

Referee Report on „The acceptance of earnings losses after voluntary mobility” by Stefan Schneck

The paper deals with the interesting question of the determinants of wage cuts for voluntarily mobile individuals. The author argues that compensating differentials are the main reason for this phenomenon and relates these particularly to the two variables commuting and homeownership. He finds that daily commuters are more likely to trade off wages and commuting time even more so when they are homeowners.

Overall, although the paper is potentially interesting it gives the reader the impression that it is not yet a paper which is publishable. There are too much details missing or wrong and there are several big points that put the analysis into question.

Major points:

- 1) The theoretical part is too sparse from my point of view. I would like to see a thorough discussion, how wage cuts can be explained from different theoretical strands, when the interest of the paper is to empirically test determinants of wage cuts. Both traditional economic theory, search theory and the theory of compensating differentials contribute to explaining this phenomenon (see below). Then, I do not find the concentration on the two variables commuting and homeownership convincing. This is from my point of view pretty much an ad hoc choice and should be founded somehow in economic theory. Why does the author concentrate so strongly on those two variables? From my point of view, there are many candidate characteristics that may matter for compensating differentials. Moreover, commuting does not so much coincide with amenities of jobs but more with classical economic arguments. Commuting time and costs could be directly calculated as being part of the wage. Homeownership, although important, has nothing to do with job amenities. When the interest of the paper is to testing compensating differentials, I would suggest using the variable “subjective improvement of the job” as lhs variable, controlling for the objective or subjective wage cut and then testing the different potential determinants of compensating differentials. When testing a theory of wage cuts, then the theoretical part must discuss the theory of wage cuts and deduce the different determinants. This is not done here.
- 2) To explain the determinants of wage cuts, the author estimates a simple Probit model on all individuals that change jobs voluntarily. This is critical from several points of view. First, the author throws away information on the amount of the wage change, which could be used for example using a Tobit model or so. It might make a difference whether one accepts a wage cut that leads to a smaller, but roughly similar wage or whether the wage cut leads to a, say, 25% lower wage. More critical might be further, second, the population for which the determinants are analyzed. Here, the population looked at is the individuals that experience voluntary mobility. However, whether the (voluntary) mobility takes place or not is very likely a function of the wage offer obtained. Thus, the population looked at will depend strongly on the wage offers obtained. In addition, the population is likely to depend on the business cycle. While there may be individuals with “hobo syndrome” (cf. Munashinge/Sigman, 2004) that (try to) change jobs independently of the business cycle, there may be many individuals that change jobs, once they obtain a good job offer. The composition of those groups in the group of voluntary mobile persons is likely to change with the business cycle and thus, the population considered changes. The author himself describes that commuters are likely to search for new jobs as a function of commuting distance, which again implies that the population considered changes with changes in the rhs variables. A further problem for the empirical analysis is that other relevant decisions of the individuals are not observed. The author discusses that non-homeowners are more likely to change residence instead

of changing jobs. A rational individual would weigh the discounted value of commuting against the cost of migration. So again, the population for which the author attempts to make statements or the decision model is not well defined, when individuals have several relevant choices but the author observes only one.

In my mind, the task undertaken by the author to identify determinants of wage cuts by compensating differentials, requires a careful rethinking of the population considered. I.e., for whom are the estimation results likely to hold - in light of economic theory. It might be more valuable to estimate a two stage probit model, where the first stage consists in the question of whether somebody changes a job or not and the second stage in the probability to have a wage cut (or the size of the wage cut, see above), allowing the errors between the probits to be correlated (because job change decision depends on the wage offer). Alternatively, a multivariate probit model taking the states, no job change, job change with wage increase, job change with wage cut, might be a feasible approach in light of economic theory.

- 3) The empirical strategy, besides the population problem discussed above, is not convincing. First, the wage variable has the problem that it is not an hourly wage but the gross monthly wage. It is however not unlikely that, for example, homeowners reduce voluntarily their working time for having more time for the house construction. Thus, the measured wage reduction could be spurious, even more so when considering the group of home owners separately. Second, using "change in job satisfaction" as a rhs variable is not a good idea. This is the case, since it is at least as likely that the wage has an impact on job satisfaction as the other way round. Thus, the variable used is severely endogenous and leads to inconsistent estimates. In addition, if the variables commuting and homeownership are as important as suggested by the author, it is likely that the job satisfaction variable is highly correlated with those, potentially leading to multicollinearity. It is a better idea using the subjective improvements in the job characteristics asked in the GSOEP and leave the overall evaluation out.
- 4) The author did not deal appropriately with the interaction effect in the nonlinear model. Both calculation and interpretation of interaction effects in binary models is non-standard (see Ai/Norton, 2003; Ai/Wang/Norton, 2004). It appears that simply interpreting the coefficient of an interaction effect in a binary model is misleading, because it does not provide an answer to the question how the probability changes the two interacted variables change (the cross derivative). It appears that even the sign of the coefficient can be wrong in term of the desired interpretation. The author should deal with this problem.

