Referee report on Fairness and the disinflation puzzle (MS 1232)

The paper extends the model by Driscoll and Holden (2004), Fairness and inflation persistence, Journal of the European Economic Association, 2, 240-251 by using the job finding rate instead of the unemployment rate as measure of workers' opportunities outside the firm. The aim is trying to offer a better explanation of the effect of disinflations.

The paper is often sloppy which hampers the possibility to give a fair assessment of the underlying model and of achieved results.

Starting from the abstract, at the third line, a much larger effect is mentioned, without explaining neither larger with respect to what nor the effect of what variable on what variable.

The paper needs to be reviewed by a native professional proof-reader. Here are some examples:

- page 2 first paragraph: "are never existent" is not proper English;
- page 2 second paragraph: the subject suddenly changes from "I" to "it". There should be consistency throughout the paper about this;
- page 4 fourth paragraph: "which have their ... to fit the data" is not proper English;
- there is a typo in equation 5. By the way, how is this equation derived?

Mixing an imperfect monitoring model with a reciprocity model would need more reflection. Underlying the former one, there is a conflict. Workers naturally shirk unless there is some punishment. Underlying the latter one, there are fairness considerations: workers are ready to exert effort if they perceive their contract as fair. The two views might not be exclusive, but I feel more argumentation is needed to get them together.

The literature review is sparse. It does not give a real feeling of the state of the art. Section 1.2 needs to be drastically rewritten. It is difficult to accept that the sticky information model can be disproved by some (difficult to understand) descriptive statistics. Authors should instead give a flavor of what is the state of the art in this specific subfield.

Figures 1 to 3 are obscure. It is not clear why the percentage of bad times responses is normalized. If the scale is the problem, why do not use two subplots to project the data? The normalization procedure is unclear and cumbersome and it would need more explanation.

The first equation at p. 7 is difficult to read. The usual order of parentheses and brackets is not respected. v and ϕ are not defined. The last inequality is not commented.

In equation (2) do the terms dw_t^j with j=k,R refer to absolute or percentage changes?

In equation 7 $E_{t-1}[V_t^S]$ appears twice. Why $E_{t-1}[P_t]$ is in (8) and not in (7)?

I do not understand the role of expectations. What is the meaning of arrows in equation (6). Are expectation rational or not? Either way could be good for me, but the relevant choice should be motivated.

Danthine and Kurmann (2010) is indicated as "forthcoming" in the references.

A model that does not consider consumption and, therefore, is not a general equilibrium model lacks of interest in my view.

I do not deny that the paper might potentially contain some interesting result. The thing is that bad English, lack of precision, motivation, strong assumptions that limit the scope of the paper (such as a utility function not depending on consumption) and lack of transparence at key passages (see for instance also eq. 11) hamper a fair assessment of possible good points.