Swedish Institute for Social Research (SOFI)

Stockholm University

WORKING PAPER 4/2014

MINIMUM WAGES AND THE INTEGRATION OF REFUGEE IMMIGRANTS

by

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Minimum Wages and the Integration of Refugee Immigrants**

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March, 2014

Abstract

This paper is the first to estimate the effects of minimum wages on the unemployment of refugee immigrants. The collectively agreed minimum wages raise both the incidence of unemployment and days in unemployment considerably for Swedish male refugees; different estimation methods and models yield robust elasticities in the 1.8–2.0 range. The effects for young natives are about half as large. There are heterogeneous effects with regard to country of origin and time of residence in Sweden for both male and female refugees. We account for spatial trends – a concern in some of the recent literature – as well as industrial trends. It turns out that only the latter affect our results.

**We acknowledge comments on an early version from Per Hjertstrand, Eskil Wadensjö and participants at the NORFACE conference "Migration, Global Development, New Frontiers", University College, London, and at seminars at IFN and SOFI. Skedinger gratefully acknowledges financial support from the Marianne and Marcus Wallenberg Foundation.

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When evaluating the employment effects of minimum wages, a crucial aspect is the proper identification of groups exposed to these wages. Several studies have focused on teenagers, rendering a particular policy relevance to the minimum wage studies in countries with high youth unemployment. Another group typically exposed to minimum wages, and for whom the labour market situation in many countries is even more pertinent, is the refugee immigrants. Following the increasing inflow of refugees from different trouble spots around the world, many Western countries today face severe integration problems. The need for a successful labour market integration of refugees in those countries has become more pressing, for social, economic as well as political reasons. The social situation of refugees is characterised by exclusion and high dependence on the welfare system, to a large extent caused by failed labour market integration.

The widespread use of minimum wages in Western countries may represent obstacles for successfully integrating large inflows of, particularly low-skilled, refugee immigrants. Due to low education many refugee immigrants have little of transferable skills and may be in poor psychic and physical health at arrival. Hence, minimum wages could potentially be far higher than the productivity of many refugees. However, it is not obvious a priori that minimum wages are more detrimental for the labour market prospects of refugees than for the employment of native workers. To the extent that refugees can escape high minimum wages, for example through geographic mobility or self-employment, any adverse employment effects may be mitigated.

Despite a voluminous literature on minimum wages no study deals with refugees specifically, presumably due to lack of data in which refugees can be reliably identified. To the best of our knowledge, this study is the first to do so and we examine the integration of refugees in unskilled jobs in Sweden, which, for a variety of reasons, is an ideal country for an analysis on the relation between minimum wages and labour market outcomes for refugees. First, the collectively agreed minimum wages are high by international standards and vary a great deal across industries, as documented in Skedinger (2010). We have collected information on these rates from various collective agreements.

A second reason for Sweden being a useful research object is the high rate of refugee immigration, yielding many observations in our data. Among the rich countries in 2008, Sweden was the country with the largest influx of refugee immigrants in relation to its population and ranked seventh in absolute numbers, after Germany, the UK, the United States, Canada, France and the Netherlands (Hatton, 2012).

Thirdly, we are able to reliably identify refugees among the immigrants in our register data. During the period covered by our data (1998–2007) Sweden pursued a strict policy concerning work-related immigration from countries outside of the European Economic Area (EEA). In effect, immigration from these countries for purposes of work was banned entirely for the unskilled occupations we examine up to December 2008, when rules for labour immigration from non-EEA countries were relaxed. Consequently, information on country of origin, coupled with a date of immigration that falls within a period during which Sweden granted asylum for immigrants from that country, enables us to identify refugee immigrants in our data.

Due to high coverage of collective agreements in Sweden, the negotiated minimum wages act as a wage floor much in the same way as a legal minimum would, with the important difference that the former differ by industry, implying larger variation in minimum wages than is usual in countries with a national minimum wage. We examine how industry-wide minimum wages affect unemployment of the individual, conditional on current or previous employment in the industry, which provides the link between the individual and the relevant minimum wage. Unemployment is measured both as a binary and as a continuous variable, that is, the number of days in unemployment during the year (which need not necessarily be consecutive days). We experiment with various models that take into account the censored nature of the dependent variable and the potential for selection. Our results indicate that minimum wages increase unemployment among male refugees considerably, and more so than among a comparison group of young natives. Our preferred Heckman model yields an elasticity of 1.82 for male refugees and other models yield higher elasticities.

The strong effects remain when we account for spatial heterogeneity in trends, a concern in the recent literature relying on regional variation in minimum wages in the US (discussed in more detail in the literature review in Section 3). The inclusion of industry-specific matter more for our results and raises substantially the estimates of the minimum wage on unemployment days.

The paper is organized as follows. The next section contains the literature review, in which we briefly discuss previous studies dealing with the employment effects of minimum wages, both in general and concerning effects estimated specifically for immigrants. Then we go on to describe our register data on refugees and the minimum wage data collected from collective agreements, in Section 2. The following section describes the econometric specifications in detail, while the results are presented in Section 4. Concluding comments appear in Section 5.

