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Does immigration reduce wages of natives? Evidence for nurses

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Abstract

Previous research has not reached any definite conclusion on whether immigration has a negative effect on the wages of natives. This paper reexamines the question by looking at the effect of immigration of foreign born nurses on the income of native nurses in Sweden. Using full population individual level data for nurses in Sweden for the period 1989-2000, we relate the share of foreign born nurses in counties to the income level of native nurses. We find no evidence of the share of foreign born nurses negatively affecting the incomes of native nurses, with estimates being either positive or statistically insignificant.

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1. Introduction

Does immigration reduce the income of natives? This has been a long debated question among economists, with research yielding no conclusive evidence for either side. Owing to the ever present debate about immigration and the important policy implications of the question, the current state of knowledge is unsatisfying by far.

Looking to previous research, most studies have tried to measure a general income impact on all workers. However, as Card (2001) asserts, looking at aggregate measures of immigration might prove to be a too crude instrument. A more fruitful approach might be to look at one specific occupation group. Building on that, we intend to study the income effect of immigration on nurses in Sweden. We believe that studying this specific occupation group can contribute to the general debate about the income effects of immigration. As a result of our focus on nurses, our study has two main advantages compared to most previous literature.

First of all, healthcare is mainly a non-tradable good. One reason why many previous studies find no effect on wages or income from immigration could be due to factor prize adjustments resulting from trade within a country. Healthcare, and in particular the care provided by nurses, cannot be traded with ease (or at all). Consequently, this specific problem should be mitigated with our approach.

Second, nurses are a very easily defined and delineated group. As a nursing education is a vocational training, we should expect the majority of nurses to try to get jobs in nursing. At the same time we know that only registered nurses with the appropriate education can actually work as nurses. It follows that we have a very clearly defined labor market, in which we can neglect any problems resulting from illegal immigration and undocumented workers. In more general studies the authors usually define people with similar age and years of education as substitutes. This is obviously a simplification. In reality, there are numerous cases where people with the same years of schooling and age simply cannot compete for the same jobs.

All the above positive reasons aside, we realize that focusing on a specific occupation will lower the general applicability of our study. In addition, looking at nurses does not only entail benefits, it also introduces several problems of its own.

The main problem concerns wages. First, minimum wages are given by collective agreements, so any negative wage effect is limited by this (Calmfors and Richardson 2004). The high degree of unionization among nurses also has implications (Thomasson 2000). Wages in Sweden tend to be rigid, especially in the public sector and in occupations with high union membership (Agell and Bennmarker 2007). Indeed, according to Agell and Bennmarker, barely one percent of the Swedish workforce experienced a nominal wage decrease during the crisis years of the early 90's. Of those employers who choose to lower wages for their employees, the great majority were in the private sector. We thus do not expect to see an immediate decrease in nominal wages, but instead a real wage decrease through comparably worse wage growth. This case of sticky wages is shared with many other occupations, but will nonetheless be a problem when looking at nurses.

But as the benefits of focusing on nurses helps to alleviate some of the main problems discussed in the literature, we still argue that examining nursing wages and immigration is a valuable pursuit.

The results would give an indication about whether those problems have been the obstacles to finding a negative wage effect.

Following from the above, the purpose of this paper is to investigate the effect of immigration on the income of nurses in Sweden. Given our purpose and method at hand, the research question addressed becomes:

- *Does the share of foreign born nurses in a region negatively affect the income of native nurses in the same region?*

To answer the above question, we use an empirical strategy based on the established area correlation approach. In summary, we find no evidence of a negative income effect from immigration. We thus conclude that our results are in accord with the sizable share of research not finding a negative income effect.

In what follows, section 2 provides background on the labor market situation for nurses during the period 1989 to 2000. Section 3 and 4 details applicable theory and summarizes the current state of knowledge, both generally and for the specific case of nurses, while section 5 presents our data and method. Results are presented in section 6, and section 7 concludes.

2. Background

Requirements for Becoming a Nurse

Nursing is a regulated profession and a license is required to practice it, with the main prerequisite for acquiring a license being an education in nursing. Currently the Swedish education is three years, and has been so since 1993. Before that, the education was only two years.

Things are somewhat different for a person who already has a nursing education from abroad, since that education will not be automatically recognized. However, the recognition process is simple for those coming from countries within the European Economic Area (EEA) and Switzerland. All that is needed is to apply to the Nation Board of Health and Welfare for formal recognition of one's education (National Board of Health and Welfare 2012a).

The process is significantly more rigorous for people coming from countries outside the EEA. All in all, it consists of 7 steps. Included among these steps are formal knowledge tests, a Swedish language test and practical service (see Appendix A for a detailed list of steps)(National Board of Health and Welfare 2012b).

The Labor Market

The labor market for nurses is distinctive in many ways. The product sold, healthcare, is highly regulated and the end-consumer pays only a fraction of the cost. Furthermore, for a long time there were barely any private sector employers at all, only public sector ones. Two final peculiarities of the market are the large share of women, about 92% in our period, and the constantly low level of unemployment (Calmfors and Richardson 2004).

Research into the relation between nurse labor supply and wages has generally found a weak relationship, implying that the supply is inelastic[1]. It follows that only a substantial increase in wages would increase supply.

Employers of nurses have long regarded the supply as too low. Following the economic crisis in the beginning of the 1990's, the share of employers that found the supply as too low increased substantially. By the end of the decade, almost every employer of nurses regarded the lack of nurses as a problem (Calmfors and Richardson 2004).

Wage Setting for Nurses

As described by Calmfors and Richardson (2004), the system for wage setting has drastically changed since the 1970s. In that period, wage setting was centralized and decided on the national level through collective bargaining between the Federation of County Councils (FCC) and the Swedish Association of Healthcare Professionals (SAHP). In practice, local wages followed the national collective agreement in full, which stipulated a single wage rate for all nurses which would only differ by age.

[1] See for example Shields (2004) and Askildsen (2003)

Beginning in the 1980s, this system started to change as a new perspective on wage setting gradually took hold. More emphasis was put on individually determined salaries. The national collective agreement from 1988 broke with previous agreements in that it did not set the actual wage levels for nurses instead opting to only set a minimum wage. An explicit policy for individual wage setting was introduced not long after in 1989.

The economic crisis during the early 1990s put a temporary halt to the decentralization of wage settings. With the so called Rehnberg agreement, wages setting partially returned to centralized levels during 1991 to 1993. Nonetheless, once the crisis ended, progress toward decentralization returned. The five-year agreement between the FCC and the SAHP in 1995 pronounced an acceptance of individual and differentiated wages for nurses. The new view regarded differentiated wages as an incentive for further training and a more proactive workforce.

Several other policies were introduced during the 90s, all working toward decentralization of wage setting. Among others changes, bargaining should now occur at the local level and the responsibility for setting wages should belong to the line manager. In addition, wages should reflect differences between different labor markets.

In conclusion, the major part of the 1989-2000 period has been characterized by an intended move toward wage setting at the local level with a focus on individual wages.

3. Theory

In this section we will describe theory explaining the effect immigration has on wages, starting out by looking at the total labor force. Following that we will limit our scope and show what happens if we only concern ourselves with the immigration of nurses. It is important to remember that wage adjustment is just one mechanism through which an economy reacts to immigration. Accordingly, we will also describe other mechanisms.

All Workers

Wage Adjustment

We start by letting the economy be represented by a constant return to scale production function with three factors of production: capital, skilled labor and unskilled labor. Throughout the text, we assume a perfect market. Further, we distinguish between natives and immigrants. Starting out, we make a number of additional strong assumptions, all of which we will relax later on. They are as follows:

(1) The supply of capital is perfectly elastic.
(2) The supply of skilled and unskilled labor is perfectly inelastic.
(3) There is only one product.
(4) Natives and immigrants are perfect substitutes.

Suppose now that we have an economy where the labor market is in equilibrium with a given share of skilled and unskilled workers. Consider an influx of immigrants of which all are unskilled. The immediate difference is that there is now higher share of unskilled workers. In other words, the skill composition of the economy has changed. At the current wage there will be an excess supply, leading to a decrease in wages for unskilled labor. At the same time, demand for unskilled labor will increase as a result of the lower price. These two mechanisms will lead to a new equilibrium, albeit at a lower wage than before.

Unskilled workers see their income decline in this case while skilled workers, being a complement, see theirs being raised. As the supply of unskilled workers increase, the *relative supply* of skilled workers decreases. A lower relative supply of skilled workers would mean that their marginal productivity becomes higher. As workers are paid a wage equal to the marginal product of the last employed worker, skilled workers will now have a higher wage than before. The result from these effects is that only the relative wages between different groups of labor change, leaving the average wage of all labor unchanged.

In the previous example, we showed the effects from immigration when a majority of immigrants were unskilled workers. Having an immigration wave where the majority is instead made up of skilled labor would simply reverse the effects. Unskilled workers would see their wage increase while skilled workers would see theirs decrease. The wage effects from immigration thus depends on the ratio of skilled to unskilled among immigrants in relation to the skill distribution that prevails in the country. There will be no effect in the specific case where immigrants have the

same skill distribution as natives. That would be nothing more than an increase in the scale of the economy.[2]

The rationale behind the above idea is that immigrants not only supply labor, they also demand goods. With only one good we can neglect any potential differences between natives and immigrants in what sorts of goods they prefer. But even with multiple goods, as long as immigrants prefer the same goods in similar ratios as natives do, production will just increase without any changes in the output mix. The demand side thus mimics the supply side in that differences in the compositions drives change. In order to simplify, we will in all our examples assume that immigrants have similar demands as natives.

