We thank the reviewers and Dr. Nölscher for their careful reading of the manuscript. We address the comments of the reviewers and the on-line comment below.

Blue – reviewer comment
Black –response
Green – proposed changes to manuscript text

Reviewer #1

1) The authors describe a new metric α as the ratio of total OH reactivity to OH reactivity from isoprene and show through simulations that the overall reactivity of isoprene approaches 10 times the reactivity of isoprene itself due to the reactivity and accumulation of intermediates in the overall isoprene mechanism. They attribute the factor of 10 to the number of OH attackable bonds in isoprene, and generalize it to all VOCs in section 4.6. This is not intuitive given that the reactivity of each oxidation product is different, but it likely reflects the overall reactivity of the accumulated oxidation products. It would be helpful to illustrate this relationship between α and the number of attackable bonds with a simple VOC to better demonstrate the overall applicability of this metric to other VOCs.

The description of this concept has been expanded in the text for simpler VOCs, and new model simulations for ethane and ethene used for illustration. We have added the text below to Section 4.1 as well as adding calculations for 2 other simple VOCs to Figure 6:

The α value can be considered the flux multiplier for reactivity, i.e. how many OH radicals are lost per VOC molecule emitted or how much extra OH reactivity over the initial oxidation step an emission of a VOC represents. To help expand upon this, Fig. 6 also shows the calculated OH reactivities and corresponding α's for two simple VOCs, ethane and ethene. In these initial simulations we assume that the primary VOC (either ethane, ethene, or isoprene) emission flux is being balanced by its loss by the OH radical alone, and so for simplicity we can consider the primary VOC flux to be equivalent to the loss flux from the reaction with OH

In these simple simulations, where there is no physical loss of species or photolysis and the only primary radical present is OH, the steady state α value achieved should equal the number of OH radical reactions required to oxidise the given VOC to CO_2 and water vapour. Individual compounds in the oxidation chain react at different rates with OH, their lifetime is related to their reactivity. More reactive compounds are shorter lived and thus are present in lower concentrations. Their concentration and their rate of reaction with OH on some levels balance out, so at steady state the reactivity reflects the flux of OH reactive bonds into the system, and as OH radicals can either react with a VOC by addition to a C=C double bond or *via* a hydrogen abstraction reaction, one might expect this steady state α value to reasonably equal the number of OH attackable bonds available in the primary VOC. This simple reasoning works in the case of ethene, with a steady state α value of 5 (4 x C-H + 1 x C=C). This value of 5 can be attributed to 5 reactions during ethene oxidation that result in the loss of an OH radical. These reactions are not necessarily all OH + C=C

or OH + C-H reactions but the number of available OH reactions are preserved, for example the formyl-radical produced through the reaction of OH + HCHO does not react with OH but O_2 to yield a CO molecule which subsequently reacts with OH, thus preserving the number of OH attackable bonds. In the case of ethane, however, the steady state value of α achieved also equals 5, despite the 6 available C-H bonds. This reduction in α occurs is due to the breaking of a C-H bond during the decomposition of the alkoxy radical, produced following OH hydrogen abstraction from the primary VOC and subsequent RO_2 + RO_2 self reaction. This decomposition reaction breaks a C-H bond without the loss of an OH radical or the preservation of the number of potential OH attackable bonds, thus reducing α. This effect is not seen in the ethene simulation as the ethene alkoxy decomposition preserves the available reactivity. Simulations (not shown) of other small VOCs follow a similar pattern, with small alkenes achieving a steady state α value equal to the number of OH attackable bonds present in the primary VOC (e.g propene α = 7, butene α = 9), and simple alkanes achieving a steady state α value equal to the number of OH attackable bond – 1 (e.g propane α = 7, butane α = 9). Thus the OH reactivity of an airmass can be considered (at steady state) to be related to the flux of primary compound into the system together with a multiplier to reflect the number of OH attackable bonds associated with the primary emission that need to react. Thus the reactivity can be considered to have a 'lifetime' in the air mass related to processes that remove these bonds. Given the reaction of one compound doesn't remove all the OH reactivity from the system, the lifetime of total OH reactivity in the airmass will be longer than the lifetime of the specific individual compounds.

