Stefan Karpinski <stefan.karpinski@gmail.com>

[INFOCOM'07] Your paper #1568998545

8 messages

infocom07chairs@ecse.rpi.edu <infocom07chairs@ecse.rpi.edu> **Tue, Nov 14, 2006 at 8:31 AM**
To: Stefan Karpinski <sgk@cs.ucsb.edu>
Cc: Elizabeth Belding-Royer <ebelding@cs.ucsb.edu>, Kevin Almeroth <almeroth@cs.ucsb.edu>

Dear Stefan Karpinski:

Thank you for submitting your paper #1568998545 ('Wireless Traffic: The Failure of CBR Modeling') to IEEE INFOCOM 2007. We regret that your paper could not be accepted for inclusion in the INFOCOM program.

This year, INFOCOM again received over 1,400 submissions. All papers underwent a thorough review process including an on-line discussion among the reviewers. As a result of these reviews and on-line discussion, as well as in-person discussion at the TPC meeting in Chicago, 252 papers were accepted for publication, with an acceptance rate of roughly 18%.

Attached below and also available at http://edas.info/showPaper.php?m=1568998545 you will find the feedback from the reviewers, which we hope you will find useful in your research. Again, thank you for submitting your paper to INFOCOM and we hope that you will be able to attend the conference in Alaska.

Regards,

Infocom 2007 Program Chairs

===== Review =====

*** Recommendation: Your overall rating.
Accept if room (top 30% but not top 15%, borderline for INFOCOM) (3)

*** Contributions: What are the major issues addressed in the paper? Do you consider them important? Comment on the degree of novelty, creativity, impact, and technical depth in the paper.
This paper examines if CBR traffic and uniform destination distribution is an appropriate workload for evaluating wireless networks.

*** Strengths: What are the major reasons to accept the paper? [Be brief.]
An overdue study.

*** Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief.]
It is not clear if the data source is appropriate and
if CBR is indeed the appropriate way of recreating a realistic
setup. Furthermore the paper ignores mobility.

*** Detailed Comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper. Also provide feedback to the authors.
You tackle a problem that needs to be addressed and execute what you do very well. Yet even your study has significant

shortcommings.

1. There is a lot of work on realistic workload generators for evaluating protocols in the Internet. For example the work on how to generate Web-like traffic, or how to capture the variability inherent in Internet traffic, e.g., Harpoon, ...

2. It is not clear that using CBR and ignoring the TCP feedback loop does not lead to significant artifacts.

3. It is not clear why the workload that you consider is appropriate for the is study as the IETF meetings has a completely different set of dynamics than lets say a coffee shop.

4. You seem to ignore mobility, e.g., if a participant moves from one access point to another.

5. It is unclear how the capacity constraints intact with the workload generation.

Nevertheless this paper is nicely done and represents a first step towards showing that a more sophisticated workload model is need. This reviewer just wishes the authors would have gone a number of steps further along this road.

===== Review =====

*** Recommendation: Your overall rating.
Likely Reject (top 50% but not in top 30%, needs more work) (2)

*** Contributions: What are the major issues addressed in the paper? Do you consider them important? Comment on the degree of novelty, creativity, impact, and technical depth in the paper.
A very interesting paper about the problems of making studies relying in CBR traffic only. The paper covers many issues of the problem, and presents a _very_ interesting work. However, it is basically FLAWED. The usage of the trace data, as it is, is not a good reference for comparison with CBR-only synthesized traffic. The data should be filtered first - and then the work would become very good.

*** Strengths: What are the major reasons to accept the paper? [Be brief.]
- the theme of the work itself
- the methodology taken to analyse traffic from trace to synthetic.
- the discussion on error measure
- (partially) the results, and the impact of flow characteristics in these.
- the discussion on "novel models".

*** Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief.]
- unbelievable the action of confronting real (generic) data for comparison with CBR only models!
- the implicit unclearness about the usage of CBR models, and the value of the claims extracted from these models. Ad-hoc simulations use NOT only CBR, and do not claim that these models capture all aspects of the reality
- the implicit "work in progress" from the last lines. Some added results are in some places noticiably missing from the paper.

