*We would like to thank Reviewer 1 for their patience, and their thorough review of our manuscript. In response to the feedback from the three reviewers, we have made significant revisions to the manuscript, which we believe has strengthened the study and manuscript.*

*Upon performing the sensitivity analysis of minimum TCCO value used, it was discovered that the QA filter for the TCCO values for the matchup fires was not being correctly implemented. This has been corrected in the analysis code, which has led to new biome specific emission coefficients being calculated. This has been corrected in the analysis and manuscript. Additionally, at the time of initial submission GFEDv4.1 data for 2024 was not available. At the time of the revision, it is available, so has been included into this study.*

*We will answer your comments and suggestions in the presented order. The original comment will be displayed, followed by the response, in red italics for clarity.*

This manuscript presents a methodologically innovative and scientifically significant advancement in the assessment of high-latitude wildfires (≥ 60° N), achieved through a robust integration of multiple satellite-derived data sources within a well-conceptualized methodological framework. The study is well aligned with the scope of Biogeosciences and represents a genuine advance over existing wildfire emission inventories.

I recommend minor revisions, not due to flaws in the core methodology, but because several conceptual, statistical, and reproducibility aspects should be strengthened.

1. Scientific Questions and Hypotheses

The manuscript would benefit from explicitly stated research questions and testable hypotheses in the Introduction. This would improve conceptual clarity and narrative structure.

*Our study does not have a testable hypotheses, as we are attempting to adapt the proven FREMv2 methodology to the high latitudes, by replacing geostationary FRP observations with polar orbiting FRP observations (through the use of GFASv1.4). However, we have made this clearer in the introduction, which now reads: "Given that geostationary observations of FRP are unsuitable for fire-prone HL regions, we aim to take advantage of the higher latitude, higher temporal resolution sampling of FRP of polar orbiting FRP observations to develop a HL version of the current FREM approach of Nguyen et al., (2023); generating a dataset of HL fire carbon and trace gas emissions completely independent of satellite-derived burned area measures and/or of the FRP-to-fuel consumption conversion coefficients influenced by them."*

2. Manual Plume Identification and Selection Bias

The manual digitization of matchup fires may introduce subjectivity and selection bias. A quantitative comparison between matchup fires and the full fire population is recommended.

*We have conducted spatial distribution analysis of the matchup fires, with respect to the wider HL GFASv1.4 FRP dataset (Figure 8). We find that the zonal distribution of matchup fires is consistent with the full fire population, and that the longitudinal distribution shows that some fire activity between 60-90E was not sampled as part of the matchup fires. Additionally, the matchup fires tended to consist of a larger proportion of FRP values than full fire dataset, as shown both graphically, and by a Mann-Whitney U test. This is ultimately because higher FRP values are likely resulting from larger fires, which are more likely to produce a visible smoke plume (needed for plume identification).*

3. Statistical Treatment of Emission Coefficients

Biome-specific emission coefficients are derived using zero-intercept OLS regression. The physical justification for this assumption should be clarified, and sensitivity analyses or alternative regression approaches should be considered.

*The physical justification of the choice of OLS has been included, as well as the consideration of orthogonal distance regression (ODR).*

4. Organic Soil and Peat Burning

The framework does not explicitly distinguish soil carbon combustion, which may lead to underestimation in peat-rich regions. This limitation should be clearly discussed.

*We agree that the framework does not explicitly distinguish soil carbon combustion. The PEATMAP data product was originally used as an indicator of peat-rich environments, but was ultimately discarded due to reducing the sample size of biome specific matchup fires. This consideration has been discussed briefly in Sect. 3.7 (Biome Specific Emission Coefficient Generation) and Sect. 5 (Summary and Conclusion).*

5. Uncertainty Propagation

Uncertainties are not fully propagated into long-term emission totals. A clearer description of uncertainty propagation is encouraged.