2) The authors focus on the discrepancy between the measured and modeled reactivity during the daytime, but there is little discussion regarding the nighttime measurements. As noted by the authors on page 5248 the simulated OH reactivity at night agrees to within the uncertainty of the measurements, suggesting that intermediates in the isoprene oxidation mechanism are not contributing significantly to the measured reactivity at night. Clearly deposition of these compounds at night likely increases as the boundary layer height decreases, but what do the nighttime measurements suggest about the lifetime of these intermediate oxidation products and the "mismatch" between the lifetime of isoprene and the lifetime of the oxidation products? The paper would benefit from an expanded discussion of the nighttime measurements.

We have included the discussion below of the day vs. night observations to Section 3.

The nighttime OH reactivity observations show significantly less variability than the those during the day, with a mean and standard deviation of 9.6 s^{-1} and 3.0 s^{-1} respectively compared to 20.3 s^{-1} and 10.1 s^{-1} for the daytime, suggesting different chemical regimes operating between the day and night. This difference is in part due to local meteorological conditions, as Pike et al. [2010] show that the Bukit Atur site was above the surrounding boundary layer on many nights during OP3, as a consequence of the complex local topography, resulting in increased dilution and thus a reduction in both the magnitude and the variability of the total OH sink. In contrast, during the day, the low winds experienced throughout the campaign (Hewitt et al., 2010) allow BVOCs emitted from the forest to be confined to the local

boundary layer. This daytime meteorology likely also increases the variability seen in the OH reactivity observations, as the measurement site is no longer separated from the majority of the surrounding forest and can thus be influenced by more variable air masses. This complex meteorology, combined with the temperature and light dependencies of many BVOC emission rates (Guenther et al., 1993), results in higher and more variable OH reactivity during the day.

Minor comment not addressed in text

p. 5236, line 27, reference to Di Carlo et al., 2004 should be removed here, as the measurements at this site were not done under "high NOx" conditions. This reference is correctly included on the following page as under "low NOx" conditions.

Although the low NO_x PROPHET forest study is the main focus of their paper, Di Carlo et al., 2004 also discuss OH reactivity observations made in the high NO_x urban plume of Houston, during the TexAQS project, and we wanted to ensure that mention of the Houston OH reactivity work was covered.

Reviewer #2

Abstract: Line 20-23: "Notable in these calculations is that the lifetime of OH reactivity is significantly longer than the lifetime of isoprene and critically depends on the chemical and physical lifetime of intermediate species." The wording in the above lines is quite confusing. OH reactivity is the inverse of the chemical lifetime of the hydroxyl radical under steady state conditions. So what is meant by "lifetime of OH reactivity"? Please clarify.

The wording in the abstract has been changed (see below) to clarify this definition.

Notable in these calculations is that the lifetime of OH reactivity from isoprene, defined as the total lifetime of an emitted isoprene molecule and all of its oxidation products, is significantly longer than the lifetime of isoprene itself and critically depends on the chemical and physical lifetime of intermediate species.

Abstract: Page 5236: "A clear argument for a significant missing flux of primary emitted VOC compounds to account for the unmeasured reactivity is not found and the development of techniques for the measurement of secondary multifunctional carbon compounds is needed to close the OH reactivity budget" Based on the information and analyses contained in the manuscript, the above statement appears to be too strong. Can the authors rule out the potential contribution of unmeasured primary reactive species (say 20-30) emitted at few ppt level with reactivity comparable to isoprene (several hundred such species exists, see for e.g Goldstein and Galbally, ES&T, 2007)? These would not have been captured by the suite of VOC instrumentation (detection limits would be a challenge) deployed in OP3 but could all together still contribute significantly to the missing OH reactivity and also scale with the isoprene reactivity (which as the authors note is also a proxy for primary emissions).

If one cannot rule out the above possibility then it should be made clear that both unmeasured oxidation products and primary biogenic emissions could together account for the missing OH reactivity. This should be clarified both in the abstract and elsewhere in the manuscript.

The abstract has been re-worded to clarify the above point.

Although the presence of unmeasured primary emitted VOCs contributing to the measured OH reactivity is likely, evidence that these primary species account for a significant fraction of the unmeasured reactivity is not found.

Page 5237; Lines 1-4; "between measured and calculated OH reactivity are usually found under low NOx conditions in the presence of elevated levels of Volatile Organic Compounds (VOC), typical of those found in forested locations (Di Carlo et al., 2004; Sinha et al., 2008, 2010)."