*** Detailed Comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper. Also provide feedback to the authors.
Strictu sensu, the simulated network should reflect the physical one where the trace was obtained. In fact, mobility and traffic patterns (both are indistinguishable in the trace) are associated with AP coverage, and as

such you could be putting too much load in a single AP. Also, the traffic in the trace is probaly biased by the source (an IETF meeting) which may not be so general as desired: definitely the traffic is typical of an ad-hoc.
The trace IS NOT composed of UDP traffic only. The work should be redone with the trace previously mined for CBR-alike traffic. Otherwise the work proves that synthetic CBR traffic is not equal to real traffic.
This reflects the major issue with the work: comparing synthetic CBR with a mix of real CBR and non-CBR.

===== Review =====

*** Recommendation: Your overall rating.
Accept if room (top 30% but not top 15%, borderline for INFOCOM) (3)

*** Contributions: What are the major issues addressed in the paper? Do you consider them important? Comment on the degree of novelty, creativity, impact, and technical depth in the paper.
This paper argues that CBR is not a good model for wireless traffic, by demonstrating that performance metrics of interest (and conclusions on protocol performance) are significantly skewed when CBR is used instead of real traffic. The main contribution of the paper is to discourage people from using CBR in wireless simulations/analysis. A secondary contribution is the statistical methodology itself that can be reused. Although the point about CBR is not particularly novel, the paper does a thorough job in arguing against it.

*** Strengths: What are the major reasons to accept the paper? [Be brief.]
The paper is very well written, and makes a strong point against the CBR model for wireless traffic. In addition, the statistical methodology, for accurately defining the misrepresentation of performance metrics, is useful in itself.

*** Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief.]
- The fact that CBR is not a good model for wireless traffic is not particularly novel (although the fact that conclusions about protocol performance can be inverted if CBR is used, is a novel observation).

- It seems that the authors go purposely through a lengthy procedure instead of the straighforward approach, in order to be able to give some definitions and crunch some numbers. The CBR model is evaluated by its effect on performance metrics of interest. Wouldn't it be more direct to compare the wireless traces to their CBR model and measure that difference?

*** Detailed Comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper. Also provide feedback to the authors.
In general, the paper is well executed. Some highlghts include (i) the methodology for processing the trace in Sec. III.A (ii) the choice of the error measure and tests for statistical equivalence in section III.C and III.D (iii) the demonstration that the relative perofrmance of protocols can be switched if CBR is used, in Sec.V.
The paper is also very well written, without typos or other presentation problems.

The main weaknesses, mentioned in the previous section, have to do with the problem statement itself. The paper could be further improved by also addressing the following points:

- CBR is a good model fo VoIP and for stream traffic in general.
- Considering CBR with respect to jitter has the advantage that it allows to evaluate the jitter caused by the network alone. In contrast, considering a bursty source results in observing the jitter caused by the

source+the network
- The packet trace is collected using tcpdump on a router through which all wireless traffic goes. How is this trace wireless-specific? I understand that it goes through a wireless network at the edge, but shouldn't a traffic model used as input to a simulation be independent of the simulated network?
- The proof about the log(y/x) being the only metric with the desirable properties is nice. However, the reader can't help but wondering whether the desirable properties themselves have been purposely chosen for their tractability. Are these really the most approrpiate desirable properties for an error function? How is this typically done in math/statistics? - please reference as appropriate.
- Is there a consistent effect of CBR in the misrepresentation of metrics?

===== TPC Review =====

Elizabeth M. Belding <ebelding@cs.ucsb.edu> **Tue, Nov 14, 2006 at 1:39 PM**
To: almeroth@cs.ucsb.edu, sgk@cs.ucsb.edu

Well, the good news is that it looks like we are on the right track with this area of work. Everyone agreed it was an important topic and that the paper made a contribution. There are some questions about our methodology and our assumptions, however. We should meet to discuss these comments and what to do next, as well as to discuss Stefan's latest results.
Kevin, when will you have time to meet?