*The uncertainties reported in Fig. 7 have been included in the long-term time series plots, as shading either side of the HLFREM trace. Additionally, an increased description of potential sources of uncertainty has been included in Sect. 3.7 (Biome Specific Emission Coefficient Generation)*

*We would like to thank Reviewer 2 for their patience, and their thorough review of our manuscript. In response to the feedback from the three reviewers, we have made significant revisions to the manuscript, which we believe has strengthened the study and manuscript.*

*Upon performing the sensitivity analysis of minimum TCCO value used, it was discovered that the QA filter for the TCCO values for the matchup fires was not being correctly implemented. This has been corrected in the analysis code, which has led to new biome specific emission coefficients being calculated. This has been corrected in the analysis and manuscript. Additionally, at the time of initial submission GFEDv4.1 data for 2024 was not available. At the time of the revision, it is available, so has been included into this study.*

*We will answer your comments and suggestions in the presented order. The original comment will be displayed, followed by the response, in red italics for clarity.*

The manuscript by Maslanka et al. “Direct Estimation of Wildfire Emissions at High Latitudes from Combined Polar Orbiter FRP and Sentinel-5P CO Data “ presents a methodology to obtain CO emission coefficients from satellite CO columns and hourly GFAS FRP. The authors derive emission coefficients for four different broad vegetation classes that are common in Northern latitudes. Total CO emissions are estimated from this and compared to three other common wildfire inventories: GFAS, FEER and GFED. The manuscript s well written and scientifically sound. I believe that the manuscript fits well in the scope of Biogeosciences and should be of interest to the readers of this journal. I suggest publication after the suggested revisions are addressed (see details below).

General Comments

1. Uncertainty Analysis

Overall, the uncertainties, limitations of this method, and assumptions need to be expanded in this manuscript. The approach used to estimate the background CO concentration also requires further justification and uncertainty analysis (sensitivity tests). Using the minimum CO value within the study area may not be the most appropriate choice, given the inherent noise in satellite observations; an upwind background concentration may provide a more robust estimate or at least a lowest percentile of CO. Since the excess CO calculation depends directly on the background concentration, this is likely one of the largest sources of uncertainty in the emission coefficient estimates.

Additionally, the manuscript would be strengthened by a more comprehensive discussion of the uncertainties associated with the methodology, including the uncertainty in the excess CO estimates as illustrated in Figure 6.

A discussion on the influence of combustion phase (flaming versus smoldering) on the emission coefficients is also missing, I assume this has been neglected because TROPOMI does not have too many overpasses per day. But a discussion of this needs to be included, as well as the assumptions underlying the approach.

*As part of the HLFREM, or from the FREMv2, study, we have not used wind data in order to determine plume orientation, so cannot easily determine “upwind” to get an upwind background concentration for CO. However, we have completed a sensitivity analysis, by increasing the number of pixels in the buffer, to determine the minimum background CO value, from one-pixel (used in the analysis) incrementally to six-pixels, which equates to roughly 20-30km. Using these pixel buffers, we calculated new EC values, and found that increasing the pixel buffer had diminishing returns after three-pixels for the forested and the shrubland biomes, but had less of* *an effect on the grassland biomes. This sensitivity analysis, and tabular results has been included in Sect. 3.7, with associated scatter plots shown in Fig. A2.*

*An increased description has been added, detailing potential sources of uncertainty in Sect. 3.7 (Biome Specific Emission Coefficient Generation). These potential sources of uncertainty include the variability and impact of peat-rich soils on the CO emissions from the matchup fires, bias within the TCCO observations, and variability and uncertainty within the FRP observations.*

*Combustion phase was not considered, as we are assuming that a single relationship between CO emission and FRE exists for a given biome. In reality, this is a simplistic view, as the combustion phase, as well as the proportion of flaming vs smouldering will impact the relationship. This would introduce some intra-biome scatter, seen within the EC plots (Fig. 7). This influence has been briefly added to the introduction and Sect. 3.7.*

2. Description on TROPOMI CO

The manuscript would benefit from either a separate section on TROPOMI CO or including this as part of section 3.4, describing TROPOMI CO in more details including data version used, quality filters used, uncertainties and validation of the CO data product (meaning citation and discussion of appropriate studies for this, I don't mean validating this product as part of this study).

