIS. This would seem to place SFNSW in a very tenuous position with regard to its current Pollution Control Licence.

Under 8.3.3 -Water Quality Monitoring, the EIS states:

> "SFNSW proposes to continue its water quality monitoring program in the EMA." (8:7)

Seventeen (17) 'routine' water quality monitoring stations are listed in Table 8.2 (8:8), in addition to the Tantawangalo and Yambulla Hydrology Research Catchments. All these sites are in the southern part of the EMA. Yet many of the LSA are in the northern section of the EMA, in the coastal forests.

There is no additional water quality monitoring proposed in the EIS for these areas.

Is it possible that SFNSW have forgotten their responsibilities under the Pollution Control Licence? In a discussion of 'External Licensing Controls', the EIS states:

> "The Pollution Control Licence requires immediate reporting of water pollution incidents, compliance with the Code of Logging Practice, provisions to minimise the pollution of waters and post-operational audits by SFNSW." (2:79)

Yet, in 'Part 6: Monitoring', the Pollution Control Licence (dated 8/8/94) clearly states:

> "42 :(2) State Forests must indicate on each harvesting or operations plan the water quality monitoring site which is representative of the logging operations in the compartment or operations area.
> (3) Where there is no water quality monitoring site that is representative of a particular logging operation, State Forests must propose additional monitoring to be undertaken for that operation."

As it stands, the EIS proposes that the water quality monitoring sites in the south-west of the EMA are 'representative' of the small catchments common to the coastal ranges over 100 kls. to the north-east. Is this satisfactory?

EPA staff have commented -

> "...upstream water clarity in the granite/sand bed-load streams of the south-east forests is almost invariably crystal clear even in large storm events. The natural variability is **far** less than the logging-induced variation. Is this really so surprising? 'Integrated harvesting', with 90% tree removal rates [and then coupes are burnt] on steep, highly erodible/dispersible soils, equates to "land clearing" at its worst, in terms of environmental impacts." (EPA Memo 14/3/94)

This is not new information. In May 1993, staff from the Department of CaLM indicated that current integrated harvesting in the EMA is

> "....probably too intensive for the erosion hazard of the land, especially by virtue of soil type and climate." (EPA Memo 10/6/93)

7. EROSION MITIGATION

7. 1 Standard Erosion Mitigation Guidelines for Logging (SEMGL 1993) and Universal Soil Loss Equation (USLE)

Both forestry practice and this draft EIS place great reliance on the Standard Erosion Mitigation Guidelines for Logging (SEMGL; Appendix 7, Vol. C) to mitigate against a variety of erosive forces and the high intensity impacts of integrated harvesting.
The problems and serious limitations of the USLE have been discussed. It is of great concern that the SEMGL are also the subject of much criticism and scientific challenge, across a range of professional areas and government agencies -e.g. soil scientists, hydrologists, soil conservationists, CaLM, CSIRO, EPA.

The capacity of measures like the SEMGL, based on the USLE, to minimise erosion, has been questioned by many experts. The serious conceptual and practical flaws in approaches which attempt to statistically average major soil loss in the first twelve months, over 40 years, are further compromised when it is realised that in much of the LSA, there has been contiguous coupe logging over a shorter term, and this will continue during the period of this EIS.

> "The underlying philosophy of the SEMGL's is that they are to achieve soil loss rates over a 40 year logging cycle. C&LM are using 10 tonnes/ha/yr as a sustainable long term soil loss rate objective. They will manage to achieve the sustainability objective in theory by allowing a soil loss rate of 400 tonnes/ha/yr from the coupe in the first year and by assuming the rehabilitation is complete within twelve months. Thus, by assuming nil soil loss over the next 39 years, when the coupe is logged again, they get their desired average 10t/ha/yr over a 40 year period.....
>
> There are a number of difficulties with this. Firstly, true natural soil loss rates in undisturbed forest are given by C&LM elsewhere as less than 1 tonne/ha/yr. It is difficult to avoid the conclusion that the threshold has been shifted upwards to enable the SEMGL philosophy to be satisfied. Secondly, the completion of rehabilitation within twelve months is questionable." (EPA Memo: 10/6/93)

Note that the draft EIS states, in regard to forest- floor leaf litter and groundcovers,

> ".......that recovery to pre-logging state typically occurs...within ten years in the drier forests..."(6.3.3.2, p. 6.25),

and in regard to soil erosion,

> "Indications are that erosion rates are likely in most cases to return to natural rates within five to ten years after logging.." (6.3.1.1; p. 6.12)

Erosion mitigation techniques outlined in the SEMGL have been criticised on grounds of incorrect input data and unanticipated consequences. For instance:-