In the case of a different skill distribution, the average wage for all workers will in the worst case not change at all. Only the relative wages for different skill groups are affected, with the share of national output going to labor remaining constant. The reason why we receive such results can be found in assumption (1), that the supply of capital is perfectly elastic. If the labor supply increases, we should expect the marginal productivity of capital to increase and, as a result, capital will have a marginal productivity higher than its price. It follows that it will become profitable to supply capital, and it will continue to be supplied until the price is brought down to a normal level.

However, if we allow the supply of capital to be inelastic, the results will be different. The relative scarcity of capital leads some of the redistribution that previously went to skilled labor to instead go to capital. The average wage can now decrease due to immigration since capital will take a larger share of national income. Given a small open economy and small immigration flows, the capital assumption is not unreasonable.

Other Mechanisms for Adjustment

As mentioned, wage is not the only mechanisms through which an economy can adjust to immigration. First off, relaxing assumption (2), that the supply of labor is perfectly inelastic, slightly changes the results from above. We now allow the supply of labor to be elastic, although still upwards sloping. Continuing the example with unskilled workers, what happens is that a group of people are no longer willing to work for the lower wage. Supply still increases, but by less than in the previous example, since some people choose not to work anymore. Previously, everyone worked at whatever wage was offered. The extent of the downward pressure put on the wages of unskilled workers depends on the size of the supply increase. It follows that if the supply increase is *lower*, wages will decrease by *less*. Relaxing assumption (2) thus opens up another way for the economy to adjust to immigration. All in all, the economy can now adjust in two ways: unemployment and wage decrease. Which of these two mechanisms is used most intensely depends on the elasticity of the labor supply.

Assumption (3) states that there is only one sector that produces a single product. Relaxing this assumption and allowing for multiple sectors with multiple goods brings about two additional adjustment mechanisms: output mix and technology.

[2] Intuitively, this can be understood by imagining what would happen if we created an exact copy of Sweden.

The first of these, output mix, simply refers to the Rybczynski theorem (Rybczynski 1955). In this example, we assume that we have two industries that differ as to which type of labor that they use intensively. Each industry produces a good that is traded on the world market. Further, we assume that we have a small open economy whose production is small relative to total world production. Given all this, we can assume fixed prices for both goods on the world market. Going back to our example with immigration of unskilled workers, initially we will see the same effects as before. Wages for unskilled workers decrease and wages for skilled workers increase. Consequently, the production costs for the unskilled-intensive sector will see a relatively larger decrease in production cost. Given fixed prices, the profitability in that sector will be higher. New firms will then enter the sector and production will be expanded. This drives up the wages of unskilled labor until profitability is brought back to normal. Altogether, wages should be brought back to the pre-immigration levels through a change in the output mix.

The second mechanism works through technology[3]. By technology we mean the different production methods that exist to produce the same good, each of which uses different ratios of inputs. Immigration of unskilled workers will once again initially reduce wages for all unskilled workers. But once companies notice this reduction, they will switch production technology to one that uses unskilled labor more intensively in order to reduce costs. The subsequent increase in demand for unskilled labor can increase wages. The economy can thus adjust to the increased supply of unskilled workers by changing production technologies, all without wages necessarily changing from pre-immigration levels.

Substitutability of Immigrants

The last of the assumptions we provided in the beginning of this section, (4), states that immigrants and natives are perfect substitutes. It is this substitutability that allows immigrants to compete with natives for the same jobs, and thus also affect wages for those same jobs. We could, on the other hand, assume that natives and immigrants are not substitutes at all. In that case, immigrants would only compete with each other and not negatively affect native wages. Indeed, immigrants would even increase the wage for those natives they are complements to. The most realistic assumption is probably somewhere in between and depending on a multitude of other factors.

In conclusion, when we use all our assumptions, the effect from immigration is clear: wages decrease for those who are substitutes for immigrants. But when we relax the assumptions, we give rise to a number of other ways for adjustment. Furthermore, any effects for natives rely on immigrants being able to compete for the same jobs as natives. Of interest is the fact that, except for employment, all listed adjustment mechanisms needs time to work. The incentive to supply more capital, change industry or production technology comes from wages *first* actually decreasing. This implies that short-run effects on wages are negative and only long-run effects have the potential to become zero.

With this as background, we now turn to describing how it all relates to the specific case of nurses.

[3] See Lewis (2005) for a more thorough theoretical review, in addition to an empirical analysis, of the technological adjustment mechanism.

Nurses

This section outlines how the previously presented theory is applicable when examining the immigration of nurses. To be exact, we are limiting ourselves to the effects on nurses from the immigration of nurses.

As previously noted, healthcare (and by extension nursing) is a highly regulated industry. For example, a nursing or medical license is required by law to perform certain tasks.

Consider an example in which a group of nurses migrate to a specific country. Comparing this to our previous example, nurses can be considered a specific production input akin to one of the previous skill groups. All other things equal, immigration of nurses increases the supply of nurses and, most importantly, nurses' share of the total workforce. As a starting point, we should see a negative effect on either one or both of nursing wages and employment. But as we have previously noted, the supply of nurses is very inelastic, with the result that the effect should thus be concentrated to wages.

If we assume that immigrants demand the same amount of healthcare on average as natives, then the demand side of immigration will have no effect on wages. The assumption regarding the supply of capital is beside the point now. The effect from different levels of capital concerns only how other labor groups and capital will divide the gain from lower nursing wages.

The effect on nursing wages or employment depends on the nurses immigrating alone, or at least being a disproportionally large share of the immigration wave. If the immigration of nurses was correlated with other immigration (in that they do not immigrate alone), then any wage effects could disappear by nurses being a similar share of immigrants as nurses are in the native work force.

Other Mechanisms for Adjustment

Owing to the peculiarities of the healthcare industry, some of the previously discussed adjustment mechanisms might not be applicable. Considering the highly regulated nature of healthcare, there is arguably less scope for changing production technology than in most other sectors. But that is not to say that there is no flexibility in the production at all. It is therefore difficult to say beforehand how applicable the technology adjustment mechanism is. Hence, we will have to consider it.

Healthcare, and especially that provided by nurses, is a nontradable good[4]. Local demand will therefore have to be met by local supply. The economy cannot adjust to an increased supply of nurses by increasing healthcare's share of production. Consequently, the output mix adjustment is not possible.

[4] Of course, you could consider traveling to another country to get healthcare as a form of trade. But this kind of trade is presently very low (Herman 2009). The only major international trade in healthcare is currently the migration of healthcare workers.

Substitutability of Immigrants

Implicit in our reasoning is the substitutability between native nurses and immigrant nurses. But this may not be a correct assumption, since nursing requires, among other things, communication skills. If an immigrant nurse cannot speak the native language, that person may have a difficult time being hired as a nurse, since mastering the language may be a requirement for working. In that case, that nurse would not compete with native nurses and thus not affect nursing wages negatively.

Theoretical Expectations

Summarizing the above section, theory gives that immigration of nurses should in the short-term lead to a decrease in wages, given native-immigrant substitutability. Long-run effects depend on the possibility of changing production technology, the elasticity of supply changing (so that more people choose not to work) and other immigration returning nurse's share of the workforce to normal. Of course, if the immigration was expected, any adjustment might have already taken place before the immigrants arrived. In that case we would only concern ourselves with the long-run effects.

However, nurses do not act on a perfect market, meaning some of these predictions might not be applicable. Since wages are sticky, we should for example not expect to see any short-run effects happening immediately upon increased immigration. Long-run effects will thus be the most important. As previously stated, theory predicts lower wages in the long-run, albeit with the possibility for adjustment through other mechanisms, leaving wages unchanged.

Decision to Emigrate

Until now we have neglected the fact that people do not migrate without a reason. In many cases, immigrant's reasons for migrating have no bearing on our question. But in some cases it will prove to be important.

Migration theory separates the reasons for migration into two categories: supply-push factors and demand-pull factors.[5] Supply-push factors refer to conditions in the home country that makes people emigrate. Normally these factors are negative. Examples include unfavorable employment conditions, low wages and more extreme factors such as war and political unrest. Nevertheless, supply-push factors are of no interest to our question since they do not concern the destination country directly.

Demand-push factors refer instead to those factors in the destination country that attracts immigrants. Typical examples are higher wages, better living conditions and more employment opportunities. But foreigners can also be enticed to migrate by the presence of fellow countrymen in the destination country.

It follows that a demand shock might induce foreigners to migrate. If the demand for healthcare in a country increased (due to for example an increasing share of elderly), and if supply did not

[5] See Clark, Hatton and Williamson (2002) for a more detailed and formal exposition of economic theory on the decision to migrate.

increase with it, wages for nurses would certainly increase. Foreign nurses could in that case find it worthwhile to move to that country. Immigration can in turn depress the nursing wages, returning prices to status quo. While this makes no difference for our theory, it can pose a problem when we later try to empirically test our question.