1. Previous literature on the employment effects of minimum wages

The literature on the employment effects of minimum wages is vast and various methods have been used (see Neumark and Wascher, 2007, for an extensive survey). An early approach, still used by many researchers, relies on exploiting variation over time and across regions in the minimum wage, using aggregate or individual data, typically for vulnerable groups. Bazen and Marimotou (2001), Burkhauser et al. (2000) and Neumark and Wascher (1992) provide examples of this approach.

Other studies use a minimum wage hike in one region as a natural experiment and then apply difference-in-difference methods to capture the minimum wage effect on employment. Some of these study effects on specific vulnerable groups using firm-level data or individual data, see, for example, Card and Krueger (1994) or Kim and Taylor (1995). Focusing on a specific group may underestimate the overall dis-employment effects, to the extent that the group assumed to be affected by minimum wages is larger than actually is the case. Consequently, other studies rely on the individual’s position in the wage distribution, comparing employment outcomes for workers close to the minimum wage to those of workers with slightly higher wages, regardless of demographic characteristics. Examples of using difference-in-difference estimation in this context are Abowd et al. (2000) and Machin et al. (2003), while Neumark and Wascher (2002) and Pacheco (2011) use probabilistic approaches.

One concern in the recent literature relying on regional variation in the minimum wage, mentioned in the introduction, is the risk that regional trends drive the results, implying spurious correlation between employment and the minimum wage. Specifically, Allegretto et al. (2011) and Dube et al. (2010) argue that the failure in the previous literature to account for spatial heterogeneity in trends causes an upward omitted variable bias in the estimates of the dis-employment effect and to be close to zero when region-specific trends are included (for a retort to this criticism, see Neumark et al., 2013).

The results in the literature are mixed. While several of the above studies find evidence of negative employment effects (Abowd et al., 2000, Bazen and Marimotou, 2001, Burkhauser et al., 2000, Neumark et al., 2013, Neumark and Wascher, 1992, 2002 and Pacheco, 2011), there are other studies that do not (Allegretto et al., 2011, Card and Krueger, 1994, Dube et al., 2010, and Machin et al., 2003).

There is scant evidence on the employment effects of collectively agreed minimum wages, in Sweden or elsewhere, and on the effects on immigrants of minimum wages in general. Skedinger (2006, 2013) use methods exploiting the individual's position in the wage distribution and examine effects in Swedish hotels and restaurants and retail, respectively. The findings indicate dis-employment effects in both industries, but immigrants could not be identified in the data. Orrenius and Zavodny (2008) find that minimum wages do not hurt the employment of immigrants in the US, but do so for young natives. They argue that this may be explained by the higher inclination among immigrants to re-locate to states with lower minimum wages and non-compliance among employers of undocumented workers. However, they do not distinguish between labour market immigrants and refugees and their data are likely to include mostly non-refugees.

2. Data

Since we do not have access to wage data that are precise enough to use methods that rely on the individual's position in the wage distribution (these methods being highly sensitive to measurement errors in the wage), this study relies on the variation in the minimum wage over time and across industries and regions for identification.[1] However, it is plausible that many refugees in our data are employed at, or close to, the minimum wage. According to a recent, large employer survey in Sweden, around 30 per cent of refugees hired by the firms were employed at the minimum wage (Lundborg and Skedinger, 2014).

[1] We lack information on a suitable wage measure, corresponding to the definition of minimum wages in the collective agreements, which refers to a basic pay rate per hour or month, without remuneration for shift pay, pay for unsocial hours and bonuses. The wage measure in our data refers to total earnings, in full-time equivalents, so it is difficult to assess whether an individual worker is actually bound by a minimum wage.

From Swedish register data 1998-2007, we have selected immigrant groups, refugees and individuals who have arrived for reasons of family reunification, based on country of origin and period of immigration.[2] For simplicity we refer to this group as ‘refugees’, though the selected group covers also individuals who arrived for family reunification. Since labour immigration was basically not allowed from these regions during these periods and after 1970, the selected individuals can reliably be identified as refugee immigrants.

Our basic results refer to refugees aged 19–65. Since young native workers have been analysed extensively in the previous literature, we present results also for a comparison group of natives aged 19–21. The group consists of both natives and a relatively small number of labour immigrants, but we refer to the comparison as ‘natives’ for convenience. For both refugees and natives, we exclude the self-employed and white-collar workers, since minimum wages are either non-existent or not binding for these groups. Moreover, we exclude individuals with no link to the selected industries, through regular or subsidised employment (to be explained in more detail later in this section). After these exclusions there are more than 300,000 observations of refugees and around 650,000 observations of young natives in our data.

