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# STATISTICAL THEORY AIDS INFERENCE IN SCIENTOMETRICS

## (COMMENTS TO PUBLICATION RATE AS A FUNCTION OF THE LABORATORY/GROUP SIZE BY M. M. QURASHI*)

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The number of publications of a laboratory will fluctuate over time, even with a fixed number of scientific personnel. The main point of this note is that in order to decide whether there is a difference in the productivity (publications per capita) of laboratories of different sizes (number of scientific personnel), it is necessary to take such fluctuations into account. Even a very conservative estimate, as will be provided below, of the extent of random fluctuation in the number of publications shows no convincing evidence, in the data I have collected,** that there exist systematic peaks in productivity for laboratories of particular sizes.

I am grateful to Dr. *Qurashi* for drawing my attention to, and sending me copies of, his studies. I was not aware of them when I wrote[1,2].

Here I will first review *Qurashi*'s treatment of my data. I will then comment briefly on the data and claims of *Qurashi* and of *Wallmark* et al.

The original analysis of the data[1,2] is largely standard statistical practice, appropriate to the data. The following more ad hoc procedure for analyzing the fluctuations in these data is geared to Dr. *Qurashi*'s approach.

In his Table 1, *Qurashi* computes the ratio R of the total number of publications to the total number of scientists in laboratories in each given range of size 1–3, 4–6, and so on. How can one decide whether a large value of R represents a real peak or merely a random fluctuation?

One systematic approach, which we shall follow, is to estimate the distribution of R, viewed as a random variable, and then to see whether the data reject the null hypothesis that the distribution of R is independent of laboratory size. If the

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**Data from the National Cancer Institute, National Institute of Medical Research and Rockefeller University null hypothesis is rejected, then the data provide evidence for a peak. If the null hypothesis is not rejected, then the data do not provide evidence for a peak.

A rough way to do this is to suppose that within each range of laboratory size, the total number of publications, denoted by $\Sigma E$, is a Poisson random variable whose mean is equal to the observed, or sample, value of $\Sigma E$. The Poisson distribution is a conservative model for the variation in $\Sigma E$ because it neglects all differences between individuals and between laboratories. Such differences would generate additional variance in $\Sigma E$.

Under the Poisson model, the standard deviation is equal to the square root of the mean. When the mean is large, the Poisson distribution is approximately normal. Thus we can compute a crude 99 percent confidence interval for $\Sigma E$. The lower end is given by $L = \max(0, \Sigma E - 2.57[\Sigma E]^{1/2})$ and the upper end is given by $U = \Sigma E + 2.57[\Sigma E]^{1/2}$.

The lower bound on a 99 percent confidence interval for R can then be estimated as ranging from $L/\Sigma F$ to $U/\Sigma F$, where $\Sigma F$ is the total number of scientists in the laboratories in the given size range. This is a very conservative estimate of a confidence interval for R because all random variation in $\Sigma F$ is ignored. E.g., this estimate of a confidence interval for R makes no allowance for the possibility that the scientists enumerated are not the same in number as the scientists who did the work being published. Hence this estimate is likely to understate the actual range of random fluctuations in R.

The net effect of the two conservative assumptions made, first that $\Sigma E$ has purely Poisson variation, and second that $\Sigma F$ has no variation whatever, is that the estimated 99 percent confidence interval for R is likely to exaggerate the statistical significance of any difference between an observed R and the quantity R*, defined as the ratio of the total number of publications of an institution to the total number of scientists of that institution. This means that no real peak in R is likely to be overlooked, although some apparent peaks in R may not be statistically significant.

My Table 1 shows R, the corresponding confidence intervals, and R* for the National Cancer Institute[2] (corresponding to Dr. *Qurashi*'s Table 2), the National Institute of Medical Research[2] (corresponding to Dr. *Qurashi*'s Table 3), and the Rockefeller University[1] (omitted by Dr. *Qurashi*). Asterisks* show when the ratio R* for the entire institution falls outside the crude confidence interval for laboratories in a given size range.

