Report on the dissertation "*Essays on Citizenship Policies and Labor Market Integration*", presented to the Center for Economic Research and Graduate Education at Charles University by Vahan Sargsyan

Introduction

The thesis comprises three independent essays on the economic integration of migrants, with particular emphasis on administrative obstacles to integration, namely citizenship and the hukou, a permit to reside and work in a given area in China. The thesis is based on publicly available datasets and uses standard econometric models that are frequently used in labor economics, namely wage decompositions. In most parts, the thesis is well-written and presents some interesting ideas. However, there are several limitations that need to be addressed before the thesis can be defended. In the following, I separately present comments to each chapter before providing final recommendations. In my judgment, I apply the same criteria as I would for a thesis at top US or UK schools.

Chapter 1

The first chapter addresses the question whether the hukou system is responsible for the wage gaps between locals and migrants in Chinese cities. This is an important question, given that the hukou system is seen as an obstacle to development in China, and a major source of friction in Chinese labor markets. The author uses data from the RUMIC survey and performs Oaxaca-Blinder decompositions to test whether rural hukou holders who live in cities are discriminated against, i.e. whether they have lower wages compared to urban hukou holders with the same observable characteristics. To address potential biases from selection into changing one's hukou, into migration as well as into employment, the author focuses on a sample of people whose hukou got converted after their municipality became part of an urban area, and estimates a Heckman selection model to account for selection into migration. The central finding is that the hukou system explains less of the conditional wage gap than previously thought.

This chapter is well-written and performs a simple but solid analysis that delivers interesting results. The section begins with a comprehensive literature review that summarizes and critically analyses the literature on rural-urban migration in China as well as the hukou system and its consequences for the Chinese labor market. After reading this literature review, it almost seems surprising that no one else had the idea of applying decompositions before. I definitely think that, with this chapter, the author has spotted an fills important gap in the literature. The literature review is followed by a discussion of the hukou system, which outlines its history, in particular the institutional changes. This section is important to readers not familiar with the Chinese labor market.

In the main empirical analysis, the author briefly explains the methodology (Oaxaca-Blinder/ Ransom decompositions) as well as the insights this method can deliver. I greatly appreciate that the author is upfront about potential selection biases. Decompositions are all about comparing similar individuals, and selection on unobservables can lead to false comparisons of seemingly similar people differing fundamentally in their unobservable characteristics. The author identifies three sources of selection bias, namely selection into hukou change (from rural to urban), selection into migration, and selection into employment. All three are potentially problematic. Changing one's hukou is a choice that may have the same determinants as labor market outcomes. Similarly, migrants are often self-selected, such that they are not a representative sample of the population at origin. And people's unobservable differences may explain why some are employed while others aren't. The author proposes solutions to all three sources of selection bias, one of which I find very credible while I have some reservations about the remaining two. To address selection into Hukou change, the author focuses exclusively on people who were forced

to change their hukou after their rural areas were incorporated into a greater urban area and the rural hukou was automatically converted. This is a clever way of circumventing the first selection problem.

For the second selection problem, he applies a Heckman correction whereby he first estimates a participation equation for migration, from which he obtains the inverse Mills' ratio, which is subsequently included in the wage regression to control for selection. As with most two-step procedures, identification hinges on an exclusion restriction. The author uses the number of children and the number of elder siblings as excluded instruments that should predict migration but not earnings. I understand the rationale for using both — after all they provide a plausible and strong first stage — but I have some trouble understanding how they can be excluded from the wage regression. The number of children may affect earnings through labor supply, investment in education, and other channels, whereas it also affects migration. Likewise, if there exists a quantity-quality trade-off in fertility, then people with more siblings received less investment in their human capital, which also affects earnings. Without an in-depth discussion of the validity of the exclusion restriction, and without further robustness checks, this result is not credible, and the author needs to do more to justify his choices.

The third problem, namely selection into employment, is not directly addressed. The author justifies this choice with the high employment rates of migrants. This makes sense, but it only solves the problem for one out of two groups. Obviously, migrants would not move without the perspective of employment. But their wages are compared to those of locals in cities, whose wage function may be more biased by selection into employment than that of migrants. While I am not sure how this problem can be solved, it would be worthwhile to devote some space discussing it.