4. Previous Research

There has been considerable previous research on the labor market effects of immigration, with most studies finding little to no effect. Theory gives us clear implications for how an immigration-induced labor supply shift should affect wages, at least while neglecting potential adjustments through trade and technology. Not finding a negative wage effect from immigration thus still fits with the theory. Nevertheless, research has been inconclusive. While most results hover around zero, some find a strong negative effect from immigration. Research has nonetheless continued, in most part driven by debate about the empirical methods used. The problem has not been in theory, where nearly all researchers agree, but in empirically testing the theory, which is fraught with many difficulties. There have been two main approaches to estimating the effects of immigration on wages: the area correlation approach and the factor proportions approach. We will in this section elaborate on these two methods, as well as the equally important research that uses natural experiments. Following that we will describe those studies that specifically address this issue for nurses.

Area Correlation Approach

The area correlation approach builds on the finding that immigrants tend to cluster in certain areas instead of being evenly spread out in a country. The method starts out by assuming that different regions in a country can be considered independent closed labor markets. Using that assumption, it is possible to relate the share of immigrants in different local labor markets to the wage and employment levels there. Influenced by the findings of Bartel (1989), researchers using this approach have in most cases used an instrument based on previous immigrant settlement. The idea behind this instrument is that immigrants tend to settle in areas where previous immigrants with similar origin already live. The usage of this instrument is meant to alleviate the problem resulting from immigrants settling in places with high wages or wage growth, a problem which would otherwise bias results.

An early and widely cited study is Altonji and Card (1991). The authors use 1970 and 1980 census data to estimate both the cross sectional and first difference estimates of the correlation between labor market outcomes and immigration shares in American metropolitan statistical areas (SMSA). Their cross sectional results show an unexpected but weak positive correlation between immigration share and wage rate. The first difference approach yields, on the other hand, a weak negative correlation. Although, as both results are so weak, it is hard to argue that they are very different. To attempt to control for endogeneity, Card adds a version of the traditional instrument. Nonetheless, using the instrument barely changes the results.

Using 1980 and 1990 census data instead, Borjas, Freeman and Katz (1997) estimate the relationship between the ratio of immigrants in a region to the log of weekly earnings of natives in the same region. To control for factors such as changes in local labor market conditions and education achievements between 1980 and 1990, the authors apply a difference-in-difference approach. When using relatively small geographical areas, such as an SMSA, the authors find a positive but nearly zero effect on wages. However, using successively larger regions, such as states, turns the effect negative, although still close to zero.

Reacting to criticism against the area correlation approach, Card (2001) introduces two changes to it. First, the traditional instrument is developed further. Focusing on recently arrived immigrants, Card intends to increase the relevance of the instrument. The idea behind the change is simple and follows from the fact that the correlation between residence and previous immigration should be the strongest for the recently arrived. From this, the author develops an "exogenous supply-push component" based on previous migrant's decisions on where to live, the total number of immigrants from a specific country and the share of different occupations for those immigrants. Further developing the area correlation approach, Card explicitly tries to incorporate the theory that asserts that wage changes results from immigration that changes the skill composition. Skill groups in this paper are defined as different occupations. Consequently, an influx of immigrants that increases the population share of an occupation would put downward pressure on wages for that occupation. Similarly, an influx of immigrants that do not change the relative share of any occupation group would not affect wages. Using US census data pertaining to the late 1980's, Card finds that the wages of low skilled workers were reduced by 1 to 3 percent as a result of immigration.

The early studies have mostly used American data, but in recent years there has been a proliferation of studies using European data. The results from these should be interesting, seeing as how both labor market and immigration policies differs in most of Europe compared to the US. However, on average these studies seem to confirm the American ones. Dustmann, Fabbri and Preston (2005) use British Labour Force Survey data for 1983 to 2000 (for employment data) and 1992 to 2000 (for wage data) to examine immigration and wages in the UK. Three statistical methods are used: simple OLS, regional fixed effects and regional fixed effects with instrumental variable. The OLS results imply that a one percentage point increase in immigrant share would increase native wages by 0.8 percent. The authors propose that this effect is due to persistence in economic conditions and immigrant concentrations. Accordingly, they use regional fixed effects to control for these effects. Incorporating the fixed effects still yields results that imply a positive, but lower, effect at 0.2 percent. However, the results are not significant at conventional levels. Going a step further, the authors add the traditional instrumental variable. Nonetheless, this does nothing but *increasing* the effect to 0.9 percent, leaving it still positive and insignificant.

Spain has also been of interest to researchers owing to the great number of immigrants the country received during 2001-2006. During this period, the foreign-born share of the population increased from 6 percent to 13 percent. Gonzalés and Ortega (2011) uses Spanish sample data to estimate the labor market effects from this immigration. Even using earlier immigrant settlements as an instrument combined with regional fixed effects, the authors find a very small negative effect on wages. A 10 percent increase in labor supply would lead to a 0.6 percent reduction in wages.

Glitz (2012) uses the approach developed by Card (2001), in which immigration affects local labor markets by changing the relative supplies of different skill groups, and applies it to German data. But instead of using the traditional instrumental variable, Glitz uses the fact that German immigration policy for ethnic Germans between 1996 and 2001 worked in a way which made the

settlement choice of the immigrant plausibly independent of economic conditions[6]. As inflows of ethnic German immigrants can be considered largely exogenous under this policy, the author uses this as an instrument for changes in relative factors shares. In contrast to Card, who only used cross-sectional data, Glitz uses longitudinal data and is thus able to difference away region and skill specific fixed effects. The results acquired imply that a 10 percent increase in relative skill shares in a region, through additionally employed individuals, decreases relative wages by 0.62 percent. Even so, the results are not significant.

Despite the multitude of studies using it, the area correlation approach is not without problems. Borjas, Freeman and Katz (1992, 1996, 1997) criticize it on several points. First, immigration to a region might cause a subsequent migration of people to other regions, thus not changing the total labor supply and, by extension, not changing wages. Second, local demand shocks might raise wages, which can in turn attract immigration. Third, an increase in the supply in one region could in the long run be diffused across the economy by interregional trade. The authors propose that the failure to find any significant effects on wages is simply due to this adjustment through trade. Finally, as described in the theory section by us, if increased labor supply was coupled with increased capital supply, there might be no wage effect.

The potential migratory respone of natives, the first criticism mentioned, has been empirically examined by several researchers. A recent study by Peri and Sparber (2011) found no evidence of any migratory response in the US. Card (2001) also looks at the migratory response of natives, finding results similar to those found by Peri and Sparber. However, not all research point in the same direction. For example, Borjas (2006) argues for a large displacement effect and estimates that roughly three natives are displaced for every 10 immigrants.

As should be quickly realized, using only cross-sectional data opens up for spurious correlations between immigration and wages. This is another problem put forward by Borjas et al. (1997). Consider a region with high immigration coupled with high wages, and where the wage level is due to reasons independent of immigration. While there would be no causal connection between high wages and high immigration in this case, cross-sectional OLS will find a correlation. The solution to this problem has been to use panel data and, through fixed effects, relate the *change*s in immigration shares to the *changes* in wages. However, fixed effects estimation brings new problems. Aydemir and Borjas (2011) put forward one important aspect: the risk for attenuation bias. There is typically very little variation in the share of immigrants in a region over time, meaning fixed effects estimation has very little information to work with.

Factor Proportions Approach

While many seem to treat it as one single method, the factor proportions approach can really be divided into two separate methods. One of these mimics the area correlation approach, in that it relates immigrant shares to labor outcomes. The difference is that instead of using immigrant shares in different geographic areas, it uses immigrant shares for different nationwide skill

[6] Ethnic German Immigrants who arrived in Germany were allocated their place of residence according to a policy that tried to even out the distribution across the nation. Not complying with the law would result in losing all benefits. The Ministry of Interior and the Association of German Cities and Towns regards that this policy was successful in ensuring compliance with residence allocation (Glitz 2012).

groups. This division should be understood as an attempt to group workers with those workers they are substitutes for. But apart from that, the method is very similar to the area correlation approach. Seeing as it is so similar, most of our discussion will concern the other type of factor proportions approach.

The other factor proportions approach builds more directly on economic theory by imposing structure on the estimation procedure through an assumption of production technology. We will accordingly call it the structural approach. The structural factor proportions approach takes off in a general equilibrium perspective of the economy. As a result, the level of analysis is widened to encompass the whole nation. The typical way is to first allow the economy to be represented by an aggregate production function (typically Cobb-Douglas) with two main inputs: capital and labor[7].

Referring back to theory, immigration should put downward pressure on workers with similar characteristics as the immigrants and upward pressure on workers with different characteristics. It follows that any wage effects depends on the degree of substitutability between different types of workers as well as the size of the immigration.

The structural approach takes this into account by dividing labor into different groups and letting total labor be represented by a nested CES aggregate. The idea is simply to divide labor according to some characteristic into different groups. These groups are then further divided according to some other characteristic. This continues for as many different characteristics as the researcher deems necessary. The result is a structure with different levels of aggregation and labor groups with different level of detail in their division, a division very similar to the simple factor proportions approach. A typical nesting approach is to first divide workers by education, followed by age, with a possible third division according to native or immigrant status.

Having built a structure for characterizing labor, the next step is to estimate the elasticity of substitution between different groups. The estimated elasticity can then be used to compute the implied percentage change in the wage for workers in a specific group caused by a percentage change in the supply of workers of the same or a different group.

