Comments on Optimisation of the application of GRAL in the Australian context

David Carslaw, July 2017

Overall comments

The report (and appendices) represent a thorough and comprehensive assessment of the GRAL/GRAMM models for use in Australia. The report clearly identifies the important steps involved from the use of input data, the set up of models through to verification. The report is well-written and the main issues clearly identified. The authors have a good understanding of the main processes involved in air pollution modelling and some of the limitations.

The study is unusual in my experience for being so thorough: from the effort to capture vehicle characteristics e.g. speed, fixed site and passive measurements, the use of meteorological models, a Lagrangian dispersion model, combined with a wide range of usefully selected analyses is most impressive. A strength of this report is how well all of the different inputs, models and analyses have been pulled together, which I think is a difficult challenge to realise in a cohesive way.

In comparison with many other studies I am familiar with, the GRAL study is among the most comprehensive in terms of data input, use of leading models (most studies of this kind would use Gaussian plume models) and evaluation. Moreover, such comprehensive work undertaken for *local* modelling I would say is rare. The authors have also been balanced in their treatment of some of the limitations of the important steps involved (e.g. emission factors, difficulty in predicting hourly concentrations – in particular, the additional effort required to predict hourly NO_2 concentrations). In this regard, I would consider the report overall to be a high-quality study. It is clear the study has gone to great lengths to optimise the approaches used for prediction through many sensitivity tests, which I think is enormously useful. Overall, I consider the study to be an excellent state of the art, 'modern' approach to air quality modelling – a tour de force!

The main limitation of the study is the treatment of NO_2. However, as I describe below in more detail, this situation arises more because of the currently lack of robust NO_2 modelling even in state of the art models. I have suggested some analyses that could be conducted to at least better understand/characterise the NO_2 performance.

The appendices are comprehensive and provide a lot of useful information about some of the important detail. Overall, I consider the study to be of high quality and a good example to others in the field about how similar studies should be undertaken.

Specific comments

Treatment of traffic

The overall treatment of traffic flows and emission calculations I think is as comprehensive as can reasonably be expected. The average speed approach using the Google API is great – it's often difficult to obtain reasonable estimates of speed and using observation-based estimates is a very useful approach. I suspect, as is common with all studies, the hourly prediction of emissions is highly uncertain (see comments on Appendices later). However, given the types of data available I believe the study is as robust as can be reasonably expected in this regard. The diurnal and day of the week variations in flows etc. all look to be sensible – and this is reassuring.

4.4.4.3 Calculation of NO_2 concentrations

In this section the authors consider the methods by which NO_2 concentration can be calculated. I think this is probably the weakest part of the study, which comes down to the lack of chemistry treatment in the models used. In some respects, it is a shame that all the excellent work on meteorological and dispersion modelling etc. is let down by a 'bolt-on' solution to calculating NO_2.

As the authors have discovered, no empirical approach works well. It is (as the authors recognise) a challenging problem because of the processes involved. These processes include: adequate representation of background concentrations, quantification of primary NO_2 (uncertain) and the short-term chemical formation of NO_2 through its reaction with ozone. The latter point is particularly important for this study – the time scales for atmospheric mixing and chemical reactions are every similar, which makes this apparently simple task genuinely highly challenging. Ideally, what is required is a closely coupled treatment of mixing and chemistry.

Having said that this part of the report is disappointing, it really reflects the state of the art of the models involved. Models at the local scale (including CFD) are primarily focused on dispersion rather than chemical processes. There is a lack of models available that give even-handed treatment to both. The criticism above therefore is not of the authors per se but the current state of the art for modelling chemically reactive pollutants (which for local scale modelling is really dominated by NO_2). I think the study does usefully consider several empirical 'bolt-on' approaches for estimating NO_2 and gives an honest appraisal of model performance.