Elizabeth

[Quoted text hidden]

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Elizabeth M. Belding <ebelding@cs.ucsb.edu>

Stefan Karpinski <sgk@cs.ucsb.edu> **Tue, Nov 14, 2006 at 3:49 PM**
To: "Elizabeth M. Belding" <ebelding@cs.ucsb.edu>
Cc: almeroth@cs.ucsb.edu

Yeah, bummer about the rejection—the second reviewer especially missed the point—but overall good feedback.

[Quoted text hidden]

Stefan Karpinski <sgk@cs.ucsb.edu> **Wed, Nov 29, 2006 at 2:47 PM**
To: Elizabeth Belding-Royer <ebelding@cs.ucsb.edu>, Kevin Almeroth <almeroth@cs.ucsb.edu>

Individual responses to various reviewer comments. I've also asked Kimaya and Krishna to read the paper and give me feedback. I'll combine the reviewer comments with their feedback and whatever you think of it to produce the version for Broadnets.

> ===== Review =====
>
> *** Recommendation: Your overall rating.
> **Accept if room (top 30% but not top 15%, borderline for INFOCOM) (3)**
>
> *** Contributions: What are the major issues addressed in the paper? Do you consider them important? Comment on the degree of novelty, creativity, impact, and technical depth in the paper.
> **This paper examines if CBR traffic and uniform destination distribution is an appropriate workload for evaluating wireless networks.**

> *** Strengths: What are the major reasons to accept the paper? [Be brief.]
> **An overdue study.**
>
> *** Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief.]
> **It is not clear if the data source is appropriate and if CBR is indeed the appropriate way of recreating a realistic setup. Furthermore the paper ignores mobility.**

I'll address these below, where they are discussed in detail.

> *** Detailed Comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper. Also provide feedback to the authors.
> **You tackle a problem that needs to be addressed and execute what you do very well. Yet even your study has significant shortcommings.**
>
> **1. There is a lot of work on realistic workload generators for evaluating protocols in the Internet. For example the work on how to generate Web-like traffic, or how to capture the variability inherent in Internet traffic, e.g., Harpoon, ...**

I'll have to talk more about this kind of work in the related work section. Currently, this is, IMO, the weakest part of the paper since I never got a chance to fix it up before we had to submit. I don't think it will change the content of the paper at all, but should address these relationships.

> **2. It is not clear that using CBR and ignoring the TCP feedback loop does not lead to significant artifacts.**

This is true. The main reason we don't use TCP streams is because Qualnet doesn't support it. However, the fact that most experiments use UDP CBR traffic, while much real traffic is actually TCP is only a stronger argument against standard traffic models. So while this is a limitation of the research, it actually doesn't hurt the conclusion of the paper at all.

> **3. It is not clear why the workload that you consider is appropriate for the is study as the IETF meetings has a completely different set of dynamics than lets say a coffee shop.**

The main reason that the IETF data set is a good one is that it has a broad range of behaviors. There's times when the usage is much like a coffee shop, and others when it's completely different (e.g. during the plenary session). The next obvious step is to use multiple different data sets, but before we do that, this is a good first data set because it is unusually diverse in behaviors.

> **4. You seem to ignore mobility, e.g., if a participant moves from one access point to another.**

This is true, we ignore mobility entirely. It's on purpose since we want to isolate the impact of traffic behavior by itself. Mobility is another, additional layer of complexity which would only serve to muddy the results.

> **5. It is unclear how the capacity constraints intact with the workload generation.**

I'm not sure what this means. Any ideas?

> **Nevertheless this paper is nicely done and represents a first step towards showing that a more sophisticated workload model is need. This reviewer just wishes the authors would have gone a number of steps further along this road.**

well, I guess that's nice.

