Brief answer to Referee Report 1 dp 2013-27

First, let's start by thanking the referee for the comments. I think that the paper will benefit from addressing these comments and I hope to have the opportunity to submit a revision of the paper that takes into account the points made by the referee. In what follows, I rewrite what I interpret as the main referee's comments and try to provide a brief answer to them and to explain how a new version of the paper could deal with them.

1.*In the view of the referee, the references to the Spanish labour market reform in the Introduction are misleading because one would expect to see a model with firing costs and wages set by collective bargaining. Moreover, there are no cyclical fluctuations.*

I agree with the referee on the whole. I consider the role of this paper as a first step in the formal discussion about the possible implications of introducing more internal flexibility in the Spanish labour market. Of course, it would be very nice to have a model with those details. In particular, it would be very nice to see if the results follow once a dual market characterized by collective bargaining and different firing costs for permanent and temporary workers is introduced. As Bentolila and Jansen (2012) has noted, the 2012 reform introduces interesting complementarities between internal and external flexibility, which are beyond the scope of this paper, although they are part of my research agenda. In fact, I do have a working paper (joint with J.I. García-Perez) where we introduce these elements in order to evaluate the implications of the 2012 reform concerning the changes in the external margin (the reduction in the severance costs gap between permanent and temporary contracts). We are now working on extending that model by including the intensive margin decision.

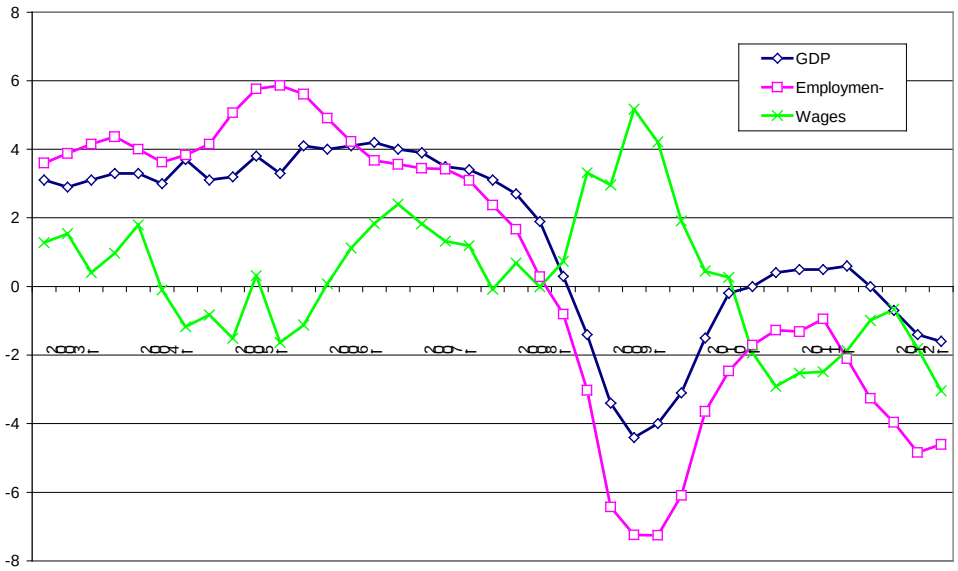
To sum up, I see the point made by the referee, but I consider that the references to the functioning of the Spanish labor market are needed to understand the relevance of the changes on internal flexibility introduced by the reform. In particular, the references to the existence of a dual labor market characterized by different degrees of protection for permanent and temporary workers, and to the fact that working conditions cannot be easily adjusted because they are governed by collective bargaining agreements, are relevant to understand the main mechanism of adjustment that firms have used over the last twenty five years: temporary employment instead of adjusting hours and wages. In the graph provided in the next page, one can see that since the 2012 reform was put in place wages are more responsive to the cycle. This was not the case at the beginning of the crisis, where wages have shown a countercyclical pattern.