The most important feature of Table 1 is that *there is no evident pattern in the location of the asterisks when the three institutions are compared.* Only one of the "peaks" identified by Dr. *Qurashi* is starred (namely, R = 2.24 for laboratories of size 25–27 at the National Cancer Institute). Even if the confidence intervals

Table 1.
**Minimal 99 percent confidence interval for R**

| Size range | R | Lower end | Upper end |
|---|---|---|---|
| | | National Cancer Institute (R* = 1.18) | |
| 1–3 | 0.50 | 0.00 | 1.79 |
| 4–6 | 1.49 | 1.00 | 1.98 |
| 7–9 | 1.55 | 0.86 | 2.23 |
| 10–12 | 0.83 | 0.43 | 1.22 |
| 13–15 | 1.29 | 0.51 | 2.06 |
| 16–18 | 1.27 | 0.92 | 1.62 |
| 19–21 | 1.13 | 0.90 | 1.35 |
| 22–24 | 0.99 | 0.77 | 1.20 |
| 25–27 | 2.24 | 1.70* | 2.77 |
| 28–30 | 1.09 | 0.73 | 1.44 |
| 31–33 | 1.06 | 0.73 | 1.39 |
| 34–36 | 1.21 | 0.94 | 1.49 |
| 37–39 | 0.79 | 0.43 | 1.16* |
| 40–42 | 0.88 | 0.50 | 1.25 |
| 43–45 | 1.44 | 0.97 | 1.91 |
| 46–48 | 1.04 | 0.66 | 1.43 |
| | | National Institute of Medical Research (R* = 0.90) | |
| 1–3 | 0.67 | 0.00 | 1.88 |
| 4–6 | 1.00 | 0.44 | 1.56 |
| 7–9 | 0.67 | 0.30 | 1.03 |
| 10–12 | 1.00 | 0.55 | 1.45 |
| 16–18 | 1.00 | 0.55 | 1.45 |
| 19–21 | 1.11 | 0.81 | 1.41 |
| 22–24 | 0.51 | 0.24 | 0.79* |
| 34–36 | 0.94 | 0.51 | 1.37 |
| | | Rockefeller University (R* = 1.02) | |
| 1–3 | 1.21 | 0.68 | 1.73 |
| 4–6 | 1.28 | 0.95 | 1.62 |
| 7–9 | 1.00 | 0.54 | 1.46 |
| 10–12 | 0.88 | 0.62 | 1.13 |
| 13–15 | 1.09 | 0.82 | 1.37 |
| 16–18 | 0.96 | 0.65 | 1.26 |
| 19–21 | 0.79 | 0.49 | 1.09 |
| 21–24 | 1.24 | 0.89 | 1.58 |
| 25–27 | 0.88 | 0.64 | 1.11 |

were really exact 99 percent confidence intervals (and they are very probably too small), one would not be surprised to find one interval lying above its corresponding R* in 33 tests (16 size ranges for N.C.I., 8 for N.I.M.R., and 9 for R.U.). Such a "peak" is not distinguishable from a peak by chance alone.

I conclude that there is no convincing evidence in these data for peaks of productivity in laboratories of different size. A conservative analysis of the likely magnitude of the random fluctuations in the ratio R of publications to scientists indicates that the "peaks" identified by *Qurashi* may be entirely attributed to random fluctuations.

## Data of Qurashi and Wallmark et al.

*Qurashi*'s Figures 1a and 1b display frequency distributions of the size of laboratories or research groups, but do not bear directly on the effect of laboratory size on research productivity. I am not aware of evidence, offered by Dr. *Qurashi* or others, to support his "assumption that the stable or most probable system would correspond to maximum output."

