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# The Role of Migration in Regional Wage Convergence: Evidence from Sweden 1860–1940

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*ABSTRACT: Sweden experienced a decline in inter-county real wage differentials for agricultural workers between 1860 and 1940, historical evidence of early labor market integration well before widespread unionization in agriculture occurred. By means of dynamic panel data analysis, this paper examines whether internal and external migrations caused real wage beta convergence across Swedish counties. To account for statistical problems such as endogeneity of migration, time-invariant county characteristics and autocorrelation in the regression model, we adjust our estimates using fixed effects, instrumental variables and GMM. The preferred model shows that both internal and external migrations contributed to wage convergence before the First World War and internal migration mainly during the interwar years. The agglomeration effects of urbanization were not sufficiently pervasive to offset the labor supply effects of internal and external migrations.*

## 1. Introduction<sup>1</sup>

Migration is one important option for people who want to improve their economic prospects and overall quality of life. Generally, migration streams tend to seek out prosperous areas, although the evidence is not clear-cut. The actual stream of people from poor to rich countries is a great deal smaller than economic theory predicts (Hatton and Williamson, 2005, ch. 12). Internal migration and migration between neighbouring countries probably outweigh long-distance migration from rich to poor areas, but we tend to direct the searchlight towards conspicuous cases of long-distance migration. For instance, emigration from the Old World to America was massive in the late nineteenth and the early twentieth century, driven by a large trans-Atlantic wage gap, and has been the subject of endless studies. Many potential emigrants, though, preferred an internal move to emigration to the New World, the large transatlantic wage gap notwithstanding.

An important question is how migration flows, both internal and external, affect the spatial distribution of a country's wages and incomes. Theoretical models produce conflicting results. The first group of models stresses the labour supply effect, arguing that migration from low- to high-wage areas spurs convergence by altering the supply of workers relative to other factors of production (Barro and Sala-i-Martin, 1991). Labour mobility leads to convergence rates that exceed those in the closed-economy framework in which only the capital-to-worker ratios determine the convergence rates (Solow, 1956). The second predicts that migration drives spatial divergence; for example, models in new economic geography (NEG) emphasize the effects of agglomeration economies in densely populated areas and predict that the qualities migrants carry, such as young age, skills and entrepreneurship, have growth-enhancing effects in the receiving regions (Kanbur and Rappaport, 2005; Krugman, 1991; Rappaport, 2005). Ultimately, the role of migration in the spatial distribution of incomes and wages is an empirical question.

During the last decades, there has been a surge in econometric studies that assess the role of migration in wage or income convergence,<sup>2</sup> most of which focus on the post-war period. Ozgen et al. (2010) survey the recent literature and conclude that migration has negligible effects on income and wage convergence. This result lends some support to the models that predict migration to entail agglomeration effects and alter the composition of human capital rather than just giving rise to wage convergence.

For historical periods, studies show conflicting results regarding the effects of migration on wage convergence. While Taylor and Williamson (1997) find that migration accounted for a sizeable proportion of the observed wage convergence between the Old and the New World in 1870–1910, several studies of internal migration and wage convergence show instead that the impact of migration on wage convergence within countries was limited (Boyer, 1997; Rosés and Sánchez-Alonso, 2004; Silvestre, 2005).

This paper analyses the role of migration between 1860 and 1940 in Sweden, a country that ranked second in terms of per capita emigration rates in the late nineteenth and the early twentieth century. In addition, Sweden hosted large internal migration rates. We draw on a recently compiled data set on real regional wages and emigration and internal migration rates by county (Hofsten and Lundström, 1976) to examine the link between migration and

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<sup>2</sup> When surveying the literature on migration and its role in convergence, we refer to studies that use GDP per capita, GDP per worker or wages as their dependent variable.

regional real wage convergence. We provide a counterfactual analysis of the wage effects and estimate the effects on  $\beta$ -convergence by means of dynamic panel data analysis. To account for statistical problems such as endogeneity of migration, time-invariant county characteristics and autocorrelation in the regression model, we adjust our estimates using fixed effects, instrumental variables and GMM.

The paper contributes to the debates about migration effects on regional wage convergence in three ways. Firstly, we examine the role of migration in wage convergence when labour markets were unrestricted, human capital was relatively evenly distributed and the agglomeration effects from industrialization were still modest. Compared with many recent studies, such as those surveyed by Ozgen et al. (2010), we expect that the labour supply effect played a larger role in this historical setting than in the post-Second World War period. Secondly, we examine simultaneously the effects of emigration and internal migration on wage convergence. The effects of emigration on the regional distribution of wages in the sending country have received relatively little attention. One exception, however, is Gomellini and O'Grada's (2011, p. 23) recent study, which finds that emigration pushed regional convergence by contracting the north-south income gap in Italy between 1905 and 1970. Thirdly, we address the methodological problem that arises because migration determines wages endogenously. We show that conventional OLS estimates fail to capture the true magnitude of the effects migration exercised on regional wage convergence. Our alternative GMM estimates instead show that the regional wage convergence attributable to migration was large, larger in fact than comparable historical studies for the late nineteenth and the early twentieth century show (Boyer, 1997; Rosés and Sánchez-Alonso, 2004; Silvestre, 2005). Migration accounted for more than half of the estimated  $\beta$ -convergence rates. We also find that the wages in Stockholm would have been up to 30% higher per decade in the absence of migration while the wages in the poorer counties, where emigration took place on a large scale, would have been about 30% lower for the late-nineteenth-century decades. The effects of migration on wages were substantial before the First World War, when the largest migration streams took place. The effects of emigration petered out in the interwar period.

We propose two explanations for migration's large impact on regional wages. (i) High mobility of migrants. The Swedish labour market appears to have been quite dynamic, with vast streams of net migration.<sup>3</sup> (ii) Small agglomeration effects. We argue that the Swedish agglomeration effects from urbanization were still too small to offset the labour supply effects on wage convergence since industrialization in Sweden was mainly a rural phenomenon until the early twentieth century (Berger et al., 2012; Söderberg, 1985). This explains why the agglomeration forces in Sweden, unlike those in Britain (Boyer and Hatton, 1997), did not offset the labour supply effects of migration on the convergence of regional wages.

## 2. The relationship between migration and wage convergence: A review

Standard economic theory predicts that migration should work to even out the differences in factor prices among countries and lead to convergence in wages and GDP per capita. In the Solow (1956) convergence model, diminishing returns to capital imply that regions with low capital intensity will grow faster than regions with high capital intensity, which will lead to convergence. Migration will add to convergence by increasing the capital intensity in sending

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<sup>3</sup> Compared with, for example, Southern Europe, where a low level of industrialization and income barriers to migration hampered labour mobility (Rosés and Sánchez-Alonso, 2004; Sánchez-Alonso, 2000; Silvestre, 2005).

regions and decreasing it in receiving regions (Barro and Sala-i-Martin, 1991). When technologies are spread across the globe, migration should even out the differences in capital per worker, wages and incomes. Two major channels can serve to modify this hypothesis. Firstly, migration increases the population density in the receiving regions and may propel the potential agglomeration effects in receiving regions. The recent concentration of population in particularly dynamic metropolitan cities coincides with increasing regional disparities and suggests that agglomeration forces could be a prime driver of regional divergence (Fujita and Thisse, 2002). Secondly, selective migration may alter the distribution of human capital in sending and receiving regions (cf. Borjas, 1987; Cohen and Haberfeld, 2007; Feliciano, 2005). If those migrants who leave on average possess more human capital than those who stay, a brain-drain scenario may occur, damaging the growth prospect of the sending regions and enhancing it in the receiving regions. In such a scenario, migration drives divergence.<sup>4</sup>

Since the theoretical predictions about the role of migration in wage convergence are ambiguous, the issue needs to be resolved empirically. The methodological strategy to determine these effects thus becomes crucial. In the empirical literature, we may discern two main methodological strands.

The first one addresses the issue using partial equilibrium analysis and counterfactual statements. For example, Taylor and Williamson (1997) analyse the role of migration from the Old to the New World between 1870 and 1910 by comparing the actual convergence in wages with a simulated model without net migration. They find that migration accounts for a sizeable proportion of the observed wage convergence in 1870–1910. Swedish wages would have been 8% lower while American wages would have been 9% higher in the absence of migration (Taylor and Williamson, 1997, p. 40). In a similar vein, O'Rourke and Williamson (1995) estimate that a sixth of the increase in Swedish urban unskilled wages was due to migration, and Boyer et al. (1994) conclude that the real wage levels in Ireland would only have been 81% of the actual levels if there had been no emigration between 1851 and 1911.

