Discussion Record

SESSION NO. 63 - STATISTICAL ANALYSIS OF TRAFFIC DATA

Chairman - K. RAHKO (Finland)
Discussion Leader - K.M. OLSSON (Sweden)

Paper No. 631
Author: G. LIND (Sweden)

P. LE GALL (France): Special observations on B-subscribers stages prove that the mean call congestion (before the called subscriber) may be much greater than the mean time congestion, particularly in the case of a single line.

You give an explanation by considering the effect of the skewness of the subscriber's traffic distribution, with the lost call model. In my paper (No. 125), I gave another explanation: the consequence of the repeated attempts, and I presented some formulae. Don't you think it would be interesting, now, to consider the two phenomena simultaneously to investigate the effects?

G. LIND: I agree with you. Perhaps I should mention that one of my models, is in fact, a sort of repeated attempt model. In a rough way, it is a Poisson stream of incoming calls when the subscriber is busy. We know it is not a Poisson stream but I have a parameter here that I can adjust, so that I can at least allow for the proportion of repeated calls as compared to the other incoming calls. So, in this model there is some account of repeated calls also. Of course, it might be possible to simultaneously work on it in another way - more according to your principles - and I will investigate that.

P. REID (Australia): Assumption 2.1(b) assumes a constant incoming intensity, λ, independent of whether the sub is busy or free. I agree that for free sub and first calls to busy sub that this assumption will hold, but, as shown in other papers, this does not hold when the sub is busy, as the repeated attempts will alter the arrival intensity.

1. Did you consider including this effect in your study?
2. Would you please comment on the effect that this inclusion would have on your results?

G. LIND: I think I have answered the first question in my reply to Dr. Le Gall, actually. As for the effect, in question 2, there could be some comments made. I expect that it should be increased; but of course I would like to mention that we have super-position effects here for the total group of subscribers.

J.P. FARR (Australia):

Comment 1:
Some elementary calculations which I have done support your hypothesis that a badly skewed distribution of traffic per subscriber would adequately explain the observed fact that calls meeting wanted sub busy (about 20%) is much greater than the simple probability of a sub being busy (e.g. 8% for a total traffic of 0.08 e/subscriber, based on uniform traffic per subscriber).

Comment 2:
The effect of repeated call attempts to busy subscribers would seem to be a useful aspect to consider in future studies of this subject, as was also suggested by Dr. Le Gall in the first question of this session.

G. LIND: The first one, I have no special comment. I agree with you there and the other one is about the same question as we had from Dr. Le Gall and that has already been answered.

Paper No. 632
Authors: L.E. NOBELA and J.M. PILLADO (Spain)

K.M. OLSSON: In the introduction to your paper you state that in the so-called Karlsson rate method the mean number of call meter pulses is dependent on the distribution of the conversation times and not only on their mean. I know that there are some variants of Karlsson's method, but if you have in your mind the original Karlsson method it is easily proved that the expected number of pulses is equal to the ratio of the mean conversation time to the distance between two successive pulses. I proved this at the first ITC in Copenhagen. Would you comment on this?

J.M. PILLADO: Thank you for your question Dr. Olsson. I think you are right, as you suppose in your theory, that in Karlsson's rate method the time base for the pulses does not depend on the moment in which the call has been originated: knowing only the mean conversation time, one may estimate the number of metering pulses. But, in these cases, it is not the case when the base for pulses depends on the moment in which the call has been originated, it is necessary to know the distribution law of the call holding time. Obviously, ours is the latter case.

This subtle difference must be considered by those people who intend to apply the method described in our paper. Furthermore, it will be seen that as the pulse intervals tend to zero, the two cases come together.

M. CHIN (Australia): Your practical paper is most useful. Looking at the FORTRAN program listing, page 7, line 790 and line 800, I presume "... GAMMA (P) ..." is the Gamma function. Would you care to comment please?

J.M. PILLADO: As you presume GAMMA (P) represents the value of the Gamma Function when the variable takes the value of P previously defined - see line 770 - as P = 1 + 1/2. According to formula 21 in the paper, the mean holding time for a Weibull distribution is:

\[ \text{mean holding time} = \frac{\text{gamma function}}{1 + 1/2} \]

You can find the demonstration of this formula, for instance, on reference (2) shown in my paper. By using the facilities supplied by FORTRAN subroutine you can get the value of \( \Gamma(1 + 1/2) \) writing directly "GAMMA (P)", P being the value between the brackets. Reminding that \( \gamma \) has been defined as X/2 and \( \lambda + \gamma \) as \( \lambda \), it is easy to prove the sentence labelled on line 790.

Similarly the variance for a Weibull distribution is expressed by the following expression:

\[ \sigma^2 = \frac{\gamma^2}{1 + 2/\lambda} - (1 + 1/\lambda)^2 \]

which is represented by the sentence on line 800. Thank you for your question.
V.B. IVersen (Denmark): I agree it is important to take account of non-exponential holding times. Palm's form
factors \( \frac{\text{mean}}{\text{variance}} = 1 + \text{variance} \) appears in many formulae and is 2 for exponential holding times, but 3 - 5 for real
holding times. You have to take account of the measuring principle, (i.e. the scanning principle) when you use a time unit of 9
seconds [ref. formula (5.1) in my paper #312]. The first class in your tables is always so small because of the measuring
principle. If you take account of the scanning principle, I think you will get a strictly decreasing failure rate in table 2. Also the scanned exponential
distribution ( = the Westerberg distribution, ref. paper #312) fits still better than your corrected values for the
asymptotic distribution.