Compared to both the other factor proportions approach and the area correlation approach, this method imposes considerable structure on the estimation through the implicit assumption of production technology. However, the method carries several advantages compared to the area correlation approach. The perhaps most obvious advantage is that it doesn't have to assume that a country is made up of closed local labor markets. Instead the whole country can be treated as a single labor market. Hence, adjustment of wages due to internal trade and migration can be disregarded. Given a country with negligible emigration, one can also plausibly assume that the labor market is closed. Of course, the question still remains whether the national labor market can be treated as fully integrated.

[7] Nesting the production function into a standard Ramsey (1928) or Solow (1956) model gives us two interesting implications: in the long run the economy follows a balanced growth path with constant real interest rate and capital-output ratio with a capital-labor ratio growing at a rate of $1/\alpha$ times the growth rate of TFP. See Ottaviano and Peri (2012) for a further elaboration on this.

Arguably the most influential study using this approach is Borjas (2003), in which the author uses both the simple and structural factor proportions approach. Borjas uses a nesting structure with two levels: education (four groups) followed by age (eight groups). The results from the study imply that a 10 percent increase in the supply of labor in a specific education group reduces wages in that group by between 0.2 to 0.8 percent. Likewise, a similar increase in an education-age group decreases wages in that group by about 3 percent. The wages of other groups, being complements, increases by between 0.02 to 0.33 percent.

Building on the structural approach of Borjas (2003), Ottaviano and Peri (2012) introduce several important extensions to the model. First of all, in addition to experience and education, Ottaviano and Peri divide workers according to whether they are immigrants or natives. In consequence, they treat the substitutability between natives and immigrants as an empirical question. Moreover, they specifically focus on long-run effects by assuming full adjustment by capital. This is in contrast to Borjas (2003), which assumes constant capital stock, and thus focuses on short-run effects. Of interest is that Ottaviano and Peri also attempt to compute the *total wage effects* from immigration. The authors define total wage effect as the change in wage experienced by the total workforce, not just substitutes. The reason for using it is that immigrants do not simply lower wages for similar workers, immigrants also raise wages for complementary workers. Since immigration usually consists of people with varying education and experience, this creates a myriad of different effects. Looking at US immigration during 1990-2006, they find that natives have on average had their wages marginally increase from immigration. Looking at educational groups, the wages for those with less than high school education and those with college education slightly decreased. Wages for high school graduates and those with only some college experience slightly increased. The ones who lost from immigration were actually previous immigrants whose wages decreased on average by between 6.7 and 6.8 percent. Although all education groups among immigrants had their wages reduced, the ones who lost the most were high school dropouts and high school graduates.

Manacorda, Manning and Wadsworth (2012) use the same model as Ottaviano and Peri (2012) did for US data, but instead apply it to UK data. Starting from the fact that the UK has experienced a significant rise in immigration over the past 30 years, they examine why this has had no apparent effect on the average wages and employment of native-born workers. Their conclusion is similar to Ottaviano and Peri (2012): due to imperfect substitutability between natives and immigrants, the ones who lost from immigration were previous immigrants. Of specific interest is the fact that Manacorda et al. find even lower substitutability between immigrants and natives than Ottaviano and Peri did. Ottaviano and Peri put forward an explanation to this: Manacorda et al. uses yearly inflows of immigrants while Ottaviano and Peri use ten-year flows. Using yearly inflows could mean that the elasticity of substitution is identified on very recent immigrants, who obviously have yet to become fully integrated into their new home.

While the structural approach has been used extensively in recent research, it also has its problems. In contrast to the area correlation approach, little work has been done to alleviate the problem with supply-driven immigrant shocks (by e.g. using instrumental variables). But the main problem is still that results may be too dependent on too strong assumptions. It is not hard to imagine that the choice of nesting structure could potentially cause problems. However,

Ottaviano and Peri (2012) found no differences when they examined the differences resulting from changing the order of education and experience. Card (2012) lists three additional areas in which different assumptions can cause problem: division of skill groups, substitutability between immigrants and natives, and supply of capital.

The first of these problem concerns the number of skill groups. This critic is thus also applicable to the simple factor proportions approach. Borjas (2003) has used four education groups (with both approaches): high school dropouts, high school graduates, people with college experience and college graduates. Traditionally, labor economics has only used two skill groups (see for example Katz and Murphy 1992). The crucial difference is whether to view high school graduates and dropouts as separate inputs. Intuitively it is easy to understand why this is important. If immigrants with low education were substitutes for only high school dropouts, that group's share of the workforce would drastically increase. If, on the other hand, both high school dropouts and graduates competed with low educated immigrants, the relative share of workforce would not increase by as much.

The second problem is whether to assume perfect or imperfect substitutability between immigrants and natives within the same skill group. By using only education and experience to divide labor, Borjas (2003) implicitly assume that natives and immigrants are perfect substitutes. Needless to say, the degree of substitutability between natives and immigrants is important in estimating the wage effect on natives. Both Ottaviano and Peri (2012) and Manacorda et al. (2012) find evidence that natives and immigrants are imperfect substitutes. However, Borjas, Groger and Hanson (2008) dispute their results. They argue that the imperfect substitutability found is the result of too much heterogeneity in the skill groups. As an example, they used the same data as Ottaviano and Peri, but removed students still in high school. This immediately removes any immigrant-native complementarity. In response, Ottaviano and Peri argue that the specifications of Borjas et al. were saturated with fixed effects, leaving little variation left. While the question is far from answered, the debate about native-immigrant substitutability has at least turned researchers to regard it as an empirical problem, not something to be assumed away.

The third problem concerns how to treat the capital supply. Comparing Borjas (2003) and more recent studies like Manacorda et al. (2012) reveal a difference in assumptions. While Borjas assumes that capital is fixed, Manacorda et al. assumes that there is a perfect elastic supply of capital. In the short-term, we could expect there to be little capital adjustment. However, assuming no change in the capital stock seems unrealistic in the long-term. Little empirical work has been done on how capital flows actually respond to immigration. As an example, Ottaviano and Peri (2012) estimated a 10 percent per year convergence rate.

A further problem was identified by Dustmann and Preston (2012). Both factor proportions approaches typically use years of education and age to create their skill groups. As previously noted, the idea is to create groups where workers are as close substitutes as possible and thus competes with each other. The problem is that immigrants may compete in lower skill levels than their education and age predicts, especially directly after arrival. Immigrants may for example lack necessary language skills, a social network or face discrimination. After staying in a country for a long enough time an immigrant will "upgrade" into his predicted skill level. Dustmann and Preston argue that estimating an elasticity of substitution between immigrants and natives will be

both sensitive to the time span used and biased, all due to the downgrading and the subsequent upgrading process.

Natural Experiments

A problem with most of the previously mentioned studies is their risk of endogeneity problems. Immigration is not always exogenous to labor markets. For example, a person may immigrate to a country because of an increasing demand there. Given a high enough increase in demand, immigration may be correlated with higher wages, even though the immigration did not cause the wage increase.

A few natural experiments have been found where an immigration wave can be considered exogenous, the most famous being the study by Card (1990) on the Mariel Boatlift. On 20 April 1980, Fidel Castro unexpectedly allowed Cubans wishing to migrate to the US to leave Cuba from the port of Mariel. About 125,000 choose to emigrate in a period of a couple of months, increasing the population in Miami by 7 percent and its Cuban population by 20 percent. Comparing labor market outcomes in Miami and other comparable cities, Card finds no appreciable effect. Miami seems to have absorbed the sudden increase in labor without any negative effect on workers already living there.

Further similar studies seem to confirm the results. Hunt (1992) examined the repatriation following the independence of Algeria from France in 1962. Within one year, about 900,000 persons had returned to France. Still, Hunt found no sizable effect on the labor market. Carrington and de Lima (1996) did a similar study using the independence of Mozambique and Angola in the 1970's when nearly 600,000 persons returned to Portugal. In this case as well, the authors found no sizable effect.

Still, natural experiments are not a panacea. Consider the labor demand curve implied by the Mariel Boatlift experiment: perfectly elastic, in that wages are constant regardless of the level of labor supply. We can then compare that demand curve to the one implied by another famous natural experiment, regarding the effect of the minimum wage on employment in New Jersey (Card and Kruger 1994). In that study the authors found that increasing the minimum wage did not lower employment in the fast food industry in New Jersey compared to Pennsylvania (where no wage increase had taken place). The labor demand curve implied by this study is thus perfectly inelastic. Both curves cannot be true at the same time, given that they are each other's opposites. It follows that one must interpret the results with caution, while allowing that they may be context dependent.

Immigration and Nurses

There are few studies on specific occupation groups, and even fewer on nurses. However, nurses and physicians seem to be the occupation groups that have attracted the most interest. The results the studies have yielded mirror the general studies in that they are inconclusive, with results pointing both ways.

Of special interest is a recent study by Kaestner and Kaushal (2012), where the authors use the area correlation approach to estimate the effect of immigrant nurses on the labor market

outcomes of US nurses. The data they base their study on comes from the National Sample Survey of Registered Nurses (NSSRN). The NSSRN has information on approximately 35,000 nurses and is made with the purpose of being representative for all licensed registered nurses, both nationally and at the state level. Information is collected for every fourth year by telephone and mail, with so far having achieved a high response rate of 70-80 percent. Incorporating the traditional instrument and using states as separate labor markets, they find that a 10 percent increase in the total number of nurses due to immigration is associated with a 1 to 4 percent decrease in annual earnings. However, most of their results when using instruments are not significant.