The other strand of the literature uses econometric models that identify the amount of so-called  $\beta$ -convergence. These econometric models typically estimate unconditional convergence regressions and compare the estimates with regressions that control for migration. However, since the decision to migrate could partly be determined by the relative wage levels, the migration variable counts as endogenous in econometric studies. Barro and Sala-i-Martin (1991, 1992, 2004) provide a detailed description of this phenomenon in the context of the neo-classical growth model. They argue that the estimated  $\beta$ -convergence should become smaller when migration enters and the issue of potential endogeneity is properly taken into account. However, when Barro and Sala-i-Martin (2004, table 11.7) follow this strategy, they find limited effects of migration on regional convergence in the USA, Japan and five European countries between 1950 and 1990. After controlling for endogeneity of net migration with instrumental variables, they still cannot find any effect of internal migration that is statistically significant. Rosés and Sánchez-Alonso (2004) use a similar model in a study of Spanish provinces from 1850 to 1930. They show that although wage convergence was significant, migration did not play an important role in explaining it. Instead, Rosés and Sánchez-Alonso (2004) suggest that the limited extent of internal migration before the 1920s explains why the model does not identify migration as an important driver of regional convergence.

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<sup>4</sup> Migration also alters the age distribution of the sending and receiving regions. In the receiving regions, the share of the population of working age will increase and it will decrease in the sending regions. If migration moves from low- to high-income regions, it will lead to divergence in the GDP and GDP per capita.

Ozgen et al. (2010) review the econometric literature that examines the role of migration in internal wage convergence after the Second World War by selecting 12 studies with 67 observations from different countries. Their conclusion is that the overall effect of net migration on growth in real per capita income is positive, but small. A 1 percentage point increase in migration boosts the rate of growth in per capita income by about 0.1 percentage points.

The small effect of migration on convergence contrasts with what the standard neo-classical model forecasts, and indicates that economies of scale and agglomeration effects counteract the labour supply effects of migration. Ozgen et al. (2010) also highlight the importance of using econometric techniques to overcome the two-way causality problem in the relationship between migration and growth. They additionally stress the need to include time dummies and fixed effects to circumvent the problems of correlated economic shocks and regional heterogeneity.

### 3. Swedish wage convergence and regional integration: A background

Swedish wage convergence between 1860 and 1940 was rapid in international comparison. Figure 1 visualizes  $\sigma$ -convergence (coefficient of variation), and figure 2 shows  $\beta$ -convergence. Since the major share of convergence took place before the First World War, when there was virtually no unionization of labour, we look for market-driven factors to explain the contraction of inter-regional wage differentials.<sup>5</sup> Labour mobility, by shifting the relative supply of labour in sending and receiving regions, may explain the Swedish regional convergence. Hence, what do we know about migration rates?

Figure 1 about here

Figure 2 about here

Sweden also appears to have had rather large internal migration rates in the first half of the nineteenth century. Most of the migration was circular and short-distance, though. Movers were typically unmarried young people, servants in agriculture, frequently moving from one farm to another within the local labour markets (Dribe and Lundh, 2005; Lundh, 1999). Larger farms and manors in agriculture also employed married servants, who were quite mobile, and during harvest seasons migrant workers from distant areas were employed on a temporary basis (Eriksson and Rogers, 1978; Lundh and Olsson, 2011). Even though gross migration was considerable, the circular character of migration served to limit net migration. However, until the mid-nineteenth century, long-distance migration was more restricted by institutional barriers. The gradual disappearance of patriarchal laws and customs circumscribing labour mobility, in particular in the agricultural sector, contributed greatly to the formation of a modern labour market (Lundh, 2010, pp. 50–53).

Still, the costs of long-distance migration were substantial inasmuch as the information and transportation networks were still underdeveloped by the mid-nineteenth century. Before the arrival of railways in the late 1850s, a trip between Stockholm (the capital city in the east) and Gothenburg (the second-largest city on the west coast) was a week-long journey that involved several changes of transport mode (Sjöberg, 1956). Estimates show that the railways significantly lowered the transportation costs and accounted for 50% of urban growth in 1855–1870 (Berger and Enflo, 2013). Following Crozet (2004), who modelled the migration decision as a trade-off between the employment probability in the host region and a migration

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<sup>5</sup> Only 7% of agricultural workers were unionized as late as 1930 (Lundh 2010, p. 99).

cost that increased with distance, we argue that in Sweden, with its large distances between north and south, the migration costs were substantial before the integration of the transportation network. At the same time, large regional imbalances in wage differentials and job opportunities existed, serving as a pull factor for migration.

Allegedly, some of the regional wage differentials could reflect differences in amenities, in particular those that affected health. Williamson (1981, p. 79) finds a substantial and positive association between the British infant mortality rate (IMR) and the regional nominal wages of unskilled construction workers in 1905, which he interprets as a wage premium for living in urban areas. In Sweden, the IMR was about 140 per mille in the 1860s and significantly higher in urban than rural areas. The IMR fell over time, though; in the 1890s it was about 100 per mille, and in the 1930s it was 46 per mille (Historisk statistik, Del 1, tab. 4, p. 115). There were in addition large regional differences in the IMR over the entire period. Compared with the rest of the country (19 counties), the IMR was on average 60% higher in Stockholm and 20% higher in the northernmost (4) counties in 1860–1940. Furthermore, while regional wages tended to converge, we find little or no convergence in regional infant mortality rates. There is, however, a general positive correlation between the regional wage levels and the regional IMR for the investigation period (the average for the panel years is about 0.3). Especially for Stockholm (before the First World War) and the northern part of Sweden, higher wage levels were associated with higher infant mortality rates. The evidence suggests that part of the earnings differentials stemmed from amenities, which supports Williamson's conclusions.

Nevertheless, migration appears a more plausible explanation for the rapidly changing wage levels and the resulting convergence. This view squares well with the finding of Söderberg (1985) that there was considerably greater mobility across and within Swedish regions compared with other European countries. He actually characterizes the Swedish evolution with large internal population movements as exceptional in a European comparison of 1840–1914 (Söderberg, 1985 p. 296).

Thus, in 1860, 7% of the members of the Swedish population resided in a county other than their birth county. Long-distance migration was important for Stockholm city, where the majority of the inhabitants were immigrants from other counties. Factors such as industrialization and urbanization made long-distance migration more important during the latter half of the nineteenth and the early twentieth century. Moreover, the establishment of railways, nation-wide telegraphs and postal services linked labour markets together, and from the end of the century newspapers facilitated the flow of information (Schön, 2010 pp. 132–3, 210–11). In 1900, 16% of the Swedish population resided outside the birth county, and in 1930 the proportion had risen to 22%. Migration streams poured from agriculture to cities and industrial centres, located in urban and rural areas (Bengtsson, 1990; Institute for Social Science, Stockholm University, 1941, pp. 58–9).

In addition to having increasing internal migration rates during the late nineteenth century, Sweden was one of the largest sending countries of emigrants to the New World in relation to its population size (Hatton and Williamson, 1998, pp. 197–8). Between 1880 and 1893, marking the era when emigration peaked, no fewer than 550,000 Swedes embarked on a trip to the New World (Bohlin and Eurenus, 2010). Schön (2010, p. 192) estimates that every twelfth Swede emigrated during the 1880s. Because 84% of the emigrants departed from Swedish towns, above all Gothenburg (67%) and to a lesser extent Malmö (12%), the mass emigration also boosted the rate of internal migration (Emigrationsutredningen, Bil. II, p. 68). Many emigrants furthermore lived in the cities for some time before embarking on their trip across the ocean.

The emigration rates varied considerably by county, though; 6 out of 24 counties hosted 44% of all emigrations between 1881 and 1910 but were home to only 28% of the total

population (Bohlin and Eurenus, 2010). Contemporaries knew that regional imbalances characterized Swedish emigration (Sundbärg, 1910). Figure 3 displays emigration rates per county during the 1870s. As seen from the figure, counties closer to the main port, Gothenburg, experienced higher emigration rates than the rest of the country.

Figure 3 about here

Despite Sweden becoming an increasingly integrated labour market during the last half of the nineteenth century, the effects of emigration and net internal migration on Swedish regional wage convergence remain an unexamined nexus.<sup>6</sup> This paper seeks to bridge this gap in the literature.

#### 4. Data

The Swedish official statistics provide most of the regional wage data that underlie the empirical part of the paper. The 24 Swedish counties correspond to the level of NUTS 3 according to the European Union classification system of spatial territories.<sup>7</sup> All the county-specific information that underlies the analytical sections of the paper either appears every tenth year (population census) or forms decadal means from 1860 to 1940. Our set of variables hence forms a panel that includes 24 cross-sectional and 9 time units.<sup>8</sup>

Our 24 regional wage series pertain to the daily wage of male day labourers in agriculture and originate from the official statistics of the agrarian sector in 1865 to 1910 and from the annual official wage statistics in 1911 to 1940.<sup>9</sup> Three arguments warrant the use of day labourers in agriculture. First, there are no regional wage series for industrial workers (or any other worker group) before 1931. Second, in contrast to farm hands and contract workers (*statare*), day labourers' employment terms resembled those of industrial and construction workers. They were paid cash wages on a day-to-day basis while benefits in kind made up a minor share of their earnings.<sup>10</sup> They were typically employed on shorter notice for seasonal work during the summer half-year, especially during production peaks, and combined work in agriculture with other employment during the winter, for instance in manufacturing industries located in the countryside. Third, day labourers were scattered throughout Sweden

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<sup>6</sup> Some previous studies investigate the role of migration in the overall agricultural wage level (Ljungberg, 1997) and the unskilled urban wages (O'Rourke and Williamson, 1995). These studies conclude that the effects of emigration on the growth in agricultural and unskilled wages were large between 1870 and 1910, ranging from one-third (Ljungberg, 1997) to one-sixth (O'Rourke and Williamson, 1995).