Our estimates are based on data on minimum wages collected from the Swedish collective agreements for blue-collar workers for the following seven industries: Hotels and restaurants, retail, engineering, slaughter-houses, bakeries, construction and local government.[3] The agreements specify several different minimum wages, depending on various worker characteristics, like age, experience and occupation. In each agreement we have picked the lowest minimum wage for adults, applying to workers without previous experience and in unskilled occupations. This minimum wage should represent the lowest threshold for entering employment in a specific industry. The industries we have chosen include the most important ones, as far as low-wage employment is concerned, and also the largest industries in terms of employment. Few individuals outside of the samples are likely to be affected by minimum

[2] Our basic register data set is LOUISE, compiled by Statistics Sweden. The data base was extended to include additional information on immigrants. Classification as a refugee is based on immigration from the following countries and time periods: Argentina (1976-82), Baltic countries (1945-89), Bolivia (1981-2001), Bosnia-Herzegovina (1990-95), Brazil (1964-74), Bulgaria (-1989), Chile (1973-89), Colombia (-2003), Czechoslovakia (-1989), Ethiopia, Greece (1967-75), Hungary (-1989), Iran (1980-), Iraq (1980-), Lebanon, Paraguay (1954-89), Peru (-2001), Poland (-1989), Portugal (-1975), Romania (-1989), Somalia, Soviet Union (-1989), Spain (-1975) and Uruguay (1973-85).

[3] See Skedinger (2010) for a detailed discussion of the Swedish minimum wage system and information on the evolution of minimum wages in these industries, covering around 75 per cent of all blue-collar employees.

wages, although the samples also include a substantial fraction of workers with wages well above these rates. We also control for the median wage in the relevant industry and region, for those aged 35–50 to avoid influence from the minimum wage, in the regressions.

With the chosen regression methods the dependent variable is used both as a binary indication of whether the individual has been unemployed or not during the year and to represent the number of days unemployed. The measure of unemployment is based on registration at the Public Employment Service (PES). This implies a deviation from the definition used in the Labour Force Surveys (LFS), according to which an individual is defined as unemployed if without a job and prepared to accept a job offer within 14 days, regardless of registration at the PES.

The variable 'days unemployed' covers days in open unemployment as well as time spent in some form of active labour market programme. Every individual who has had some paid work during a year, including jobs offered in programmes, is then assigned an industry code (ISIC), associated with the negotiation area from which we have collected minimum wages.[4] We are thus able to identify the minimum wage that the individual is most likely to be exposed to. Individuals with links to other industries are thus excluded.

A person in a programme could be registered with the PES for all of the 365 (or 366) days during the year but still be assigned an industry code, because of subsidized employment in the programme or part-time unemployment. Our basic assumption is that a higher minimum wage in an industry increases the unemployment risk for persons linked to that industry.

A limitation with our data is that persons who have been openly unemployed every day of the year or participated in a programme without attachment to some specific workplace, will not be assigned an industry code and hence not be associated with a specific minimum wage. Since the regression analysis assumes some association with a defined industry, some new entrants to the labour market will not be included in the regressions. It should therefore be borne in mind that we study workers who already have established some connection with the labour market and that there may be other mechanisms through which minimum wages may affect employment than the one we examine.

[4] The selected negotiating areas overlap reasonably well with the ISIC codes.

Table 1 shows descriptive statistics of the central variables included in the analysis, separately for refugees and young natives. The probabilities of having been unemployed during the year are lower among refugees than among young natives, 0.32 and 0.40, respectively. Thus the vast majority of the individuals in our data have zero days in unemployment. However, the refugees accumulate more days in unemployment (64.6) than young natives (51.9) on average.

There is considerable variation in real minimum wages for both refugees and young natives, from 10,577 to 15,733 SEK per month, with an average of 11,831 for refugees and slightly higher, 11,963 for young natives (1 SEK was equivalent to 0.148 USD or 0.108 EUR in 2007). In relation to the relevant median wage, the average minimum wage is quite high – 67 per cent for refugees and 68 per cent for young natives.

There are notable differences in the distribution of workers across industries. Compared to young natives, many refugees are employed in engineering and local government. The young natives have a larger share of persons with education up to high school, but a larger share of the refugees have attained less than 9 years of education. The variation within the latter group may be considerable, with quite a few having just a few years or even no education whatsoever. No data are available on the exact number of years of education among the least educated. Young natives also have smaller shares of individuals with at least 2 years of tertiary education, but the figures are affected by the fact that the low average age means that many of them have not yet completed their education.

The vast majority of refugees in our data, around 60 per cent, originate from only five countries: Bosnia-Hercegovina (20.1 per cent), Iran (15.6), Iraq (11.5), Poland (11.6) and Chile (10.8). There is also a sizeable share of refugees from the Horn of Africa (5.6 per cent), i.e. Eritrea, Ethiopia, Somalia and the Sudan. About two thirds of the refugees originate from non-European countries.

3. Regression models

One concern with using unemployment as the dependent variable is that unemployment reflects factors on both the demand and supply side of the labour market. A high minimum wage may encourage individuals to enter the labour force, which may cause unemployment to rise because of an increase in labour supply. Another possibility is that workers who are laid off due to a minimum wage increase exit from the labour force, thus reducing labour supply and unemployment.

