This paper analyses the impact on Spanish unemployment of reductions in employers' social security contributions (ESSC). My main concern is that the adoption of wages as numeraire in such a setting is a problematic modelling strategy. The authors follow Kehoe's et al. (1995) approach to capture feedback effects between the real wage and the unemployment rate. In fact, Kehoe et al. (1995) adopt a commodity price index in their original framework, not the wage, which looks much more appropriate. Of course, one can choose any numeraire but then the interpretation of results should be done accordingly, and I think in this paper it has not been well addressed. The authors do not report the capital remuneration and other variables in their tables that would help us to get a better idea of the evolution after the shock.

Another important comment is about the way in which the authors present their results and derive conclusions. In my view, it would be less confusing if they had presented the impact on unemployment in percentage point differences with respect to the benchmark level (as Kehoe et al. (1995) do). The authors, instead, present it in percentage change with respect to the 17.86% initial unemployment level. The strongest effect derived for unemployment arises in the DT compensation scenario of a fall of 5.00 of the ESSC and is of a -4.324% (in last column of Table 1). This implies that unemployment turns from 17.86% to 17.08% (=17.86 *(1-0,04324)). So an important conclusion to be derived from the results should be that the policy measures they are analyzing would have a very limited effect on the unemployment rate. This does not seem to be very clear in their conclusions.

In general, the paper does not develop very much which sort of contribution they are making to the literature, neither justify their modelling approach and the underlying data they use. As they mention very briefly at the end of the introduction, there are other predecessors looking at what seems similar issues. What is, then, their contribution? In order to better motivate the paper a more detailed discussion of close modelling attempts would be appropriate. A similar comment applies to their modelling strategy. Why this approach to model the labor market? Other alternatives should be, from my point of view, discussed, explaining their choice. A reference to a review of CGE modelling strategies in labor markets would be very much welcome (e.g., Boeters and Savard, 2012). Additionally, the paper uses a database that is not publicly available, since it is derived from a PhD Dissertation. Any advantages in doing so? No comments or explanations are given in that regard. Furthermore, there is very little information on the underlying data in this modelling exercise. A table presenting information (and not just the names, as Figure 1 does) of the sectors in the economy: their weight in production, unemployment, value added… would be important. Or more detail on the weight and values of the different macroeconomic aggregates of Spain in the base year. This looks particularly important if, as said before, the base data are not publicly available.

The results presented only cover macroeconomic aggregates. One of the main strengths of CGE models is their capacity to derive micro and macroeconomic effects in a consistent manner. If one is to concentrate on the macro results then maybe other methodologies would be better tuned (DSGE models, for example). Furthermore, even within the macroeconomic results it is common to look to a broader set of elasticities of unemployment to wages, as in Kehoe et al. (1995). In particular, in this setting looking at the results for its infinite value (rigid wages case) and zero value (perfectly flexible real wages) is a common strategy to test the model more deeply.

Other minor comments are that the authors could better explain the role of their saving-driven closure in the model and how they are measuring the "activity level" they report in the results (aggregate output? GDP?). The latter variable remains nearly unchanged. It is not clear how its variation is expressed. If activity levels do not vary, how can unemployment vary?

The comment "the labor supply is perfectly elastic up to the level of the total labor endowment where it turns inelastic" seems inconsistent with the idea that there are feedback effects between the real wage and the unemployment rate. The authors could also mention what the source for the rest of elasticities (apart from the one of the real wage with respect to unemployment) is.

I would encourage the authors to use another numeraire, (or, at least, to report and discuss in more depth the evolution of nominal variables) and keep on working in such an important avenue of research, while better stressing the motivation for their choice of modelling strategies and benchmark data.

Reference:

Boeters. S. and Savard, L. (2012) "Labor market modeling in a CGE context" in Dixon, P. And Jorgenson, D. (Eds.) *Handbook of Computable General equilibrium modeling*, Elsevier, North-holland, available at: discussion-paper-201-labour-market-cge-models%20.pdf