This study can be compared to Kalist, Spurr and Wada (2010) where the authors also looks at nurses, but use the simple factor proportions approach in addition to a cross-sectional area correlation approach. Kalist et al. also use the same survey, NSSRN, as Keastner and Kaushal. The difference is that Kalist et al. only use 1980 and 2000 data for their area correlation. With that approach, both with and without the traditional instrument, they find a significant positive effect from immigration (with the instrument actually increasing the positive effect). They estimate results for different types of local markets: counties, regions and states. Although the larger markets give a more positive result, the differences are not that large. For the factor proportions approach, the authors supplement their previous data with NSSRN data from 1984, 1988, 1992 and 1996. The simple factor proportions approach then yields an implied negative wage effect for nurses, although it is far from statistically significant.

A recent study by Schumacher (2011) also uses NSSRN data. Using the area correlation approach, Schumacher differs by his choice of geographic scope of the market. This study instead mimics the general area correlation studies for the US in that it uses metropolitan statistical areas as local labor markets. Nonetheless, Schumacher gets results similar to the area correlation part of Kalist et al. (2010): a slightly positive effect on wages from an increased immigration share.

In summary, no conclusive evidence has been provided for either a positive, negative or nonexistent effect from immigration, neither for nurses nor workers in general.

5. Method

Data

Our starting dataset consists of all people who have worked in the healthcare sector in Sweden at some point during 1989 to 2000. Our definition of the healthcare sector comes from the 1992 version of SNI, where healthcare is defined as any SNI code starting with the numbers 85.[8] From this we build a dataset consisting of all individuals living in Sweden with both an education in nursing and who have worked in healthcare at some point between 1992-2000 (see appendix B for which SNI and SUN codes we have used to define healthcare and nurse). As we are interested in the wages of native nurses, we drop all observations on foreign born nurses before running our regressions. The data is based on Swedish register data and has been compiled by Statistics Sweden. The dataset details a number of individual level factors for each year: age, sex, highest education achieved, labor status, gross income, income from active business, county of residence and place of birth. Place of birth is given by a rough division of the world: Sweden, Nordic countries except Sweden, EU15 except Denmark and Finland, New EU-Countries, Europe except EU25, North America, South America, Asia, Oceania and the former Soviet Union. The number of observations varies by year, meaning we have an unbalanced panel dataset.

TABLE I. NUMBER OF OBSERVATIONS PER YEAR

1989	1990	1991	1992	1993	1994
49,570	49,925	52,043	51,815	53,172	55,872
1995	**1996**	**1997**	**1998**	**1999**	**2000**
54,634	54,626	53,173	52,899	53,038	51,694

We remove everyone with a yearly income of less than 100 000 SEK to limit the number of part-time workers. We also remove outliers at the other end (those with an income of more than 500 000 SEK). This is because these people are likely to be senior management in healthcare, which are not likely to compete with regular nurses for regular nursing jobs. All in all, we drop a total of 123 634 observations with an income of less than 100 000 SEK and 221 observations with an income of more than 500 000 SEK.

It should be noted that the system for classifying education in Sweden changed in 2000. Due to this we lack information on education for that year. To get around this problem we will assume that everyone has the same education in 2000 as in 1999.

Data on the total population in Swedish regions as well as the number of foreign citizens has been taken from public statistics provided by Statistics Sweden.

[8] Swedish classification system for grouping organizations according to business activities. Based on five number codes where the first two numbers gives a company's broad sector, and the other three which detailed subsector it belongs to. The 1992 version is based on the European NACE classification system.

In table I and II below we present various descriptive statistics of the variables used:

TABLE II. DESCRIPTIVE STATISTICS

Variable Name	N	Mean	Std.Dev	Min	Max	Unit
Income	632461	156	70.0	0	1287	SEK/year (1000's)
Ln Income	632272	11.8	0.81	5	14	Ln SEK
Age	632461	40.1	9.39	18	75	Years
Female	632461	0.93	0.26	0	1	Dummy
Foreign Share	252	0.06	0.03	0.02	0.16	Proportion
Specialist	632461	0.35	0.48	0	1	Dummy
Urban	3468	0.14	0.34	0	1	Dummy
Suburban	3468	0.13	0.34	0	1	Dummy
Large City	3468	0.34	0.47	0	1	Dummy
Medium City	3468	0.16	0.37	0	1	Dummy
Industrial	3468	0.07	0.25	0	1	Dummy
Rural	3468	0.03	0.18	0	1	Dummy
Sparsely Populated	3468	0.02	0.14	0	1	Dummy
Other Large City	3468	0.07	0.25	0	1	Dummy
Other Small City	3468	0.04	0.20	0	1	Dummy

Note: Observations with less than 100 000 SEK or more than 500 000 SEK are included

TABLE III. NUMBER OF NURSES AND THEIR ORIGIN

	Number	Percent (%)
Swedish	70631	91.94
Foreign born	6191	8.06
Nordic except Sweden	3453	4.49
Western*	720	0.94
Eastern**	988	1.29
Other***	1030	1.34
TOTAL	76822	100

**Western: EU15 except Nordic Countries, North America & Oceania. **Eastern: New EU countries, Europe except EU15 and Nordic & Soviet Union. ***Other: South America, Asia & Africa.*

Table II above presents the total number of different individuals in our dataset as well as their place origin. The total share of foreign born is only 8.06 percent. But as can be seen from table I, this varies a lot between different counties.

Empirical Model

Both of the established methods discussed above have problems, with the area correlation approach being no exception. Among other problems, one specific has been discussed in the literature: that of intraregional trade. Even if every region was a closed labor market, they might still trade with each other. As a result, any wage effect might be diluted across the whole country through trade between regions. However, healthcare is a nontradable good. Focusing on nurses thus allows us to neglect this problem.

Given the above, we base our empirical model on the area correlation approach. This approach uses the fact that immigration intensity tend to vary across a country, with Sweden not being an exception. We assume that Sweden can be divided into a number of closed local labor markets. Given this, we can relate the income level in different regions to their immigration intensity and thus estimate the effect of immigration on income. Income will here work as a proxy for wage.

It is not obvious what kind of area to use as a local labor market, and especially what geographic scope it should have. Choosing a very large area will minimize the risk of having a too large share of the population actually seeking work or working in another area. Instead there is a risk that any effect from immigration is diluted as we look at something larger than just one labor market. We have chosen to use Swedish counties as local labor markets for two reasons. It is first of all a large area, but not so large so as to expect that any effects from immigration will completely disappear. However, our primary reason for choosing counties is that this is the level at which wages can be considered set.

Going into more detail about our method, each individual is assigned to a local labor market (in our case county) depending on where the person lives. Immigration intensity is measured by the share of foreign born nurses in each region. We then use OLS to regress nurse wages on the immigration intensity in each person's respective labor market. Our basic model is as follows:

(1) OLS, Basic model

$$\log(Income) = \beta_0 + \beta_1 ForeignShare_{tj} + \beta_2 Female_{it} + \beta_3 Age_{it} + \beta_4 Age_{it}^2 + \beta_5 Specialist_{it} + \beta_6 Year + \varepsilon_{it}$$

This specification might suffer from omitted variable bias, specifically in that there could be regional attributes that affect the income level that we have failed to control for. We thus estimate a model where we have added dummy variables for the different municipal types used by Statistics Sweden to categorize Swedish municipalities. All in all, there are nine different types, giving us eight dummies. In contrast to a regional fixed effects approach, this leaves part of the cross sectional variation intact, and will as a result use more information.

(2) OLS with controls for municipal characteristics

$$\log(Income) = \beta_0 + \beta_1 ForeignShare_{tj} + \beta_2 Female_{it} + \beta_3 Age_{it} + \beta_4 Age_{it}^2 + \beta_5 Specialist_{it} + \beta_6 Year + \beta_7 MuncipalType + \varepsilon_{it}$$

Of course, that is probably not the only regional attribute that could be important. By adding regional fixed effects we can control for all differences between regions that remain constant over

time. Specifically, regional fixed effects isolate the change in nursing income due to changes in the share of foreign nurses. The problem with this specification is that there is very little variation in the share of foreign born over time. As discussed in the previous section, this is a typical problem in the literature. Nonetheless, we still estimate a fixed effects model on account of all the advantages it can provide.

(3) OLS with regional fixed effects

$$\log(Income) = \beta_0 + \beta_1 ForeignShare_{tj} + \beta_2 Female_{it} + \beta_3 Age_{it} + \beta_4 Age_{it}^2 + \beta_5 Specialist_{it} + \beta_6 Year + a_j + \varepsilon_{it}$$

These three specifications provide the foundation for our analysis. A noteworthy distinction between them is that the fixed effects specification, (3), will solely focus on short-run results. Specifications (1) and (2), although probably biased, will provide us with a view of long-run effects.

A detailed list of our variables follows in the table below:

TABLE IV. LIST OF VARIABLES

Log(Income)	Individual yearly income (natural logarithm).
β_0	Intercept.
ForeignShare	The share of foreign born nurses within each labor market, calculated by dividing the number of immigrant nurses in an area by the total number of nurses in that area.
Female	Dummy variable for the sex of the individual: 0 if male, 1 if female.
Age	Age of the individual.
Age²	The squared value of age.
Specialist	Dummy variable for whether an individual has a specialist nurse education: 1 for specialist education, 0 for not. Note that all individuals have at least a basic nurse education.
Year	Dummy for year specific effects.
MunicipalType	A series of 8 dummy variables for the nine different municipal types used by Statistics Sweden to categorize Swedish municipalities. Baseline is *Other Small*.
A	Regional fixed effects.
E_{it}	Error term.