<sup>7</sup> In the case of Stockholm, the official statistics distinguish between Stockholm city (urban) and Stockholm county (rural). In two other cases, Elfsborg county and Kalmar county, they distinguish between "north" and "south". In order to make the data homogeneous, we merge the geographical units into 24 counties that include both urban and rural populations.

<sup>8</sup> First-differencing the data results in a panel with 24 cross-sections and 8 time units. We use decadal data because the population census from which we compute the net internal migration and net external migration by county only appeared every tenth year. Testing for panel unit roots with Levin-Lin-Shu test we reject unit roots in the wage data (with 0-2 lags) and in migration with 0 and 2 lags. Using the Im-Pesaran-Shin test we reject panel unit roots in specification with 2 lags (wage panel) and 0 lags (migration panel). Since our data appears to not contain unit roots, we did not proceed to test for panel cointegration. But our T is very small, thus we caution against a strong interpretation of the tests. Results are available from the authors upon request.

<sup>9</sup> 1865–1910: Hushållningssällskapens berättelser; 1911–28: Arbetarettillgång, arbetstid och arbetslön inom Sveriges jordbruk; 1929–40: Lönestatistisk årsbok.

<sup>10</sup> A state report on agrarian workers' living conditions in 1910 estimates the value of free housing and fuel, garden plot and pasture of day labourers as approximately 10% of the daily wage (Arbetsstatistik I, p. 45). A rural household survey in 1920 reports the in-kind part of the wage of a male day labourer as 7% (Levnadskostnaderna på landsbygden, table 3, pp. 18–19).



and accounted for about 50% of the rural working class during the period of investigation (Jungenfelt, 1959, pp. 106–8). We include data for day workers who brought their own food and did not belong to the master's household. Furthermore, the wage level of each county is an arithmetic mean of winter and summer wages, and an arithmetic mean of permanently and temporarily employed workers for sub-periods when such details are reported. In order to extrapolate the official wage statistics, which begin in 1865, back to 1860, we use Jörberg's (1972) series of wages for day labourers based on the market price scales (*markegångstaxorna*).<sup>11</sup>

As was already mentioned, we rely on agrarian wages because the official statistics do not give information on the regional wages of industrial workers until 1931. Three additional arguments justify the use of agrarian wage statistics as indicators of labour market outcomes. First, the quality of the regional series of day labourers' wages in agriculture is high; for instance, they do not suffer from inter-temporal inconsistencies in the classification of labour characteristics, which is often a feature of official wage series for manufacturing. Second, the agrarian sector was important for most of the period of investigation. In 1870, more than 70% of the population depended on agriculture for their livelihood; it was not until the 1930s that industry (mining, manufacturing and construction) exceeded agriculture in terms of manpower (Lundh, 2010, p. 236). Third, we find a clear correlation between the regional wages of day workers in agriculture and those of manufacturing workers for the period 1931–1940, during which the official wage statistics contain the regional wages of industrial workers. The correlation coefficient (Pearson) is 0.59 and the rank correlation is 0.61. Thus, there was a clear association between agrarian and industrial wage levels for this sub-period, indicating that the labour markets were quite integrated. A comparison of wages in the mechanical engineering industry and agriculture in 1898/1900 indicates that even previously there was a positive association (the correlation was 0.20–0.25).<sup>12</sup> Thus, we conclude that in general, high agricultural wages accompanied high manufacturing wages.

Another indicator of the integration of the rural and the urban labour market is the size of the urban–rural wage gap. A previous study shows that the real urban–rural wage gap was about 25% in Sweden just before the outbreak of the First World War (Lundh, 2012). The nominal ratio was quite stable between 1865 and 1914. The interwar years then brought a dramatic divergence of wages because of two macro-economic shocks in 1920–1921 and 1930–1931, both of which had asymmetric consequences for wage developments in the two labour markets. A reduction of working hours in 1919–1920 that excluded agriculture expanded the wage gap additionally. A similar expansion of the wage gap in the interwar years also occurred in other countries (Alston and Hatton, 1991; Greasley and Madsen, 2006; Hatton and Williamson, 1991). The wage gap started to decline from the mid-1930s and continued to do so until well into the post-Second World War era. The effect of the macro-economic shocks in the interwar years was short-lived, interrupting only briefly the long-term trajectory of stable or diminishing wage gaps between the labour markets. Consequently, the regional wages for agricultural workers may also serve as a proxy for labour market outcomes at large.

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<sup>11</sup> Jörberg's (1972) wage series of day workers in agriculture end in 1914 and originate from a different source. The official statistics allow us to draw on unbroken series from 1865 to 1940 and extrapolate backwards by using Jörberg's series only from 1865 to 1860.

<sup>12</sup> Here the daily wages of day labourers in agriculture were compared with the earnings of blue-collar workers in mechanical workshops. The source presents the average wages for 61 workshops in different parts of Sweden, representing 66% of the workers in the workshop industry (Arbetsstatistik III, pp. 329–346; Arbetsstatistik IV, pp. 395–433). A comparison was possible for 19 counties for which the average wages were estimated, weighted by the number of workers in each workshop.

County-specific cost of living indices transform nominal regional wages into real regional wages. The regional cost of living indices in 1860–1912 are based on the prices of 11 food items, which accounted for about 60% of the total household expenditure in the 1860s (Jörberg, 1972; Myrdal and Bouvin, 1933, p. 123).<sup>13</sup> Most of the price series were county-specific. The indices from 1912 to 1940, meanwhile, are based on the prices of 47 items, which is probably very close to complete coverage of a consumer budget. The price series are no longer county-specific. Instead, they relate to seven broadly defined regions (Detaljpriser och indexberäkningar; Konsumentpriser och indexberäkningar). Myrdal and Bouvin (1933) provide the household budget weights for the years between 1860 and 1912, and the Social Board provides the weights for the two sub-periods of 1913 to 1930 and 1931 to 1940.<sup>14</sup> Since we apply fixed weights to each sub-period, the procedure results in three separate Laspeyres index series. The three Laspeyres indices are spliced to cover the whole period. In the final step, each county's weighted price level is divided by the average price level of all the counties in 1913. Constructed thus, the indices capture the cross-county price differentials and differences in the movements over time. Our set of real wages for 24 Swedish counties appears in the appendix (table A1).

The migration rates are based on the official population statistics. The number of immigrants minus the number of emigrants yields the external net migration. This procedure underestimates emigration in 1861 to 1900, on average by about 10%. In the twentieth century, the errors are insignificant.<sup>15</sup> The internal net migration is calculated indirectly by subtracting the natural increase in the population and the net external immigration from the intercensal population increase. The net total migration is estimated in a similar way.<sup>16</sup> The net migration rates are per 1,000 of the average population size.<sup>17</sup>

Data on industrialization and urbanization are taken from the censuses.<sup>18</sup> The degree of industrialization is measured as the proportion of the population making their living in the industrial sector, and urbanization is measured as the proportion of the population that were resident in urban municipalities. Data on infant mortality are gathered from the official population statistics.<sup>19</sup> The IMR is calculated for the 10-year period up to the census years and measured as the number of living born who died before the age of one divided by the number of births.

## 5. How potent was Swedish migration?

To view the effects of the Swedish migration rates from an international standpoint, we follow in the footsteps of Boyer (1997), Silvestre (2005) and Williamson (1990), by calculating the counterfactual effects of migration on the regional wage levels by county for

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<sup>13</sup> We choose 11 food items: fresh pork and other meats, milk and cream, butter, margarine, cheese, eggs, bread, flour, grain, potatoes and sugar. This basket represents 80/86% of the food costs of households in the household surveys in 1920/1923 (Levnadskostnaderna på landsbygden; Levnadskostnaderna i städer och industriorter). We lack the regional costs for clothing, housing and heating, the latter being of minor importance since the wages are for farm workers.

<sup>14</sup> Myrdal and Bouvin (1933, p. 119); *Detaljpriser och indexberäkningar; Konsumentpriser och indexberäkningar*.

<sup>15</sup> Hofsten and Lundström (1976, table 4.2, p. 66 and table 8.1, pp. 139–40).

<sup>16</sup> Hofsten and Lundström (1976, tables 8.2–8.3, pp. 142, 144).

<sup>17</sup> The population statistics from the Swedish censuses are assembled in *Historisk statistik för Sverige*, also available at [www.scb.se](http://www.scb.se). The printed original statistics from the censuses are also available there.

<sup>18</sup> Statistiska Centralbyråns underdåning berättelse 1856–60, 1870, 1880, 1890, 1900, 1910; *Folkräkningen 1920, 1930, 1940*.

