Report on MS 509 "Senior activity rate, retirement incentives and labor relations"

This is an interesting study that theoretically and empirically examines cross-country differences in the relationship between old-age pension benefits and senior labor force participation. The authors investigate how work patterns of older males respond to the implicit tax on work in the country's pension system and the predicted disutility from work in that country. The theoretical model is a nice lead-in to the empirical part of the paper; most of my comments deal directly with the empirical section and the presentation/discussion of those results.

1. It would help if the authors were more specific about the definition of "activity rate." In particular, I wonder how much an individual must work in a given year in order to be coded as "active" (one day, one week, full-time?) My main concern with the variable is whether it is consistent across countries, since patterns of transition into retirement vary across nations (e.g. in some countries it is more common to transition to part-time work before exiting the labor force entirely; an individual who has left a career job but is in a transitional or "bridge" position may self-report as working or may actually self-report as retired). I also wonder whether there are other, similar variables that could also be tested for consistency (e.g. fraction of the population that is "retired").

2. I am having trouble thinking about the meaning of the results in Table 2. With only 37 observations (and why are there 37 observations, if the data include 19 countries in 3 cross-sections?), it is difficult for me to believe that these point-estimates actually demonstrate a causal relationship. It therefore does not seem to me that there is any more information in this table than is already demonstrated in Figure 2 and Figures 5-7, so I would advocate for keeping the figures but eliminating the table. If the authors do retain the Table 2 results, they should be more specific about the meaning of the coefficients. In particular, they should not only discuss the significance of the coefficients of interest but should also interpret their magnitudes. (e.g what would the coefficients on *rel management* and *rel workmates* imply for the impact on labor force participation of a one-standard deviation increase in either variable).

3. While I like the idea of using labor relations as a proxy for disutility from work, I wonder at the consistency of the meaning of the five-point rating scale across countries. For example, would a rating of "4" really mean the same thing to a Canadian worker as to a South Korean worker? I am particularly concerned about the emphasis on these variables in the study since the variance seems quite low (can we really be sure that a "3.6" average in one country means something different than a "3.8" in another?). I realize that these measures may be the best available for this type of cross-country comparison, but I wonder whether there has been any previous work with these variables to investigate

their predictive power vis-à-vis more "objective" labor force indicators (e.g. strikes or other labor unrest, % of labor force that is unionized, etc.). I also wonder about the relationship between these variables and the survey question about self-reported job satisfaction (a rating that is probably even more subjective and may vary even more across cultures than the labor relations variables, but may contain interesting information, nonetheless).

4. As a follow-up to point 3 above, it is somewhat difficult to interpret the meaning of the coefficients of interest (i.e. the coefficients on *Implicit tax* and *Interaction term*) in Table 4, since the coefficient on *Implicit tax* represents an elasticity for a case that doesn't actually exist (i.e. the case where W=0). The reported minimum and maximum effects are useful (it would actually help if the authors said a bit more about these in the text, rather than leaving it to the reader to back out what the average labor relations rating is for each min and max) but I would find it more intuitive if the authors also ran regressions with a 0-1 indicator variable for labor relations (i.e. a 0-1 "W" term). This variable could be coded as "0" if the relevant average labor relations variable is below the mean, and as "1" if the variable is at the mean or above.

5. It would be useful to have some summary statistics for the dispersion of labor relations variables. The authors present quite a bit of information on the average labor relations measure (in the figures) but nothing for the dispersion measure. Based on the min/max effects reported in Table 5 it also seems that the variance in this measure is not very high, raising similar concerns to those discussed in point 3 above.

6. For the results as currently reported in Tables 4 and 5, it would be useful to have more discussion of what these elasticities actually mean. Since the range between the minimum effect and the maximum effect is very small (i.e., the elasticity seems to hover around zero, on average) how should the reader interpret the importance of labor relations (and therefore disutility of work) in the retirement decision?