Author's response to reviews

Title: The cost of uncomplicated childhood fevers to Kenyan households: implications for reaching international access

Authors:

Bruce A Larson (bruce.larson@uconn.edu)
Abdinasir A Amin (aamin@nairobi.kemri-wellcome.org)
Abdisalan M Noor (anoor@nairobi.kemri-wellcome.org)
Dejan Zurovac (dzurovac@nairobi.kemri-wellcome.org)
Robert W Snow (rsnow@nairobi.kemri-wellcome.org)

Version: 2 Date: 2 August 2006

Author's response to reviews: see over
We received reviewer comments on our paper (MS: 1062452707105894) on 17 July 2006. As requested, we have completed a revision of the manuscript paying careful attention to each comment and suggestion offered by each reviewer.

In the following, we would like to explain how we have revised the manuscript based on each reviewer comment and suggestion. We proceed point by point for each reviewer. We first provide the reviewer’s comment in italics and then our explanation of revisions and response.

In closing, we would like to thank the editors and reviewers for their input and advice on this manuscript. We think the revision has benefited substantially from this important input.
Reviewer 1: Gerry Killen

Thank you very much for your comments, suggestions, and information related to this manuscript. We have tried to respond constructively to each in our revision of the paper. In the following, we discuss in detail how we have responded to each of these points. For ease of review, we have provided your comments in order, and then provide our response and explanations.

**Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)**

1.1. *My greatest concern is that this model assumes all fevers are caused by malaria and will be resolved by antimalarial drugs. There are some quite useful estimates of the attributable fraction of fever which is caused by malaria that have become available (e.g., recent articles on urban malaria by Wang et al.) and should be quoted. Comparable rural estimates are available in previous analytical work by the same group and recent field studies in rural Tanzania (TMIH 11:441) confirm that at least half of all fevers are unlikely to be of malarial origin.*

We do not assume all fevers are caused by malaria and will be resolved by antimalarial drugs. On page 2 under ‘definitions and approach’, we state that in this paper we address all fevers regardless of cause, and on page 3 under ‘data sources’ we explain that our data are for all pediatric fevers experienced in the two weeks prior to the interview (and that had resolved). We also note on page 1 the poor specificity of fever for diagnosing malaria.

What we have assumed is that fever, as defined by caretakers of young children, is a prompt to decide on seeking treatment. In all malaria endemic areas of Africa, such as our study location, it has been recommended by the international research community and by the World Health Organization (WHO) that 80% of these events must be treated with an effective antimalarial within 24 hours.

Thus, we acknowledge in the introduction that not all fevers are malaria and we also agree with reviewer’s point that the proportion that can be attributed to malaria decreases dramatically in urban areas of low transmission (Wang et al. 2006), and that even in rural areas a significant proportion of fevers are not due to malaria (Kachur et al., 2006). However, all of the four districts included in our study are classified for clinical management purposes as “high” malaria risk areas by the national malaria guidelines (in accordance with national and international IMCI criteria of at least 5% of childhood fevers due to malaria), and in all four districts presumptive antimalarial treatment of all febrile children below 5 years of age is explicitly recommended (MoH, 2006). We are sure Dr Killen is aware of the risks of not treating fevers in malaria endemic areas and thus the rationale for this strategy (Greenwood et al., 1987; Marsh et al., 1996; Gove et al., 2007; Chandramohan et al., 2002). To make this point clearer in the text we have
revised “data sources” section and explicitly stated that presumptive antimalarial treatment of all febrile children below 5 years of age is recommended (see page 3, data sources, sentence 3).

In sum, while we are fully aware and write in the paper that not all fevers are malaria, we appropriately focus on ‘fever’ for two reasons. First, households manage fevers although they often do not know the precise cause. And second, national and international milestones for malaria case-management make no distinction between malaria fevers and those far less potentially fatal due to other causes.