<sup>19</sup> Statistiska Centralbyråns underdåning berättelse 1856–60, 1870, 1880, 1890, 1900. *Befolkningsrörelsen. Översikt 1901–1910, 1911–1920, 1921–1930, 1931–1940*.

each decade between 1860 and 1940. This requires assumptions about the rates of labour force participation of the total population, the rates of labour force participation of immigrants and the demand elasticities of labour in destinations and origins. In accordance with the previous literature, we assume that the rate of labour force participation was 67% for the population at large and 90% for immigrants (Rooth and Scott, 2012). In line with Williamson's (1990, pp. 93–5) study of British non-agricultural workers, we assume that the long-run elasticity of demand for agricultural workers was  $-1.6$ .<sup>20</sup> Table 1 provides the full gamut of the estimated counterfactual effects of migration on labour, by county and for each decade. Our crude calculations give reasons to believe that the effects of net total migration on wages were quite substantial.

Table 1 about here

In general, migration affected wages more before 1910 and most in 1880–90. In the 1880s, the wages of Stockholm county would have been about 28% higher per decade in the absence of migration. In contrast, the counties with the largest out-migration, Jönköping and Kronoberg, would have had wages that were about 31–5% lower. Comparing the estimated impacts for Sweden with the literature on Britain and Spain, we conclude that the migration to Stockholm in the 1880s gave rise to similar effects on convergence as migration to London between 1860 and 1901 but four times the effects of migration to Barcelona in the 1920s.<sup>21</sup>

Table 2 displays the correlations between net internal migration and net external migration.<sup>22</sup> The first panel refers to the full period, whereas the middle and the bottom panel show the split periods of 1860–90 and 1900–40. The table shows that the net internal migration rates correlate positively with the external ones in the full period as well as in the two sub-periods. The coefficient of the full period is large and statistically significant, but driven mainly by the effects of the earlier period. The counties from which many people emigrated also experienced negative net internal migration rates. We find these counties in the south and west of Sweden. The counties from which few people departed overseas also enjoyed positive net internal migration. These counties are to be found mostly in the northern parts of Sweden.

Table 2 about here

Figure 4 about here

Figure 5 about here

The left panel of figure 4 shows the pooled relationship between the net total migration and the percentage wage changes a decade later. The relationship is evidently negative, with higher wage increases in the areas from which most people out-migrated (the negative

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<sup>20</sup> Actually, Boyer (1997) assumes that the elasticities were about  $-2.0$  for nineteenth-century England. On the other hand, Hamermesh (1993, ch. 3) assumes that the confidence interval for developed economies ranges from  $-0.75$  to  $-0.15$ .

<sup>21</sup> Boyer (1997) and Silvestre (2005) make similar calculations for England and Spain. Boyer (1997, p. 212) estimates that London wages would have been 24–8% smaller in 1901 (assuming elasticity of  $-1.6$ ) if no immigration had occurred between 1860 and 1901. For Spain, and assuming elasticity of  $-1.6$ , Silvestre estimates that the wages of Barcelona in 1920–30 (which was the most dynamic decade in Spanish migration history) would have been 7% lower in the absence of migration, and that the largest effect of the sending region was 10% higher wages in the Ebro Valley (Silvestre, 2005, p. 155).

<sup>22</sup> In the international literature, there is no consensus about the relationship between regional emigration rates and net internal migration. For the English counties, Baines (1985) concludes that there was no significant relationship in the rates of emigration and internal migration in 1861–1901. In Spain, the relative roles of internal migration and international migration varied over time (Rosés and Sánchez-Alonso, 2004).

correlation is statistically significant with a p-value of 0.025). The right panel of figure 4 substitutes on the horizontal axis the share of the urban population for migration, and hence shows the pooled relationship between wages and urbanization. It confirms our expectation that urbanization did not have a significant impact on wage changes in our investigated period. This is because Sweden's scattered population pattern thwarted agglomeration forces and industrialization took place mainly in the countryside. Since the urbanization rates did not drive agglomeration and wage increases, we believe that the quantity effects of migration were more important in explaining the wage changes. (The correlation between urbanization and wage growth is statistically insignificant with a p-value of 0.56.)

Figure 5 investigates the role of  $\beta$ -convergence, i.e. the percentage growth rates of wages vs. the log of wages in the initial year, conditional on the extent of migration. As the left panel indicates, areas that experienced out-migration rates of the highest 25th percentile enjoyed strong convergence forces (the coefficient is -0.5166 and statistically significant at the 1% level). The right panel, showing the areas where in-migration was on the lowest 75th percentile, brings forth a more mixed pattern, partly because the wage pattern of Stockholm (#1) blurs the picture somewhat (the coefficient is still negative, -0.2463, but no longer significant). In sum, convergence prevailed above all across counties prone to large levels of out-migration.

Table 3 about here

Models (1)–(4) in Table 3 report the regression results for the influence of migration on wage changes, using pooled data and controlling for alternative factors that could influence wage convergence. Models (5)–(8) include the same control variables, using fixed-effects regressions instead. The coefficient of migration is negative and stable in all the regressions. In most regressions, the coefficient is also significant at the 5% level (and in all cases significant at the 10% level). Thus, the migration coefficient is not altered by including candidate explanations such as urbanization, industrialization or infant mortality rates. The evidence suggests that we should really look more closely at migration in order to explain convergence.

A concern with the migration estimates is that they are endogenous and therefore potentially biased. The bias with regard to endogeneity probably leads to smaller coefficients (most people move to the burgeoning areas and the inflow of workers there presses wages downwards). Hence, we do not run the risk of overstating the effect of migration on wage convergence. In addition, we use migration in the previous decade to explain wage changes in the following one. However, to establish the true impact of migration on wages, we still need to address the problem of endogeneity properly. Following the migration model outlined by Crozet (2004), we believe that a measure of geography could provide information on the costs of migration that individuals faced, above all for the early period when the transportation network was still in its infancy.

As shown in table 2, there was a positive correlation between net external and net internal migration. Counties with large net internal out-migration rates also experienced large net external migration. Since most emigrants had to pass the port in Gothenburg to embark on their journey to USA, the distance to the port should have influenced the cost of emigration, without having affected the growth rates of wages or given rise to agglomeration effects.<sup>23</sup> Therefore, we use each county's distance to Gothenburg as an instrument for migration.<sup>24</sup>

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<sup>23</sup> Gothenburg is Sweden's second-largest city, but agglomeration effects and spatial spillovers also affected large cities like Stockholm and Malmö.

<sup>24</sup> Here, the distance to Gothenburg is measured as the km distance from each county's largest city to Gothenburg.

(For the location of Gothenburg, see figure 3 above.) We also use data on road density (public highways per square km) and railway density as a time-varying instrument for migration. Data on roads per county were taken from the official statistics (K. M:ts femårsberättelser). Unfortunately, county-level data were only reported between 1860 and 1900, which limits the period for which the instrument can be used. The railway density was easier to come by thanks to Caruana-Galizia and Marti-Henneberg (2013), who report railways per county for large parts of Europe covering our entire period.<sup>25</sup> The results of our use of these instruments for migration in fixed-effects regressions on wage growth appear in table 4. The models refer to different combinations of instruments and whether they include time fixed effects. We interact the distance variable with dummies for each year to capture the time-varying effects that distance may have implied as transportation networks improved during the course of the nineteenth century.

Table 4

As expected, the instrumented coefficient for migration is about 2–3 times larger than the ones obtained in table 3. This may illustrate that a downward bias afflicted the original estimates. The use of instruments yields higher standard errors than in the original equations in table 3. There are reasons to believe that instrumenting migration with the distance to Gothenburg and time-varying infrastructure gives more plausible results for the beginning of the period, when the costs of migration were still heavily influenced by these two variables. Since we are confined to the scant information on road density, we can only examine the effects of this instrument for a limited part of our study (models 2 and 4). The improvement of the infrastructure and transport facilities might explain why the instrumented variable only becomes significant at conventional levels in specification 6, railway density and distance to Gothenburg between 1860 and 1910. Nevertheless, the estimated coefficients show the expected negative sign in all the specifications.

This section demonstrates that there is a negative, stable correlation between migration and wage changes in the subsequent period. The relationship becomes even stronger when we use instruments relating to distance and transportation networks to account for the potential endogeneity problem of migration.

## 6. Methods: The use of panel data in convergence regressions

Barro and Sala-i-Martin (1991) pioneer a regression model designed to capture the effect of migration on regional convergence. Their original, unconditional model assesses the strength of the negative relationship between the growth of incomes in a particular time span and the log of the income levels in the initial year. They coin the estimated coefficient  $\beta$ -convergence, the speed at which a particular region approaches a steady state. To examine the role of migration in regional convergence, they hold migration rates constant in the convergence regression. The change in the unconditional parameter equals the effect of migration.