> "...the aim of cross-banks on snig tracks is to divert surface water to undisturbed ground [and thus reduce 'slope' length in the USLE....]. In the Eden operations however, there is little undisturbed ground. The cross-banks may in fact be making the problem worse by raising a hydraulic head, concentrating flow, and causing sub-soils to be exposed. This has apparently been a concern to some foresters all along. When quizzed...they stated they continued with the practice simply because "it's in the Codes"...........
>
> Additionally, the methodology is based on soils which do not have a propensity for gully erosion, unlike Eden district. USLE underestimates soil loss in such situations.........
>
> The issue of constraining snig track construction to lower erosion hazard situations may overlook the reality that snig tracks are effectively formed

unintentionally simply by the logs being dragged across the ground. As expected, sub-soils are commonly bared in the process, with erosion-prone channels forming downslope..........

> Finally, there is no attempt to look at the sustainability of impacts on water resources. The proposition that a catchment stream being loaded up with roughly 400 tonnes/ha of sediment is OK if you average it out over 40 years - appears dubious, to say the least....the SEMGL's suffer from the lack of a catchment/water quality perspective and may even in some circumstances unwittingly increase water pollution....C&LM have been stressing the need to collect and shift stormwater off the site as smartly as possible. This approach, combined with the high Eden level of sub-surface water flow which is brought to the surface by road/snig track interception, will affect catchment hydrograph characteristics and increase stream- power, with subsequent siltation from stream bed and bank erosion." (EPA Memo 10/6/93)

In view of these serious documented concerns, how much confidence can be placed in guidelines such as SEMGL, and subjective equations like the USLE?

What if, instead of the 40-400 tonnes/ha/yr -(low to moderate erosion hazard)- assigned to most of the EMA/LSA, a correction to input data in the USLE, - such as the 'K' factor for soil erodibility, or 'R' factor for Rainfall Erosivity, or 'C'(0.45), the standardised factor for Ground cover management, or 'P'(1.0), the standardised factor for supporting practices or management, resulted in 'A' - the estimated soil loss - becoming 400-800 tonnes/ha/yr, or worse, more than 800 tonnes/ha/yr?

How many years would these figures have to be averaged over in order for the soil loss from integrated harvesting to be considered "acceptable"? In short, what if SFNSW calculations are incorrect? The EIS gives a reader no way of knowing whether this is the case.

8. CONCLUSION

To what extent is the soils information in this draft EIS an improvement on that presented in previous EIS', over the past several years?

Under the sub-heading "Claimed lack of adequate depth of information" in Section 3:2:3 of the Environmental Impact Assessment Report 1992 on the Supplementary EIS it is stated, (p. 12):

> "Detailed soil data are not available for the region or indidual compartments, but observed correlations between soil characteristics and geology provide a working knowledge of the forest soils and their distribution...This guides the prescriptions to be used."

The 1994 draft EIS has failed to demonstrate any evidence that SFNSW managers have improved their understanding of soil science and hydrology. A close examination of the EIS leads a reader to the inescapable conclusion that SFNSW have no intention of either attempting to rectify their poor knowledge-base, or restrain their activities in the LSA native forests over the period of the EIS, in recognition of the inadequate, misapplied data which is coupled with an almost total reliance on estimates, guesses, and a very broad brush.

For these reasons alone, the integrated harvesting of 60,000 ha proposed in the draft EIS is an extremely risky proposition for SFNSW, for the Department of Planning, and more importantly, for the forest environment of the LSA in Eden Management Area.

P.S. A JOKE
What do the draft EIS and the Bible have in common?
The draft EIS is almost as thick, certainly as dense, and reading it is an exercise in faith requiring a complete suspension of all critical faculties.

<u>REFERENCES</u>

Doeg, T.J. and Koehn, J.D. (1990): "A Review of Australian Studies on the Effects of Forestry Practices on Aquatic Values". Fisheries Division, Department of Conservation and Environment, September 1990.

Dissmeyer, G.E. and Foster, G.R (1980): "A Guide For Predicting Sheet and Rill Erosion On Forest Land" USDA - Forest Service, Southeastern Area, Atlanta, Georgia. Technical Publication SA -TP 11)

SFNSW (1994): Draft EIS for Eden Management Area Vols. A and C.

FCNSW (1992) : Environmental Impact Assessment Report on 1992 Supplementary EIS /EMA.

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Andrea Powell
Resident, LSA/EMA
22nd. January 1995.

ATTACHMENT 1:

NOTES IN RESPONSE TO CHAPTERS 3 and 6: DRAFT EIS for EMA (SFNSW)

by: Jetse D. KALMA,
Senior Principal Research Scientist
CSIRO Division of Water Resources
Canberra, 15th. January 1995.