While acknowledging these concerns, we believe them to be of less importance in our particular context, in which we condition unemployment on previous employment. This means that all entrants in the labour market with no employment experience are excluded from the sample. Given the characteristics of the Swedish minimum wage system, and the fact that we aim at performing an industry-wide analysis, we also argue that it is more useful in this particular context to look at unemployment than employment.[5] The individual's labour supply cannot be connected with any other sector or region than that determined by the current or last employment. Particularly for people not yet established in the labour market, the labour supply by sector and region cannot be determined since low-skilled job searchers, be they employed or unemployed, may search for jobs in many sectors and regions.

For the refugee group we specify the following equation:

$$\begin{aligned} Y_{jrt} \; &= \alpha + \beta_1 \ln MW_{jt} + \beta_2 \ln W_{jrt} + \beta_3 AGE_t + \beta_4 AGE_t^2 + ED_t'\beta_5 + COUNTRY'\beta_6 \\ &\quad + \beta_7 YSIM_t + \beta_8 YSIM_t^2 + \beta_9 URATE_{rt} + REGTREND_{rt}'\beta_{10} \\ &\quad + INDTREND_{jt}'\beta_{11} + \alpha_j + \alpha_r + \alpha_t + \epsilon_{jrt} \end{aligned} \tag{1}$$

For convenience, individual subscripts are suppressed. Due to the characteristics of our data, we define the dependent variable as either the probability of being unemployed *Pr(U*) or the log of days in unemployment (*ln UDAYS*).[6] *UDAYS* refers to the number of days the individual has been registered as a jobseeker at the Public Employment Service during the year. The minimum wage is included in logarithmic form (*ln MW*). To control for the possibility that the effect of *ln MW* depends on the log of the median wage, *ln W*, we include this variable.

[5] The latter type of analysis would involve calculating a minimum wage index across all industries, which is far from straightforward. The minimum wage bite differs greatly across sectors and the weights, if based on employment in the sectors, are likely to be endogenous with regard to the minimum wage.
[6] Since the number of unemployment days is zero for a large number of individuals, a logarithmic specification works only if the zeros in UDAYS are replaced with positive values, so we have assigned very small values for these observations (the deviation from zero is –0.0000001). See Cameron and Trivedi (2010, p. 546) for a detailed discussion of this procedure. We also performed several robustness checks doubling these values, with no effect on the estimates with seven decimals.

We also include variables intended to capture the productivity of the individual and additional factors affecting the demand for labour. Thus we control for individual characteristics like age (*AGE*) and its square, which we use as proxies for experience, and vectors of dummies for education (*ED*) and country of origin (*COUNTRY*) as well as the regional unemployment rate (*URATE*), according to the definition in Labour Force Statistics, LFS. We add the number of years since immigration (*YSIM*) and its square mong the explanatory variables, which is standard procedure in the empirical literature on labour market integration.

As discussed in the literature review, to avoid potential omitted variable bias we control for spatial heterogeneity in trends by including region-specific trend variables, the vector *REGTREND*. Moreover, since minimum wages are set at the industry level, omitting industry-specific trends could bias our results and we therefore also include a vector for such trends, *INDTREND*. Finally, we allow for fixed effects specific to industry, region and year represented by α's, using subscripts j, r and t, respectively. Dummies for industry and region capture structural differences that impinge on unemployment, while the year term controls for aggregate effects of the business cycle. ε denotes the error term.

The dependent variables, *Pr*(*U)* and *ln UDAYS*, are by definition the difference between supply and demand for labour. We thus think of the response to *ln MW* as emanating from both sides of the labour market.

For young natives, we assume the same specification as in (1), with the sole difference that we leave out *COUNTRY* and the two *YSIM* variables.

4. Econometric results

We apply various estimation methods and take into account the censoring in the data and/or sample selection. The estimation methods impose different restrictions. Some of our methods imply that the same variables be used for the estimations of the dichotomous *Pr(U)* and the continuous variable *ln UDAYS*. Other estimation methods rely strictly on linearity, implying that some variables must be excluded.

Given the censoring in our data, it seems appropriate to start with standard tobit estimation. We also account for the upper threshold of 365 (lumped together with the observations of 366 days in leap years).[7] The tobit model is sensitive to certain characteristics of the data, one of which is heteroskedasticity, prompting us to estimate standard errors that are robust in this regard. Tobit estimates are also sensitive to divergences from the normal distribution, albeit less so with large samples (Wilhelm, 2008). Despite the fact that we have large samples, large enough divergences from normality could cause our estimates to be biased. To mitigate the non-normality problem, the dependent variable *UDAYS* is estimated on the logarithmic form, which also entails less of heteroskedasticity.[8] A formal conditional moments test rejects the normality assumption. While criticism has been put forward against this test (Skeels and Vella, 1999), this rejection, together with potential selection problems involved, cause us to pursue an eclectic approach by estimating alternative models, namely the Two-part model and different Heckman selection models.

