Reviewer's report

Title: What determines income inequality between French male and female GPs - the role of medical practices.

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Reviewer: anne-laure samson

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Referee report for
“ What determines income inequality between French male and female GPs: the role of medical practice ”

In this paper, the authors measure and analyze the income gap between male and female GPs, using a data set containing information on about 320 GPs practicing in one French region (Midi Pyrenees).

In my opinion, even the results are obtained using a small sample of GPs that may not be representative of all French GPs, the paper is interesting and original (it is the first time this topic is addressed using French data). I have some comments, that I explain hereafter, that mainly concern the methodology used and the interpretation of the results.

I - Major Compulsory Revisions

1. Comment on the background: I think the authors should motivate more their analysis: why is it important to study male/female GPs income differentials in France? This is not presented in the introduction. For example, maybe the authors could emphasize that there is currently a rapid feminization of the profession in France (and give figures). Female GPs working less than males, it may lead to potential problems for the regulation of ambulatory care in France and especially the regulation of the supply of physicians. It is therefore important to understand the reasons for this income differential.

2. The authors should detail more the different results obtained:

   a. The descriptive statistics (table 2) are not commented at all (p.5). The authors should give the main results of this table and the main differences between male and female GPs (and for example say something about a nice result that could motivate the empirical analysis, which is that male GPs work 48% more than female GPs but only earn 34% more). I think the 26% difference in income (obtained from the regression) is stated in the wrong paragraph (“male/female differences”), a paragraph that only deal with descriptive statistics.

   b. Results of the estimates are not new and can already be found in the literature (p.5) but I think the authors should explain them a bit more : i) the experience effect shows that income increases until 24 years of experience and then
decreases: this could be an age or a generation effect; ii) how to explain the “rural” effect? The “sector 2 effect”? I think the paper should be presented slightly differently because the expected effects that the variables included in the regression should have are very well described (p.11-12) but the estimated/obtained effects are not. Maybe the authors should combine both parts of the paper (specifications p.11-12 and estimates p.5) and give more importance to the interpretation of the effects obtained in the paper;

c. Results of the decomposition: interpretation of the constant (p.7 and p.9): could the constant also reflect discrimination towards female GPs, for example from some kinds of patients (in rural areas, or older patients) that are less likely to see a female GP?

d. Results of the decomposition: could the authors interpret a bit more the portion attributable to characteristics? For example I am quite interested in the result that female GPs have an advantage compared to all GPs in terms of belonging to the sector 2 and in terms of location. What does it mean? What are the consequences in terms of public policy? (when female GPs are sector 2 GPs, they overbill more than male GPs for example?)

Concerning this second part of the decomposition, I think the authors should make clear that it is an unexplained part of the income gap, and therefore I think only hypotheses can be made.

3. The authors should test the robustness of their results to the definition of the income variable: Could the authors check if their results are maintained when they use an alternative definition of the income variable, i.e. an income variable that only contains incomes earned from the self-employed practice (exclude income from salaried activities, etc.). In fact, variables used in the regression and that explain the differences between male and female GPs are mainly variables that characterize the self-employed activity. As a consequence, it is not clear to me why the global income, including salaried activity, should be used.

4. On the regressions:

a. Could the authors control for other demand variables? i) the % of CMU patients; ii) the level of medical density in each department (specialists and GPs) could reflect the potential level of demand. There is a strong heterogeneity in terms of medical density in the Midi-Pyrenees region that may not be entirely captured by the rural/urban dummy.

b. Because of its endogeneity, I understand why the authors do not include the number of hours worked in the regression (even if they could instrument it using the number of children under the age of 15 for example). But I do not understand why the authors do not present another regression that would explain the hourly income. In the section “background” they state that the literature finds significant differences and smaller male/female gaps using the hourly income instead of the annual income. In their conclusion, the authors interpret their results in terms of difference between male and female productivity. Another analysis that deals with hourly income would therefore be more convincing to say things about productivity. (And I don’t understand well the argument given by the authors p.9
for not doing this kind of analysis)

c. I understand that the authors are constrained by the small size of their sample and that they cannot include lots of explanatory variables at the same time. However, I am a bit embarrassed by the fact that the authors interpret their results by saying that women use more lucrative services (specialized procedures) but without testing it empirically (they mostly rely on descriptive statistics). However, the mix of services used by GPs (ie the fact that they performing pediatric / gynecological follow-ups, …) is not a variable included in the regression. Why? Why don’t the authors perform an alternative regression (if needed, without the variable “number of services” and maybe without the rural/urban variable because it may be highly correlated) but with the variables “gynecological follow-ups, pediatric follow-ups” (or a mix of these variables) in order to quantify the effect of these variables on the income gap.

5. Income decomposition (table 4): Would it be difficult to give standard errors to all variables included in the decomposition to see which variables statistically influence the decomposition?

6. On the representativeness of the sample: The authors use a small sample of GPs, that contains information on GPs practicing in only one region. For the interpretation/potential generalization of the results, maybe the authors should give information on the representativeness in terms of medical density (p.10). Midi-Pyrenees is a region where medical density is quite high compared to the average level: what consequences does this characteristic have on the results and on their generalization?

II – Minor Essential Revisions

7. Minor comments:
   - p. 5: Oaxaca-Ransom?
   - p. 8: The authors refer to Delattre and Dormont (p.8), but I don’t think they should use the term “target income” because the authors do not test for this hypothesis and do not have data to test for this (and the authors are proposing a utility maximization model, rather than a model consistent with the target-income hypothesis). More generally, I think the authors should be very cautious when they use the term “target income” to interpret their results (p. 8) because they do not test whether female GPs have a target that they try to reach (note that, in the literature, it has also been shown that female GPs are less sensitive than male GPs to their target income, as shown by Rizzo and Zeckhauser).
   - p. 11: the creation of the income variable is not very clear to me : why do the authors need to use an expense rate from the ministry of health while they have most expenses in their database (rent for the office, …)? I also don’t understand why the expense rate is so low (25.3% while I thought it was about 45%). Maybe the authors should explain a little more the construction of the variable.
- p.13: the description of the reference is unclear: what is the reference point in the Oaxaca-Blinder? What is the usual reference in the Oaxaca-Ransom decomposition? How sensitive is the decomposition to the choice of this reference?
- p.21: use the term experience and not age in table 4.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests