Reviewer's report

Title: Geographical variation in radiological services: a nationwide survey

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Reviewer: Vijay Rao

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General
Authors have examined geographic variation in radiological examinations in Norway.
It would help, enormously to include a preliminary table of the counties, the population, density, number of radiologists, etc.

Method

1. Not frequencies, rates per thousand.

2. It seems that for the two large hospitals in Oslo mentioned, exams were attributed to the county of the patient; for other providers, it was assumed that the patients were residents of that county. Any check on this assumption? I would assume that many other providers in the metropoles were seeing patients from other areas.

3. "Pearson's R describes correlations" might better be "Pearson's r (small r) is a measure of association between two continuous variables that ranges from -1 through 0 to +1" or some such.

4. Measures of accessibility and their source ought to be described in the methods section--population density, number of ordinary radiological facilities, number of radiographers (is this word used in English?) are probably pretty clear, but "the proportion of the population living in municipalities with general radiological services" is not. What was the rule used for deciding this and calculating the proportion? Distance from a provider above and below a certain limit? Existence of a provider in the same municipality? What is the unit of the municipality? (In the US, a city might be in a village in a town in a township in a county in a state--these are all municipalities. Clearly, authors are using a unit smaller than the nineteen counties of their study to calculate this, so what is it?

Results

1. It would be preferable to present both High/low ratio and COV in table 2 and High/low ratio and COV for each modality in table 1. High/low ratio would have the advantage of being very easily comprehensible, COV of not being distorted by a extreme high or low value--and authors might want to explain this.

2. Table 3. As well as noting which correlations are high, authors might note that 23 of 24 correlations are positive (and you can calculate the binomial probability of this). Since these correlations are based on only 19 cases, you might want to indicate which correlations differ significantly from zero. I realize these are correlations conducted on a population not a sample, but authors could consider year 2002 data to be a sample of some true value that varies randomly across years...

3. "The correlation for head/brain examinations is illustrated in Figure 1" Since authors are dealing with Pearson correlations, which are typically illustrated with a scattergram and a regression line whose slope is the r, calling a bar chart of the raw data an illustration of the correlation is idiosyncratic. Is this a stacked bar chart? It would be better call this table "Rate/1000 inhabitants of CT and MR brain examinations.

4. Contribution from private and public institutions: the use of the coefficient of variation is not very enlightening here. Authors have 19 counties. 5 have no private providers. Of course the COV is going to be higher than for providers who have values in 19 counties. Authors should probably just discuss the contribution of private providers vs. public providers. This whole section really contributes nothing to the paper and neither does the ad hoc argument about private providers in the discussion. It would be best to
just drop this section.

Discussion

1. Migration--authors cite one study of three private institutions (three counties) as evidence that migration is not a problem. This is such a restricted study it amounts to answering the objection anecdotally. Authors then go on to a couple of arguments about some neighboring counties which are also anecdotal. Arguments presented by authors are based on three counties with tiny population densities.

2. Morbidity--this section is completely anecdotal. If authors want to deal with morbidity, I'd prefer authors do what they did with accessibility--create some indexes, like prevalence of chronic disease (statistics which they probably have) for each county, and do a quantitative analysis.

3. The final paragraph in the morbidity section on substitution has nothing whatever to do with morbidity.

4. Accessibility--While arguments about Telemark and Oppland make sense, arguing a point on the basis of two counties with population densities of 7 and 12 /km2 is again anecdotalism. Authors need to consider all the other permutations, and give at least several examples.

5. Now that I think about it, if measure of accessibility means (essentially) geographic proximity to an x-ray machine or better, overall findings may simply reflect the high proportion of x-ray exams within radiology. It really is critical that authors define this measure.

6. Discussion--"economic incentives cannot be ruled out" This is a pretty speculative paragraph.

7. Public-private has just not been developed well in this paper and this section doesn't help. Better dropped. By the way, the treatment of the example of US of the pelvis, involves an incredibly roundabout way of saying (probably) that women go to private gynecologists. (also roundabout discussion of Figure 2 in results).

8. "However, when geographical variation does not correspond to differences in the prevalence of disease, it is reasonable to assume that the probability of failing to receive necessary examinations is higher for patients in low use counties, and conversely, that the probability of receiving examinations that are not strictly medically indicated is higher for patients living in high use counties."

This is a remarkable sentence. Authors haven't established #1, except by a couple of examples for Oppland and Telemark. Neither of the clauses that follow are anything but assumptions. Authors might equally say, only the people with the highest level of utilization are getting adequate care. Or the lowest.

Conclusion

Socioeconomic differences ... may also play a part. This factor appears in the conclusion with little evidence except for a citation in the discussion.

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This paper is a very valuable contribution. I wish that the authors had gone further in doing quantitative analysis of factors such as socioeconomic factors, morbidity, etc. Instead, authors present what is essentially a descriptive paper to an explanatory level by questionable assumptions and anecdotal reasoning. If they remove these, the paper is very good and important.

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

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

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Discretionary Revisions (which the author can choose to ignore)