If the prediction of NO_2 is key to this project, I would suggest that the empirical predictions are unpicked further (using many of the approaches the authors have used elsewhere in the report). It would be useful to know for example, whether the NO_2 predictions worse under certain conditions (comparing the temporal variations of the model + observations could be very useful), are they worse under high background ozone conditions, what do the polar plots of the predictions look like compared with observed value polar plots, can the fall-off with distance from road be assessed using passive samplers etc.? As I comment later, the latter analysis that eh authors conducted for NO_x is highly valuable – a similar treatment of NO_2 would also be very useful. These types of analysis would at least help reveal where the limitations of the empirical approaches are and point to future refinements that could be implemented.

Some of the data in Figure 19 looks a bit odd i.e. high NO_2 but lower overall NO_x concentration.

I would consider adding a recommendation on this issue i.e. models need to properly account for local chemical processes.

5.5.3 Directional analysis

This section is very interesting and useful. The authors have correctly identified the important processes involved and their interpretation of the polar plots is spot on. These plots also nicely reveal the differences between the roadside and background site in their different characteristics. It would have been useful to also plot the same plots using the predicted concentrations, as this would help determine whether the dispersion / chemical processes have been adequately captured.

5.8 Road traffic contribution and 6.6.1.1

This is an important section and set of analyses. This use of the passive sampling data in particular is very useful. The fall-off in concentration with distance from road is a critical element that is rarely considered.

The results later in 6.6.1.1 significantly strengthen the study. I would say this comparison alone is among the most useful that can be made. The comparison encapsulates so much that is important: the emission factors, configuration of the road in the model and meteorology that represents the situation at hand. If any of these important elements are wrong, then it will be exposed in this comparison. In this respect, the results (particularly from GRAL) are very encouraging.

The authors might want to compare some of their finding with a recent study from the US:

High-Resolution Air Pollution Mapping with Google Street View Cars: Exploiting Big Data

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Conclusions

The sensitivity tests reveal that road gradient can be important for the emissions estimate – but is there actually so much uncertainty in the gradient in the study domain?

I would avoid the use of terms like "quite poor" when discussing the results. It's better to refer to an actual typical numeric value.

Probably useful to comment on the lack of explicit treatment of NO_2 in these models. This is especially important where the focus is to assess against health standards rather than total NO_x.

Many of these findings should be useful for other studies.

Appendices

The appendices provide very comprehensive information related to the performance of the meteorological and dispersion models. The authors have been through in the comparisons made and the choice of analyses undertaken. For example, comparing the temporal variations in model performance is a 'tough test' of models that inherently includes information on the quality of the inputs used, the meteorological model performance as well as dispersion (and chemical) processes. It is unusual for model evaluation to adopt such comprehensive approaches, which will expose any weaknesses involved. The combination of comparing a wide range of temporal components, Q-Q plots, hourly scatter plots and Taylor Diagrams is an excellent (and tough) selection of evaluation choices. It is more common to see simple statistics comparing all data, which is not very helpful for understanding the underlying limitations of a modelling study.

In considering the temporal variations in more detail, it does seem that the combination of meteorological model and dispersion model capture the diurnal, seasonal and weekday variations well – even if there is a lot of scatter in the hourly comparisons

It is clear from looking through many of the plots and data that it is highly challenging to accurately predict hourly concentrations. This does not surprise me given the complexity of the processes involved. It can be difficult enough to predict annual concentrations adequately, especially across a range of different sites. Moving to hourly concentrations places a huge emphasis on *all* aspects of modelling. A key issue with hourly predictions is that all components of model prediction need to be at a similar level of sophistication – even if (for example) the meteorological and dispersion processes are well-represented, the use of estimated hourly traffic and emissions estimates can easily reduce the overall performance.

From my review of the hourly data I would say the results are similar to the best that can reasonably be achieved. I would also congratulate the authors for presenting this information in such an open way – often I think these types of output are conveniently ignored. It seems to be difficult in these types of modelling studies to generate models that produce good hourly predictions – a large element appears to be stochastic.

One further comment though is whether similar results could be achieved with simpler approaches – that is perhaps an issue for the review group to consider.