References in above discussion:


1.2. The cost of non-resolving fevers is not considered although I accept this would be very difficult to address. This might be another underestimated cost of a) malaria fevers that are not treated with modern drugs (branch 3) and b) Non-malarial fevers regardless of antimalarial treatment. I note that because only resolved fevers are considered, and fevers which persisted but eventually treated, all go into branch 1, the duration of home management may be substantially underestimated.
To be clear on definitions and associated data, we focus our analysis on ‘resolved’ pediatric fevers as defined by the primary caretaker by the day of the interview. This focus on resolved fevers allows us to identify the complete treatment histories of children included in the study. Those children remaining febrile at the time of the interview would have been in the process of treatment actions by their caretaker and, hence, the children’s treatment histories would be incomplete. Given the random sampling procedures of the survey, we have no reason to believe that household actions for children febrile 7 days ago but not febrile today were different than for children who were febrile on the interview day. While we do not have data to test this assumption, we believe it is a reasonable one and the inclusion of only those completing their treatment histories allows for a more complete picture of actual treatment actions.

Since a two-week recall period was used for collecting the primary data, it is true that this truncates the distribution of fever resolution to 2 weeks or less. Given the focus on uncomplicated childhood fevers, we do not expect that many would resolve in more than two weeks; indeed our primary data suggests most fevers resolved within 4 to 5 days with or without treatment.

1.3. This decision tree approach should be readily compared with similar approaches by Goodman et al and these should be compared in the discussion.

We now note in the model presentation that decision trees are widely used in many disciplines to organize information, evaluate the implications of various decisions, and sometimes to identify a best course of action. After consulting with Dr. Goodman, we provide two appropriate references for her work. The work by Goodman and her colleagues focuses primarily on the cost-effectiveness of various treatment strategies for malaria (nets, spraying, old versus new drugs, and so on), where a clear end point is essentially a case of malaria avoided, or a disability adjusted life year not necessarily broken down by age.

In our paper, we organize our data on fever management by households into a decision tree simply to show how households actually manage fevers in their children. Thus, this is our best attempt at explaining objectively the decisions that households make. While we could include a discussion of the decision trees used in studies by Goodman and colleagues, we would have to devote some substantial space to reviewing the work and explaining how it is essentially not relevant to the focus of this paper. As a result, we have provided appropriate references to the Goodman literature for the interested reader, but not taken the extra space to delve into this discussion in more detail.

1.4. While I am at a loss to suggest an alternative approach, $1 per day for women’s opportunity costs seems a bit excessive under most typical rural Kenyan conditions. This is not really my area of expertise but I hope some of the other reviewers can suggest more appropriate figures. My understanding and experience is that most rural women do not receive cash wages but rather contribute to household income and
wellbeing through providing a) non-income generating essential services including care-giving or b) services that lead to cash income which will be received through the man of the house and is already calculated under his income. I suggest de Plaen et al's work from Cote D'voire (TMH 8: 459; Acta Tropica 89: 135) is also useful in understanding the interactions between daily activities, financial flows and decision making ability in impoverished rural households. Kenya is clearly a different setting but some features are probably common to both. If this figure is retained it should be more clearly justified and explained to non-specialists such as myself.

Our response below is related to both comment 1.4 and 1.5.

We have substantially revised our discussion of opportunity costs to clarify the discussion and provide additional details. First, we moved the discussion of opportunity costs, now titled “Measuring the monetary value of caretaker time” to after the data sources discussion. This helps keep clear what information in our analysis is based on primary data (treatment seeking behaviour and cash expenses), what is estimated from secondary data (the monetary value of a day of caretaker time), and what is clearly an assumption (the amount of caretaker time per fever day).

For analyzing the cost of childhood fevers to households, we need to consider both direct cash expenses paid during each episode and the ‘value’ of caretaker time in the household devoted to a fever. We have direct information on cash expenses for travel and treatment (both at home and at health care facilities) from our primary data sources. Thus, to compare and add up with the same units, we need an estimate of the monetary value or equivalent of caretaker time, which is a combination of both how much time a caretaker allocates to each fever AND the monetary equivalent of the caretaker’s time. Clean data on each indicator are not available in our data.

Let us take each in turn. We know from our data the duration of each fever. We also know from our primary data that, for children taken to a health care facility, the time spent traveling back and forth to the facility is on average 0.5 days (four hours based on an 8 hour productive day) and the average amount of time spent at the health care facility (a little less than 1.5 hours, or 0.18 days). What we do not know is, for each day of a fever, how much additional time caretakers allocate to childcare instead of what they otherwise would have been doing. On average, just because a child has a fever for that day, it is unlikely that caretakers do nothing else for the day especially since we focus our analysis on uncomplicated fevers.