Following the estimations of the tobit model, we perform post-estimations in which we predict the effects of an increase in *ln MW*, first on the probability of being unemployed, and secondly on the number of unemployment days. In Table 2, the first four columns present the post-estimation elasticities on *Pr(U)* and on *ln UDAYS* of *ln MW*, evaluated at the means of the regressors. The first two columns refer to male and female refugees and the last two ones to male and female natives (young natives and labour immigrants). To save space, we only display the estimates for the main variable of interest, namely *ln MW*.[9]

The elasticities of *MW* have expected positive signs for male refugees and for young native females for whom the results imply that a higher minimum wage significantly increases the probability of unemployment as well as the number of days in unemployment. For male refugees the elasticities of *ln MW* with regard to *Pr(U)* and *ln UDAYS* are 0.40 and 2.03, respectively. The corresponding elasticities for young native females are 0.59 and 2.63. The

[7] Sometimes a minimum value can arise because lower values have simply not been measured and in such cases the variable is explicitly latent. In other cases, including ours, the variable cannot assume a value lower than, say, zero. In both cases the tobit model is appropriate and it is conceivable in our case that there is an excess demand for the services of individuals with zero days in unemployment. The excess demand being latent then causes OLS estimates to be biased.

[8] Fixed effects models for censored data, while available for individual data, suffer from inconsistency. Exploiting the pooled character of our data also yields fragile results since these crucially depend on homoskedastic, normally distributed errors.

[9] Full results are available from the authors.

elasticities for female refugees and for young native males come out with the unexpected sign and are not significant.

The coefficients of the other, unreported variables come out as expected. Unemployment decreases with increasing age, educational level and, for refugees, length of residence in Sweden. Moreover, higher median wages and unemployment in the region increases unemployment among the individuals in our data.

The tobit model has been criticised for being non-robust, complex and not always easily interpreted (see, e.g., Angrist and Pischke, 2009). As a test for robustness we estimate an alternative model, that unlike tobit does not depend on homoskedasticity and normality for unbiasedness, namely the Two-part model. This model specifies a probit for the censoring part followed by an OLS regression conditional on the observed outcome. A key assumption is that the conditional expectation is linear in the regressors. For this reason, we estimate the models without their squared variables. The results are presented in Table 2, columns 5 through 8.

For refugees, the probit *U* yields a positive and significant elasticity for males, 1.15, that is larger than in the tobit model, while there is no significant effect for females. The outcome variable *ln UDAYS* is, as in the tobit model, affected by *ln MW* only for males and the elasticity is only slightly lower, 1.87. For young native males, *Pr(U)* continues to be not significantly affected, while *ln UDAYS* is now significantly increasing in *ln MW*. The estimate, 1.01, is smaller than the one for male refugees, even though we have included only the youngest among the natives. For young native females the effect on *Pr(U)* is significant and positive and considerably higher than that yielded by the tobit. The estimate for *ln UDAYS* is insignificant in contrast to the tobit results.

The two parts of this model are estimated independently, which is a potential problem since individuals may not be randomly selected. In our case, such selection would imply that individuals with positive numbers of unemployment days are not selected randomly after we have controlled for other characteristics. To account for selection, we first apply Heckman's sample selection model (Heckman, 1976, 1987). While the outcome equation is linear, the selection equation is non-linear and the model is estimated using maximum-likelihood.[10] The outcome equation is specified as in the Two-part model.

The results are provided in Table 3, columns 1 through 4. Compared to the Two-part model, allowing for sample selection does not change the results concerning *ln MW* other than marginally; concerning *Pr(U)* the elasticity is 1.09 for male refugees, while it is still not significant for young native males. For *ln UDAYS*, the elasticity for male refugees comes out as 1.91 and for young native males it remains at 1.00. The similarities between the two models do not necessarily mean that selection is not present, but in any case it does not seem to affect the estimate of interest.

This model, also known as the Type-2 tobit model, rests on bivariate normality assumptions, i.e., that the residuals in both equations are normally distributed. This requirement is often put forward as an argument against this model and an explanation for why it sometimes tends to yield non-robust estimates. A related selection model, based on univariate normality and generally considered to be more robust, is Heckman's two-step model, in which the non-selection standard, i.e. the inverse of Mill's ratio, enters explicitly in the regression equation (Heckman, 1979). Since the selection variable may be collinear with the regressors in the outcome equation, the standard errors are usually larger in this model. Again we estimate the equation as for the Two-part and Heckman sample selection models, i.e., without higher powers, and the results are presented in columns 5 through 8 in Table 3.

The first step is identical to the first part of the Two-part model, so we consider the estimates for *ln UDAYS* . For female refugees, the estimate is still non-significant. For male refugees, the estimated parameter (1.82) is very similar to the ones obtained for the Two-part model and the sample selection model. However, for young native females the effect on the outcome variable *ln UDAYS* now comes out positively and significantly, as 1.29. Moreover, for young native males the estimates are again significant, at 0.82. Mill's lambda is significant only for young natives, implying that selection bias is not a problem in the estimations for refugees.