This is a good reference for TROPOMI CO in smoke:
Jake P. Rowe, Kyle J. Zarzana, Natalie Kille, Tobias Borsdorff, Manu Goudar, Christopher F. Lee, Theodore K. Koenig, Johana Romero-Alvarez, Teresa Campos, Christoph Knote, Nicolas Theys, Jochen Landgraf, and Rainer Volkamer, ACS Earth and Space Chemistry 2022 6 (7), 1799-1812, DOI: 10.1021/acsearthspacechem.2c00048

*Tropomi TCCO, as well as VIIRS and MODIS data used in this study, was not introduced sufficiently. A new section (Sect. 3.1 HLFREM Earth Observation Data Used) has been included to rectify this.*

3. Comparison to previous studies

The claim in the abstract (Lines 23–24) that this is the first study of its kind should be reconsidered or better justified. Previous studies have derived emission coefficients for these biomes, and emission coefficients have been reported using different methodologies. It would therefore be valuable to discuss how the emission coefficients presented in this study compare with those reported in previous work (e.g., Adams et al., 2019; Hayden et al., 2022; Griffin et al., 2024). In addition, I suggest considering the following references and determine whether they are relevant to cite and discuss in the context of the current study:

Adams, C., McLinden, C. A., Shephard, M. W., Dickson, N., Dammers, E., Chen, J., Makar, P., Cady-Pereira, K. E., Tam, N., Kharol, S. K., Lamsal, L. N., and Krotkov, N. A.: Satellite-derived emissions of carbon monoxide, ammonia, and nitrogen dioxide from the 2016 Horse River wildfire in the Fort McMurray area, Atmos. Chem. Phys., 19, 2577–2599, https://doi.org/10.5194/acp-19-2577-2019, 2019.

Mebust, A. K. and Cohen, R. C.: Space-based observations of fire NOx emission coefficients: a global biome-scale comparison, Atmos. Chem. Phys., 14, 2509–2524, 2014.

Mebust, A. K., Russell, A. R., Hudman, R. C., Valin, L. C., and Cohen, R. C.: Characterization of wildfire NOx emissions using MODIS fire radiative power and OMI tropospheric NO2 columns, Atmos. Chem. Phys., 11, 5839–5851, https://doi.org/10.5194/acp-11-5839-2011, 2011.

Voshtani, S., Jones, D. B. A., Wunch, D., Pendergrass, D. C., Wennberg, P. O., Pollard, D. F., Morino, I., Ohyama, H., Deutscher, N. M., Hase, F., Sussmann, R., Weidmann, D., Kivi, R., García, O., Té, Y., Chen, J., Anderson, K., Stevens, R., Kondragunta, S., Zhu, A., Worthy, D., Racki, S., McKain, K., Makarova, M. V., Jones, N., Mahieu, E., Cadena-Caicedo, A., Cristofanelli, P., Labuschagne, C., Kozlova, E., Seitz, T., Steinbacher, M., Mahdi, R., and Murata, I.: Quantifying CO emissions from boreal wildfires by assimilating TROPOMI and TCCON observations, Atmos. Chem. Phys., 25, 15527–15565, https://doi.org/10.5194/acp-25-15527-2025, 2025.

Chelsea E. Stockwell, Megan M. Bela, Matthew M. Coggon, Georgios I. Gkatzelis, Elizabeth Wiggins, Emily M. Gargulinski, Taylor Shingler, Marta Fenn, Debora Griffin, Christopher D. Holmes, Xinxin Ye, Pablo E. Saide, Ilann Bourgeois, Jeff Peischl, Caroline C. Womack, Rebecca A. Washenfelder, Patrick R. Veres, J. Andrew Neuman, Jessica B. Gilman, Aaron Lamplugh, Rebecca H. Schwantes, Stuart A. McKeen, Armin Wisthaler, Felix Piel, Hongyu Guo, Pedro Campuzano-Jost, Jose L. Jimenez, Alan Fried, Thomas F. Hanisco, Lewis Gregory Huey, Anne Perring, Joseph M. Katich, Glenn S. Diskin, John B. Nowak, T. Paul Bui, Hannah S. Halliday, Joshua P. DiGangi, Gabriel Pereira, Eric P. James, Ravan Ahmadov, Chris A. McLinden, Amber J. Soja, Richard H. Moore, Johnathan W. Hair, and Carsten Warneke, Environmental Science & Technology 2022 56 (12), 7564-7577, DOI: 10.1021/acs.est.1c07121

Griffin, D., Chen, J., Anderson, K., Makar, P., McLinden, C. A., Dammers, E., and Fogal, A.: Biomass burning CO emissions: exploring insights through TROPOMI-derived emissions and emission coefficients, Atmos. Chem. Phys., 24, 10159–10186, https://doi.org/10.5194/acp-24-10159-2024, 2024.