Significant missing OH reactivity has also been observed in aged urban air masses in France (e.g. Dolgorouky et al, 2012) which the authors cite in error as a study where the OH reactivity budget was well understood. Very high OH reactivity has also been observed in aged urban air masses containing very low NOx levels in Spain (Sinha et al., ACP, 2012). The authors should include these works in their Introductory section and clarify that missing OH reactivity and high OH reactivity has also been observed in urban sites when influenced by aged air masses and not only in forested sites.

The description of previous OH reactivity observations in this introductory section has been expanded to include / clarify the points made by the reviewer, and the new references have been included.

Page 5238; Lines 18-24: The Kim et al. 2011 paper only studied four tree species (red oak, white pine, beech and red maple), and therefore conclusions derived in that study cannot be generalized to the most bio-diverse natural forest ecosystem, namely the rainforest. This should be noted in the discussion.

This note has been added to the text.

This role of oxidation products is supported by recent observations by Kim *et al.* [2011] of OH reactivity within branch enclosures on four different tree species, where no significant oxidation chemistry had taken place, which found that the observed OH loss rate could be accounted for by the measured BVOCs. Although this result cannot be generalised to the plethora of plant species present in a rainforest, this work suggests that the missing OH sink found in the forest boundary layer may be due to unmeasured oxidation products of the initially emitted primary BVOC rather than unmeasured primary species.

Mogensen et al. 2011, ACP included secondary species in their modeling analysis of the boreal forest OH reactivity but were still unable to explain the 30-50% missing OH reactivity. They even assessed uncertainties in rate coefficients as a contributory factor and the authors should discuss how their approach fares better/differently in comparison to the modeling analysis of Mogensen et al. as it would benefit the readers.

The study by Morgensen et al. (2011) has now been included in the introductory Sect. 1. However, explaining the differences between the two studies is outside the scope of this work. Below is a brief summary of the major differences we have identified:

-Morgensen et al. (2011) use a 1D model with calculated VOC emissions (i.e. not constrained to the observed VOCs even if they were available).
- It is not obvious in Morgensen et al. how deposition of organics is treated, and could thus be very different to this work.
- The two studies are of very different forest environments, with the terpene emissions from coniferous trees in Morgensen et al. resulting in a calculated peak in OH reactivity at night when the boundary layer confines emissions to a surface shallower layer. No discernable diurnal cycle was seen in the OH reactivity observations, unlike during OP3.
- We chose not to change the rate constants for reactions used in the model from the recommended literature values used in MCM v3.2. For uncertainties in observed reaction rates to explain the missing OH reactivity seen during OP3, either the recommended OH + isoprene rate constant is slow by a factor of 2, or all recommended OH reaction rates in the model are wrong in the same direction We acknowledge that there are uncertainties in rate constants, but feel that these are likely to either cancel out or are small in comparison to the uncertainties in the field observations.

Figure 5: The correlation plot does not look that good to me and the trend lines tend be determined at the extremes by just 1-3 points. While the authors have certainly used a novel approach here and I appreciate it, some comment is warranted on why at OH reactivity of _ 75 /s, the isoprene reactivity is only 12 /s while at a total observed OH reactivity of 40 /s, isoprene reactivity is as much as 22/s. Could this be due to unmeasured primary biogenic reactive emissions during the instances you observe close to 75/s ? Some comment is required regarding this feature. Perhaps colouring the data points by the intensity of the solar radiation or by the ratio of MVK+MACR/isoprene might help to elucidate the relative importance of primary emissions versus oxidation products?

The description of the figure in the text has been modified, as shown below, to reflect the authors' acknowledgement of the large degree of variability in the data. Although the "best fit" line only shows a correlation coefficient of r^2=0.5, the conclusions from this figure, and the paper, are not effected by this variability, as the aim of this line is purely to show it has a slope > 1. Unfortunately the reviewer's suggestion of colouring the data by solar radiation did not shed any light on the variability. This is likely due to the nature of the measurement site being in a small clearing surrounded by forest, making the measured photolysis rates not representative of the

surrounding forest from where the OH sink species originated. As mentioned in the response to reviewer #3 comment 2 and in the response to the on-line comment, MVK and MACR were not measured concurrently with OH reactivity, so this colouring of the data was not possible. The lack of useful MVK and MACR observations has also been made clear in the text.