With our eclectic approach we have estimated a number of models with alternative properties. When estimating the effects on male refugees' unemployment days the four models yield

[10] Hence the selection equation may include variables of higher powers, while the outcome equation may not.

elasticities within the 1.82–2.03 range. Also for native males the elasticities for the selection models and the Two-part model are within a narrow range, between 0.82 and 1.01, i.e., around half of those for male refugees. Of interest is that these fairly consistent results emanate from models with different properties. The estimates remain relatively stable despite the fact that the Two-part model does not account for selection and the Heckman models, unlike the Two-part model, are sensitive to non-normality and heteroskedasticity. This offers some support for us having correctly identified the effects.

The results point to Heckman's two-step model as our preferred model. We therefore proceed by further evaluating the results for *Pr(U)* and *ln UDAYS* from this model. Consider first the results for male refugees of a 1 per cent increase in *ln MW*, evaluated at the mean. For male refugees our estimated parameter (1.15) implies an increase in the probability of becoming unemployed, from .2900 to .2933. Given that the refugee is unemployed at least one day during the year, the number of days in unemployment will rise from 191.89 to 195.38. This means an increase by 3.5 days per year for a representative individual. For young native males no significant effect was obtained for the probit component. Based on our estimated elasticity of 0.82 for *ln UDAYS*, the corresponding effect on the number of days in unemployment is an increase from 113.91 to 114.85, i.e., about one day. For young native females the elasticity is higher, 1.29, and the increase is almost two days, from 140.31 to 142.11.

4.1 Spatial and industry heterogeneity in unemployment trends

Given the potential importance of spatial trends, suggested by some studies on US data (Allegretto et al., 2011; Dube et al., 2010), it is of interest to assess the importance of our results to spatial heterogeneity. Our starting point is the results from Heckman's two-step model presented in Table 3 for male refugees, i.e. 1.15 and 1.82 for the effects of *ln MW* on *Pr(U)* and *ln UDAYS*, respectively. Excluding the spatial trend variables from the regression equation, the elasticity related to PR(*U)* is unchanged, while the one for *ln UDAYS* only becomes slightly higher,1.86.[11] Thus, regional heterogeneity in unemployment trends seems to be of little concern in our Swedish data.

The cross-industry dimension of our data necessitates an evaluation also of the industrial trends. Excluding the industry trends, while leaving the regional trends in the regression equation, has a major impact as the elasticities are substantially reduced in magnitude for

[11] The full results are available on request.

male refugees: from 1.15 to 0.30 for *Pr(U)* and from 1.82 to 0.50 for *ln UDAYS*. Thus, the inclusion of industry trends, which seem to be negatively correlated with *ln MW*, is of vital importance to our quantitative results. Not including the industry trends thus causes a severe omitted variable bias. All estimates are significant at the 5 per cent level, except the one for *ln UDAYS* which is significant at the 10 per cent level.[12]

4.2 Heterogeneous effects across subgroups

We have included country of origin as a regressor in the estimations, but since the effects may be very different across these countries it seems warranted to proceed by estimating the preferred model, Heckman's two-step, by country of origin. Non-European refugees typically perform less well in the labour market than European refugees. Hence, we select the refugees from Iran, Iraq and the Horn of Africa and estimate the same models again.

As the first columns of Table 4 shows, Heckman's two-step model continues to yield significant results for male refugees. The elasticities of *ln MW* rise from 1.15 to 1.44 for *Pr(U)* and from 1.82 to 2.16 for *ln UDAYS*, thus yielding stronger effects than when refugees from all countries are included. For female refugees, only the estimates for *Pr(U)* remain significant, but still with the unexpected sign.

We proceed to examine if there are effects on refugees from these countries that differ for those having spent only a short time in Sweden. These estimates may reflect selection, to the extent that refugees vulnerable to unemployment are more prone to return migration, as well as increasing labour market opportunities over time, and we cannot distinguish between these mechanisms with our data. In the third and fourth columns of Table 4 we explore the effects of length of residence in Sweden by further restricting the sample to having spent less than 10 years in the country. For male refugees from Iran, Iraq and the Horn of Africa, the elasticity of *ln MW* on *Pr(U)* is now even larger, 1.56. The estimate for *ln UDAYS* increases further to 2.61. For females, a high and significant elasticity for *Pr(U)* of 2.21 obtains and we continue to see no effect on *ln UDAYS*. For refugees from other regions with the same length of residence, in columns 5-6, no significant effects are obtained.

[12] Excluding both spatial and industry trends yields significant elasticities of 0.36 and 0.61, for *Pr(U)* and *ln UDAYS*, respectively.

When estimating the model for male refugees, the estimates generally become larger the more narrowed the group is to those perceived to be the most vulnerable. Including all male refugees, the elasticity for *Pr(U)* is 1.15, rises to 1.44 when the sample is limited to refugees from Iran, Iraq and the Horn of Africa and increases further to 1.56 for the refugees from these countries having spent less than 10 years in the country. The corresponding estimates for *ln UDAYS* rise from 1.82 to 2.16 and to 2.61. For females, however, there are mainly no significant effects until we narrow the sample to females from Iran, Iraq and Horn of Africa having been in the country less than 10 years.