> ===== Review =====

> *** Recommendation: Your overall rating.
> **Likely Reject (top 50% but not in top 30%, needs more work) (2)**
>
> *** Contributions: What are the major issues addressed in the paper? Do you consider them important? Comment on the degree of novelty, creativity, impact, and technical depth in the paper.
> **A very interesting paper about the problems of making studies relying in CBR traffic only. The paper covers many issues of the problem, and presents a _very_ interesting work. However, it is basically FLAWED. The usage of the trace data, as it is, is not a good reference for comparison with CBR-only synthesized traffic. The data should be filtered first - and then the work would become very good.**

I'll talk more about the filtering bit later, but this really misses the point of the paper.

> *** Strengths: What are the major reasons to accept the paper? [Be brief.]
> **- the theme of the work itself**
> **- the methodology taken to analyse traffic from trace to synthetic.**
> **- the discussion on error measure**
> **- (partially) the results, and the impact of flow characteristics in these.**
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> *** Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief.]
> **- unbelievable the action of confronting real (generic) data for comparison with CBR only models!**
> **- the implicit unclearness about the usage of CBR models, and the value of the claims extracted from these models. Ad-hoc simulations use NOT only CBR, and do not claim that these models capture all aspects of the reality**
> **- the implicit "work in progress" from the last lines. Some added results are in some places noticiably missing from the paper.**

These comments are barely intelligible, but in general they're just wrong, since not all of the traffic models we compared with were CBR. I'm not sure how to make this more clear. Ideas?

> *** Detailed Comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper. Also provide feedback to the authors.
> **Strictu sensu, the simulated network should reflect the physical one where the trace was obtained. In fact, mobility and traffic patterns (both are indistinguishable in the trace) are associated with AP coverage, and as such you could be putting too much load in a single AP. Also, the traffic in the trace is probaly biased by the source (an IETF meeting) which may not be so general as desired: definitely the traffic is typical of an ad-hoc.**

This is a common immediate reaction to this work: shouldn't you be trying to reproduce the original IETF network? Of course, the answer is that this is not the point. We're not trying to reproduce the IETF network, but rather use it's traffic to examine the difference between real and synthetic behaviors.

There's a single common issue here: people want to couple all aspects of behavior together — mobility, TCP feedback, the physical setup of the IETF venue, the locations of nodes — because their intuition is that they are all inextricably linked. And the response is that, yes, they are linked, but the linkage is not inextricable. Being able to approximate traffic behavior accurately without considering all the other factors is still better than not being able to approximate it at all, which is the current situation. And we need to walk before we can run. Trying to model all of these aspects together realistically is like expecting an infant who just learned to crawl to run a triathlon. Moreover, if we cannot accurately reproduce realistic traffic behavior in isolation, we certainly don't have any shot at doing so when all these other factors are included.

> **The trace IS NOT composed of UDP traffic only. The work should be redone with the trace previously mined for CBR-alike traffic. Otherwise the work proves that synthetic CBR traffic is not equal to real traffic.**

> **This reflects the major issue with the work: comparing synthetic CBR with a mix of real CBR and non-CBR.**

See the above comment. It's the same fundamental issue.

[Quoted text hidden]

Ah yes. The "straightforward" approach. But what do you compare? We already know that real behavior is not CBR. The question is whether it matters or not. The reason for writing this paper is so that people cannot say, "well, yes, we know that CBR isn't perfect as a traffic model, but it's good enough." It is not good enough in any sense. But how do we address this reaction? I don't really know.

> *** Detailed Comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper. Also provide feedback to the authors.
> **In general, the paper is well executed. Some highlghts include (i) the methodology for processing the trace in Sec. III.A (ii) the choice of the error measure and tests for statistical equivalence in section III.C and III.D (iii) the demonstration that the relative perofrmance of protocols can be switched if CBR is used, in Sec.V.**
> **The paper is also very well written, without typos or other presentation problems.**
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> **The main weaknesses, mentioned in the previous section, have to do with the problem statement itself. The paper could be further improved by also addressing the following points:**
>
> **- CBR is a good model fo VoIP and for stream traffic in general.**

I would argue that this is in fact not the case: VoIP traffic is not really CBR, and I bet I could look take a trace of VoIP traffic and show that CBR does not accurately reproduce it's characteristics. It's actually an interesting research idea. We could easily apply the same techniques to specific kinds of traffic. All we really need is access to the traces.