The data in *Qurashi*'s Fig. 1c show a rise in annual research output with increasing numbers of scientific officers. These data are based on a time series for the Karachi Laboratories rather than on a sample of many laboratories at a single time, and hence are not strictly comparable with the data from my papers. *Qurashi* does not compare the goodness of fit of a straight line with that of a quadratic function fitted to these data, so it is not possible to conclude that they provide statistically compelling evidence in favor of a maximum per capita output at a particular number of scientific officers. (Contrast this with my earlier analysis[2]: "For a more formal test of nonlinearity, the mean square residual from a linear regression $Y = A + BX$ is compared with the mean square residual from a quadratic regression $Y = A + BX + CX^2$. The data from NIMR and NCI give no evidence of nonlinearity significant at the 5 percent level. Neither do the data from RU. . . Finally, the pooled data from all 3 institutions also give no evidence of nonlinearity significant at the 5 percent level.")

The paper of *Qurashi*[3] from which Fig. 1c is taken includes also the graph that I reproduce as Fig. 1 along with *Qurashi*'s original capiton. The data in this graph are also drawn from a time series, rather than from a collection of laboratories at one time. The evidence for a levelling off in output beyond 200 scientists is clearly very weak, and the linearity in the relationship between total output and total number of scientists is visually compelling. Fig. 1 is nicely consistent with the cross-sectional findings in my papers[1/2].

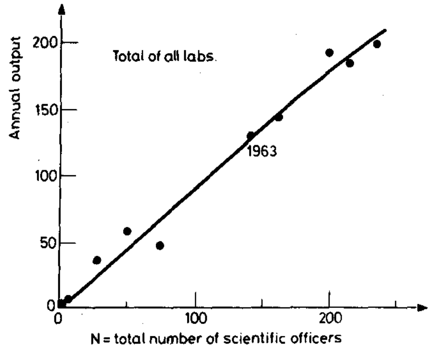


Fig. 1. Dr. QURASHI's caption: "The total annual output of all the P.C.S.I.R. Laboratories taken together, plotted against the total number of Scientific officers, N, responsible for this output, showing a practically linear initial rise, followed by a levelling off or saturation trend beyond N = 200 scientists." Source: QURASHI[3]

In his conclusion, *Qurashi* repeats the claim of *Wallmark* et al.[4] al.[4] that "research efficiency, as defined, increases exponentially with size of the research team.", I have already reported[2] a detailed statistical re-analysis of their original data, from which I concluded: "These data thus do not provide evidence that research efficiency increases, linearly or exponentially, with the size of a research group. The data are consistent with the hypothesis that net references per scientist are independent of the number of publishing scientists in a team." Merely repeating the conclusion of *Wallmark* et al. in no way strengthens their claim.

*Qurashi* concludes his observations with the assertion that (at least for small numbers N of research scientists) per capita output R is proportional to N. If this assertion were true, there would be no peaks in per capita productivity as a function of increasing N, but rather a linear increase. Hence this assertion appears to contradict *Qurashi*'s claims to find peaks in per capita productivity in his Fig. 1c and in his Tables 1–3. Moreover, this assertion implies total output per laboratory should be proportional to the square of the number of scientific workers. As the quotation above from my article[2] shows, I have examined my data for the presence of a quadratic dependence of total output on laboratory size and have found none. Thus *Qurashi*'s concluding assertion that per capita output R is proportional to the number N of scientists appears both to contradict his earlier claims and to lack empirical foundation.

## Conclusions

Laboratory size may affect the number of scientific publications per capita. However, no analysis I have seen of my data or of other data has demonstrated the existence of such an effect convincingly.

An analysis[5] of patentable research among 500 leading industrial firms concluded that the number of patents filed per year divided by annual sales is independent of the annual sales, i.e., on the average, small, medium and large industrial firms are all equally inventive. If patents are viewed as a measure of research productivity in the commercial sector, and if sales are viewed as a measure of size or research effort (a company's research budget would be a better measure), then this finding that the ratio of patents to total sales is independent of total sales exactly parallels my finding that publications per capita are independent of laboratory size. The parallelism of these findings suggests the possibility that, in general, the productivity per capita of a research effort is independent of the total size of the research effort, whether the setting is academic or commercial.

Whether or not this empirical generalization is ultimately confirmed, the methodological message of this note seems to me less arguable.

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