5. Concluding discussion

Our main conclusion is that minimum wages hamper the labour market integration of refugees in Sweden, a country with relatively high minimum wages by international standards. With a large number of observations on refugees and considerable variation in the variable of interest, i.e. the collectively agreed minimum wage, we have been able to also examine heterogeneous effects for several subgroups. In general, we find that unemployment of male refugees increases more than that of young natives. The elasticity of 1.82 for male refugees in our preferred specification, implies that a one per cent increase of the minimum wage raises unemployment by 3.5 days. The corresponding elasticity with respect to the probability of unemployment is 1.15, which translates into an increase from .2900 to .2933.

The effects are even more adverse as we limit our sample to refugees from Iran, Iraq and Horn of Africa having been less than 10 years in Sweden; the unemployment elasticity is then 2.61. The estimates for female refugees are in almost all instances small or insignificant. However, also here there appears to be heterogeneous effects in terms of origin country and time in the country. The estimated effect on probability of unemployment is 2.21 for females from Iran, Iraq and Horn of Africa having spent less than 10 years in Sweden.

The strong effects are consistent with employment statistics for refugees from these countries (SCB, 2009), indicating that the employment rate after 10 years in Sweden is as low as 35 per cent for those from Somalia and around 50 per cent for refugees from Iran and Iraq. For refugees from former Yugoslavia, the corresponding figure is much higher, 70 per cent. The larger estimates for refugees from Iran, Iraq and the Horn of Africa suggest that workers from these countries cluster in certain sectors, which could reflect their background in terms of education and work experience.[13] Another potential explanation is discrimination. Experimental studies based on fictitious job applications have revealed discrimination against job seekers with Arab-sounding names in Sweden (Carlsson and Rooth, 2007). However, it is not obvious why discrimination against refugees from non-European countries should increase with increasing minimum wages. A case for such a relationship can be made if minimum wages tend to reinforce discriminatory behaviour. To the extent that high minimum wages attract more skilled job applicants, employers with a latent taste for discrimination can then afford to be more selective in their recruitment of workers. Discrimination may also force highly educated refugees to look for low-skilled jobs, for which minimum wages are binding.

Our findings are basically robust to variations in estimation method. However, the findings suggest that omitted variable bias is an important concern. The inclusion of regional trends leaves our results basically unchanged. The fact that Sweden is a considerably smaller country than the US may explain why spatial heterogeneity matters little in our context. But heterogeneity in industry trends affect our estimates positively, which may be explained by Swedish minimum wages, unlike those in the US, are set at the industry level. Hence, the negative correlation between minimum wages and industry trends could generate strong downward bias if these trends are omitted.

The unemployment elasticities we find are considerably larger than those found in most previous studies, which, however, pertain to employment. This may partly be explained by the fact that we have focused on refugees, a group previously not examined. Our elasticities for native youngster are, however, also relatively large. An important factor behind our results may be the high level of minimum wages in Sweden. Adverse employment effects from these minimum wages have historically been mitigated by increases in educational attainment. However, for increasing numbers of low-skilled refugees, and high-skilled refugees not well matched with the Swedish labour market, the minimum wages may represent serious employment obstacles.

[13] A possible explanation for the larger estimates for refugees from non-European countries may be unobserved heterogeneity in human capital; for example, the fairly crude education variable may fail to truly reflect fewer years of schooling among refugees from these countries. Moreover, their education may be less well adapted to the Swedish labour market than that of refugees from European countries.

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Table 1. Descriptive statistics

Variable	Refugees[a]			Young natives[b]		
	Mean	Min.	Max.	Mean	Min	Max
Probability of unemployment	0.32	0	1	0.40	0	1
Days unemployed	64.6	0	366	51.91	0	366
Real minimum wage, SEK per month	11,831	10,577	15,733	11,963	10,577	15,733
Real median wage, SEK per month	17,596	11,712	36,364	17,511	11,712	36,364
Age	38.9	16	65	20.0	16	21
Years since immigration	15.3	0	61			
Industry:						
Engineering	0.295	0	1	0.145	0	1
Construction	0.039	0	1	0.117	0	1
Retailing	0.159	0	1	0.348	0	1
Hotel and restaurants	0.131	0	1	0.180	0	1
Local government	0.337	0	1	0.179	0	1
Bakeries	0.020	0	1	0.017	0	1
Slaughter-houses	0.018	0	1	0.012	0	1
Education:						
< 9 years	0.058	0	1	0	0	1
9-10 years	0.100	0	1	0.128	0	1
High school	0.533	0	1	0.827	0	1
Tertiary, < 2 years	0.044	0	1	0.034	0	1
Tertiary, ≥2 years	0.259	0	1	0.003	0	1
Doctoral	0.006	0	1	0	0	1
Country of origin:						
Iran	0.156	0	1			
Iraq	0.115	0	1			
Asia (excl. Iran and Iraq)	0.060	0	1			
Horn of Africa	0.056	0	1			
Chile	0.108	0	1			
Latin America (excl. Chile)	0.019	0	1			
Poland	0.116	0	1			
Bosnia-Hercegovina	0.201	0	1			
Eastern Europe (excl. Poland and B-H)	0.092	0	1			
Other countries	0.077	0	1			
N	301,200			649,010		

Notes: [a] Aged 19-64, including individuals who arrived for family reunification. [b] Aged 19-21, including labour immigrants. The observation period is 1998-2007. The collectively agreed minimum wages refer to rates for low-skilled, blue-collar workers without experience and vary with industry and year, while median wages, calculated for all workers aged 35-50, vary with region, industry and year. Both wage variables are expressed in 1998 prices. The Horn of Africa includes Eritrea, Ethiopia, Somalia and the Sudan.