In this paper, we follow the approach of Barro and Sala-i-Martin but adjust their model somewhat. First, we include all the observations in our data set by formulating a panel data model of conditional  $\beta$ -convergence in wages:

$$\Delta w_{i,t} = (\beta - 1)w_{i,t-1} + \delta m'_{i,t} + \gamma x'_{i,t-1} + \eta_i + \lambda_t + v_{i,t} + \varepsilon_{i,t}, \quad (1)$$

<sup>25</sup> We are very grateful to the authors for sharing their unpublished data with us.

where  $\Delta$  is the difference operator between  $t$  and  $t-1$ ,  $w$  is the log of the real wage in county  $i$  at time  $t$ ,  $m$  is a vector of migration rates in county  $i$  at  $t$ ,  $x$  is a vector of control variables,  $\eta$  are regional fixed effects and  $\lambda$  are time-specific dummies. The time-specific dummies are included to control for period-specific trends in wage growth and migration rates that otherwise might give rise to results that are spurious. The convergence parameter of interest is  $(\beta-1)$ , which measures the relationship between the changes in wages from  $t$  to  $t-1$  on the left-hand side of the equation and the lagged wage rate on the right-hand side.

The estimated coefficient,  $\beta$ , indicates the speed of convergence, and is calculated as

$$Conv = -\ln(\beta) \quad (2)$$

The advantage of the panel formulation in relation to Barro and Sala-i-Martin's original model design is that the panel data model makes use of the richer variation in the time dimension of the data set. In addition, panel data models have become increasingly influential in convergence studies because they enable the researcher to control for time-invariant heterogeneity, known more commonly as fixed effects. Islam (1995) shows that cross-sectional analysis leads to a systematic downward bias in the convergence coefficient because cross-sectional techniques fail to control for unobservable factors. The use of panel data to examine convergence entails an interpretation of the  $\beta$ -coefficient that differs from the cross-sectional specification, because the estimated convergence parameter reports how rapidly a region approaches its own steady state rather than the sample average. Since a region is closer to converging to its own steady state than it is to the entire sample's, the estimated  $\beta$ -coefficient in panel data models is larger; it typically indicates a faster convergence rate than the standard 2% a year closing of the gap that Barro and Sala-i-Martin (1990, 1991, 2004) encounter.<sup>26</sup>

Equation (1) can be rewritten as a dynamic panel that takes the form:

$$w_{i,t} = \beta w_{i,t-1} + \delta m'_{i,t} + \gamma x'_{i,t-1} + \eta_i + \lambda_t + v_{i,t} + \varepsilon_{i,t}, \quad (3)$$

If we furthermore transform it into differences to remove the county fixed effects, it takes the following form:

$$\Delta w_{i,t} = \beta \Delta w_{i,t-1} + \delta \Delta m'_{i,t} + \gamma \Delta x'_{i,t-1} + \eta_i + \lambda_t + v_{i,t} + \Delta \varepsilon_{i,t}, \quad (4)$$

Equation 4 may, however, suffer from the econometric problems of endogeneity. Firstly, county-specific characteristics may not determine migration rates exogenously; instead, it is likely that changing migration rates modify county-specific characteristics. Therefore, time-invariant county characteristics may be correlated with the migration rates and give rise to problems of endogeneity, which in turn may result in biased estimates of the  $\beta$  parameters. Secondly, the lagged dependent variables on the right-hand side of the equation could be endogenous if the model suffers from first-order autocorrelation. In section 5, we found a way to remedy the endogeneity problems by using the distance to Gothenburg, railway

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<sup>26</sup> For instance, de la Fuente (2002) estimates that the speed of convergence was about 12% per annum in the Spanish regions, and Canova and Marcet (1995) report that the convergence rate was 23% per annum when each region converged to its own steady state.

density and road density in the sending area. Through geography's external influence on the cost of migration, these instruments should affect migration without affecting wages. In addition, Arellano and Bond (1991) show that second or deeper lags of the levels of the variables are suitable instruments for endogenous variables. Because the lagged wage rates on the right-hand side of equation (4) may also be endogenous if there is first-order autocorrelation, we also instrument this variable with second lags of the wages. In addition, we conduct a statistical test to ensure that there is no second-order autocorrelation in the error term.

The Arellano–Bond difference GMM estimator provides a way to estimate the model represented by equation (4) (Arellano and Bond, 1991). The difference estimator uses lagged levels of the variables as exogenous instruments of the endogenous variables. It is designed for small T and large N panels, which fit this study with 8 time periods and 24 cross-sectional units. However, Bond et al. (2001) show that the difference estimator behaves poorly in small samples. Instead, they suggest using the system GMM estimator developed by Arellano and Bover (1995) and finalized by Blundell and Bond (1998) because it has superior finite sample properties (Blundell and Bond, 1998). The system GMM estimator combines the standard set of equations in first differences with lagged levels of the variables. In addition, it uses a set of equations in levels in which lagged differences are used as instruments.<sup>27</sup> Several recent convergence studies recommend the system GMM estimator for dynamic panels with small samples (Østbye and Westerlund, 2007; Wolszczak-Derlacz, 2009). We compute one-step and two-step estimators because Windmeijer (2005) argues that two-step estimators for difference GMM regression have a lower bias and standard error.

## 7. Regression results

Table 5 displays the estimates of four models based on GMM system regressions.<sup>28</sup> The first model (1) reports the unconditional convergence rate in a panel specification setting, in which wages are instrumented with their second lag. The estimated  $\beta$ -coefficient indicates an unconditional convergence rate of 13.5% annually. This estimate is higher than the standard 2% a year that is typically estimated using the Barro and Sala-i-Martin approach, but in fixed-effects specifications such as these, higher speeds of convergence rates are usually obtained (Canova and Marcet, 1995; de la Fuente, 2002).

The second model (2) reports the conditional convergence rate, after controlling for migration, and indicates that migration does not play a role in wage convergence. The implied annual convergence in models (1) and (2) changes from 13.5 to 27%, suggesting that migration has a divergent effect.

Table 5 about here

In models (3) and (4), we compare the unconditional convergence estimates with the estimates that control for migration when instrumented to control for its potential endogeneity. One problem with the Arellano–Bond procedure is the potential weakness of the instruments. We follow the common recommendation to keep the number of instruments smaller than the number of groups; second lags of differenced wage levels therefore suffice as instruments.

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<sup>27</sup> The properties of the estimator in the presence of endogenous covariates are discussed further by Blundell and Windmeijer (2000).

<sup>28</sup> The one-step estimates are reported in table 4. The two-step estimates, available from the authors on request, have larger standard errors.

The Hansen J test examines the null hypothesis that the last instrument in the over-identified model is valid (but unfortunately there is no way of knowing which instrument was the last one).<sup>29</sup> In none of the specifications reported in table 4 is the instrument rejected at standard confidence levels. Since we use the second lags as instruments, we also examine carefully whether there is second-order autocorrelation in the model. All the models pass the test by displaying insignificant second-order autocorrelation.

However, we only instrument for migration in models (3) and (4) to show how large the effect of migration is on the convergence estimate. In model (2), we only instrument wages with the second lags but use no instruments for migration, which means that we do not control for the possibility that the migration rates are correlated with the fixed effects. When we do not instrument for migration (2), the point estimate of  $\beta$  suggests that migration has a divergent effect compared with model (1).

In model (3), we instrument migration with its second lag. The inclusion of the instrument changes the implied annual convergence rate from 13.5% (1) to 12.3%, suggesting a very weak effect of migration on convergence. However, the standard errors of the  $\beta$ -estimate are large, which implies problems with the weakness of the lags of migration as an instrument. The Hansen J test and the autocorrelation tests do not indicate any problems with the validity of lagged wages and migration rates as an instrument.

However, we suspect that the results in model (3) may be biased by weak instruments. Therefore, we decide to instrument migration with distance to Gothenburg and railway density (4). Then, the point estimate of the  $\beta$ -parameter becomes 0.622, implying a yearly convergence rate of 6.2% and holding constant the effect of migration. The result of (4) indicates that migration may have contributed to slightly more than half of the estimated convergence rates in Sweden. Based on the results attained in table 5, we gauge the effect of migration to be quite substantial; migration explains about half of the convergence rates, even if the instruments are weak. This underlines the importance of using proper instruments of migration in wage regressions in order to determine the sign of the effects of migration on wages.

Table 6 about here

In table 6, we turn to the question of whether net external or net internal migration played the larger role in Swedish wage convergence. The table reports the system GMM estimates of migration on wage convergence divided into the effects of net internal migration and net external migration. In the models, lags are used to instrument for wages and the distance to Gothenburg and railroads are used to instrument for migration to avoid problems of endogeneity. We repeat models (1) and (2) from table 5 to facilitate the comparisons. Model (1) reports unconditional convergence, whereas models (2)–(4) report conditional convergence, controlling for net total migration (2), net external migration (3) and net internal migration (4). The results are quite similar whether we control for net external or net internal migration. In (3), the counterfactual convergence rates without emigration become 4.6%, implying that net external migration by itself contributed to about two-thirds of the total wage convergence of about 13.5%. Controlling for net internal migration only gives a similar result with a counterfactual convergence rate of 5.3%, but the coefficients of net external and net internal migration are not statistically different from each other. We interpret the similarity of our control variables as an indication that net external and net internal

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<sup>29</sup> Hansen's J test is preferred to the Sargan test, since Hansen is robust to heteroskedasticity in panel estimates.



migration were so spatially correlated during the period when the effects of migration were the highest that it is hard to distinguish between the effects.<sup>30</sup>

From the exercise in table 6, we conclude tentatively that net external migration and net internal migration had similar effects on the convergence rate. Nonetheless, the sole force of external migration appears to drive the entire wage convergence, a result that is probably due to the very fast rate of wage convergence before the First World War, when net external migration correlated with net internal migration.