In our original version of the paper, we completed our analysis using 0.5 days as the base case assumptions, and also provided results for 0.25 days as well. Following the reviewers comments and after considering the situations again ourselves, we have revised the paper so that 0.25 of caretaker time per fever day is used as our base case assumption for the analysis reported in Table 1 and Table 2. We’ve also provided all results for 0.5 and 0.125 days of caretaker time per fever day in Table 2 so that the readers can evaluate the sensitivity of our results to this assumption. We think this range of possibilities provides a very good indication of how the results change with this assumption, without
unnecessarily complicating the presentation of results and the discussion. Since expected costs are linear in this parameter, discontinuities that would define thresholds do not exist.

In sum, we have used what we think is a more reasonable base case assumption, and provide all details in our paper that allows any reader to reevaluate all estimated costs for any value of the parameter.

The second part of this topic is the monetary ‘value’ or equivalent of a day of caretaker time. The correct notion of value here is “opportunity cost”, which is defined as the value to the caretaker and their household of the next best use of their time, where value is converted into a monetary equivalent.

As we wrote on page 4 of the original manuscript, we of course know that a large portion of women work in subsistence agriculture and complete valuable household activities (fuel and water collection) without cash earnings. We also know that caretakers may not control the income (cash or non-cash) that their labor generates for their household. The question is what to do?

Perhaps because this information is difficult to obtain and/or estimate, the concept of opportunity cost or value of women’s and/or family time is commonly discussed in the development and health literature but rarely estimated directly. While there is a large literature, including that mentioned by de Plaen by the reviewer, documenting many details of household cash and non-cash income earning and control, we think delving into this literature in a reasonable fashion would require too much space and time to be appropriate for the journal. At the same time, however, we want to emphasize the importance of this number and provide a reasonable number for our analysis. Our discussion of opportunity costs or value of caretaker time in the paper (beginning in paragraph 3 of the section titled “measuring the monetary value of caretaker time”) is designed simply to show that $1 a day is a very reasonable starting point as an average value of adult time in rural areas in Kenya.

Based on a review of existing literature on poverty, adult daily income, and wages in Kenya, we know that the Kenyan rural population is poor, whether working in wage or non-wage environments such as farming for home consumption. Between the information we found on incomes in Kenya, rural wages, and the fact that $1 a day is well recognized as a standard poverty level, we think $1 a day is a pretty reasonable starting point for the monetary equivalent of the value of women’s time in a household.

We arrived at this figure using three sets of information. First, the Kenyan poverty line was defined by the government as $0.50 per day in 2002 (see Barrett et al. 2006), which overlaps the time period when the primary data discussed in the next section was collected, and we know that some substantial percentage of the rural population lived at or below this poverty line in 2002. Using the average household size of 8.91 (adults and children) and 5 adults per household from the primary data sources used in our analysis, daily adult income would be $0.89 per day if average per capita income was $0.50.
Second, the World Bank reports for 2002 that national income was $11.1 billion, population was 31.3 million people, and per capita national income was $360. Per capita income underestimates adult income because children are included in the calculation. However, per capita income overestimates average income because the distribution of income in Kenya is very skewed: 20% of the country’s total population is estimated to earn 50% of national income. We therefore estimated a daily income per adult based on national income for the bottom 80% of the country ($5.55 billion for 25.04 million people), which implies that per capita annual income for the poorest 80% of the population is $221. Using the average household size in the four study districts included in our analysis of 8.91 people (see data sources discussion above), average household income would be $1,975. Finally, using the average of 5 adults in households, average daily income per adult can be estimated at $1.08.

And third, survey data on rural wages referenced in the paper (Dolan and Sutherland, reference 28) show a somewhat higher figure (about $1.30 per day), but we recognize that this figure is probably too high. Given the high poverty prevalence in rural areas in Kenya and $1 per day as a well recognized poverty line figure, which is very close to the Kenyan official poverty, we think the $1 per day used as the monetary equivalent of the value of caretaker time is reasonably justified.

We also provide all information in Table 1 that would allow any reader to re-estimate the model for any other figures based on new contexts or conditions.

We have also revised how we present our results so that the total estimated costs are clearly broken down into cash expenses (Kenya Shillings (KES) and $), caretaker time (in days), and then the monetary value of this time (in KES and $).

References cited in above discussion:


1.5. I also question whether caretakers typically spend 50% of their day on caring for a child with a non-severe fever of the type referred to in DHS. My feeling is that it is probably far less in reality. Perhaps rather than making an assumption of 50% or 25% in the absence of literature reports, a sensitivity analysis with graphs exploring the ranges of values and identifying key thresholds could be conducted?
Our response is part of the above discussion under 1.4.