Figure 5 shows the observed OH reactivity plotted against the reactivity calculated for isoprene alone (i.e. the OH + isoprene rate constant multiplied by the observed isoprene concentration at each point). Although the data shown in Fig. 5 show a large degree of variability, likely due to observed variations in the mix of primary BVOC and secondary compounds contributing to OH reactions as well as the influence of different meteorological conditions, it does have some correlation (r^2 = 0.5). The best fit gradient of this plot (2 +/- 0.2) gives the average number of OH radicals lost per isoprene molecule, and despite the observed variability in the data is significantly higher than the gradient calculated from the observed compounds which react with OH (1.1 +/- 0.01). The small increase in the gradient calculated from the observations above unity reflects the correlation in the diurnal profiles of the BVOCs i.e the correlation of isoprene and monoterpenes which can be seen Figure 1.

Section on “Missing OH reactivity owing to primary emitted species”: The authors highlight the limitations of different analytical techniques in this section. This section could benefit from a specific discussion regarding the detection of green leaf volatiles (GLV) such as hexenols and hexenals? How well would the triad of GC-FID, GC-MS and PTR-MS be able to measure/constrain these seemingly ubiquitously produced GLVs in the forest air?

The following paragraph has been added to Sect. 7:

Green leaf volatiles (GLVs) form a subset of biogenic emissions, being oxygenated hydrocarbons produced from the biochemical conversion of linoleic and linolenic acid within plant cells. GLVs are not released from all vegetation types, and are most commonly associated with wheat, oilseed rape, grape and birch trees. These oxygenated species are generally released when mechanical damage occurs to leaves, either cutting, abrasion or the grazing of animals. Detection of common GLVs, such as cis-3-hexen-1-ol and cis-3-hexenylacetate, is possible by adsorption tubes, GC-MS methods and also by PTR-MS (albeit with some potential for isobaric interference amongst each other). During OP3 neither the GC nor PTR-MS made a positive identification of these species, and it is therefore assumed that GVLs were not present in any significant concentration to account for the “missing” OH sink.

<u>Minor comments not addressed in text</u>

Figure 1: Why do both isoprene and monoterpenes show a dip at around 0.6 fraction of day before rising again? The plot for propene goes till -50 pptV. What does this suggest about the measurements? Figure 1: Were the isoprene oxidation products methyl vinyl ketone and methacrolein measured using the PTR-MS? If so, their profiles should also be shown in Figure 1. The ratio of these compounds to isoprene could also be a good tracer for oxidation of isoprene.

We believe this dip likely reflects the balances between the sources and sinks for isoprene and monoterpenes. The site is in a clearing and the photolysis rates jump at that point as the sun comes into the clearing. OH concentrations track this increase in photolysis well, thus the BVOC sink increases at this time. When photolysis starts to drop in the early afternoon so does OH production, resulting in a reduction in the BVOC sink. This reduction in the loss rate of BVOCs at a time where their emission is peaking results in the second spike in their observed concentrations.

Figure 3: There seem to be large breaks in the dataset. What are these due to?

The gaps in the data are due to instrumental difficulties/failures, and also power cuts during the campaign. The instrument was not always run at night due to logistical difficulties with transport to the site and health and safety regulations regarding operations at a remote rainforest site that was located a significant distance from the accommodation.

Figure 11: The features (rising, peaks and fall) in the day time modeled and measured mean OH reactivity profiles do not appear to be in phase and neither does the trend seem to mirror. Can the authors speculate why?

It is unclear to us what the review is referring to here. The model and the measured reactivity show differences which we attribute to failures in the model to reflect the variability in a range of processes, probably most notably deposition.

Reviewer #3

1) One wonders how realistic are the model simulations performed here and in Whalley et al. 2011 and in Stone et al. 2011. In the very dynamical interplay between emissions, chemistry, transport and deposition the diurnal steady state for all species does not seem to be necessarily reached, does it? Furthermore, such an approach is bound to maximize (overestimate?) the accumulation of products and therefore OH reactivity. Another way of doing it is to let the model run until the photochemical age is reached like in Karl et al., ACP 2009. In isoprene-dominated environments this can be defined by the (MVK+MACR)/ISOP ratio. Thus, I would like to see first how the MVK+MACR sum and the (MVK+MACR)/ISOP ratio from the DSMACC model compare to the observations. If not close to observations the case for computing the model OH reactivity constrained by the photochemical age becomes even more compelling. Both of these results should be shown.