## 8. Conclusions

Swedish regional wages converged quickly in the late nineteenth and the early twentieth century. We argue that migration contributed to this process by moving people from low- to high-wage areas. In particular, during the large migration flows before the First World War, most people migrated from low-wage counties. The large inflow of people into Stockholm during the 1880s served to make wages there 30% lower than they would have been in the absence of immigration.

The previous literature using regressions to measure the impact of migration on convergence suffers from methodological flaws owing to the endogeneity of migration. Our comparison between OLS and GMM estimates stresses the importance of tackling these problems. The OLS estimate does not identify a significant contribution of migration to wage convergence. It requires a model that takes the effects of geography and migration costs into account by using instruments such as distance to the main emigration port of Sweden (Gothenburg) and railroad density to capture the effects migration had on wage convergence. The model with instruments finds that migration could have contributed to slightly more than half of the obtained  $\beta$ -convergence rates for Sweden. Finally, we separate the effects of emigration and internal migration, indicating that they contributed equally to the convergence in real wages. However, in the late nineteenth century, when the convergence rates peaked, emigration and internal migration were correlated spatially. Low-wage counties lost people because of both emigration and internal migration.

Compared with the previous literature, we attribute a relatively large role to migration in explaining wage convergence. We argue that Sweden exhibited two characteristics that explain the large effects we attribute to migration. First, labour mobility, both internal and external, was high according to international standards. Second, late-nineteenth-century industrialization took place largely in the countryside, which provides an explanation for why the agglomeration forces of growing cities did not offset the power of labour supply economics. In addition, we suspect that quite a uniform distribution of human capital among migrants and stayers counteracted the brain-drain forces common in the contemporary world. Future research is required to establish whether that conjecture holds.

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<sup>30</sup> Given that we hypothesize that our results are mainly driven by the evolution before 1910, we also run our models for the sub-period 1860–1910. Although the standard errors of our estimates increase, the qualitative effects do not change. This suggests that the earlier sub-period is a prime driver of the results. The results are available from the authors on request.

Arbetsstatistik III. Undersökning af den mekaniska verkstadsindustrien i Sverige. I. Större egentliga mekaniska verkstäder (1901)

Arbetsstatistik IV. Undersökning af den mekaniska verkstadsindustrien i Sverige. II. Mindre, egentliga mekaniska verkstäder samt vissa specialverkstäder m.m. (1904)

***Arbetsstatistik L (Kommerskollegii afdelning för arbetsstatistik, Stockholm)***

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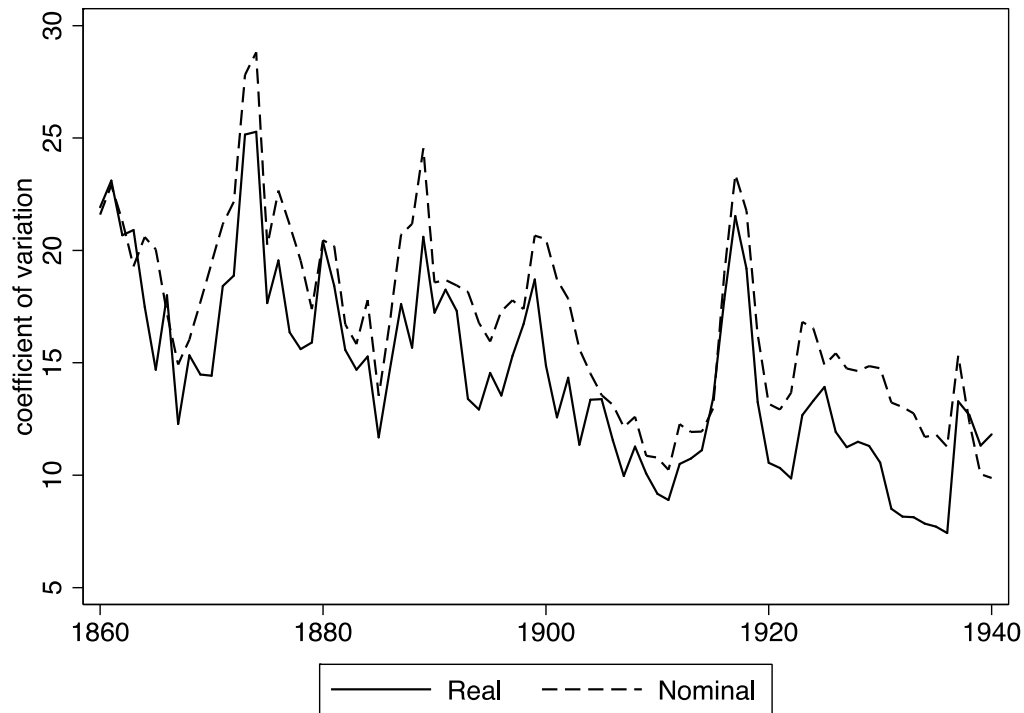


Figure 1. Coefficient of variation in nominal and real wages across 24 Swedish counties, 1860–1940

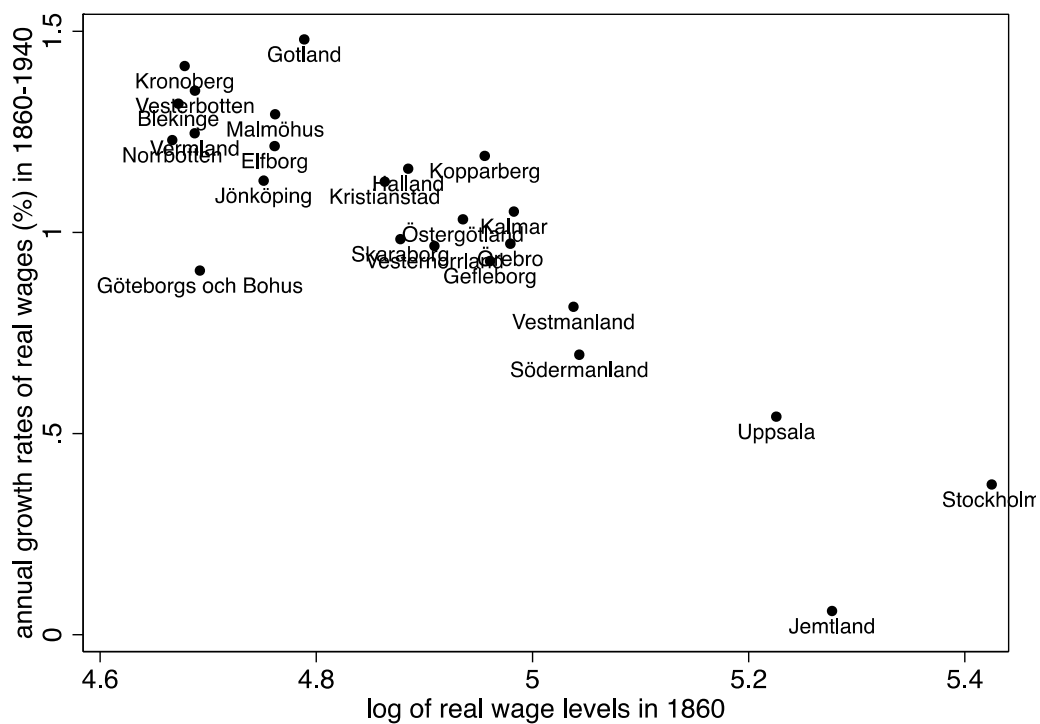


Figure 2.  $\beta$ -convergence, log of the initial real wage levels in 1860 against the average annual growth rates (%) in 1860–1940 in 24 Swedish counties