After discussing selection, the author goes further into the methodology by describing how the fair wage is constructed. He chooses the Oaxaca-Ransom (1994)-method of using a weighted average of betas between both groups. This discussion lacks methodological reflection. While there is nothing wrong with following the literature, two issues are unclear in this paragraph. First, it is unclear how the weights for the fair wage are computed. Second, there is no explanation why the Oaxaca-Ransom method performs much better than others. As correctly stated by the author, the choice of method affects the results and, therefore, the conclusions drawn from the chapter. To make this chapter publishable, it will be important to discuss this issue in greater depth, and to run a series of robustness checks with other definitions of fair wages.

The methodology section is followed by a fairly short description of the data. In this section, the author briefly describes the Rumic-survey, a popular household survey in China that covers rural-urban migrants as well as non-migrants in rural and urban areas. While this dataset is very interesting, the author needs to devote more time to a careful description of the dataset, answering questions such as: (i) how did the data collection work? (ii) what was the initial sample design, and did the sampling work in the actual survey? (iii) how representative of the Chinese population is this survey (and can you show this by comparing observable characteristics with those in similar surveys)? These questions, and several more, should be discussed in detail. This is very important, as it affects the conclusions that can be drawn from the study, as well as the reliability of the results. Moreover, the descriptive statistics are not very well presented. Table 1.1c does not include sub-headings, which makes it difficult to compare the groups. Moreover, it only includes means, but for the interpretation of the results, the standard deviation could be helpful.

In the results section, the author first presents the estimation results from Mincer wage regressions, which are subsequently used for the income decomposition. The decomposition results are very interesting. They suggest that, the difference in hukou does not explain the urban-rural wage gap. This result emerges from the comparison of hukou converters to an urban control group, whereas the unexplained component is virtually zero when workers with rural hukou and converters are compared. From the beginning of this chapter, the author puts forward the result that hukou explains less of the wage gaps than previously thought. However, I am not so sure about this conclusion after seeing table 1.3. I strongly recommend that the author provide a more in-depth discussion about the results. It would also be helpful to shape a reader's expectations *before* presenting the results. If hukou mattered, what would the unexplained wage component

tell us about that? And what could we conclude about hukou if we found no explanatory power of the unexplained component?

To summarize, I think this is an interesting and well-written chapter that shows potential. However, there is room for improvement along several dimensions, which will be very important if the author wants to publish his work in economics journals. Conditional on these improvements, I could see this paper being of interest to journals like the Journal of Population Economics, Demography, International Migration Review, Review of Development Economics, or the Journal of Development Studies. I would strongly recommend that the author make the following adjustments:

- Discuss the validity of the exclusion restriction of the Heckman selection model in great detail. As it stands, the assumption is not credible. Try to find a more credible instrument. That failing, he needs to discuss the implications for the estimation. I am positive that the effect can be bounded once one determines the sign of the bias.
- Discuss the potential biases arising from selection into employment of locals in cities.
- Discuss the choice of fair wages in greater depth. In an appendix, provide robustness checks with different definitions of fair wages.
- Discuss in depth the sample design, representativeness of the sample, as well as descriptive statistics. Convince the reader of the quality of the dataset, but also discuss its limitations (no dataset is perfect).
- Provide a more in-depth discussion of the decomposition results. Make Table 1.3. self-contained (notes, clear explanation, etc).

Chapter 2

Chapter 2 estimates and decomposes the returns to naturalization of immigrants in France and Denmark. Using data from the Luxembourg Income Study (LIS) and applying Oaxaca-Ransom decompositions, the author shows that returns to naturalization are high in France but not in Denmark.

The chapter begins with an introduction to the economic analysis of naturalization. The most important findings on the economic effects of naturalization are first summarized, before the author discusses two gaps in the literature that his paper aims to fill. First, there are few — in his view too few — studies showing the cross-country differences in the returns to citizenship, and second, there is little evidence on which personal characteristics drive the returns to naturalization. I agree with both. Given the large returns to naturalization, it is important to learn more about its determinants and compare them across countries. However, some more recent papers, above all Gathmann & Keller (EJ, forthcoming), are not included in the literature review, and I would strongly suggest that the author take a close look at this paper and other papers using citizenship reforms for causal identification.

The introduction is followed by a brief and intuitive explanation of the econometric model (Oaxaca-Ransom decompositions). As in chapter 1, the idea is to study the unexplained component of a wage decomposition, and discuss how much of this component can be explained by naturalization. To this end, the author plans to estimate Mincer equations for three groups (naturalized and non-naturalized immigrants, natives), based on which he will perform the decomposition.