Regarding the absence of cyclical fluctuations in the presentation of the results, this has been done on purpose. The aim of the paper is to focus on the flexibility that firms need when idiosyncratic shocks (not aggregate shocks) hit them in the steady-state. In fact, I do perform business cycle analysis in order to calibrate the values of the parameters that govern the friction because I need to match the relative volatility of hours and employment, but this analysis is just an input in the calibration process.

To summarize, I see things rather differently myself. I think that the fact that the paper abstracts from all these elements can be seen as a virtue of the analysis. In fact, in the homogenous workweek case, one can work out an explicit solution (abstracting from capital in the production function) which is very nice because it helps in identifying

where the results come from. And, of course, it is always possible to relate the adjustment cost parameter to some measurement of firing costs: a low value of this parameter implies higher adjustment costs which makes hours relatively more attractive than employment. However, I would not go that far in this model because I think that in order to properly to talk about the relevance of firing costs in Spain, one should use a framework that reflects the duality of the Spanish labour market.

On the other hand, the fact that I abstract from collective bargaining does not seem to me very important because the 2012 reform has introduced important changes in the system of collective agreements: first, priority has been given to firms' collective agreements; second, opt-out clauses have been introduced for firms experiencing economic difficulties; and third, the automatic extension of collective agreements once they expired has been reduced to one year. In fact, the reform allows for an internal devaluation by facilitating the adjustment of hours and wages to changes in a firm's economic conditions as an alternative to job destruction. For the first time, the firm will be able to unilaterally modify working conditions, such us hours worked and wages, when subject to negative shocks. In think that these changes resemble more to a competitive labor market than to a labour market where unions are very powerful.



2. *Calibration strategy concerning the cross-sectional distribution of hours.*

I understand the concerns of the referee regarding the calibration strategy and I think they might be due to a misunderstanding. I would like to clarify that the value of the parameter σ_ε that shows up in Table 1 is not the variance of the idiosyncratic shock. It is the variance of the aggregate process for the Solow residual, which explains its small

value as usual in the literature. I need this variance to carry out the business cycle exercise in order to calibrate the parameters that govern the friction.

In any case, I would like to explain the strategy that I followed and an alternative that was not shown in paper, but whose results are similar. I calibrate the heterogeneous workweek version of the model, using effective weekly hours as reported by the Spanish Labour Force Survey in the period 2005-2001. The underlying assumption is that the cross-sectional distribution of workweeks in the data should provide an indication of the desired degree of flexibility regarding the workweek. That is, I am assuming that in the data firms are free to set a particular working-week, but once this working-week has been chosen, deviations are not allowed under the status quo.

In order to calibrate the model, I compute the average effective weekly hours for those who work less than, equal to and more than forty hours a week and the percentage of workers in each of these groups. I use these percentages as the weights for a three-valued idiosyncratic process (of course, these groups are not equally large, the majority are in the middle group). The values of the three shocks are determined such that the model replicates the percentage deviation of each of the previously computed workweek averages with respect to the whole sample average in a scenario where firms are free to change the working-week when hit by a productivity shock. The calibrated cross-sectional hours distribution is such that 21% work 1.18 of mean hours, 63% work 0.98 and 16% work 0.67 of mean hours, that is, the cross-sectional distribution of hours is 32, 40, and 48 hours**.**

An alternative strategy would be to use the same degree of flexibility that I found for the US in Osuna and.Ríos-Rull (2003) making the assumption that the US economy reflects better the desired degree of flexibility regarding the workweek. In fact, the workweek distribution for the US turns out to be very similar (30.6, 39.2 and 45.7 hours) to the calibrated cross-sectional hours distribution that I have in the paper (32, 40, and 48 hours). The difference, though, is in the weights. For the US case I divided the sample into three equally large groups, and then computed the average working week in each group and the deviations from the legal workweek as explained above. If I were to follow the same procedure (dividing the sample into three equally large groups) using the Spanish data, the variability of the cross-sectional distribution would be largely reduced, from 32-40-48 hours to 36-40-46 hours. I think that, based on the evidence provided in the paper, the resulting cross-sectional distribution would not be consistent with the actual degree of desired flexibility, and this is the reason why I used the procedure explained above. Of course, I admit that using this procedure implies that good and bad shocks have less weight in the simulations because these weights are related to the proportion of people with hours above and below the 40 hours working week. But I think that the strategy in itself consistent