> **- Considering CBR with respect to jitter has the advantage that it allows to evaluate the jitter caused by the network alone. In contrast, considering a bursty source results in observing the jitter caused by the source+the network**

This is a very good point, and I'm not sure if Qualnet evaluates jitter by looking at the variance between inter-packet arrivals or by the variance of packet transit latencies. The latter metric measures jitter due only to the network, while the former combines it with source jitter, which would definitely make for an unfair comparison.

The problem here is that to change the way the metrics are defined would require re-running all the simulations. In fact, getting all the metrics nailed down properly is something that really ought to be done now. I'm going to meet with Krishna this week to talk about this. I may end up having to instrument Qualnet to collect metrics the way I want them collected.

> **- The packet trace is collected using tcpdump on a router through which all wireless traffic goes. How is this trace wireless-specific? I understand that it goes through a wireless network at the edge, but shouldn't a traffic model used as input to a simulation be independent of the simulated network?**

This is true. The only reason it's wireless-specific is that traffic models don't matter very much for wired applications, while they do in wireless applications. There's also an assumption that wired and wireless traffic patterns are different. It's not entirely clear that this is the case, but it might be.

> **- The proof about the log(y/x) being the only metric with the desirable properties is nice. However, the reader can't help but wondering whether the desirable properties themselves have been purposely chosen for their tractability. Are these really the most approrpiate desirable properties for an error function? How is this typically done in math/statistics? - please reference as appropriate.**

Any ideas on how to address this suspicion. The presentation in the paper was as good as I could give. The implicit assumption
of this entire analysis is that error will be introduced proportionally; i.e. that erroneous values will generally be of the form y = c*x, where x is the "true" value and y is the observed value. I'm not sure how to argue that this is the case, or even that it always is.

> **- Is there a consistent effect of CBR in the misrepresentation of metrics?**

No.

> ===== TPC Review =====

[Quoted text hidden]

Kevin C Almeroth <almeroth@cs.ucsb.edu> **Thu, Nov 30, 2006 at 1:46 PM**
To: Stefan Karpinski <sgk@cs.ucsb.edu>
Cc: Elizabeth Belding-Royer <ebelding@cs.ucsb.edu>

Hey, I'm swamped for the next couple of days, however, a quick look says you're on track. As for ideas to help solve problems, give me through the weekend.

In the meantime, I'm assuming you've got enough to keep you busy. :-)

[Quoted text hidden]

Stefan Karpinski <sgk@cs.ucsb.edu> **Thu, Nov 30, 2006 at 2:06 PM**
To: Kevin C Almeroth <almeroth@cs.ucsb.edu>

Great. I certainly do have plenty to keep me busy.

[Quoted text hidden]

Kevin Almeroth <almeroth@cs.ucsb.edu> **Wed, Dec 13, 2006 at 10:12 AM**
To: Stefan Karpinski <sgk@cs.ucsb.edu>

2. It is not clear that using CBR and ignoring the TCP feedback loop does not lead to significant artifacts.

This is true. The main reason we don't use TCP streams is because Qualnet doesn't support it. However, the fact that most experiments use UDP CBR traffic, while much real traffic is actually TCP is only a stronger argument against standard traffic models. So while this is a limitation of the research, it actually doesn't hurt the conclusion of the paper at all.

KCA->And this is something I think you can talk about in the paper... just include the kind of justifcation you've got here. Going forward, will need to find a way to look at TCP or even some variant thereof.

> **3. It is not clear why the workload that you consider is appropriate for the is study as the IETF meetings has a completely different set of dynamics than lets say a coffee shop.**

The main reason that the IETF data set is a good one is that it has a broad range of behaviors. There's times when the usage is much like a coffee shop, and others when it's completely different (e.g. during the plenary session). The next obvious step is to use multiple different data sets, but before we do that, this is a good first data set because it is unusually diverse in behaviors.