General Comments:

1. It was very difficult to review this material without the entire text, a table of contents, list of abbreviations, the list of references and the appendices. Several pages were also missing in the text sent to me.

2. The material I have read is very poorly organized, badly written and the reader is expected to plough on, regardless.

Chapter 3 material: pp. 3.22-3.58

1. Potential soil erodibility is assessed on the basis of general geological information. Five broad geological groups are identified with 90 geological subclasses. What is the difference between geological subclass (not further described or used) and a parent rock type? Four parent rock types are dominant in LSA. How are they used in this study?

2. The important observation is made that the soil information in the EMA is incomplete and much of the available information relates to cleared, private land. So what happened elsewhere? How are soil types defined? What is a soil data card? (Some selected references are provided on the erodibility of some of the soil associated with different geological groups). The definition of soil landscape is not very clear. 16 soil landscapes are found in the EMA. But only 65% of the state forests in the region are included in the soil landscapes described so far. Some general comments follow on the erodibility of soils in various soil landscapes.

3. The identification of soil types as part of a subjective classification of lands to determine susceptibility to erosion is unclear. Climate is mentioned. Is there adequate information on rainfall intensity, EI 30etc for the entire EMA? The erosivity and erodibility information in the GIS is largely subjective. However, once it is stored, it suddenly becomes hard objective fact upon retrieval.

4. In the section on soil physical properties the parent geology is used to assess soil erodibility and to make general observations on soil physical and physico-chemical properties. This is based on very limited field measurements and one wonders how this information can be used in a spatial sense.

5. The section on existing soil erosion is critical. It provides generalized information on the basis of field surveys. The generalized observations are misleading. I believe that Erskine (1992)'s research should not be used to give the impression that the influx of sand into the SE NSW rivers is not due to logging. Also, to what extent are flood flows (and the subsequent reworking of material in the lower parts of the river) intensified by logging?

6. In 3.2.6.8 erosion hazard at regional and local scales is discussed. The SCAV method assumes that erosion processes within the "uniform" land units will also be relatively uniform. This clearly is not the case as the use of the USLE and field observations will show. This gets to the core of the problem. Every regionalisation approach will ignore very real variability within those land units. How do field observations and surveys assess that variability and at what scale?

7. On p. 3-47 the uncertainty with local erosion hazard predictions is acknowledged. In the next paragraph, the EIS goes back to a very subjective land unit based classification of erodibility and in Tables 3-16 and 3-17 we are confronted with potential erosion hazard assessments of soil landscapes and soil types despite the inadequacy of the soil information, the subjectivity of the assessments and the unknown spatial variability in the field. I have very serious reservations about the usability of maps such as Figure 3-12 for management purposes. It seems to me that somebody should attempt to identify what the key determining factors are in order of priority.

8. The use of the 0.27 conversion factor for all 7 catchments in Table 3.20 to calculate water yield seems at odds with the paragraph referring to observed differences in water yields!

9. Turning to streamflow, surely a critical aspect is what logging in its various forms is doing to peak flow and streamflow variability.

10. The section on water quality should have devoted much more attention to the dependence of water quality indicators on streamflow levels which in turn must be linked to forestry operations.

.../3

Chapter 6 material: pp. 6.9-6.47

p. 6.10: What percentage of terrain is subjected to topsoil disturbance and displacement? How much compaction takes place away from roads during harvesting operations?

p. 6.12: It is stated that significant soil movement takes place over short distances but that it has little measurable impact on water quality and sediment delivery. This seems to be a very dangerous generalisation. It also completely ignores the fact that there is a serious loss of fertility.

If things are back to normal in 5-10 years, it would be nice to know what damage has been done to the forest system over that period in terms of fertility loss, decline in biodiversity etc. The system is not a closed system (top p. 6.13) and losses are increased due to logging. The question is how much and where does it go to? Why are many estuaries (e.g. Tuross lakes etc) deteriorating as quickly as they do?

p. 6.13: There is a very frequent use of the terms “much lower”, “not significant” and “expected to be insignificant” etc. throughout this document, which hides the fact that the knowledge and data are just not available.

p. 6.14: I just note: “Whether biomass removal or redistribution of nutrients as a consequence of harvesting has a significant impact on the nutrient cycle and general site productivity is difficult to determine.” This is followed a few lines further down by “...and changes in the soil nutrient status caused by harvesting operations are unlikely to be observed in the short and medium term.” How do we know?

p. 6.15 and Table 6-2: A lot is made of Lacey’s observations on 41 coupes. This is very critical to the overall argument that the total area significantly disturbed is only about 10%. It seems to me that it is absolutely essential to indicate when these observations were made in the weeks, months, years following the actual harvesting. How is disturbance measured or assessed?

p. 6.16: Who enforces the provisions in the SEMGL? How successful have we been in the past in enforcing rules and codes?

p. 6.16: “Revegetation generally occurs rapidly on eucalypt forest sites in the EMA.” Just like erodibility will vary greatly from site to site so will the rate of revegetation. Why is nobody paying any attention to those spatial differences which will impact on soils, hydrology and water quality?

p. 6.17: How successful have the various rehabilitation strategies been?