Finally, I'm afraid I entirely disagree with the referee when he concludes that the changes in the working week legal arrangements have no significant impact on employment, output, productivity and wages. I do think that the effects regarding productivity and wages are particularly important, and I am happy to find that in most of the cases these gains in productivity do not come at the cost of losing employment. Probably, it would have been a better idea to have shown the elasticities and how do they compare to the elasticities found in related empirical studies.

3. *The referee points to the fact that the equilibrium allocation is not efficient due to the externality that governs the friction and that paper says nothing about that*.

I entirely agree with the referee. The reason why I did not mention it was that I already discussed this matter in Osuna and Ríos-Rull (2003). It is true that employment is inefficiently high, not only because of the externality, but also because of the tax on long workweeks. This is the reason why, from a technical point of view, the procedure to compute the model is interesting in itself. In Osuna and Ríos-Rull (2003) we developed the methods needed to compute the equilibria of non-convex business cycle economies where the Second Welfare Theorem does not hold because of both, the externality and the distortionary taxation. These features forced us to compute equilibria directly. This turns out to be a relatively daunting task because households must know the wage functions in order to compute their decisions, and to make things more complex, wages in these models are a non-linear function of hours. These wage functions are part of a fixed-point problem that must be solved in order to compute the equilibrium. All these details were thoroughly discussed in that paper, but I agree and thank the referee for reminding me of that. I will include a footnote about it in the paper.

Related to this comment, the referee would like to see how the efficient allocation would look like. Unfortunately, I cannot tell because, even if I internalise the externality (which I already tried) the problem remains because of the tax on overtime. At the same time, there is another important element, mentioned by the referee in the first paragraph, that is missing from the model, and that could be relevant for efficiency considerations, which is the effect of unemployment on worker's human capital. The consideration of this effect is out of the scope of this paper, whose main focus is on quantifying the trade-offs between employment and productivity, but on my research agenda, because I think this could provide a rationale for implementing policies that generate an inefficiently high employment rate.

4. Calibration strategy concerning the friction.

The referee asks whether I am solving the stochastic version of the model and wonders why I do not provide the business cycle statistics. As mentioned before, in order to properly match the degree of substitutability between hours and employment I use business cycle information to match the relative volatility of hours and employment. The reason for not having written down a section on business cycle implications is twofold. First, the focus of the paper is on the steady-state effects. And second, the business cycle exercise is needed only to calibrate the two parameters that govern the friction. Again, as in the previous comment, I did not want to put the emphasis on this exercise, and on why it is or it is not appropriate to use business cycle statistics instead of using microeconomics observations because this question was thoroughly discussed in Osuna and Ríos-Rull (2003). Footnote 16 in the present paper refers the interested reader to this discussion in Osuna and Ríos-Rull (2003).

5.*The main differences between this paper and Osuna and Ríos Rull (2003)*

The referee is interested in knowing the main differences between this paper and Osuna and Ríos Rull (2003). I see this paper as an application of the model that J.V.Ríos-Rull and myself developed in 2003. In that paper, the focus was not only on the implications of overtime taxation for the workweek, but also on the technical details,

such as the already mentioned methods needed to compute the equilibria of non-convex business cycle economies where the Second Welfare Theorem does not hold because of both, the externality and the distortionary taxation. Moreover, in that paper the baseline model was an homogenous workweek model, while in this paper the focus is on a model with heterogeneous workweeks. In addition, the model presentation in Osuna and Ríos-Rull (2003) was more formal. In this paper I have tried to present the model in a more friendly way and I have also tried to eliminate every technical detail that was no needed to follow the discussion on the application to the Spanish case. For all these details I have referred the interested reader to Osuna and Ríos-Rull (2003).

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