AUTHORS' REPLY TO REFEREE 1

Manuscript No. 2019-44:

Job duration and inequality

We are grateful to the referee for reading our work and providing valuable feedback. In particular, some modeling suggestions are very interesting and may be used to try to improve the overall quality of the model, possibly in the future as the referee wisely suggests. However, as the referee probably knows, sometimes using more 'realistic' microeconomic assumptions not always delivers more realistic outcomes. The introduction of a new assumptions should be in fact considered only if it turns out to be useful. In addition, assumptions should be consistent with each other, so it's not in general possible to modify a single mechanism without considering how it relates to the others.

Below are our replies to single comments.

MAJOR REMARKS

1) The referee's remark is correct. Earlier studies on labor market flexibility used to refer essentially to its influence on unemployment.

We made the connection for two reasons. First, there are several recent works (we are thinking in particular of all the studies reviewed in the cited paper Brancaccio et al. 2018) that analyze the impact of flexibility on inequality. Second, it is quite peaceful that when unemployment is higher inequality may increase (for example, Carpenter and Rogers, 2004, document that unemployment disproportionately falls upon low-income groups, thus widening the income gap between different groups). Hence, if labor flexibility affects unemployment, then it can also affect inequality.

2) The referee is right, and we thank her/him for pointing out that in real economies shorter contracts appear to reduce the workers' bargaining power and depress wages' growth. So, we acknowledge that what happens in our model is not, strictly speaking, very realistic. However, we have to consider that we have not implemented an explicit bargaining process between employers and employees. So, there is no workers' bargaining power to reduce when contracts are shorter.

On the contrary, with short contracts workers' reallocation from firm to firm is stronger (in the model, at least), so it's perfectly reasonable that firms increase their wage offers more often to prevents workers from quitting the job. The referee's suggestion, therefore, while perfectly reasonable in itself, would not be consistent with the other mechanisms characterizing our labor market (that is, with shorter contracts firms should raise wages *more*, not less as the referee suggests). So, while we can try to modify the wage setting process, at the same time we do not feel comfortable with the proposed modification. Probably, we should try to implement an explicit bargaining process in which workers are "weaker" when contracts are shorter (or when unemployment is higher).

3) The referee is right. We will specify it.

4) This assumption was basically introduced to fix a flaw affecting previous versions of the model. Without this assumption, in fact, newly employed workers would earn more than "older" workers, which in reality is not in general the case (the empirical evidence on this is abundant). What's more, without this assumption there would be an implicit downward flexibility of nominal wages, which we want to avoid (downward rigidity of nominal wages is basically the rule in modern economies, see also the references on the topic in the paper).

5) We partly disagree with the referee. We want to recall that rigidities have many causes, not just monetary firing costs (the literature is enormous: imperfect information, search costs, efficiency wages, etc.). Besides, in the model we actually have some sort of firing costs. There is in fact an implicit firing cost whenever the firm revises downward its production plans but is not allowed to get rid of excess workers because of the job contracts. The firm would like to fire but it cannot, so it's as if there were "infinite" firing costs.

On the other hand, the idea of introducing explicit pecuniary firing costs is reasonable and maybe even necessary if the model is intended to describe some particular, regulated real economy (European ones, let's say). But we point out that in many Countries around the world job markets are so unregulated that firing workers is very easy and cheap, often even free. Moreover, the absence in the model of pecuniary firing costs is perfectly consistent with the assumption that jobs are temporary: when in fact the contract expires, both firm and worker are free from their contractual obligations, and the employer owes nothing else to the former employee.

Besides, there are technical issues related to the implementation of such a mechanism, because in the model firms can fire before the contract has expired only when they do not have sufficient financial resources. As a consequence, they do not even have the money to pay the firing cost! On the contrary, the referee's suggestion would be easier to implement if firms were allowed to fire also when they do not have financial problems. We have never worked with this assumption, and maybe in the future it could be considered as an extension of the model (along with the introduction of monetary firing costs).

6) We do not see why this assumption is meaningless. We like it because it is a very simple way to link the supply of credit to the number of defaults: the more often the firm defaults, the less likely it will get credit from the bank. The credit rating is normalized to be a number between 0 and 1: if the probability of default is 1, the credit rating is zero, and vice versa. Then, the actual supply of credit depends also on the bank's lending attitude. The referee might argue that, strictly speaking, this is not the "correct" way to measure credit rating. But do we really know how to measure credit rating? Banks use very complicated assessment methods to measure credit rating, but often they supply credit to the wrong persons (and go bankrupt!).

All that said, the referee's suggestions are reasonable. It is in fact worth trying to link the credit rating also to the bank's net worth or to other macroeconomic indicators. But this would require a substantial revision of the model, and it is not granted that it would produce better results. We recall in fact that 'all models are wrong, but some are useful', so the goodness of an assumption should be evaluated not only on the base of its plausibility but also from the results that it produces. And our simple mechanism of credit rating appears to work well, at least in the limited context of our model.

7) We agree, we can try different consumption rules (even though there are thousands of such rules). Also the ones proposed by the referee are feasible. We want however to note that in the past we worked with an endogenous MPC embodied through a buffer-stock consumption rule a la Carroll, discovering that this did not change things very much. Moreover, we believe that heterogeneous MPCs would affect more consumption inequality than income and wealth inequality, which are the real focal points of our paper.

8) Yes, net worth is the Ef in Table 1. We will use a more consistent notation.

9) The referee is right, but this is not a major issue. We have in fact to consider that in general firms continuously ask for new loans (because they use credit to finance current production, not fixed investments in machinery that would be done only from time to time). So, even with a debt-repayment scheme based on fixed installments as suggested by the referee, the total amount of the firm's debt would generally not decrease.

10) Honestly, we fail to understand whether the referee is against the partial inheritance of debts (in which case the new firm should take all of past debts and the bank would suffer no bad debt) or against the very inheritance of debts (in which case the bank should suffer the total loss of its credit, maybe this is what the referee would like to have).
We opted for a partial inheritance of old debts because reality is very complicated indeed. Bankruptcies are managed in very different ways: in some cases the bank simply accepts to reschedule the loan, or the firm shut down and the bank suffer a complete loss of credit, in other cases a new firm acquires the defaulted one and takes on all of its debt (so, no bad debt for the bank), or agrees to take only a part of it, and so on. Modeling all these possibilities is obviously impossible, so we decided for this solution which allows us to tune the "rigidity" of the bank's attitude toward defaulted firms. Then we chose a value for parameter k that gave the most nicely-looking results.

11) Generally, for this class of models we use to interpret a period as a quarter. A co-movement analysis was in fact conducted using U.S. quarterly data. So, we do not really know whether a debt repayment rate of 5%, corresponding approximately to an annual 20%, is too low or too high or just enough. This is another parameter whose value should be set according to the results of the simulations.

12) Yes, the first 100 periods are always discarded from the analysis. Also in Figure 1 the referee can see that the time series go from 1 to 400. We will make this clear in the paper.

MINOR REMARKS

1) The code is always available upon request. Moreover, in case of publication on Economics-Ejournal we will make the code public.

2) The referee is right, we obviously mean 'inverse', but we don't think that there might be confusion. Anyway, we can change it.

3) Again, it does not seems to us a misleading sentence, but we can modify it.

4) We will do it.

5) We will fix them as suggested.

REFERENCES

Carpenter, Seth B. and William M. Rodgers III, 2004. "The Disparate Labor Market Impacts of Monetary Policy," Journal of Policy Analysis and Management 23(4), 813-830.