We have a number of variables pertaining to human capital and socio-economic factors that we control for. The variables are as follows: sex, age, age squared and education. Following what is customary in the literature, we have added age squared in addition to just age in order to better reflect the relationship between age and income.

As previously discussed, a further problem with the area correlation approach is the potential correlation between the economic conditions in an area and an immigrant's decision to move there. If immigrants specifically choose to move to areas with positive wage trends we would find a spurious correlation between immigration and wage growth, thus biasing our estimates. Accordingly, an instrumental variable approach is used to alleviate this issue.

A suitable instrument must be correlated with immigration, but at the same time not directly correlated with income or the error term. Our instrumental variable uses the fact that immigrants tend to move to areas where previous immigrants already live. In consequence, many immigrants choose where to live based not on the economic conditions in the region but on previous immigration. Based on this, we use the share of foreign citizens in a region eight years prior as an instrument for the share of foreign born nurses in a given year. As previously discussed, the usage of this instrumental variable strategy is well established within the literature. The first assumption underlying our approach is that the residence of immigrants eight years prior does not correlate with current, unmeasured determinants of the demand for nurses. We could increase the plausibility of this assumption by choosing an immigrant share from even longer back in time. But that would introduce problems for the second assumption, that foreign born nurses decide where to live based on previous immigration to an area. Of course, choosing an immigrant share closer in time improves this assumption, but that poses problems for the first assumption about the exogeneity of the instrument. In the end, we believe eight years prior to be a good compromise. Nonetheless, we examine these two assumptions as well as our choice of instrumental variable in the results section below. All the previous specifications are estimated using our instrumental variable, giving rise to the following specifications:

(4) 2SLS, Basic model

$$\log(Income) = \beta_0 + \beta_1 ForeignShare_{tj} + \beta_2 Female_{it} + \beta_3 Age_{it} + \beta_4 Age_{it}^2 + \beta_5 SPecialist_{it} + \beta_6 Year + \varepsilon_{it}$$

(5) 2SLS with control municipal characteristics

$$\log(Income) = \beta_0 + \beta_1 ForeignShare_{tj} + \beta_2 Female_{it} + \beta_3 Age_{it} + \beta_4 Age_{it}^2 + \beta_5 Specialist_{it} + \beta_6 Year + \beta_7 MuncipalType + \varepsilon_{it}$$

(6) 2SLS with regional fixed effects

$$\log(Income) = \beta_0 + \beta_1 ForeignShare_{tj} + \beta_2 Female_{it} + \beta_3 Age_{it} + \beta_4 Age_{it}^2 + \beta_5 Specialist_{it} + \beta_6 Year + a_j + \varepsilon_{it}$$

An additional problem regards our assumption of closed local labor markets. If this assumption does not hold we could find that difference in wages disappear due to adjustment from internal migration and trade. While it is obvious that local labor markets are not perfectly closed, the

question still remains whether this is a plausible approximation. As a first indication, the Swedish National Board of Health and Welfare reports that most nurses and midwifes live and work in the same county or region in which they studied (National Board of Health and Welfare 2005, s. 42). Nonetheless, we will attempt to test this.

It is important to be aware of the risk for over controlling, which might occur while trying to avoid biases and determining explanatory variables. Hence, to avoid multicollinearity only independent variables which affect income and are uncorrelated with all of the other independent variables should ideally be used. This becomes even more important when using instrumental variables. As a precautionary measure, we test for multicollinearity among our independent variables below.

6. Results and Analysis

In this section we will present and analyze the results from running our regressions. We start by testing for multicollinearity among our independent variables. Next, we present the results from the OLS specifications (1)-(3), followed by the results from the 2SLS specifications (4)-(6). Note that as we are working with full population data, any inference is made without sampling error.

Table V. Pairwise Correlations between Independent Variables

	Age	Age^2	Female	Foreign share	Specialist
Age	1.0000				
Age^2	0.9916	1.0000			
Female	0.0564	0.0603	1.0000		
Foreign share	-0.0503	-0.0455	0.0189	1.0000	
Specialist	0.1770	0.1723	0.0028	0.0012	1.0000

Looking at the above results, we can conclude that our main independent variables do not suffer from multicollinearity. Of course, there is an expected high correlation between age and age-squared. But given our reason for using both, this is not a problem.

Ordinary Least Squares Results

TABLE VI. ORDINARY LEAST SQUARE RESULTS

Independent variable	(1) OLS Basic Model	(2) OLS Mun. Charecteristics	(3) OLS Regional fixed effects
Foreign share	0.57866***	0.37737***	-0.78831
	(0.07458)	(0.07626)	(0.51951)
Age	0.01710***	0.01756***	0.01721***
	(0.00100)	(0.00091)	(0.00098)
Age^2	-0.00013***	-0.00014***	-0.00013***
	(0.00001)	(0.00001)	(0.00001)
Female	-0.16040***	-0.15940***	-0.16110***
	(0.00418)	(0.00421)	(0.00413)
Specialist	0.01598***	0.01590***	0.01591***
	(0.00464)	(0.00484)	(0.00462)
Intercept	11.47865***	11.46579***	11.57621***
	(0.02378)	(0.02313)	(0.04531)
Urban	-	0.05501***	-
		(0.00848)	
Suburban	-	0.01739**	-
		(0.00874)	
Large City	-	0.00078	-
		(0.00711)	
Medium City	-	0.00958	-
		(0.00764)	
Industrial	-	-0.00558	-
		(0.00649)	
Rural	-	-0.01241	-
		(0.01238)	
Sparsely Populated	-	0.05668***	-
		(0.00929)	
Other Large City	-	0.00846	-
		(0.00776)	

**** = significant at 1% level; ** = significant at 5% level; * = significant at 10% level*
Note: Coefficients for year fixed effects are presented in appendix C.
Standard errors adjusted for clustering at regional level

Table I presents the results from the first three specifications of our model. Note that we only have one observation of our variable of interest, *foreign share*, for each region and year. Accordingly, we have adjusted our standard errors through clustering to reflect this. Further, to control for heteroskedasticity, the standard errors estimated are robust.

The basic model (1) shows a significant but positive relation between the share of foreign nurses in a region and the income level of native nurses. The coefficient implies that a one percentage point increase in the share of foreign nurses in a county will increase the income of native nurses by 0.58 percent in the same county. All control variables, including the year dummies, have the expected sign and magnitude.

However, regression (1) probably suffers from omitted variable bias. We could for example imagine that different labor market structures in different regions affect wages, or that some regions had trade unions that were better negotiators. Consider the fact that the major metropolitan areas in Sweden generally have *both* the highest immigrant share *and* the highest wages. This would in a simple OLS show up as a positive correlation between immigrant share and wages. But it is difficult to argue that the correlation actually shows a causal connection. Immigration may affect wages, but there are other factors that have a much greater effect (as an obvious example, more native nurses equals more supply). Additionally, the coefficient would be positive, going against the theoretical predictions. A more probable situation is that we have omitted variables that affect the economic conditions in regions.

The results from our first attempt to alleviate this problem, specification (2), are similarly presented above. Following our expectations, the coefficient for foreign share is now lower. Still, it is positive and significant. The results imply that a one percentage point increase in the share of foreign born nurses in a region increases income for native nurses by 0.38 percent.

Controlling for municipal characteristics might remove time consistent factors that are common to specific municipal types. But it is easy to imagine that there can be other omitted variables that are not shared between municipalities of the same type. As an example, the ability of the local trade union to negotiate is not necessarily consistent over a municipality type. Consequently, we can still have omitted variable bias.

We thus move on to regression (3)[9], where the coefficient for foreign share is now negative. The results now imply that a one percentage point increase in the foreign share of nurses in a region would *decrease* income for native nurses in that region by 0.8 percent. If our expectations regarding omitted variable bias and a negative wage effect were true, this is exactly the kind of change we would expect. Referring back to our reason for using fixed effects, we have now removed possible bias resulting from the omission of time invariant variables. For example, Stockholm could have had higher wages due to persistently higher living costs, a factor reasonably independent of immigration of nurses. As Stockholm also had a high share of foreign nurses, our results from regressions (1) and (2) (which also uses the cross sectional variation) could contain a spurious correlation from something like that. The end result would be a biased estimation. Problems of this kind is what fixed effects estimation helps to solve, in that it only uses variation within an observation (in our case region) over time.

[9] Performing a Hausman test gives clear evidence that fixed effects are more efficient than random effects.

However, the relation is not statistically significant at conventional levels (p-value of 0.143). Seeing as how fixed effects estimation neglects the variation between regions and that this is where most of the variation is, the loss of significance follows naturally. But the result is that we are unable to conclude anything definite using this specification. The correlation found could be the true wage effect, but just as likely a spurious correlation.

As previously noted, fixed effects estimation gives information on short-run effects. Expectation from theory is that the short-run effect should be negative, with only the long-run effect having the potential to be zero. However, this assumes a perfect market where wages can decrease. Seeing as how nominal wages almost never decrease in Sweden, actually finding a negative short-run effect would have been out of line with the facts (Agell and Bennmarker 2007).