It is natural that the Weibull-distribution fits better because it has 3 parameters, in comparison with 1 for the
exp. distr. This Weibull-distribution is, however, difficult to transform according to the measuring
principle and difficult to deal with in general. It would be much more natural to use the hyper-exponential
distribution which is a weighted sum of two exponential distributions, and this also has 3 parameters (this is the
maximum number you can estimate from data in praxis). This distribution is easy to transform according to
different measuring principles, can be included in birth and death models, and is easy to handle analytically.
This distribution has for Danish measurements given an excellent description of all types of holding times in
speech paths and also times in computer systems.

Have you investigated your data with regard to hyper-

D.J. Daley (Australia): I would like to make three points concerning the present paper and correlations that occur in
many of the types of Markov chains that occur in queueing
systems; certainly my remarks apply to all the examples in the
present paper. First, on a technical point, the
infinite waiting room chains are not uniformly
generically ergodic. Second, the chains may be
stochastically monotone and hence the correlation
coefficients \( P_n \) or \( \log n \), are positive and decrease with increasing \( n \). Third, for heavy traffic, the individual
coefficients \( P_n \) are roughly linear in \( n \),

\[
P_{\text{mean}} = n(1 - A^2) \quad \text{(const.)}
\]

where \( A \) is the relative traffic intensity, and the sum of these coefficients, which is what
really matters for the large sample variance \( V_r \), is roughly proportional to \((\text{const.})/(1 - A^2)\), much as in Dr Olsson's paper earlier in the conference. Would the author care to
comment?

D.J. Songhurst: I agree with all of your remarks. On the
first point the paper is not sufficiently explicit. It is
certainly true that the delay systems considered are not
uniformly geometrically ergodic, but this does not appear
to affect the validity of the results. I have not been
able to determine the most general conditions under which
the main theorem holds.

Paper No. 634
Author: O.A. Pedersen (U.S.A.)

D.H. Barnes (U.S.A.): Your use of ranges to determine
unbalance situations is an interesting approach. Your use
of this approach seems to be for reducing data processing.
Does this reason really apply when we have such a large
availability of computers, even micro-processor hand
computers which can very easily solve statistical problems
such as analysis of variance? The next part of my question
is: Your method seems to quite easily detect unbalance
situations with optimum sample size, but we must also
accurately estimate group to group individual differences
in order to take the best corrective action. How would you
handle this?

Lastly your example shows very large server groups. In the
more common load balance situation for line or trunk group
balance we deal with an order of magnitude smaller groups.
Do you think your range method will apply as well to these
with their higher coefficients of variation or both chance
variation and variation due to greater (possibility) for
non-homogeneity.

O.A. Pedersen: The use of range methods for estimating the
standard deviation and sequential analysis of load balance
data are intended to reduce data processing. While this
may not be significant when considering the large
availability of computer systems it is in the
large scale use of facilities in a centralized service
bureau that controls and conducts traffic studies on a
number of central offices.

After detecting the existence of an unbalanced situation,
several methods are available for testing the statistical
significance of group to group differences in order to
identify those groups requiring corrective action. Two
commonly used methods are those of Tukey and Scheffe. They
are discussed in the references, Bowing and Lieberman [12]
and especially Snedecor and Cochran [13].

The methods in my paper are applicable to smaller groups
than those appearing in the illustrative example.

Remembering that the critical value of the test statistic
used to detect unbalance is a function of \( n \) or where \( n \) is the
critical value of the test statistic, and \( \sigma \) is the
deviation of the estimate of the range due to chance
variations, relatively large values of \( n \) due to the
smaller size of groups reduces the sensitivity of the test
statistic thereby requiring more data for arriving at a
conclusion for a specified \( n \). The greater chance or
possibility of having non-homogeneity with smaller groups
increases the need for balance studies. The greater chance or
possibility of having non-homogeneity with smaller groups
experiencing relatively high line or trunk assignment activities makes more frequent balance studies
necessary.

Paper No. 633
Author: D.J. Songhurst (United Kingdom)

K.M. Olsson: I think that your method could be
characterized as an ingenious one. My question is: Do
you think it is possible to develop it further so that it
will be possible to get further terms in the asymptotic
expressions?

D.J. Songhurst: It certainly seems possible to use this
technique to obtain the second term in the asymptotic
expression. It should also be noted that, for small
samples on small systems, the variance \( V_r \) can be computed
directly using recurrence relations. This has been done
for observations of the proportion of lost calls in the
M/M/N loss system, and the results indicate that the
asymptotic variance is practically attained for sample
sizes equivalent to about 5 mean holding times. For the
large samples likely to be required in practice, the
asymptotic form of the variance will generally be
adequate.
In section 3 of your paper you define a number of constants $a_i$, characterizing group No. $i$. $a_1$ and $a_n$ are respectively the largest and smallest values of the $a_i$'s and then $w = a_n - a_1$ is the range of the $a_i$'s. You define $a_0$ and $a_2$ respectively as the smallest and largest values of the estimated values of the $a_i$'s. Then you take $w = a_0 - a_2$ as an estimate of the range $w$. In my opinion this estimate is biased. The reason for this is that $a_0$ and $a_2$ are not necessarily estimates of $a_1$ and $a_n$, respectively. Would you comment on this?

O.A. Pedersen: The fact that $a_0$ and $a_2$ are not necessarily estimates of $a_1$ and $a_n$, respectively, does not cause the estimate $w$ of the range $w$ to be biased. Any bias would be due to $w$, like the standard deviation $s$, being a measure of dispersion and therefore inherently non-negative. Thus, for example, if $w = 0$, then we would most likely have $w > 0$. It is not possible to have $w < 0$.

The identification of the groups with characteristics $a_0$ and $a_1$ is a different problem from the estimation of the value of $w$. Comparisons to determine the statistical significance of the relative differences of the estimated values $a_2$ of the group characteristics $a_0$ are necessary for making the identification. It is quite possible that no clear-cut identification can be made because of clustering of the estimates $a_2$.

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