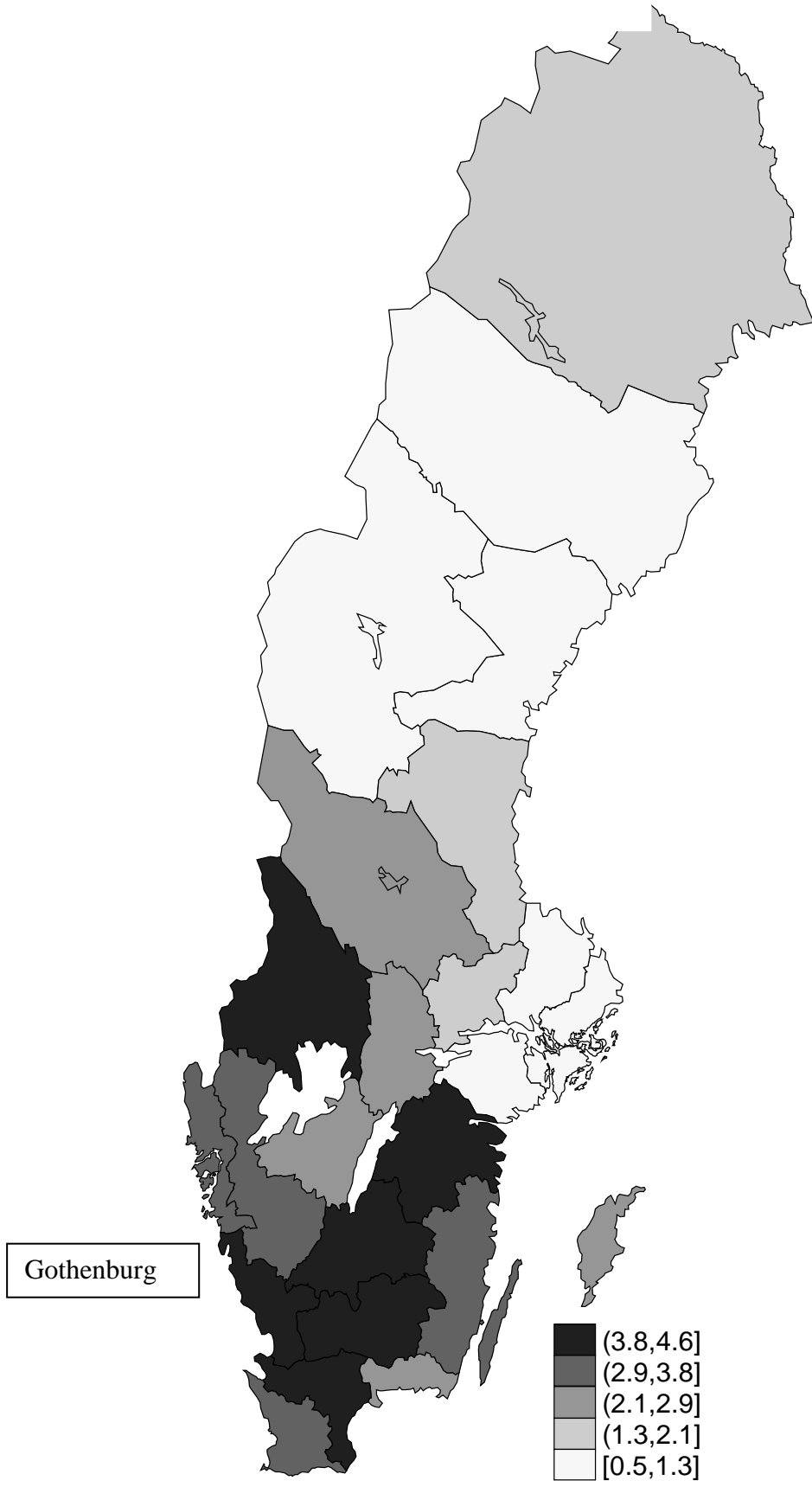


Figure 3. Net external migration (emigration) per county 1870–80, quintiles. Source: Hofsten and Lundström (1976) p. 140

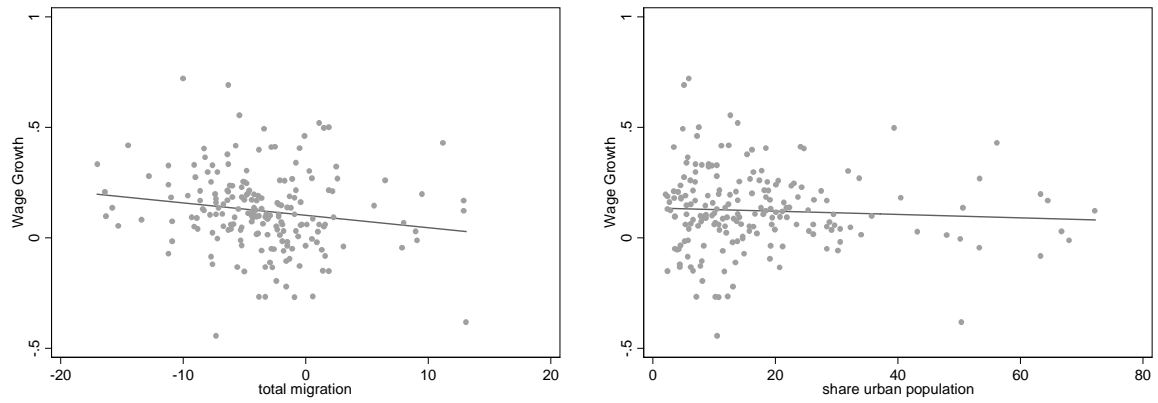


Figure 4. Net total migration vs. wage change (%) in the subsequent decade (left) and proportion of urban population (%) vs. wage change (%) in the subsequent decade (right)

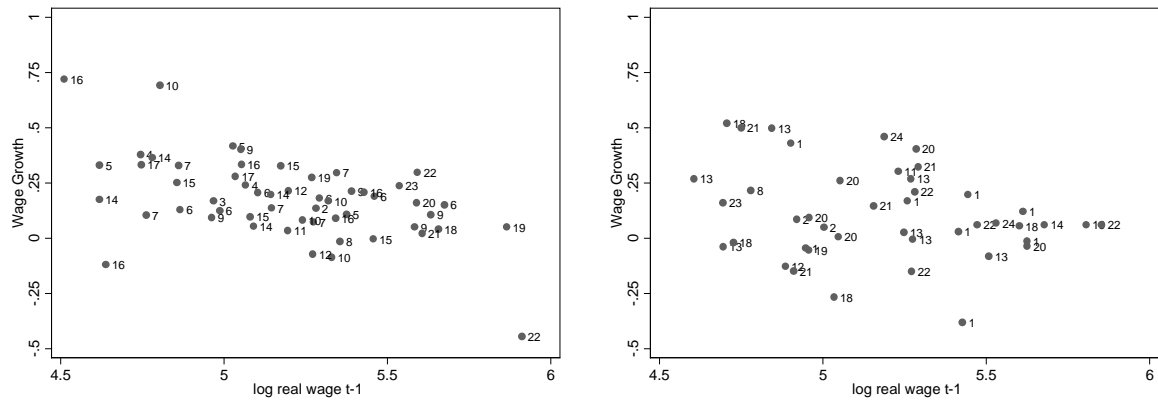


Figure 5.  $\beta$ -convergence of out-migration counties (left) and in-migration counties (right)

Note, for an explanation of the county codes, see table A1 in the appendix.



Table 1. Counterfactual effects of migration on wages (%)

	1850–60	1860–70	1870–80	1880–90	1890–00	1900–10	1910–20	1920–30	1930–40
Stockholm	39.98	16.94	24.08	27.76	19.30	20.42	19.53	27.64	26.96
Uppsala	2.81	1.29	3.22	-3.65	-15.26	-11.18	-1.72	-8.60	-4.51
Södermanland	-0.19	-4.94	-6.66	-13.76	-8.17	-8.81	-3.22	-11.18	0.86
Östergötland	-0.71	-12.68	-13.76	-24.07	-10.32	-10.75	-6.66	-8.17	0.00
Jönköping	-0.34	-19.56	-11.82	-31.16	-14.61	-11.18	-4.30	-7.74	3.44
Kronoberg	-1.20	-18.05	-17.84	-35.25	-23.64	-20.63	-13.11	-14.40	-8.81
Kalmar	-0.90	-16.76	-16.33	-33.96	-23.43	-17.19	-9.24	-9.03	-8.38
Gotland	-0.52	-3.87	-11.61	-23.43	-2.36	-1.93	-6.88	-1.72	-2.15
Blekinge	0.41	-10.75	-9.46	-19.99	-17.84	-18.48	-19.56	-15.69	-8.38
Kristianstad	-0.28	-13.54	-16.55	-28.80	-18.48	-9.46	-6.23	-10.10	-6.45
Malmöhus	-0.56	-4.08	-5.37	-13.54	0.64	-1.93	-4.51	-2.58	1.72
Halland	-0.39	-8.38	-12.25	-24.07	-13.76	-12.90	-13.11	-8.60	-3.87
Göteborg och Bohus	-0.11	-2.36	1.07	3.22	2.15	1.07	5.59	3.44	7.31
Älvsborg	0.67	-15.69	-17.62	-32.88	-15.90	-10.96	-3.22	-0.64	5.59
Skaraborg	-0.71	-11.39	-16.55	-35.03	-24.07	-15.47	-9.03	-9.24	-5.16
Värmland	-0.41	-16.33	-21.49	-36.54	-19.13	-13.54	-5.16	-8.38	-4.73
Örebro	-0.60	-9.46	-13.54	-27.51	-12.47	-10.32	-4.94	-7.09	6.66
Västmanland	-0.82	1.93	2.36	-10.10	-7.74	-12.90	-0.86	-18.91	5.59
Kopparberg	0.13	-11.18	-7.31	-19.34	-4.73	-9.89	0.21	-14.40	-3.44
Gävleborg	0.28	-1.29	13.97	-1.07	0.00	-15.69	-9.24	-5.80	-9.89
Västernorrland	0.49	4.08	12.04	5.37	-11.39	-16.33	-10.32	-6.23	-10.96
Jämtland	0.30	-2.79	4.73	2.36	-7.52	-15.47	3.44	-15.69	-3.65
Västerbotten	0.41	-6.02	-7.74	-8.60	-4.73	-13.33	-4.30	-3.44	-3.44
Norrbottn	-0.09	-4.51	-0.21	-3.22	17.19	-6.66	-4.73	-10.75	-7.74