We acknowledge the complex issues associated with representing the coupled dynamical, chemical, emissions and depositional processes occurring within a forest canopy. Our interests here lie in the complexity of the chemistry occurring within the canopy and attempting to simplify through the diurnal average some of the dynamical processes so that we can apply our focus to the chemical problems. Unfortunately MVK and MACR were not measured in the same location as the OH reactivity observations were made. The nearest observations of these species were made at a height of 75 m, above the surrounding canopy, and observations of isoprene made at this same height show significant differences with those made on

the ground [Jones et al., 2011]. This deficiency in the available constraining observations has been acknowledged in the text. Also see response to online comment #3.

2) Although OH reactivity is by definition determined by the reactivity of compounds, little is said about the impact of changes in the chemical mechanism. For example, MCM v3.2 along with the epoxide formation did not implement the OH-forming channel in the ISOPO2 + HO2 reaction (Paulot et al., Science 2009). Liu et al., ACPD 2013 confirm this experimentally. Moreover, the branching ratios for the reactions of ISOPOOH with OH are very questionable. Specifically, ISOPCOOH + OH is given to yield 93% epoxide via the formation of the tertiary radical after OH addition. However, the formation of the secondary alkyl radical, which cannot form the epoxide and recycle OH, is expected to be 30-35% of the total. Similar problem is seen for the implemented chemistry of ISOPBOOH and ISOPDOOH. However, the most extreme case is for OH addition to ISOPAOOH in which instead of yielding 100% epoxide and OH it should yield 65% of RO2. This likely leads to an underestimate of the OH sink (OH reactivity). Therefore, I would like to see results from a sensitivity run in which the OH-forming channel in ISOPO2 + HO2 (Paulot et al., Science 2009, Liu et al., ACPD 2013) and realistic OH addition branching ratios for ISOPOOH, e.g. using the SAR in Peeters et al., JPC A 2007, and subsequent chemistry are implemented.

The reviewer makes a good point about the sensitivity of this work to the chemical mechanism used. The current state of our knowledge on the mechanism of isoprene oxidation in low NOx environments is highly uncertain (although improving rapidly), and analyzing all the potential suggestions for mechanistic changes is beyond the scope of this paper. As the reviewer describes, the main focus of the uncertainty surrounding isoprene oxidation is in its ability to regenerate OH radicals. In this work we acknowledge that the MCM v3.2 chemistry scheme does not calculate the OH concentration that was observed during OP3, and we therefore constrain the model to the observed OH. We then investigate the sensitivity of the total OH sink to the OH concentration (sections 4.5 and 6.2), and thus test the impact of this facet of the isoprene oxidation mechanism (i.e. OH regeneration) without complicating the issue by testing every mechanism proposed in the recent literature. In order to test the sensitivity of the calculated OH reactivity to the nature of the oxidation products generated by the chemical mechanism used, we have included in Section 6.3 a simulation using MCM v3.1. This mechanistic scheme has significantly different isoprene oxidation chemistry than MCM v3.2, and its effect on the calculated OH reactivity, whilst still constraining to the observed OH concentration, are minimal (< 10 %) and are now shown in Figure 12.

On-line comment

1) The presented manuscript describes an interesting dataset of total OH reactivity measurements in the tropical rain forest of Borneo. The total OH sink was measured directly and compared to simultaneous observations as well as to model simulations. Although these results are discussed in detail and allow some interesting conclusions, the manuscript starts with a rather poor abstract. A motivation is

missing and the total OH reactivity is not defined correctly in the first sentence of the abstract. The total OH reactivity is the reciprocal of the OH lifetime which depends on the reaction with its sinks. Additionally, in the abstract as well as later on in the text the authors write about the “lifetime of OH reactivity”. As the total OH reactivity is defined as the inverse of the OH lifetime, I wonder what is the “lifetime of OH reactivity”?

This has been clarified in the text (see response to Reviewer #1 comment #1).