1.6. Some of the assumptions based on the initial data review sound interesting but should be presented explicitly if they are to be accepted and understood by a broad audience.

We have tried to state explicitly throughout the paper that all parameters in Table 1 related to treatment seeking behaviour and cash expenses are derived from the three primary data sources presented in the section titled “Data Sources”, that the $1 per day value of time is based on a review of secondary sources, and that the 0.25 days of caretaker time per fever day is clearly an assumption.

1.7. The finding that less than 10% of fevers were treated within 24 hours and only 23% in 48 hours is a very important observation and should be emphasized. My feeling is that the economic incentives to early treatment may be greater than presented here if the opportunity costs because of extended, unresolved fevers could be considered. For example, what proportion of the DHS-reported fevers are actually recurring surges of parasitemia from the same infection? Indeed much recent work has shown just how persistent infections can be, and one report upon which one of these authors is a coauthor (Nature 438: 492) estimates mean duration of infection of about 6 months, during which time a patient may be febrile half a dozen times (See malaria therapy data plus numerous papers analysing it, notably TRSTMH 96: 205, Parasitology 122:379 and Sama et al TRSTMH E-pub ahead of print plus references therein). If effective treatment and can prevent more than one fever, the potential saving of opportunity costs and perceptions thereof by the community could be much greater than presented here. I strongly suggest a careful sensitivity analysis considering the potential to prevent multiple fevers within the range that can be expected from the malaria-attributable fraction.

The information on fevers treated within 24 and 48 hours have already been noted in Amin et al. (2003) cited in our paper, which is why we did not focus too much on this information as a ‘new’ result, but instead use it as the starting point for the evaluation of two scenarios discussed in the paper.

As explained in the introduction of the paper and the definitions section, we focus here on fever as the unit of analysis. That fact that an infection might lead to multiple fevers over time does not change the logic of our analysis. From the household’s perspective, their costs are associated with managing a febrile event. Using the primary data available for this analysis, no information exists to consider if the costs of six fevers from the same infection are difference from the cost of six fevers each due to a different infection.
If we were evaluating the benefits of fever treatment options, then we would clearly need to define the treatment options and the dynamics of fevers over time from the same infection so that the benefits of a treatment option (vis-à-vis other competing options) was correctly estimated. Dr Killeen is correct to highlight the complexities of malaria paroxysms and the persistence of sub-clinical, sub-patent infections. However, the relationships between long duration *P. falciparum* infection and consequent risks of repeated symptoms is less than clear (Miller, 1958; Bruce-Chwatt, 1963; Clarke, 1978) even within the early syphilis treatment literature (Collins & Jeffery, 1999). The precise etiological fraction of malarial fevers has been purposively avoided in our analysis as we have focused on the fundamental aspects of operational case-management in Africa and the economic-consequences at the household level when fevers are managed in a multi-dimensional health system in Kenya (see earlier comments). What Dr Killen proposes is an interesting biological model routed in the precise knowledge of how many fevers are due to malaria. This however is far beyond the present scope of the paper we have submitted.

Literature referenced in above discussion:


Clarke IA (1978). Does endotoxin cause both the disease and parasite death in acute malaria and babesiosis. *Lancet*, ii: 75-77


1.8. *I agree absolutely that financing mechanisms to make ACTs accessible for home management should be developed and evaluated as a top priority. I suggest including this in the abstract immediately following the existing conclusions. The potential of making ACTs accessible though facilities is enormous but reaching out through the retail sector has massive potential and circumvent the leakage otherwise likely to occur with these valuable commodities (Malaria J 5: 25).*

Done.
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

2.1 Abstract and introduction, first line: While fever is certainly very good indicator of malaria disease, many people do become infected and ill without developing fever. I suggest a close look at TRSTMH 96: 206 and Parasitology 122:379.

If people are asymptomatic, they do not have fever and are therefore not included in our analysis. Our paper describes the costs to households of treatment seeking due to a febrile symptom history in young children.

2.2 Although the costing of opportunity loss in highly interesting and novel, a lot of interesting and relevant recent literature would enrich the discussion and should be included (eg TMIH11:299; Health Policy Plan. 2006 May 8; TMIH 11:441; Malaria J 5:25).