Note: Assuming -1.6 elasticity of labour demand and labour force participation rates of 0.9 among migrants and 0.67 among the population in total. Effects calculated per decade. A: The percentage change in the labour supply in region  $i$  depends on the change in population levels ( $pop$ ) and the inflow of workers ( $mig$ ), corrected for differing assumptions about the groups' participation rates. Thereafter, we multiply the percentage change with an assumption about the elasticity of the labour supply to obtain the estimated percentage change in wages:

$$\frac{pop_{i,t-1} * 0.67 + mig_{i,t} * 0.9}{pop_{i,t-1} * 0.67} * elasticity$$

Sources: Wages: Table A1; Migration rates: Hofsten and Lundström (1976).

Table 2. Correlation between net internal and net external migration

	Correlation coefficient	P. value
1860–1940	0.2237	0.0141
1860–1890	0.2970	0.0113
1900–1940	0.1096	0.2334

Sources: Hofsten and Lundström (1976) for migration rates by county.

Table 3. Pooled/fixed-effects regressions. The dependent variable is wage growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Migration	-0.006 (0.003)	-0.006 (0.015)	-0.007 (0.006)	-0.006 (0.035)	-0.008 (0.052)	-0.008 (0.053)	-0.008 (0.033)	-0.006 (0.088)
Urbanization		0.000 (0.533)	0.001 (0.070)	0.001 (0.090)		0.000 (0.951)	0.002 (0.450)	0.001 (0.630)
Industrialization			-0.001 (0.377)	-0.002 (0.301)			-0.002 (0.463)	-0.005 (0.073)
Infant Mortality				0.000 0.626				-0.001 (0.078)
Constant	0.103 (0.000)	0.094 (0.000)	0.103 (0.002)	0.143 (0.105)	0.094 (0.000)	0.096 (0.002)	0.093 (0.001)	0.143 (0.105)
R2	0.026	0.027	0.029	0.030	0.026	0.026	0.027	0.025
Obs. (N*T)	192	192	192	192	192	192	192	192

Note: Models (1)–(4) refer to pooled regressions. Models (5)–(8) are fixed effects. P-values are in parenthesis. Standard errors are clustered at the county level.

Table 4. Panel regressions. The dependent variable is wage growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Migration	-0.015 (0.149)	-0.017 (0.219)	-0.007 (0.472)	-0.038 (0.083)	-0.007 (0.455)	-0.044 (0.038)	-0.012 (0.249)	-0.03 (0.103)
Constant	0.069 (0.078)	0.077 (0.156)	0.001 (0.975)	-0.103 (0.543)	0.001 (0.988)	-0.030 (0.762)	0.082 (0.031)	-0.044 (0.762)
<b>Instrumented</b>								
Distance to Gothenburg	x	x	x	x	x	x	x	x
Road density		x		x				x
Railway density					x	x	x	x
Time fixed effects	N	N	Y	Y	Y	Y	N	Y
Obs.	192	96	192	96	192	120	192	96
Period	1860- 1940	1860- 1900	1860- 1940	1860- 1900	1860- 1940	1860- 1910	1860- 1940	1860- 1900

Note: P-values in parenthesis.

Table 5. GMM system regressions. The dependent variable is the log of the real wage, 1860–1940

	(1)	(2)	(3)	(4)
	Unconditional	Conditional	Conditional	Conditional
Beta	0.465 (0.008)	0.384 (0.142)	0.482 (0.002)	0.622 (0.000)
Migration		-0.020 (0.369)	0.009 (0.063)	0.011 (0.027)
Beta range	0.121-0.809	-0.12-0.896	0.175-0.788	0.356-0.889
Convergence rate	0.765	0.957	0.730	0.475
Implied yearly	0.135	0.271	0.123	0.062
No. of instruments	14	14	26	35
Wages instrumented	Lag 2	Lag 2	Lag 2	Lag 2
Migration instrumented	no	no	Lag 2	Dist GBG, Railroads
Hansen's J	0.197	0.266	0.197	0.870
AR(1)	0.001	0.012	0.000	0.000
AR(2)	0.871	0.993	0.577	0.677

Note: P-values in parenthesis. Time dummies included. The estimation procedure is the GMM system approach.

Standard errors are robust to heteroskedasticity within panels. Constant not reported.

Second lags of the levels and differences used as instruments. One-step estimates are reported.

The number of observations is 192 and the number of groups 24; in the GMM models, the observations per group are 8.

Table 6. GMM system regressions. The dependent variable is the log of the real wage, 1860–1940

	(1)	(2)	(3)	(4)
	Unconditional	Total migration Conditional	Emigration Conditional	Net internal migration Conditional
Beta	0.465 (0.008)	0.622 (0.000)	0.688 (0.000)	0.657 (0.000)
Migration		0.011 (0.027)	0.005 (0.515)	0.014 (0.034)
Beta range	0.121-0.809	0.356-0.889	0.454-0.921	0.403-0.911
Convergence rate	0.765	0.475	0.375	0.420
Implied yearly	0.135	0.062	0.046	0.053
No. of instruments	14	35	35	35
Wages instrumented	Lag 2	Lag 2	Lag 2	Lag 2
Migration instrumented	no	Dist GBG, Railways	Dist GBG, Railways	Dist GBG, Railways
Hansen's J	0.197	0.870	0.647	0.946
AR(1)	0.001	0.000	0.001	0.000
AR(2)	0.871	0.677	0.985	0.690

Note: P-values in parenthesis. Time dummies included. The estimation procedure is the GMM system approach.

Standard errors are robust to heteroskedasticity within panels. Constant not reported.

Second lags of the levels and differences used as instruments. One-step estimates are reported.

The number of observations is 192 and the number of groups 24; in the GMM models, the observations per group are 8.

Appendix table A1. Real day wages of agricultural workers by Swedish counties, 1860–1940

	1860	1870	1880	1890	1900	1910	1920	1930	1940
1 Stockholm	228	141	135	192	225	232	277	274	307
2 Uppsala	187	137	149	156	197	223	274	283	288
3 Södermanland	156	114	132	144	168	235	265	260	272
4 Östergötland	139	108	115	159	197	245	287	291	320
5 Jönköping	117	101	135	152	216	239	284	304	286
6 Kronoberg	108	130	147	165	199	235	280	291	336
7 Kalmar	145	117	129	172	195	209	272	305	340
8 Gotland	119	145	136	212	209	264	292	324	394
9 Blekinge	108	97	122	143	156	219	266	280	309
10 Kristianstad	129	122	206	189	204	239	263	289	321
11 Malmöhus	117	121	128	180	187	243	288	291	331
12 Hallands	132	115	138	195	181	220	271	288	336
13 Göteborg & Bohus	109	105	100	127	190	195	194	247	227
14 Älvsborg	117	101	119	163	171	205	257	293	310
15 Skaraborg	132	130	128	161	176	234	234	265	290
16 Värmland	109	103	91	157	209	228	275	290	296
17 Örebro	146	107	115	154	197	215	280	291	318
18 Västmanland	154	113	111	168	202	229	271	286	298
19 Kopparberg	142	134	130	194	248	274	332	353	371
20 Gävleborg	142	156	157	197	277	268	311	348	301
21 Västernorrland	136	116	173	199	263	272	279	333	296
22 Jämtland	195	166	197	238	253	268	348	369	205
23 Västerbotten	109	127	179	211	231	254	314	360	322
24 Norrbotten	106	142	179	262	252	270	293	338	287

Sources: Nominal wages: 1860–1865: Jörberg (1972); 1865–1910: Hushållningssällskapens berättelser; 1911–1928: Arbetarettillgång, arbetstid och arbetslön inom Sveriges jordbruk; 1929–40: Lönestatistisk årsbok. Cost of living: Price series: 1860–1914: Jörberg (1972) and Myrdal and Bouvin (1933); 1913–1930: Detaljpriser och indexberäkningar; 1930–1940: Konsumentpriser och indexberäkningar. Budget weights: 1860–1913: Myrdal and Bouvin (1933, p. 119); 1913–1930: Detaljpriser och indexberäkningar; 1930–1940: Konsumentpriser och indexberäkningar.