Furthermore, it should be remembered that our results could still suffer from the omission of variables which vary over time. Variables that vary with time but affect the whole country will be picked up by the year dummies. But local demand shocks and the like could still introduce bias. Consider a temporary increase in healthcare demand in a region, leading to increased wages for nurses. If the region experienced immigration at the same time, even the fixed effects regression would be biased.

As we have previously noted, regressions (1)-(3) potentially suffer from endogeneity. It is unrealistic to assume that the decision on where to settle in Sweden is always independent of any economic conditions in that area. Indeed, high wages in an area might be just the reason for settling in that area. In an attempt to alleviate this problem, we use an instrumental variable approach.

Instrumental Variable Results

TABLE VII. INSTRUMENTAL VARIABLE RESULTS

Independent variable	(1) 2SLS Basic Model	(2) 2SLS Mun. Charecteristics	(3) 2SLS Regional fixed effects
Foreign share	0.56844***	0.33696***	-4.78988***
	(0.07143)	(0.08101)	(0.67785)
Age	0.01709***	0.01754***	0.01726***
	(0.00096)	(0.00088)	(0.00026)
Age^2	-0.00013***	-0.00014***	-0.00014***
	(0.00001)	(0.00001)	(0.000003)
Female	-0.16037***	-0.15939***	-0.16112***
	(0.00405)	(0.00410)	(0.00110)
Specialist	0.01598***	0.01592***	0.01589***
	(0.00454)	(0.00474)	(0.00064)
Intercept	11.47962***	11.4684***	11.86533***
	(0.02153)	(0.02170)	(0.04929)
Urban	-	0.05716***	-
		(0.00818)	
Suburban	-	0.01991**	-
		(0.00863)	
Large City	-	0.00794	-
		(0.00681)	
Medium City	-	0.00997	-
		(0.00761)	
Industrial	-	-0.00549	-
		(0.00631)	
Rural	-	-0.01244	-
		(0.01189)	
Sparsely Populated	-	0.05606***	-
		(0.00899)	
Other Large City	-	0.00892	-
		(0.00767)	

**** = significant at 1% level; ** = significant at 5% level; * = significant at 10% level*
Note: Coefficients for year fixed effects are presented in appendix C.
Standard errors adjusted for clustering at regional level

We start by noting that our instrument is statistically significant in the first stage for all three cases. Regression (4) is the same as regression (1) with the addition of our instrumental variable. The coefficient for foreign share is practically the same at 0.57, differing by only 0.01, compared to regression (1). If our previous foreign share variable really was endogenous in the way thought, we should expect to find a lower or negative effect. Either the foreign share variable never was endogenous in the first place, or the instrument is not working. We will return to potential problems with the instrument later.

Seeing as the same omitted variable problem we had before is relevant here as well, we estimate a second 2SLS regression, (4), with dummies for municipal characteristics. While the estimated foreign share coefficient moves in the expected direction, it is nonetheless nearly the same as regression (2). It now implies that a one percentage point increase in the share of foreign nurses increases income for native nurses by 0.34 percent. Again, we can either conclude that our foreign share variable never was endogenous or that the instrument is not working.

Moving on to regression (6), the coefficient for foreign share has moved in the expected direction, in that it is more negative than both regression (5) and (3). However, the magnitude of the coefficient points toward a problem with this specification of our model. While a more negative effect is expected in this specification, the size of it is, at face value, unrealistic and completely out of line with previous research.[10] It now implies that a one percentage point increase in the foreign share of nurses should lead to a 4.8 percent decrease in the income of native nurses. Consider the fact that many regions only have between 1000 and 2000 nurses. The results would thus mean that the arrival of about 20 foreign nurses could reduce the wage for a native nurse with a monthly wage of 20 000 SEK by nearly 1000 SEK. Comparing the two fixed effects specifications, we can conclude that the problem does not seem to be in the use of fixed effects itself. The results from (3) are reasonable and in line with previous research. The only difference between (3) and (6) is the use of the instrument. Accordingly, we now examine the instrument.

TABLE VIII. FIXED EFFECTS ESTIMATION WITH DIFFERENT INSTRUMENTS

	10 year	8 year	4 year
Foreign share	-20.46192***	-4.78988***	-0.00833
	(3.07455)	(0.67785)	(0.50571)

**** = significant at 1% level; ** = significant at 5% level; * = significant at 10% level. Standard errors adjusted for clustering at regional level*

Suspecting that there might be something wrong with our initial instrument, we re-estimate specification (6) with two different versions of the instrument: immigrant share 4 years prior and 10 years prior. As we have discussed in our method section, choosing an immigrant share that is too old could decrease its relevance. On the contrary, choosing an immigrant share that is too recent could decrease its exogeneity. The '10 year' estimate produces an equally unrealistic estimate, while the '4 year' estimate is not far from zero. Nothing in the theory behind the instrument explains why the estimate should be so sensitive to the choice of years. Indeed, ethnic

[10] Compare for example Kaestner and Kaushal (2012) which finds a wage decrease of 1-4% from a 10% increase in the supply of nurses. Our estimate is thus at the least ten times higher.

enclaves to not do change place every few years. Given this, we turn to discussing other potential weaknesses in the instrument.

Even putting the above results aside, our instrumental variable is far from perfect. There is one specific flaw which should be made explicit, a flaw which is the result of data constraints. The theory behind the instrument is built on the idea that immigrants tend to settle where previous immigrants of the same origin have settled. A perfect instrument of this type would match previous immigrants from a specific country with new immigrants from the same country. But, with the data we have available, this is not possible. What we have is information on the total foreign population in a county, with no information on their country of origin. As a result, our instrumental variable approach implicitly assumes that immigrants from *any country* tend to settle where there is a high share of immigrants from *any country*. This is obviously a great simplification, and something that theory does not necessarily support.

It is a common phenomenon to find that an instrumental variable approach increases the coefficients. Given the shaky theoretical foundation of our instrumental variable combined with the unrealistic results produced, things points toward the instrument being flawed. Adding the fact the instrument failed to make the coefficients in regressions (4)-(5) any lower (indeed, it barely changed them at all), we believe that we have only uncovered spurious correlations in specification (6). While the instrument had promise *ex ante*, we will now have to reject the results it produced.

Importance of Results

So how important are these results? A telling fact is that the R-squared of a regression of log income on only foreign share is less than one percent. The implication is that differences in the foreign share of nurses between different counties and over time have barely any explanatory power at all when it comes to income. We further test different specifications of our model by starting off with the simple bivariate regression above and then sequentially adding our variables. The resulting R-squared are presented below.

TABLE IX. R-SQUARED FROM DIFFERENT SPECIFICATIONS

	Share foreign born	**+Year fixed effects**	**+Individual controls**	**+Regional fixed effects**
R-squared	0.0078	0.2790	0.3469	0.3520
	Share foreign born	**+Regional fixed effects**	**+Individual controls**	**+Year fixed effects**
R-squared	0.0078	0.1358	0.2304	0.3520

As can be seen, it is the year fixed effects and the individual controls which have the most explanatory power. This can be compared to the results from Korpi (2009) in which year fixed effects barely contributed anything to the total explained variation. In our case, that the year dummies have that much explanatory power can be interpreted as wage setting being very centralized. Given that wage setting was only recently moved to the individual level in our data (1989 to be exact), it is not difficult to imagine that wages continued to be set at a centralized level. What supports this conclusion is the generally even increase in income each year (see appendix for coefficients for the year dummies).

Potential Problems

Closed Labor Market

The most important assumption underlying our model is that counties can be considered separate closed labor markets. We gave a first indication of this in method section, where we reported that the majority of nurses tended to stay and work in the same place as they got their education. But still, it is prudent to test how much nurses actually move around in Sweden.

TABLE X. MOVES BETWEEN COUNTIES 1989-2000

Total	6506
Average per year	542

The table above presents how many observations of a move to a new county we have during the period 1989-2000. As can be seen, the total amount is very small. While this is no proof that counties can be considered closed labor markets, it still gives a strong indication that our assumption is reasonable.

Part-time Workers

A potential pitfall for our results is the potential, and probable, inclusion of marginal workers in our dataset. While it is doubtful that our definition of nurses instead captures a student, it is very probable that we have part-time workers in our dataset. This becomes a problem because we only have information on the actual income for different people, with no information on how many hours they work. With perfect data, we could compute full-time equivalent wages for all our observations. But now, all we can do is to remove those earning too little to reasonably work full time. Nonetheless, we have no way of removing those with a high full time equivalent wage working part-time. Our model considers these as full-time nurses with a comparably low wage.

Going further, we add another dimension to the part-time problem when basing our model on comparing different regions. Consider the fact that the general wage level can differ between different regions. Our solution, to remove those with a too low wage, will solve most of the problem in regions with low wages, but few part-time workers will be removed in regions with high wages. This introduces a bias when comparing region in that the part-time workers will reduce average wages in high wage areas.

As of now, we have discussed the part-time problem as a static phenomenon: either you work full-time all the time or you don't. But workers can naturally change from being a full-time worker to a part-time worker and vice versa. Our model would view a decrease in working hours with the same hourly wage as a wage decrease (and vice versa for moving from part-time to full-time). A negative effect on nursing wages from immigration could actually be showing how nurses choose to work fewer hours. While we see no obvious reason why that should happen, the risk for our model is still there. Moreover, if immigration caused lower wages, some nurses might not find it worthwhile to work full-time. This would bias our estimates and show up as an even more negative effect. Conversely, if wages increased, some nurses might keep their income constant by choosing to work less.