Hayden, K. L., Li, S.-M., Liggio, J., Wheeler, M. J., Wentzell, J. J. B., Leithead, A., Brickell, P., Mittermeier, R. L., Oldham, Z., Mihele, C. M., Staebler, R. M., Moussa, S. G., Darlington, A., Wolde, M., Thompson, D., Chen, J., Griffin, D., Eckert, E., Ditto, J. C., He, M., and Gentner, D. R.: Reconciling the total carbon budget for boreal forest wildfire emissions using airborne observations, Atmos. Chem. Phys., 22, 12493–12523, https://doi.org/10.5194/acp-22-12493-2022, 2022. – particularly section 3.5.2

*The claim in the abstract has been removed, as it was misleading. It was intended that this was the first time that high latitude fires had had their emission coefficients calculated via a top-down approach, using EO data only, however this not strictly speaking the case. Some of the recommended literature above has been incorporated into the manuscript (specifically: Adams et al., Griffin et al., Mebust et al., Hayden et al.,), with a brief comparison with the TROPOMI/FRE/GFED coefficients in Griffin et al., being compared (being the most directly comparable methodology). It was shown that the BORF and SAVA GFED coefficients calculated by Griffin et al., were of comparable magnitudes to those in this study.*

4. Limiting this method to high latitude fires

Hourly GFAS FRP is available globally, as are TROPOMI CO observations. Therefore, I do not fully understand the rationale for restricting this analysis to high-latitude fires. While higher latitudes benefit from multiple TROPOMI overpasses per day, the manuscript does not clearly explain how these additional overpasses are utilized. For example, Figure 4 describing the methodology appears to include only a single TROPOMI overpass.
The manuscript mentions that northern latitudes can have "up to three" TROPOMI overpasses per day. Does this simply mean that up to three independent emission coefficient estimates are included in the analysis (i.e., one for each overpass) multiple points on Fig. 6?, or are the multiple overpasses incorporated in a more sophisticated way for which multiple overpasses per day are

needed. The methodology should clearly describe how multiple overpasses improve the analysis (e.g., by accounting for temporal variability in emissions throughout the day).

Expanding this approach to fires globally would substantially increase the impact and applicability of the study. If such an analysis is beyond the scope of the current work, I recommend including a discussion of its feasibility as future work. Alternatively, the authors should provide stronger justification for why a single TROPOMI overpass, together with hourly GFAS FRP, is insufficient for applying the proposed methodology.

*Whilst Hourly GFAS FRP are available globally, it would not be appropriate to use hourly GFAS FRP observations for a global FREMv2 product. This is because, at non HL regions, geostationary FRP data is available from geostationary platforms, such as the Meteosat Second Generation across Africa (as used in Nguyen et al., 2023), as well as Himawari and GOES-EAST and GOES-WEST. These geostationary platforms give a greater temporal resolution of FRP observations than polar orbiting observations. However, at the HL, the view angles are large enough that the quality of geostationary data is poor. Additionally, at the HL, there are more FRP observations than at the non-HL from polar orbiters, because of orbital convergence, so fires are more likely to be covered by multiple overpasses at the HL than at lower latitudes.*

*This study looked to provide an alternative to the FREMv2 method for use in the HL. Outside of the HL, whilst it is possible to use the HLFREM method (using GFASv1.4 data), using the FREMv2 method (with geostationary FRP data) is recommended. This has been reinforced within the manuscript in the Introduction (including Fig. 1), as well as Sect. 3.*

*Additionally, we do not utilize multiple TROPOMI observations for a single matchup fire. Instead, when there are multiple TROPOMI and VIIRS observations of the same fire at different times of the day, these are treated as separate matchup fire instances, giving multiple estimates of FRE and associated excess CO across the day. This clarification is included in Sect. 3.5 (Excess CO Estimation).*

5. Figure Quality

Quality of figures could be improved, making label fonts larger, making the figures higher resolution, etc. see detailed suggestions in specific comments.

*Multiple figures have now been updated to have larger font sizes, and for clarity.*

Specific comments

l.133: “FRE over the time it took the plume to form”: how do you find this time?