KCA->Again, you want to include this kind of consideration. Something like: while we know the IETF meeting is a specific venue, we believe that it is generally representative. We leave for future work how to generalize our results across a wider range of scenarios.

> **4. You seem to ignore mobility, e.g., if a participant moves from one access point to another.**

This is true, we ignore mobility entirely. It's on purpose since we want to isolate the impact of traffic behavior by itself. Mobility is another, additional layer of complexity which would only serve to muddy the results.

KCA->So say this... we can certainly ignore mobility in a one-hop network in which mobility often means disconnect-and-reconnect.

> **5. It is unclear how the capacity constraints intact with the workload generation.**

I'm not sure what this means. Any ideas?

KCA->If network capacity varies, how does that impact results? For example, if there is congestion, would a model correctly capture the resulting impact on behavior? I've always wondered this myself. Though I'm not sure there is a lot you can do about it.

> **Nevertheless this paper is nicely done and represents a first step towards showing that a more sophisticated workload model is need. This reviewer just wishes the authors would have gone a number of steps further along this road.**

KCA->My interpretation of this review is that the reviewer felt we were trying to over-sell what we were doing. We should couch our contributions by saying that we feel this is a first step, and from this one case, we hope to learn more about what to analyze, but in the meantime, we've already got some interesting conclusions.

> **A very interesting paper about the problems of making studies relying in CBR traffic only. The paper covers many issues of the problem, and presents a _very_ interesting work. However, it is basically FLAWED. The usage of the trace data, as it is, is not a good reference for comparison with CBR-only synthesized traffic. The data should be filtered first - and then the work would become very good.**

I'll talk more about the filtering bit later, but this really misses the point of the paper.

KCA->The point for us though is that if there is this kind of confusion, it is our responsibility to better describe what we are doing. Similar to what happened when you presented this to MOMENT, the graphs take a LOT of interpretation to understand what is really there. Assume you need to explain more clearly what is happening so not-so-smart (not saying dumb) people don't miss the point. After all, if they miss the point of a great paper, the end result is still rejected.

> *** Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief.]
> **- unbelievable the action of confronting real (generic) data for comparison with CBR only models!**
> **- the implicit unclearness about the usage of CBR models, and the value of the claims extracted from these models. Ad-hoc simulations use NOT only CBR, and do not claim that these models capture all aspects of the reality**
> **- the implicit "work in progress" from the last lines. Some added results are in some places noticiably missing from the paper.**

These comments are barely intelligible, but in general they're just wrong, since not all of the traffic models we compared with were CBR. I'm not sure how to make this more clear. Ideas?

KCA-> Not much you can do or should do about comments like this. Even though I mentioned above about making the paper easier to follow, there is only so much of this you should do.

> *** Detailed Comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper. Also provide feedback to the authors.
> **Strictu sensu, the simulated network should reflect the physical one where**

> **the trace was obtained. In fact, mobility and traffic patterns (both are indistinguishable in the trace) are associated with AP coverage, and as such you could be putting too much load in a single AP. Also, the traffic in the trace is probaly biased by the source (an IETF meeting) which may not be so general as desired: definitely the traffic is typical of an ad-hoc.**

This is a common immediate reaction to this work: shouldn't you be trying to reproduce the original IETF network? Of course, the answer is that this is not the point. We're not trying to reproduce the IETF network, but rather use it's traffic to examine the difference between real and synthetic behaviors.

KCA->Since this is a common response, we should address it directly...

There's a single common issue here: people want to couple all aspects of behavior together — mobility, TCP feedback, the physical setup of the IETF venue, the locations of nodes — because their intuition is that they are all inextricably linked. And the response is that, yes, they are linked, but the linkage is not inextricable. Being able to approximate traffic behavior accurately without considering all the other factors is still better than not being able to approximate it at all, which is the current situation. And we need to walk before we can run. Trying to model all of these aspects together realistically is like expecting an infant who just learned to crawl to run a triathlon. Moreover, if we cannot accurately reproduce realistic traffic behavior in isolation, we certainly don't have any shot at doing so when all these other factors are included.

