Essays on Social Welfare Systems, Education and Agglomeration across the EU
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The dissertation consists of three empirical papers that use data from eight countries combined: seven European countries and the U.S. The papers cover a relatively wide range of topics, from welfare income of immigrants through returns to education to agglomeration effects. The first paper is co-authored; the other two are solo papers.

All three papers are written in a professional way, with clarity. The language, format and structure meet the standards of international publications. The methodologies used in the papers do not make significant advances over the literature, but they are adequate for the purposes of the analyses (my specific remarks on the third paper are below). From what I can tell, the first two papers provide genuine new evidence on policy relevant issues. The results of the second paper raise some interesting theoretical questions as well. The third paper adds to a specific literature by providing evidence from one more country, and it has some minor methodological contributions as well.

In my view, the three papers together meet the standards of a PhD dissertation in applied empirical economics. They demonstrate adequate skills and knowledge in applied empirical research, and they obviously are the outcome of a lot of serious work. I recommend accepting the thesis with no revisions to the dissertation itself. At the same time I have a few comments that the author may find useful if she were to submit her papers to international journals.

The first paper looks at a simple but policy-relevant question: do immigrants from poor countries to rich countries receive higher welfare income, and if yes, why? Apparently, no one has looked at this question in the framework of a systematic cross-country analysis. This is a surprising fact, but it only increases the value of the paper. The analysis focuses on four rich European countries and the U.S. The data used for the analysis is an internationally comparable survey (the LIS), which makes cross-country comparisons both simple and credible. The method is OLS regressions and Oaxaca-Blinder decompositions, and for robustness checks, we see median regressions as well. The results are not particularly surprising, or, in other words, they make a lot of sense. Immigrants from non-EU countries to European countries receive higher social income, in large part because they are poorer and have more children. The same is not true for immigrants from EU countries or for immigrants to the U.S. The paper could make a valuable publication in a good policy journal.
I found the second paper the most interesting. It looks at the returns to education for immigrants (foreign-born individuals) versus natives in three EU countries that are quite different in terms of their immigrant pool (Germany, Austria and France). The data used for this study is the same as the one used for the first chapter, the LIS. The analysis is based on OLS regressions of two types, standard Mincerian regressions and “realized-matches” regressions that allow education to enter in interaction with the modal education in the occupation the individual. Returns to education are lower for immigrants in all three countries, with France showing a smaller difference. The most interesting finding of the study is that these differences are the result of complicated matching processes of jobs and employees. Immigrants face almost the same returns to their education as natives if they work in jobs where their education is the mode. However, they are less likely to work at such jobs, and when overeducated, their return is lower than that of the natives.

These results turn out to be somewhat different from those found in the U.S. and the U.K. They are also quite intriguing. If the author wants to make this paper publishable, she should work a lot more on the theoretical part. For example, I think that the “assignment theory” (i.e. the theory of compensating wage differentials) can produce any prediction depending on the distribution of the unobserved characteristics of workers and jobs. Even the simpler setup of the Roy model is notoriously hard to use for clear predictions. The theoretical motivation is also unable to rationalize the cross-country differences in the results. Getting a more focused theoretical framework for the results is clearly beyond the scope of the thesis. It would be necessary, however, for a good publication – and for the reader to make a better sense of the results.

The third paper looks at the association between average wages and employment density in British regions. The analysis follows the “agglomeration effects” literature very closely, and it hypothesizes some mechanism from the stock of human capital to productivity. While there is no deep discussion about that mechanism, the apparent need to tackle endogeneity suggests that we are after some genuine causality; say, in a thought experiment we may put a lot of employed (or rather employable?) people in a region and see whether average wages would go up. I must say that the little reading of the literature I’ve done did not make me understand what kind of a thought experiment we should be after. (Note that this issue does not come up with respect to the first two chapters as they deal with more reduced-form relations.) Natural experiments are hard to imagine as well: David Card’s Mariel Boatlift, exogenous increases in college completion, or fertility policies that change the size of the population a generation later may come close but, from what I see, are not considered in this literature.

The literature instead searches for instruments, but it is unclear why and how those may reproduce such a thought experiment. The instruments used here are typical in the literature, but that does not make them valid. For one, population density two centuries ago is not supposed to be correlated with current productivity because technology changed so much. But did it, really? Don’t most factories and offices need, for example, a lot of clear water, which makes proximity to rivers productive just like it was two centuries or more ago? The other instrument, land area, may also be problematic. Land area is determined by the administrative decision of regional boundaries, a decision that took place sometime in the past. It seems that sparsely populated areas were defined to be larger (maybe in order to balance total population per region). But then we are back to the previous instrument with all of its problems. (In light of
this, it is not at all surprising that they pass an overidentification test: the two instruments may be endogenous the same way for the same reasons.) It is clear that I have strong reservations against the identification strategy, and I’m also not sure how to make sense of the research question itself. But many economists disagree: the paper the dissertation follows closely was published in the EER. Nevertheless, if the author plans to submit her paper for publication, I advise her to address these worries in more satisfactory ways.

All that said, the three papers obviously meet the standards of an acceptable PhD Thesis applied empirical research economics. Therefore, like I indicated above, I suggest accepting the thesis without further modifications.