Minor points:

- 1) The determinants of regional mobility have been studied for Germany (see Arntz, forthcoming) (p.11), which the author could read and reference.
- 2) There is a large literature in wage tenure contracts, which the author should know and discuss when dealing with wage cuts, namely Burdett/Coles, 2003, Stevens, 2004 and Shi, 2009.
- 3) There is another strand of the literature which explains voluntary separations by reason linked to the current job, namely changes in productivity (eg, Mortensen/Pissarides, 1994) or changes in productivity expectations (Jovanovic 1979, Moscarini, 2003). This literature can also explain wage cuts, had the wage cut at the current employer been larger. The author should be aware of this literature as well and discuss it at least shortly.
- 4) A part of the variables used are not carefully described in the text: What are, for example, subjective improvements in the job type, or job benefits? Is the wage variable measured in Euros or DM? Is it gross or net?
- 5) The language shows serious deficiencies. In some parts the text is even difficult to understand. The author should try to carefully rework the English text and try to improve it.

- 6) Are there no data including commuting distance? This would maybe more interesting to see how one kilometer more of travelling distance is valued by an individual. One could try to approximate commuting distance by using centres of the Kreise and using the IABS where one disposes over exact wage information and both place of residence and place of work.
- 7) The author might want to constrain his analysis to individuals that change jobs from time to time, because there are so called artist careers (Künstlerkarrieren) of persons working always only for short, predetermined periods for the same employer and thus the wage information is less relevant for them. But the might constitute a comparatively large part of all voluntary job changes.
- 8) What exactly is voluntary mobility? Are dissolution contracts part of voluntary mobility? This might make the term voluntary a bit more doubtful, because many employer induced separations lead to dissolution contracts.
- 9) Note that as the objective wage information compares two years, it is conceivable that in between there was a wage cut at the old job and it is also possible that the employer threatens the employee with a wage cut to come, before the employee changes.
- 10) In my mind table 3 is a bit disappointing in terms of the theory put forward by the author. A (subjective) improvement in commuting is only found in 28% of the cases of wage cuts and in 24% of the cases with wage markup, the difference being not significant. This does not lead one to conclude that commuting is especially important in explaining wage cuts.
- 11) The author should only discuss results that are shown in some table (eg, p. 16). Results should also be given for the RESET tests (p.12).
- 12) I would rescale the wage variable and use local unemployment rates instead of the overall.
- 13) I would give marginal effects for Dummy variables not at the mean but rather at zero or one. (p.12) But that's a question of taste.
- 14) The fact that chances of promotion correlate negatively with the probability of a wage cut is astonishing in the light of the wage tenure contracts literature. This could be an interesting point to be discussed.
- 15) Instead of using the interaction between daily commuting and improvement as determinant for wage cuts (as robustness check), the author could look at the improvement variable separately for the group of daily commuters.
- 16) The conclusion of the exercise is a bit too narrow.
- 17) I like the fact that the author discusses the cognitive dissonance theory in the paper. However, it is put too prominent and I would discuss this in the conclusion as being important but not challenging the analysis.

Literature:

Ai and Norton (2003): Interaction terms in logit and probit models, *Economics letters*, 80(1), 123-129.

Arntz, Melanie (forthcoming): What attracts Human capital?, ZEW discussion paper.

Burdett, Ken and Melvyn Coles (2003): Equilibrium wage tenure contracts, *Econometrica*

Jovanovic, Boyan (1979): Job matching and the theory of turnover, *Journal of Political Economy*

Mortensen, Dale T. and Christopher Pissarides (1994): Job Creation and Job Destruction in the theory of unemployment, *Review of Economic Studies*

Moscarini, Giuseppe (2003): Job matching and the wage distribution, *Econometrica*

Munasinghe, Lalith and Karl Sigman (2004): A hobo syndrome? Mobility, wages, and job turnover, *Labour Economics*, 11(2), pp.191-218.

Norton, Wang and Ai (2004): Computing interaction effects in logit and probit models, *The Stata Journal*, 4(2), 103-116.

Shi, Shouyoung (2009): Directed Search for Equilibrium Wage-tenure Contracts, *Econometrica*

Stevens, Margret (2004): Wage tenure contracts in frictional labour markets: Firms strategies for recruitment and retention, *Review of Economic Studies*