KCA->Good stuff to say in the paper!!

> **- It seems that the authors go purposely through a lengthy procedure instead of the straighforward approach, in order to be able to give some definitions and crunch some numbers. The CBR model is evaluated by its effect on performance metrics of interest. Wouldn't it be more direct to compare the wireless traces to their CBR model and measure that difference?**

Ah yes. The "straightforward" approach. But what do you compare? We already know that real behavior is not CBR. The question is whether it matters or not. The reason for writing this paper is so that people cannot say, "well, yes, we know that CBR isn't perfect as a traffic model, but it's good enough." It is not good enough in any sense. But how do we address this reaction? I don't really know.

KCA->Easy enough: I think the reviewer slightly missed the point of the paper. Given this misinterpretation of the goal (the paper is NOT about saying CBR is bad but WHY it is bad and by HOW much). If they miss the point, we are doomed in the rest of the paper. So, all of this goes back to a VERY carefully worded introduction and background. I think if we nail the introduction, we solve about 90% of the reviewer feedback problems.

> **The main weaknesses, mentioned in the previous section, have to do with the problem statement itself. The paper could be further improved by also addressing the following points:**
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> **- CBR is a good model fo VoIP and for stream traffic in general.**

I would argue that this is in fact not the case: VoIP traffic is not really CBR, and I bet I could look take a trace of VoIP traffic and show that CBR does not accurately reproduce it's characteristics. It's actually an interesting research idea. We could easily apply the same techniques to specific kinds of traffic. All we really need is access to the traces.

KCA->We should be able to find these traces in the last IETF data since we collected full packets. And I know there is VoIP traffic there. This would be another great incremental paper to work on. Heck, someplace like NOSSDAV would be great. Given that it is multimedia traffic, you open a whole new set of conferences where the work would fit.

> **- Considering CBR with respect to jitter has the advantage that it allows to evaluate the jitter caused by the network alone. In contrast, considering a bursty source results in observing the jitter caused by the source+the network**

This is a very good point, and I'm not sure if Qualnet evaluates jitter by looking at the variance between

inter-packet arrivals or by the variance of packet transit latencies. The latter metric measures jitter due only to the network, while the former combines it with source jitter, which would definitely make for an unfair comparison.

The problem here is that to change the way the metrics are defined would require re-running all the simulations. In fact, getting all the metrics nailed down properly is something that really ought to be done now. I'm going to meet with Krishna this week to talk about this. I may end up having to instrument Qualnet to collect metrics the way I want them collected.

KCA->If 90% of the comments can be fixed with the introduction, another 8% can be handled with the point that this is the first step of many. There is a fine balance to strike between saying this is preliminary work and saying this is the first step. The key is probably to have a longer "future work" section where we outline all of these kinds of improvements. This goes hand-in-hand with an introduction that properly sets expectations for what this paper DOES and does NOT deliver.

- The proof about the log(y/x) being the only metric with the desirable properties is nice. However, the reader can't help but wondering whether the desirable properties themselves have been purposely chosen for their tractability. Are these really the most approrpiate desirable properties for an error function? How is this typically done in math/statistics? - please reference as appropriate.

Any ideas on how to address this suspicion. The presentation in the paper was as good as I could give. The implicit assumption
of this entire analysis is that error will be introduced proportionally; i.e. that erroneous values will generally be of the form y = c*x, where x is the "true" value and y is the observed value. I'm not sure how to argue that this is the case, or even that it always is.

KCA->Indeed an interesting comment... probably not something you need to address in this paper, but definitely something to keep in mind longer-term. At some point you'll need to answer it. How? By looking at different metrics and evaluating which has the right properties.

Stefan Karpinski <sgk@cs.ucsb.edu> **Wed, Dec 13, 2006 at 10:57 AM**
To: Kevin Almeroth <almeroth@cs.ucsb.edu>

Great. Thanks for the feedback!

[Quoted text hidden]