Table 2. Tobit model and the Two-part model. Refugees and young natives

		Tobit model				Two-part model			
Dependent variable	Independent variable	Refugees		Young natives		Refugees		Young natives	
		Male	Female	Male	Female	Male	Female	Male	Female
Pr(U)	ln MW	0.4002***	-0.0671	-0.0266	0.5900***	1.1453***	-0.2008	-0.0584	2.4283***
		(3.73)	(-0.54)	(-0.32)	(6.10)	(3.08)	(0.51)	(0.42)	(7.95)
ln UDAYS	ln MW	2.0327***	-0.3393	-0.1107	2.6278***	1.8700***	0.2771	1.0095***	0.2389
		(3.3)	(-0.54)	(-0.32)	(6.10)	(3.64)	(0.50)	(2.86)	(0.68)
	AGE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	AGE^2	Yes	Yes	Yes	Yes	No	No	No	No
	YSIM	Yes	Yes	No	No	Yes	Yes	No	No
	$YSIM^2$	Yes	Yes	No	No	No	No	No	No
	COUNTRY	Yes	Yes	No	No	Yes	Yes	No	No
N		152,029	149,171	301,240	347,770	152,029	149,171	301,240	347,769
N-uncensored						44,562	52,553	104,945	154,894
R^2 (OLS)						0.09	0.11	0.06	0.08

Notes: The estimation period is 1998-2007. All regressions include the median wage, the regional unemployment rate and dummies for education (5), region (20), industry (6) and year (8), as well as trends interacted with the region and industry dummies, respectively. Absolute t-values, robust to heteroskedasticity, in parentheses.
* denotes significance at the 10 per cent level, ** at 5 per cent, and *** at 1 per cent;

Table 3. Sample selection model and Heckman's two-step model. Refugees and young natives

		Sample selection model				Heckman's two-step model			
Dependent variable	Independent variable	Refugees		Young natives		Refugees		Young natives	
		Male	Female	Male	Female#	Male	Female	Male	Female
Pr(U)	ln MW	1.0936***	-0.2188	-0.0739	2.4237***	1.1453***	-0.2008	-0.0584	2.4283***
		(2.94)	(0.56)	(-0.28)	(8.11)	(3.06)	(0.51)	(-0.22)	(7.95)
ln UDAYS	ln MW	1.9137***	0.3104	1.0028***	0.4162	1.8205***	0.2551	0.8172**	1.2852***
		(3.55)	(0.56)	(2.88)	(1.21)	(3.19)	(0.46)	(1.99)	(3.10)
	AGE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	AGE^2	No	No	No	No	No	No	No	No
	YSIM	Yes	Yes	No	No	Yes	Yes	No	No
	$YSIM^2$	No	No	No	No	No	No	No	No
	COUNTRY	Yes	Yes	No	No	Yes	Yes	No	No
Mill's lambda						-0.0550	0.2371	0.6891	0.4640
						(0.26)	(1.44)	(4.55)***	(6.40)***
N		152,029	149,171	301,240	433,517	152,029	149,171	301,240	347,770
N-uncensored						44,562	52,553	104,945	154,894

Notes: # Due to non-convergence for the age group 19-21, the estimates refer to the age group 16-21.See also notes to Table 2.

Table 4. Heckman's two-step model. Refugees

Dependent variable	Independent variable	From Iran, Iraq, Horn of Africa				From other countries	
		All		Less than 10 years in Sweden			
		Male	Female	Male	Female	Male	Female
Pr(U)	ln MW	1.4439**	-1.1793*	1.5607*	2.2105*	0.5969	-1.9670
		(2.49)	(1.68)	(1.67)	(1.78)	(0.43)	(1.48)
ln UDAYS	ln MW	2.1648**	1.2822	2.6091**	0.5116	0.8292	-1.4040
		(2.58)	(1.23)	(2.15)	(0.31)	(0.43)	(0.75)
	AGE	Yes	Yes	Yes	Yes	Yes	Yes
	AGE^2	No	No	Yes	Yes	No	No
	YSIM	Yes	Yes	Yes	Yes	Yes	Yes
	$YSIM^2$	No	No	Yes	Yes	No	No
Mill's lambda		0.4681	-0.2561	-0.2816	-0.4316	0.5055	0.7210
		(1.60)	(0.92)	(0.54)	(0.78)	(0.80)	(1.52)
N		53,371	44,395	18,965	14,071	22,896	19,958
N-uncensored		20,391	19,735	9,941	8,151	7,852	10,443

Notes: Dummies for country of origin are included in all regressions. See also notes to Table 2.