While the literature on malaria is indeed large and interesting, we had to decide which literature was directly relevant for this analysis of household pediatric fever management and costs to households. For example, the Njau paper (TMIH11:299) is related specifically to malaria and needed information is not broken down by age category. No information on the value of caretaker time is provided, and the principle components analysis, while ranking households within the sample, provides no direct information on income that can be compared outside the sample. The Kachur paper (TMIH 11:441) provides no information on the opportunity cost of caretaker time or cash expenses paid for fevers in children (or adults). The other Kachur paper (MJ) focused on retail prices for artemisinin compounds in the capital of Tanzania. Other than showing that AL is expensive in Dar, the paper is not directly relevant for our discussion. The Goodman et al. paper from HPP (2006, May 8) focuses on costs an intervention to improve shopkeeper performance. While very interesting, the analysis provides no directly information on caretaker time or monetary equivalent per childhood fever.

References cited in above discussion:


2.3 *It would be valuable to see the dollar cost breakdown of each branch and its component costs in table 1.*

Done.

2.4 *Table 2 should also have dollar figures.*

Done.
Reviewer 2: Umberto D’allesandro

Thank you very much for your comments, suggestions, and information related to this manuscript. We have tried to respond constructively to your recommended minor essential revisions in our revision of the paper. For ease of review, we have provided your comment first, and then the explanation of our revisions.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

This is an extremely interesting and challenging paper looking at the household cost of an episode of fever in children under 5 in Kenya. It shows that the current situation can be improved without any additional cost to the household. The paper is well written though it is sometimes difficult to follow. Indeed, the text refers to the tables and figures but the link between the 2 is not always obvious. For example, at the beginning of page 8 it is stated that the opportunity cost of caretaker time account for 85% of the total estimated average cost. It is difficult for the reader to verify this information in Table 1, unless the opportunity cost of caretaker time is divided by the average cost of fever, i.e. 193.20/227.66= 85%. The authors might consider adding (193.20/227.66) after the 85%. There are plenty of examples like this in the text and the advice to the authors is to review the text and make it more readable and obvious for the reader.

We have attempted to revise the paper throughout to make the link between the text and the tables clearly and to explain our calculations more clearly. In both Table 1 and Table 2, we provide our results in both KES and $ values, and we break down costs in terms of direct cash expenses, days of care taker time, and the value of this time. This makes it easier to compare line item totals (e.g. value of caretaker time) to the estimated total. Thus, as now discussed in paragraph 8 of the results section (beginning with the line “As shown in Table 2…”), we show the details of how we calculate line item percentages. We have tried throughout to make such information more obvious and therefore readable.
Reviewer 3: Stefan Peterson

Thank you very much for your comments, suggestions, and information related to this manuscript. We have tried to respond constructively to each in our revision of the paper. In the following, we discussed in detail how we have responded to each of these points. For ease of review, we have provided your comments in order, and then provided our response and explanations.

General

Is the question posed by the authors new and well defined? Yes, but it would benefit from stating the perspective of the cost analysis more clearly e.g. household

We have revised the paper to be clear that we mean costs to households. We state costs to households in the title, abstract, introduction, and throughout the paper.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1) Please state perspective of cost analysis more clearly

Done.

2) I am not clear on per capita income do authors mean GDP/capita? Please clarify and reference. How relevant is the 1 dollar/day valuation to a household? I can see it relevant at societal level. Please discuss see also no 4 below

This discussion of per capita income is included in the section headed ‘measuring the monetary value of caretaker time’. The World Bank measures per capita income using the “Atlas method”, which essentially is per capita gross national product adjusted to $ values based on a moving average of exchanges to smooth out large fluctuations that have occurred in the past. We have included the appropriate reference.

We use three related sets of information in our discussion (per capita income, the Kenyan poverty line, and rural wage data) to develop what we consider a reasonable estimate of per capital income for adult caretakers in rural Kenya. Each of these sets of information indicate that $1 per day in 2002 is a reasonable estimate of per capita income, which is a good measure of the opportunity cost of caretaker time.

3) The authors write average consistently is this to be taken as mean or median (or mode) all of which are averages. Which is the implication of using mean or median cost estimates?
We now state explicitly in the paper that average implies mean and/or expected value throughout the analysis. While means and medians differ if the distribution is very skewed, using means of various parameters remains appropriate when estimating average costs per fever to households and aggregating up across fevers and across children.