2) The introduction part describes in detail the current situation and explains the question of interest: Is there missing OH reactivity in the tropical rainforest? And what is its origin? However, the authors could describe more precisely the “debate” about the reactive BVOCs that is mentioned in p.5237, l.13. When explaining the recent findings in detail, the authors should add the relevant reference. E.g. the PROPHET study is published in Di Carlo et al 2004 (see p.5237, l.16-22). Another recent study might be interesting for the authors. Nölscher et al 2012 ACP observed highest missing OH reactivity of almost 90% in summertime boreal forest, a totally different ecosystem than the tropics. In this study the measurement of total OH reactivity from two different heights allowed to conclude that under high temperature conditions the forest canopy was a great source for OH reactive biogenic compounds which possibly explain a large fraction of the observed missing OH reactivity.

The state of understanding of isoprene oxidation chemistry is currently a hot topic of research, and has been the focus of many recent publications. We have modified the text to make it clear that it is the current state of isoprene oxidation chemistry in particular that is the focus of the current “debate”, but we feel that a detailed discussion is outside the scope of this work. This work uses OH reactivity observations to test our understanding of OH sinks in a highly biogenically active region, not OH sources which are at the centre of the isoprene debate. We have included a model simulation in Section 6.3 that tests the sensitivity of our conclusions to the isoprene oxidation chemistry (see response to Reviewer #3 comment 2). The Nölscher et al. [2012] study has now been included in the introductory discussion.

3) It is impressive that such a high quality set of data was obtained during OP3. This makes the analysis from the total OH reactivity measurements great and puts it into a well defined framework. The authors mention the complex local dynamics caused by the measurement site. Is there a difference in variability of the total OH reactivity measurements during daytime opposed to nighttime? As the measurements were taken in 5 m height, how big is the influence from the forest and vegetation? And which layer of the forest does have the biggest influence? Can you estimate how representative are your observations for the tropical rainforest? In other words, how dependent are the conclusions drawn to the height of the measurements relative to the canopy? Without a vertical profile through the forest it would appear that results shown may be specific to the positioning of the inlet.

A section explaining the observed day vs. night variability, as well as more detail on the complex local dynamics has been added to the text (see response to reviewer #1 comment 2). The reviewer makes a very good point about the effect of the inlet positioning on the OH reactivity observed, as it is highly likely that the forest shows a great deal of inhomogeneity with respect to OH sinks. As noted in the updated text, it is likely that a lot of the variability we see in the daytime observations is due to air with different histories being sampled from different regions of the surrounding forest. It is this spatial inhomogeneity that prevents the use of observations of species such as MVK and MACR in this analysis, as they were made at an elevation of 75 m were significant differences in the concentration of isoprene were observed. Unfortunately we were unable to make vertical profiles of OH reactivity during OP3, as this would have been very interesting.

4) The role of the most prominent OH sink in the tropics – isoprene – was investigated using an interesting new approach. The ratio of total OH reactivity and isoprene reactivity connects isoprene lifetime and OH lifetime. The greater the ratio, the less depends the total OH reactivity on isoprene. However, the authors write "The _-value can be considered the isoprene flux multiplier for reactivity, i.e. how much extra reactivity over the initial oxidation step an emission of isoprene represents" (p.5244, l.1-2). This needs more explanation since it is not intuitive and difficult to understand. Similarly, the authors draw the conclusion that the value _=10 equals the number of isoprene OH attackable bonds. This needs more explanation. In this manner, isoprene reactivity seems to be not only the OH reactivity caused by isoprene itself but also its photooxidation products? If this is the case, better wording is needed in order to differentiate between the isoprene OH reactivity and the OH reactivity caused by isoprene and all it's secondary products. Since, the ratio _ is focus of further discussion and conclusions, could you please carefully define and explain this novel approach?

See response to reviewer #1 comment 1

5) At the end, the authors conclude that a global perspective on the total OH reactivity may elucidate the role and impact of organic carbons. But, why is this referred as "the budget of "emitted" reactivity" and what is meant by the "(sources and sinks)"?

For increased clarity this has been changed to:

From a global perspective considering the budget of total 'emitted' reactivity (including all oxidation products as well as primary species) may offer insights into the fate of organic carbon and ensure that this important property is modelled appropriately by regional, global and earth system models.

<u>Minor comment not addressed in text</u>

One final question: In which season was the data obtained? Is April in northern Borneo rain or dry season?

At Danum Valley, there is no distinct wet and dry season, and wet conditions prevail year-round. The area lies under the influence of the northeast monsoon from November to March and the southwest monsoon from June to August. Rainfall is generally lowest during March and April (the period of the current study was April 2008) but significant rain fell in the afternoon on virtually everyday during the campaign.