.../4

p. 6.17-6.18: How successful have we been in enforcing the Code of Logging Practice and Erosion and Sediment Control Strategies?

p. 6.18: To quote sediment production rates from road construction based on NZ and Victorian assessments is a bit risky (last paragraph). There is also no mention, after quoting the 2 kg/m2.yr figure, what happens in the subsequent years.

p.6.20: There is mention of “local hotspots” here (paragraph 7). Their presence makes the use of averages based on field observations and integrated sampling virtually meaningless.

p. 6.21 The bulk of the coarser fractions of sediments are “expected” to remain within the forest areas (paragraph 2). However, a significant fraction of the finer sediments is “expected” to enter the natural drainage system (paragraph 3). These two quotes seem to indicate that we really do not know what is happening.

p. 6.22: re: Paragraph 3: What is “the required level of control”?
Estimates of sediment production “..are difficult to quantify” !!!
“Likely quantities of sediment production are difficult to estimate”!!!
“Assessment needs to be qualitative”!!!

p. 6.24: re: Paragraph 1: Has any attempt been made to identify the harvesting areas on unstable granitic soils and to do local assessments?

p. 6.24: re Paragraph 6: What has this sediment production in the first few years done in terms of soil degradation and impoverishment?

pp. 6.25-26: Burning will lead to enhanced runoff, increased erosion and sediment transport. Although net soil movement out will be small, fine sediment will finish up in the natural drainage system, especially with heavy rainfall events. Again this is an indication that we need to look at rainfall events rather than long term averages.

p. 6.27: re Paragraph 2: Can we really accept the Langford and O’Shaughnessy (1980) reference as the key reference for stating that the overall impact is negligible?

p. 6.28: Flannery (1994) notes the role of forest fauna in nutrient recycling. Burning represents generally a net loss of nutrients. This aspect is completely ignored. The EIS looks at individual components much more than at the whole system. Overall, the EIS seems over-optimistic about the impacts of burning.

p. 6.30: I agree with section 6.4.1.2

.../5

p. 6.31: I have real difficulties in accepting the general figure of 40 mm/yr increase in water yield per 10% reduction in crown cover. I also do not fully understand the computations of Table 6.4. The figure of 27% of rainfall as runoff is also difficult to apply generally across the area as a whole. Where are the hard figures which indicate how the frequency distribution of individual runoff events has been changed by harvesting etc? Not only is there an increase in total yield. There is also a shift in the distribution curve.

p. 6.33: re Paragraph 5: This juggling with scales is confusing and not very informative.

p. 6.35: There seems to be a contradiction here. Increased runoff following logging will change the total load in the drainage system. It does not even matter where the material comes from: sheet erosion or streambank erosion. The damage downstream is done by the increased load and the increased runoff. The deposition in the slower flowing, low gradient reaches downstream is a major environmental disaster!!

p. 6.37: re 2nd. paragraph of 6.5.1: If there are few scientific studies of the impact of forest activities on stream water quality; if the full extent of this disturbance can not yet be determined, let alone predicted, why then continue with this tentative interpretation of the limited results in the next 10 pages or so?

p. 6.37: How can these 1967 and 1963 references based on very limited techniques in entirely different management situations be relevant to the problems in the EMA?

p. 6.39 re Paragraph 5: What is meant with short term gross effects of wildfires on erosion and water quality? What are the long term consequences in terms of site fertility, biodiversity or land degradation?

p. 6.39 re Paragraph 7: There is little mention in this EIS of the overall impact of forest operations on the state of health of coastal lakes, estuaries and inlets. Just ask the local fishermen.

p. 6.42: re Paragraphs 4-7: Once again, the issue of different scales is not very clear.

p. 6.45: re Paragraph 3: The reference here to the site specific nature of many of the nutrient studies makes me wonder of this EIS is really serious about the problems.

p. 6.46-7: This section 6.5.6 on cumulative impacts deals with a most imporrtant aspect. Operations are not evenly spread over the entire region. They are also not evenly spread in time because of manpower requirements, weather conditions, wet and dry periods. The other issue not really addressed is the integrated impact on the slower flowing, lower gradient reaches downstream.

Canberra, 15 January 1995