4) The authors state that it would not increase costs to shift management to facilities. However, that disregards the fact that cash outlays for home branch 1 is 37.5 ksh, while for hfac branch 2 it is 25+37.5=60 ksh. I would suspect household decision making is most affected by the need for cash outlays far ahead of use of (women’s) time. A separate discussion of cash outlays vs time use would be in place and some reference to decision making literature in household and criteria used. Similarly a gender perspective could be discussed while presumably it is mostly women’s time that is spent/saved in this analysis.

We have now tried to revise the paper and presentation of results in Table 1 and Table to allow for a clearly discussion of this topic. In our base case analysis based on our best representation of actual behaviour, Table 1 and 2 report that average cash expenses were $0.44 per fever. For scenario 1, as reported in Table 2, cash expenses DO NOT change. The same number of children are being delivered to a HCF, they are just being delivered earlier. We have tried to revise our discussion and presentation of results to make this point more apparent. For scenario 2, however, more children would need to be delivered to a HCF. The reviewer is very correct that shifting from Branch 1 to Branch 2 would involve additional cash expenses (essentially the KES 25 noted above). However, since this travel cost figure increases for about 20% of total fevers, average cash expenses across all fevers only increase slightly (from $0.44 to $0.50).

While we could expand on a discussion of gender, we expect that most readers will understand that caretakers are primarily women, and we mention mother in our discussion under “definitions and approach”.

5) I miss a discussion (or modelling) of my observation that childhood fevers have many different reasons beyond malaria, and many resolve spontaneously e.g. all the viral fevers my kids keep getting. Presumably earlier treatment would increase the total number of fever incidents to be treated, perhaps from branch 3 to 2?

We focus on fevers from all causes in this paper, and state this explicitly in the section “definitions and approaches”. As shown in Table 1, 32% of all fevers resolved at home without treatment of any kind, and another 30% resolved at home with some treatment with a modern medicine (which could include simply an antipyretic). All of my kids fevers in the past few years have also resolved without malaria treatment, and some fevers were not treated with any medicines and some were. In our analysis, your kids’ fevers would fall into branch 1 or 3. In the discussion of Scenario 1, we assumed for our discussion that fevers being treated earlier at an HCF would come from the same children already being delivered to an HCF, so no change in the number treated in Scenario 1. For Scenario 2, we considered what would happen if fevers treated in Branch 1 were shifted
to Branch 2 and treated earlier. While we could have considered the possibility that some from Branch 3 would shift to Branch 2, we think this less realistic since they are not being treated with any medicines.

6) **Similarly a discussion of costs incurred to the health system (and thus society at large) from shifting more and beginner (?) fevers with ACT would be helpful**

As we note in the introduction, other literature has focused on the costs of ACT to health systems and financing mechanisms. No literature has focused on the costs to households; that is, the costs they actually pay in terms of direct cash expenses and the value of caretaker time.

7) **Is there no literature on similar studies in this or related field the authors could refer to in the discussion section?**

There are no similar studies that lend themselves to a meaningful discussion around the relative merits of our current study on the cost of pediatric fevers in Africa. Existing literature derived from treatment seeking for fevers in Africa addresses treatment actions and out-of-pocket expenses (see, e.g. Amin et al. 2003) without including opportunity costs for caretakers (Amin et al 2003), a limitation we have sought to address by way of the current study. The few attempts at costing childhood illness are either too broad and do not specifically look at fevers (Sauerborn et al 1995; 1996) or look at malaria specifically, hence missing non-malaria fevers (e.g Sauerborn et al 1991, Ettling at al 1994, Kirigia et al 1998). Other studies alluded to in our previous discussions under response to Reviewer 1 look at the cost-effectiveness of different malaria control strategies (reviewed in Chima at al 2003), but as noted earlier these do not inform the current study looking at the cost of pediatric fevers to households in Kenya.

References cited in above discussion:


8) *It is not clear how costs were converted between years*

All information is based on costs in 2002. All primary data were collected in the end of 2001 and early 2002, income estimates were from 2002, and the exchange rate used was 2002. Thus, as we now state this explicitly in the abstract and in other relevant places in the paper.

9) *Scenario 1 “reducing waiting time” the word waiting time gives different connotations than intended.*

We have changed our wording based on the reviewer’s recommendation. We now simply state “earlier delivery to a HCF”.

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