*With the FREMv2 method, the fire start time is more easily calculated, due to the higher temporal resolution of geostationary data. However, this is not possible with GFASv1.4 data. Therefore, the fire start time was assumed to be 0600 local time (a time of estimated FRP minima, as shown in Fig. A1) and the “fire plume time” i.e. the time when the plume was observed, was taken as the TROPOMI overpass time (stated in Sect. 3.4 Fire Radiative Energy Estimation).*

l.283/284: this is not surprising as the HL-FREM relies on GFAS FRE just a different scaling factor.

*This is correct, and been made clear in Sect. 4.1.*

Fig. 1: increase the font size

*Font size increased.*

Fig. 7: the lines could be thicker it is hard to see the different colours.

*Fig. 7 (now Fig. 9) Font size increased, and lines made thicker*

Fig. 8 it would be helpful to have the map in the same projection as Fig.2b perhaps having them side by side would make it easier as well.

*Figure 8 (now Fig. 10) has been changed to have the same projection as Fig. 3. However, we feel that changing the map projection to the same as Fig. 3. Is sufficient for comparison.*

Fig. 8b-e: the label on the bottom figures is cut off

*Figure 8 b-e (now Fig. 10 b-e) layout has been changed, in order to make the subplots clearer, and to ensure the label is not cut off.*

Fig. 9: it's quite hard to see any details in the map – perhaps removing the background land colors would make it clearer. It's also hard to see where it is– could it be zoomed out a more or showing a similar projection as Fig. 2b. Instead of each emissions, showing the difference to FREM for e-j could make it clearer

*Figure 9(now Fig. 11) has been altered to ensure that it is clearer. Zooming to the whole HL polar plot (like Fig. 3) made it less clear to see the spatial patterns of FRE and emissions. By zooming in, adding more latitude/longitude tick labels, and by changing the colour scale, the spatial patterns were more visible. Additionally the regions shown (Northwest Territories, Canada, and Sakha Republic, Russia) are detailed in the figure caption and text body of Sect 4.1. We also tried plotting the difference from FREM, but ultimately decided that this was clearer for showing the spatial patterns.*

Fig.10: why not combine GFED into the other figures (a,c,e,g) as done before?

*Figure 10 (now Figure 12) is not combined, as the Emission Factors (EF) used by GFAS and GFED are different (GFAS uses EF from Andreae and Merlet (2001), whilst GFED use Akagi et al., 2011. If we combined them on the same plot and same analysis, differences in total carbon would be impacted by both differences in EF and Emission Coefficients for CO. By separating them into different plots, and by applying the respective EF values, the differences due to EF are negated.*

Fig. A2,A3: should be higher resolution or the axes labels should be in a larger font, it is hard to read
*Figure A2, A3 (now A3, A4) have been improved, with higher resolution and larger fonts.*

Technical corrections:
l.19: GFAS listed twice is that correct?
*In the abstract, the wrong name was given to FEER. The official FEER inventory is FEER1.0-GFASv1.2, as the full inventory relies on GFASv1.2 FRP data. The FEER product used in this study was FEER-equivalent emission coefficients, generated as part of the study by Ngyuen et al 2023, which is different to FEER1.0-GFASv1.2. This has now been corrected.*

l.20: which one is higher? HLFREM or other inventories – this should be made clearer here
*HLFREM emissions are smaller, this has been made clearer in the abstract.*

l.237: Only 26 and 22 fire are identified for grassland and shrubland, respectively. Are these enough to estimate a biome specific emission coefficient? Also please specify the time period that was used here.

*The FREMv2 study uses 12-18 matchup fires for their managed land and high woodland emission coefficients. Whilst 26 and 22 are a small number of matchup fires, the increased uncertainty and higher error/intrabiome spread is represented in the 95% confidence intervals.*

*The time period for this study was Northern Hemispheric Summer (June-July-August) for 2019 (for HL-BONA-19), 2020 (HL-BOAS-20), 2021 (HL-BOAS-21) and 2023 (HL-BONA-23). This temporal specification has been added throughout the manuscript, but specifically added in Sect. 3.2 (Region of Interest Selection)*

l.273/274: do you mean FRP or FRE? Before it looks like you derived the relationship between FRE and mass

*FRE was meant, this has been corrected.*

*We would like to thank Reviewer 3 for their patience, and their thorough review of our manuscript. In response to the feedback from the three reviewers, we have made significant revisions to the manuscript, which we believe has strengthened the study and manuscript.*