It is not obvious in which direction this would bias our result. Several situations are possible, some of which we have already discussed. That we are likely removing more part-time workers in regions with low wages, and that these tend to have lower share of foreign born nurses, will for example bias our results negatively.

Centralized Wage Setting

We have previously stated that individual wage setting has been an explicit goal of all collective agreements during the period 1989 to 2000. But as we alluded to in the above section, it is not unrealistic to imagine that the policy did not immediately succeed. That the time fixed effects were able to explain such a large amount of the variation, combined with the change to individual wage setting being so recent, points toward wage setting not being completely individual.

Now, what implications does this have for our results? Our empirical model would not work if wages were set at the national level. In that case, comparing regions would yield no information at all. But if wages were set equally for all nurses in a specific region, with the wage level being based on the factors unique to that region, our approach would still work. Key to our empirical model is not variation within a region at a specific point in time, but variation between regions and over time.

Given that there is variation in wages between regions, we can plausibly reject the idea that wage setting still is done at the national level. Supporting this conclusion is another goal stated in the collective agreement during this period: that wages should reflect conditions unique to a region.

7. Conclusion

Our research question asked whether an increased share of foreign born nurses in a region negatively affected income for native nurses in the same region. Basing our method on the area correlation approach, we have not found any evidence that an increased share of foreign nurses is related to a lower income for native nurses. Our results from using basic OLS indicate a significant and positive effect on income from increased immigration. We only manage to produce negative estimates when using regional fixed effects, but these are far from statistically significant. We do not know whether this is due to there not being a negative wage effect, or whether the Swedish economy has adjusted through other mechanisms such as technology change.

Of course, it is not possible to generalize these results for all of immigration. Many people work in the tradable sector and most occupations are not protected by stringent licensing requirements. But it is telling that even when we removed the possibility for adjustment through trade and the risk of looking at a too heterogeneous group, no conclusive evidence for a negative income effect could be found.

However, while we conclude the above, our results should still be interpreted with caution. We did not succeed in controlling for immigrants specifically settling in high wage areas, implying that our results could be positively biased. Nor were we able to control for hours worked, which could bias our results as well, although it is not obvious in which direction. Our choice of using counties as local labor markets might also have been mistaken. While our results indicate that they are large enough for the closed labor market assumption to hold, they might still be too large for any income effect to show up.

Finally, our approach did not explicitly control for other immigration. There is a possibility that the immigration of nurses were never able to change nurse's share of the labor force, perhaps because there was simultaneous immigration of other occupations. The majority of previous research has neglected this confounder as well, but it might be time for it to be properly examined.

Issues of sign and magnitude of the effect aside, for the specific case of nurses, immigration has not been an important determinant of income. The total variation explained by the share of foreign born nurses in a region does not even amount to one percent. In contrast, our control variables manage to explain about a third of the variation.

Comparing our results to other research, they largely corroborate those achieved by Kalist et al. (2010) and Schumacher (2011), two other studies that look at nurses. Both of these fail to find any negative wage effects using the area correlation approach. However, not all previous research produces results along these lines, with one specific study finding evidence that contradicts our results. Using the area correlation approach, Keastner et al. (2012) estimate a negative wage effect from the immigration of nurses in the US. Be that as it may, they are still alone in reaching these results for nurses with the aforementioned method.

Research looking at immigration and the total workforce similarly tends to find results that mirror ours. While results from these studies are in no way uniform, a sizeable majority still fails

to find any negative wage effect from immigration. To sum up, we conclude that our results add to the already sizeable amount of research casting doubt on the existence of any negative wage effect. But as always, more research is needed before any definite conclusion can be drawn.

Suggestions for Further Research

There are still many problems remaining, especially of the methodological kind, in trying to estimate a wage effect from immigration. Most of these problems are already discussed in the literature, making it unnecessary for us to repeat them here. We wish however to propose two specific venues for further research that may be promising.

The first concerns the possible endogeneity resulting from immigrants moving to a country in response to favorable economic conditions there. The instrument used in this study to solve the problem proved to be inadequate, but the idea behind the instrument still has potential. Before we reject its usefulness outright, we propose using more detailed data to create an instrument that can match current immigrants to previous immigrants of the same origin.

The reason for looking at a specific occupation still holds. Using too heterogeneous groups will not necessarily give a fair estimate, as parts of the group may be affected differently. But studies looking at specific occupations have not been good at incorporating the effect other immigration may have. Wage effects are a result of the occupation's share of the labor force changing, something that is affected by the number of other immigrants. We argue that further studies should take this effect into account when analyzing a single occupation.

A last suggestion for those specifically interested in researching at nurses: when some counties are starting to privatize healthcare, the number of employers will increase. This can be expected to assymetrically affect the country, with some counties still having few employers. If future research is to compare regional wages of nurses, we propose controlling for the number of employers, for example with a Herfindahl index.

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Appendix A

The following steps are required before a foreign educated nurse from outside the EEA and Switzerland can practice nursing in Sweden:

1. An assessment determining whether the foreign education meets the Swedish requirements for knowledge.
2. A test of the applicant's proficiency in Swedish.
3. A test on general theoretical knowledge, comparable to what one learns in a nursing education.
4. A test of general knowledge about Swedish society and current legislation affecting nursing.
5. Two weeks of hospitering, i.e. observing professional nurses working.
6. Practical service for a period of about three to five months, depending on previous experience.
7. Submitting the final application form.

Appendix B

SNI92 and SUN (pre-2000) codes used to define nurses working in healthcare:

SNI92					
85110	85120	85140	85311	85312	85313

SUN (pre-2000)					
65000	65030	65030	65100	65101	65102
65182	65183	65184	65710	65713	65714
65715	65740	65741	65742	65743	65744
65108*	65109*	65110*	65111*	65118*	65119*
65120*	65121*	65128*	65129*	65130*	65131*
65138*	65139*	65140*	65141*	65178*	65179*
65180*	65181*	65188*	65199*	66300*	66308*
66309*	66610*	66611*	66612*	66613*	66614*
66615*	66616*	66618*	66619*	65745*	

* Specialist nurse eduation

Appendix C

TABLE CI. ORDINARY LEAST SQUARE RESULTS

Independent variable	(1) OLS Basic Model	(2) OLS Mun. Charecteristics	(3) OLS Regional fixed effects
Year 1990	0.06525***	0.06709***	0.07523***
	(0.00181)	(0.00178)	(0.00503)
Year 1991	0.10096***	0.10256***	0.10865***
	(0.00212)	(0.00189)	(0.00395)
Year 1992	0.13670***	0.13781***	0.14042***
	(0.00431)	(0.00397)	(0.00331)
Year 1993	0.14416***	0.14513***	0.14698***
	(0.00398)	(0.00365)	(0.00305)
Year 1994	0.17087***	0.17183***	0.17294***
	(0.00538)	(0.00501)	(0.00444)
Year 1995	0.18599***	0.18671***	0.18627***
	(0.00496)	(0.00463)	(0.00400)
Year 1996	0.24900***	0.24950***	0.24907***
	(0.00701)	(0.00666)	(0.00567)
Year 1997	0.30087***	0.30098***	0.29865***
	(0.00630)	(0.00599)	(0.00496)
Year 1998	0.33503***	0.33497***	0.33158***
	(0.00867)	(0.00841)	(0.00735)
Year 1999	0.40302***	0.40300***	0.39937***
	(0.00964)	(0.00937)	(0.00836)
Year 2000	0.45216***	0.45196***	0.44686***
	(0.00937)	(0.00906)	(0.00825)

**** = significant at 1% level; ** = significant at 5% level; * = significant at 10% level*

TABLE C2. INSTRUMENTAL VARIABLE RESULTS

Independent variable	(1) 2SLS Basic Model	(2) 2SLS Mun. Charecteristics	(3) 2SLS Regional fixed effects
Year 1990	0.06532***	0.06739***	0.10430***
	(0.00178)	(0.00173)	(0.00520)
Year 1991	0.10101***	0.10278***	0.13059***
	(0.00207)	(0.00183)	(0.00406)
Year 1992	0.13672***	0.13791***	0.15088***
	(0.00421)	(0.00385)	(0.00240)
Year 1993	0.14417***	0.14521***	0.15511***
	(0.00388)	(0.00355)	(0.00212)
Year 1994	0.17087***	0.17188***	0.17986***
	(0.00524)	(0.00486)	(0.00197)
Year 1995	0.18598***	0.18671***	0.18799***
	(0.00482)	(0.00449)	(0.00161)
Year 1996	0.24899***	0.24948***	0.24956***
	(0.00682)	(0.00645)	(0.00158)
Year 1997	0.30085***	0.30092***	0.29317***
	(0.00611)	(0.00579)	(0.00184)
Year 1998	0.33500***	0.33487***	0.32220***
	(0.00844)	(0.00817)	(0.00225)
Year 1999	0.40299***	0.40288***	0.38843***
	(0.00939)	(0.00914)	(0.00244)
Year 2000	0.45211***	0.45179***	0.43104***
	(0.00912)	(0.00884)	(0.00312)

**** = significant at 1% level; ** = significant at 5% level; * = significant at 10% level*