AUTHORS' REPLY TO REFEREE 2

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Job duration and inequality

We are thankful to the referee for reading and commenting our paper. We also appreciate that the referee understands the importance of working with (relatively) simple models, as much too often reviewers are not able to distinguish between 'simple' and 'simplistic'.

Below are our replies (in red) to the comments.

Major issues:

1. The authors should discuss more clearly the added value of their model as well as its limitations (considering that policy makers are missing from the design), in particular within the context of other agent based models of the labor market, such as Dosi et al (2018) and Dawid et al (2012)

We can connect better to the literature, specifically to the suggested papers. In general, however, we do not consider a model interesting *per se*, but if it can be used as a (reliable) analytical tool to investigate some topic. Thus, we think that the main contribution of our work lies in its application, which is also what distinguishes it more from the other works. In fact, Dawid et al. (2012) is very different from our paper, both in the model and in the application. Closer to us is maybe Dosi et al. (2018), which also focuses on the relationship between labor market characteristics and inequality. However, their analysis is quite different: they in fact consider two scenarios with alternative labor markets, and neither scenario matches the situations described by our computational experiments (see Table 1 in Dosi et al., 2018), even though a variant of the competitive scenario features temporary contracts. Basically, they focus on the way wages are adjusted, while we concentrate on labor contracts duration.

2. I appreciate the solid model foundations on the stock-flow consistency (SFC) approach, however the model description along the SFC lines is not fully correct. In particular, Table 2 is wrong since it is not correct to define savings as the sum of the upper columns, which include the variation of deposits or loans as well. Actually, the last row should include more correctly zeros, which are the difference of the cash flows and the variation of deposits (with negative sign). Alternatively, the last row could simply contain the variation of deposits as the sum of net cash flows in the upper columns. Savings can be defined as the sum of net cash flows, but only those referred to the current account, then excluding for example cash flows related to new loans. I would suggest the authors to get more acquainted with the SFC model descriptions. A standard reference in this respect is the Godley-Lavoie book (2012), a recent reference about the application of the SFC tables to agent-based model is Mazzocchetti et al. (2018).

Finally, if I represents unsold goods, it should not appear as positive cash flow on the firms column. Who paid for them? None, therefore the first row should contain only a +C on the firms cell that correctly balances the –C in the households cell.

We understand the referee's concerns about Table 2, which may look awkward.

We point out that real accounting rules are not Nature rules but change from legal system to

legal system. Under this respect, therefore, we should not consider Godley and Lavoie as the ''Bible'' for stock-flow consistent models, as what they have in mind is basically the Anglo-Saxon accounting system. Honestly, we are not able to access their 2012 book, but we know some of their previous articles.

All that said, in our model we have not followed specific or real accounting rules, and Table 2 is essentially based on ideal (theoretical) accounting principles. So, the table is not a very income statement but basically represents everything that comes in and goes out in terms of <u>value</u>. This explains why we included unsold goods I with positive sign: they represent an inflow of value for the firm, even though nobody paid for them (indeed, it's as if the firm itself paid for them).

The logic behind Table 2 also explains why the variation of deposits appears with minus sign for households and firms: even though there is no currency, we can imagine that the difference between inflows and outflows of money is deposited at the bank (for which they have positive sign). The same story applies to new loans. In principle, therefore, the last line should not even be labeled 'Savings', but unfortunately we do not have a better name. Alternatively, we could eliminate the rows relative to deposits and new loans, after which Table 2 would become a more traditional and reassuring income statement. However, practically speaking this would change nothing to the model.

Finally, in our opinion the referee is overstating the importance of those tables. In fact, what matters more is that the model variables are correctly updated, and that no money is lost or dropped into the economy from the outset. That we have done this correctly is easily verifiable by checking the equations of the model, in particular the laws of motions in Section 2.5.

Minor issues:

a) Eq. 4 is not complete. It should specify as well the case $V_{it} < 0$.

In case $V_{it} < 0$ wages remain the same.

b) The definition of leverage, see Eq. 9, is quite unusual since usually leverage is defined as debt to equity ratio or assets to equity ratio, i.e. the denominator includes equity not deposits. Please, provide some justification or reference about this choice.

Indeed there are many ways to measure leverage and financial fragility, corporate finance literature provides several indexes that measure the riskiness of a firm. In our simplified setting the proposed formula L/D is a reasonable index of the ''solvability'' of the firm, decreasing in L and increasing in D. Similarly, debt to equity ratio would amount to L/(D-L) as deposits are the only firms' asset (so, equity=deposits-debts), and so also this ratio is decreasing in L and increasing in D.

c) According to the definition of consumption budget, see Eq. 13, the marginal propensity out of wealth is identical to the marginal propensity out of income, i.e. it is defined by the same parameter c. However, this is quite at odd with empirical evidence and literature as well, which states that while marginal propensity out of income is close to 1, the marginal propensity out of wealth is much lower.

The referee is right, but this happens because normally people use income to finance current, high-frequency expenditures, while use wealth for low-frequency purchases (e.g. automobiles). This explains the difference in the two marginal propensities to spend. In the model we have only one consumption good, hence using two different propensities would not make much sense (and probably results would not be much affected).

d) Households' total contribution to refinancing defaulted firms should show up in Table 2.

The referee is right.