*Upon performing the sensitivity analysis of minimum TCCO value used, it was discovered that the QA filter for the TCCO values for the matchup fires was not being correctly implemented. This has been corrected in the analysis code, which has led to new biome specific emission coefficients being calculated. This has been corrected in the analysis and manuscript. Additionally, at the time of initial submission GFEDv4.1 data for 2024 was not available. At the time of the revision, it is available, so has been included into this study.*

*We will answer your comments and suggestions in the presented order. The original comment will be displayed, followed by the response, in red italics for clarity.*

The manuscript presents a novel approach for estimating landscape fire emissions at high latitudes using an emissions inventory developed using satellite FRE estimates and emissions coefficients derived through comparison between GFAS FRE and Sentinel-5P TCCO data over a large number of manually identified fires and smoke plumes.

The manuscript is clearly written and suitable for publication. Below are some comments and minor corrections.

Comments
It would be useful to include a section that describes the GFAS v1.2 and 1.4 and S5P datasets used in developing the emissions dataset to better understand their benefits and limitations for this application.

*Tropomi TCCO, as well as VIIRS and MODIS data used in this study, was not introduced sufficiently. A new section (Sect. 3.1 HLFREM Earth Observation Data Used) has been included to rectify this. GFASv1.4 and GFASv1.2 has already been included in the manuscript, but they have been grouped in a new subsection (Sect. 3.1 HLFREM Earth Observation Data Used) for clarity.*

Two versions of the GFAS dataset are used in the production of HLFREM. GFAS v1.4 is used to derive the emissions coefficients as it provides hourly FRP retrievals whilst GFAS v1.2 is used to estimate emissions as it provides FRP over a longer time period. Is the FRP in GFAS v1.4 derived using the GFAS v1.2 FRP but diurnally distributed (i.e. the same FRP but with a different temporal distribution)? The FRP in GFAS is cloud cover adjusted – could this contribute to some of the variability in Figure 6 or impact the relationship between energy and emissions?

*GFASv1.2 uses FRP observations from MODIS, whereas GFASv1.4 uses FRP observations from both MODIS and VIIRS/SNPP, and has a diurnal model applied to it. However it was shown that GFASv1.2 FRE values are very similar to GFASv1.4 values globally (new Figure 2). Additionally, the FRP in GFAS is cloud-cover adjusted, so could introduce some variability in the FRE for matchup fires, which would partially account for some intra-biome spread. However, this intra-biome spread is accounted for via the OLS regression and associated confidence intervals.*

Minor comments
#205 – a fire affected area polygon could cover a large area. Under this scenario, is the fire FRP\FRE that from all GFAS 0.1 grid cells that intersect the polygon?

*Yes, all GFAS grid cells that are contained by the polygon, or are at least 50% contained by the polygon, between the 0600 "fire start time" and the Sentinel-5P overpass time are considered as part of the FRE for that matchup fire. This has been made clearer in Sect 3.4.*

#220 – at higher latitudes S5P can have >1 observation. In calculating the excess CO, is the data from all overpasses used\accumulated?

*This has been clarified in Sect 3.5; we are not using multiple TCCO observations for a single matchup fire, but treating multiple TCCO observations (with different plume extents) as separate matchup fires.*

#237 – "sparce" – sparse

*This has been changed.*

Figure 1: Could add a line showing the limits of the HL region (either on both a and b or just b)

*60N shown on both 1a and 1b.*

Figure 2a – perhaps plot the EURO region FRP on a 2nd y-axis given its low magnitude

*Figure 2 (now Figure 3) has been altered. Fig2a now on log scale on y-axis (shows HL-EURO monthly FRP better). Fig 3b now shows the geographic regions linked to Fig 3a. The colours in Fig 3a, 3b, and 3d have also changed, to clarity.*

Table 4 – the EFs from FEER for deciduous forest and grassland are the same. Is this correct?

*FEER EQ EC for CO for DecNeedle and Grassland are the same. Looking into the FEER v1.0 Ce values (where FEER-EQ values were calculated from, Ichoku and Ellison (2014)), there is very little difference between Ce values for fire-biomes associated with HL-Grassland and HL-DecNeedle (Please see supplementary file attached).*

Figures – in general the figures would benefit from being higher resolution – the tick labels, legends and annotations etc could be larger \ clearer.

*Figures have been altered to include higher resolution and larger font sizes.*