Referee report on "The Italian wage curve reloaded. Does Occupation Matter?"

The twofold aim of the paper is to estimate the so called "wage curve" (i.e., a (negative) long-run relationship between the level of the unemployment rate and the level of wages ay the local level) by occupation in Italy and to test the existence of a structural break in 1993 (due to changes in the bargaining system introduced by the 1993 Income Policy Agreement).

While I like the idea of testing heterogeneity in the wage curve by occupation and checking the existence of a structural break, I think that this version of the paper suffers from severe problems affecting both the research setting and the robustness of the results of the empirical analysis.

In what follows I shall try to summarize my main concerns:

1. As stated in the paper, the wage curve implies (wage) bargaining between workers and the employer, with a sort of discipline effect played by local labour market conditions (measured by local unemployment rate) on the bargaining process. Hence, I wonder why we should be interested in finding a wage curve for the self-employed and the entrepreneurs, for whom no wage bargaining takes place. While I acknowledge that in the Italian case some forms of self-employment may actually hide dependent work, the author should state this more clearly in the paper and eventually estimate a wage curve only for this sub-sample of self-employed workers – provided they could be identified in the sample.

2. In the theoretical section the author should discuss more at length why we may (or may not) expect to find different wage curves by occupation, pointing out why for some occupations we should find a larger (negative) elasticity between the level of the unemployment rate and the level of wages.

3. The theoretical section is very confused and it is not clear what is the relationship – if any - between the more traditional wage curve and the Phillips curve. On this issue, I recommend the author to read Blanchard, O. and L. Katz (1997), What We Know and Do Not Know about the Natural Rate of Unemployment, Journal of Economic Perspectives, 11 (1), 51-72.

4. There are some mistakes in equations at page 3. For instance, I think that a1 is missing in the first right-hand term of the last equation. Furthermore, if I compare these equations with the specification used in the empirical analysis (see equation A at page 14), it seems that the author estimated the fourth equation, hence a specification of the Phillips curve instead of the wage curve.

5. While it may be interesting to test the structural break in 1993, given the relatively long time span used in the empirical analysis I suspect there may be other relevant reforms in the labour market and particularly in wage bargaining that the author should consider. For instance, in July 1992 another important tripartite agreement was reached to abolish the wage indexation clause (the so-called *Scala mobile*). The author should carefully check whether other relevant reforms were introduced in the previous years.

6. Section on the empirical results is also very confused and most relevant issues/details are not clearly spelt out: what is the number of observations used in the regressions (this is crucial to know whether the authors estimated the model using micro-data or creating cells by macro-region)? If estimates are based on individual data, are standard errors clustered at the regional level? Are nominal or real wages used in the analysis? What fixed effects are included in the last regressions: only regional fixed effects or also, as the author mentions at page 17, individual fixed effects (hence, a linear panel estimator is used)? What is the estimate for logU in specifications including the interaction term of logU with the 1993 reform dummy?

7. While I acknowledge that data on regional unemployment rate (NUT2 level) are not available for the 1977-2008 period, I recommend to estimate the wage curve also on a shorter period of time using this measure of local unemployment for the time span for which it is available, in order to test whether the definition of regions matters for the estimation of the wage curve.

8. It is very difficult to compare results across tables and I recommend to compact most of the relevant results, dropping some specifications that I do not think are necessary to support the robustness of the analysis (for example, I do not see why the author wants to present all the specifications including only one individual control at a time)

9. Econometric terminology is not always used properly throughout the paper.

10. The paper needs to be checked by a native English speaker (for example; the term employees is used – correctly - for the entire workforce, but also for the sub-sample of white collars), also because the current version contains many typos.