An obvious problem with this approach is endogenous selection into naturalization. The same factors that determine the decision to naturalize may also determine wages, and these determinants may not be observable. The author tries to solve this problem with a conditional independence assumption that naturalization, conditional on occupation, education, and other observable characteristics, is as good as random. With this assumption, the author essentially assumes away any selection bias. While any paper that presents causal estimates (or estimates free from selection bias) has to make such comparisons, a large fraction of these papers is typically devoted to defending this assumption. At the very least, the author has to discuss why this assumption is plausible (what are the likely confounders? can you really control for them?). He provides some discussion thereof in Section 2.4, but there should be more. Preferably, there

should also be some plausibility or falsification tests supporting the assumption. The ideal way of dealing with selection bias is to exploit a natural experiment such as a citizenship reform. In absence of that, the author could estimate bounds in the spirit of Altonji, Elder & Taber (JPE 2005) or Oster (JBES 2017) to provide the reader with a feel for the extent to which selection into naturalization is driven by unobservables.

The analysis is based on data from the Luxembourg Income Study, which is described in Section 2.3. Some commentators will probably find the choice of France and Denmark arbitrary, but the authors provides some good reasons for his choice. Many studies are based on one country only without anyone questioning the choice; adding another country certainly adds value. Besides that, this section offers very little information about the actual sample that is used in the study. Particularly important at this stage is the representativeness of the dataset, as well as the sampling ratio. My understanding is that the LIS is representative of the respective populations, but the sample ratio is quite low. This may not be a problem when studying natives, but studying smaller subpopulations such as naturalized immigrants may induce a severe measurement error problem. Without a comprehensive description of the sample, it is difficult to assess whether measurement error is a problem or not.

In Section 2.4, the author presents the estimation results. He begins with estimations of the naturalization premium using simple OLS regressions. While I agree with this part of the analysis in principle, the presentation of the results needs to be significantly improved. Having the main estimation tables in the appendix suggests that the results are of minor importance, which they are not. Moreover, I find Table 2.6 confusing, in particular the headlines. From what I understand, the author estimates wage regressions, whereas the headlines state "naturalization premium“, "integration“, etc. I had to go back to the main text to understand what the author is doing here, and even then some guesswork was needed. I strongly encourage the author to make the table self-contained and to clearly show the estimating equation behind this table.

In a second step, the author estimates probabilities of having a particular occupation based on observable characteristics, which he compares to the actual probabilities to see if the difference can be explained by naturalization. This analysis, while interesting, lacks detail in its description. It is not clear what model the author estimates and how the estimates are to be interpreted.

In a third step, the author proceeds to the wage decompositions. As in Chapter 1, he first estimates the fair wage given observable characteristics, which serves as a reference point. In the decompositions, which are well presented, the author finds that migrants are more different from natives in France than in Denmark, such that the explained component is larger in France. Moreover, the unexplained component in France becomes small and statistically insignificant once natives and naturalized migrants are compared. In Denmark, an unexplained gap of 22% remains. The author concludes from this result that the naturalization premium is higher in France than in Denmark.

Overall, I like the idea of this chapter, but in its current form, the chapter is far from publishable. It (still) lacks the attention to detail, which is the minimum requirement for publishing in a reputable journal. The data need to be much better described, and, in general, the reader needs to be guided through the analysis. This holds particularly for Sections 2.5 and 2.6, where the result is presented without much reflection of its meaning. What do we learn from the differences between France and Denmark? Is the difference due to labor market institutions, selection into migration, selection into naturalization, or some other reason? The author so far leaves too much about the analysis to the guess work of the reader. Nonetheless, once the author is willing to go invest more time in this project, I see publication potential in field journals in labor economics such as ILRR, Labour Economics, LABOUR, Industrial Relations, or the IZA Journal of Labor Economics, to name a few.

I would recommend the following changes and extensions:

- Correction for selection bias:
    - provide an in-depth discussion of the plausibility of the conditional independence assumption
    - provide statistical tests (and/or conduct a bounding analysis) to assess the importance of selection into naturalization

- In general, all tables need to be self-contained. For example, in Table 2.1, it is unclear what group the statistics in each panel actually refer to. Take a paper published in a top journal and mimic the design and description of the tables.
- Discuss the results in greater detail.

Chapter 3

Chapter 3 represents a descriptive study of the determinants of immigrant integration. Using data for 13 European countries from the European Social Survey (ESS), the author compares the socio-economic integration of naturalized and non-naturalized immigrants as well as second-generation immigrants and natives and relates these to the strictness of migration policies as well as native attitudes towards immigrants.

Among the three main chapters, this is by far the least advanced. The author would need to invest substantial time and energy if he wanted to turn this chapter into a research paper. My main criticism about this chapter is the lack of a clear-cut research question. From the introduction and most of the discussions I understand that the author wants to show something about integration and its relation with native attitudes towards migrants as well as countries' strictness in their migration policy. While these issues are by no means uninteresting — quite the opposite — I miss a clear research question and a clear motivation thereof.

The chapter begins with an introduction that is pretty confusing. The point of an introduction is to explain to a reader in plain English what a paper does and what the reader should take away from it. I can see none of that in this introduction. It is difficult for me to give suggestions how to make this introduction better, other than suggesting to start again from scratch.

Section 3.2. presents a literature review that is comprehensive and represents the most clearly written part of this chapter. Section 3.3. presents the dataset and briefly discusses the sample restriction and the construction of the main variables of interest. As in previous chapters, this section would greatly benefit from a more thorough description of the dataset. For a reader, it is very important to be shown the descriptive statistics and to know about the limitations of the sample. In absence of such information, it is difficult to assess the quality of the estimates later on.

The analysis is presented in Section 3.4. Most of the analysis consists of descriptive statistics, showing some correlations between two variables across different groups of migrants, and comparing them across countries. In a second step, the author estimates ordered logit regressions to estimate the naturalization premium. While in the first part, it is mostly clear what the author is doing, in the second part it is not. The bar charts in the first part are clear, although I have trouble understanding the graphs with the MIPEX score on the horizontal axis. I would urge the author to use a different layout for these graphs, one that can be easily understood by an informed reader. In the second part, without writing down an estimating equation and without proper labeling of the tables, it is difficult to understand the regression results presented in the tables. Therefore, I have a hard time commenting on the results.

In sum, this chapter seems to me like a mixed bag of correlations, from which it is difficult to draw any meaningful conclusions. The overall topic is interesting, however, which is why I would encourage the author to focus on a clearer research question and provide a state-of the art answer to it, using the most recent econometric techniques out there.

Concluding remarks and recommendation

This thesis represents an independent piece of research in labor and demographic economics. The author has shown his ability to identify interesting questions, gain a deep understanding of the relevant literature, and answering the questions with available datasets. Neither the methods nor the datasets represent major advances of the literature, but the combination of research questions, methods and datasets makes sense.

In my assessment of each of the chapters, I come to very different conclusions. The first chapter clearly stands out, and represents a solid piece of research: a well-established methodology applied to an important question, with interesting findings. Wage decompositions as a method are probably past their peak, but they have their place in the empirical toolkit, and — if applied to an interesting question — can deliver important insights. The second chapter applies the same methodology to migrants in Europe, although the quality of the writing and the overall presentation lags behind the first chapter, and the author will have quite some work to do to make this paper publishable. The third chapter, in my view, does not live up to the standard of a PhD dissertation. The research question is unclear, as are the writing and the analysis. Moreover, the analysis is too descriptive for a PhD; it is, at best, at M.Sc level. I have outlined some suggestions for improvements in my comments above, but the author should be aware that it might be best to re-write this paper from scratch.

For the revisions of the thesis, my overall suggestions would be as follows:

- The author shows that he can write well in English, and yet the language lacks precision. I would strongly recommend to write in a more focused manner and with more precision. The reader needs to be guided through each chapter, and nothing should be left to the reader's guesswork. This begins with concise writing and ends with self-contained tables that can be understood without having read the text.
- The author needs to place much more emphasis on the econometric theory as well as the description of the data. On several occasions, I did not understand what model is actually estimated, what assumptions have been made, and how the estimates should be interpreted. If this is not crystal clear to the reader at first glance, no reader will ever take the time to figure this out himself.
- In each chapter — and potentially in the thesis as a whole — I also miss a discussion of the findings and their meaning in the broader context. Each chapter begins with an introduction that (more or less) states what an important policy issue the integration of migrants is. But in each conclusion, the reader is left alone thinking about.

My overall assessment is that the candidate has shown some potential, but the thesis in its current version does not fulfill the requirements for a PhD in economics. For a successful defense, careful revisions will be needed. This will require great effort, but given the interest of the topic, should also pay off in the end. In its current form, unfortunately, I cannot recommend this thesis for defense without undergoing a careful revision.