Spoor A3:

Houses and/or jobs: ownership and the labour market in Belgian districts

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WORKING PAPER

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Abstract

A.J. Oswald argues that high rates of home ownership may imply inferior labour market outcomes. Using a panel of 42 Belgian districts since the 1970s and accounting for other key determinants of employment, this paper confirms the Oswald hypothesis. A 1 percentage point rise in the rate of ownership in a district implies a statistically significant fall in the employment rate by about 0.35 percentage points. This negative effect declines in the fraction of high-skilled in a district. Our results underscore the importance of controlling for unobserved district-specific fixed effects and common time effects, and of appropriately dealing with endogeneity.

JEL classification: E24, R23

Keywords: employment, home ownership, Oswald hypothesis, Belgian districts, panel data

1. Introduction

Throughout recent history, governments in many countries have encouraged home ownership. Ownership is seen as a secure way for the population to accumulate assets. Moreover, ownership generates significant social benefits. Owners are more likely to have long residence spells, which contributes to local neighbourhood stability and to the accumulation of social capital (DiPasquale and Glaeser, 1999; Rohe et al., 2002; Dietz and Haurin, 2003; Engelhardt et al., 2010). Renters do not bring about the same returns due to their higher degree of geographical mobility. From a labour market perspective, however, rising degrees of home ownership are much more controversial. Home ownership may restrict geographical mobility, and imply inferior labour market outcomes, both for the individual and in the aggregate. Oswald (1996, 1997a,b,c) was among the first to advance this argument. If demand for labour falls in a region, home owners will be less inclined to move to more prosperous regions mainly due to high costs of selling and buying homes. Renters by contrast can move at much lower cost. In equilibrium, higher degrees of home ownership imply higher unemployment. Empirically, Oswald's evidence in favour of his hypothesis relies mainly on cross country macroeconomic data, and on aggregate data for regions within individual countries. Oswald (1996) observed higher unemployment rates in OECD countries with a higher fraction of owners (versus renters). Also, he found that since the 1970s the unemployment rate increased most in those countries with the strongest growth in the rate of ownership. According to his results, an increase of the rate of ownership with 10 percentage points causes an increase of the unemployment rate with 2 percentage points.

The Oswald hypothesis has provoked a large body of theoretical and empirical work. Coulson and Fisher (2009) show in a survey that a change of theoretical assumptions may generate results that differ from Oswald's, both for individual home owners and for the aggregate labour market. Empirically, a wave of studies has not settled the issue, although it may be possible to observe some structure in the results. Studies using microeconomic data very often challenge the Oswald hypothesis, and find home owners to have better employment positions (e.g. Coulson and Fisher, 2002, 2009; Robson, 2003; van Leuvensteijn and Koning, 2004; Munch et al., 2006, 2008). Studies using macroeconomic data are more often in line with Oswald (e.g. Partridge and Rickman, 1997; Pehkonen, 1997; Nickell, 1998; Nickell et al. 2005; Cochrane and Poot, 2007), although various researchers obtain dissident or insignificant results (Flatau et al., 2002; Barrios García and Rodríguez Hernández, 2004, Coulson and Fisher, 2009). Overall, it is difficult however to draw convincing conclusions from these macro studies due to their imperfect or limited econometric setup.

This paper tests the Oswald hypothesis in a panel of 42 Belgian districts ('arrondissements') since the 1970s[1]. Along the time dimension we have data for six years between 1970 and 2005. Our dependent variable is the employment rate, the fraction of working age population with a job. Our approach and data availability are such that this may be the first paper to avoid three limitations in existing empirical macro studies. *First*, many macro studies lack data along the time dimension, which makes it impossible to control for unobserved fixed effects, and which may lead to seriously biased estimates. The availability of data since the 1970s enables us to control for fixed effects. Moreover, it allows us to include in this study the periods with most labour market turbulence since the Second World War, and to embed the Oswald hypothesis in a broader model including various other determinants of employment like labour costs and productivity, skill level of the population and demographic variables. If the time dimension is short, and data availability limited, it clearly becomes difficult to estimate the effects of only slowly changing variables like the rate of ownership, the skill level, and demography. Only Oswald (1996, 1997a, 1997b), Partridge and Rickman (1997), Green and Hendershott (2001) and Nickell et al. (2005) exploit data for the 1970s and the 1980s. *Second,* most existing macro studies neglect the possibility of reverse causality. Yet, due to the potential influence of employment in a region on permanent income and tenure choice of households in that region, ownership may be endogenous to changes (shocks) in employment. If not dealt with, positive correlation between shocks to employment and the rate of ownership may bias the estimated Oswald effect upwards. Empirically, this would impose the use of IV techniques. We employ these in this paper. Barrios García and Rodríguez Hernández (2004), Cochrane and Poot (2007) and Coulson and Fisher (2009) are the only studies we know to have used IV-methods before. *Third*, observing the Oswald effect is (only) one thing. Another may be to understand what determines its size, and economic significance. Our relatively large panel along both the cross-sectional and time dimension allows us to test various interaction effects which may shed light on this. We test the role of structural geographic and schooling related variables. Among these variables are the proximity of a border (country or language border), population density, the skill level of the population, etc.

Our main findings are as follows. We find evidence confirming Oswald's hypothesis for Belgium. We observe that a 1 percentage point rise in the rate of home ownership in a district implies a statistically (and economically) significant fall in the employment rate by about 0.35 percentage points. Our results show the importance of controlling for both cross-sectional fixed effects and common time effects. If we do not do this, the estimated Oswald effect can be totally different, highly insignificant, close to zero and sometimes even positive. Our results also demonstrate that the estimated Oswald effect may be biased when endogeneity of home ownership is disregarded. Not using IV techniques, we find a much smaller (less negative) Oswald effect. As to the determinants of the size of the Oswald effect, we find that it falls in the fraction of high skilled in a district. We also obtain indicative results that the Oswald effect may be stronger in districts closer to borders, and in districts farther away from major cities and centres of economic activity, but these findings are not statistically significant.

Our main result in favour of Oswald's hypothesis survives various robustness checks. These concern changes in the imposed functional form of the relationship between ownership and employment, changes in our panel along the time dimension, and changes in the dependent variable. Changing our focus to unemployment rather than employment, does not affect our main conclusion.

Among our other results, we observe negative effects on the employment rate in a district of the ratio of wage costs to productivity, and (insignificant) positive effects of the fraction of high skilled. We also see a (time-varying) influence of some demographic variables, like the age structure of the population. Often, however, this influence is not statistically significant either.

In the following section of the paper we briefly review the existing theoretical and empirical literature on the relationship between housing and jobs. The third section describes our econometric model. Our analysis of equilibrium employment is situated within the New Keynesian competing claims approach developed mainly by Layard et al. (1991). Here we also take into account specific characteristics of wage setting in Belgium. To define instruments for the rate of home ownership in the employment regression we build on the literature on tenure choice initiated by Rosen (1979) and Rosen and Rosen (1980). The fourth section describes our dataset. The fifth section presents the results of our econometric analysis. We summarize our main findings in the final section.

2. Home ownership and employment: a brief review of the literature

(Un)employment rates differ widely across regions in most countries, including Belgium. Geographical mobility can be a vigorous instrument to eliminate these differences by shifting labour supply from high to low unemployment regions. Theoretically, higher wages or a higher probability to find a (suitable) job in prosperous areas could bring about this shift. Empirical evidence shows that the latter is the most important motivation for workers to be mobile (Blanchard and Katz, 1992; Böheim and Taylor, 2002). However, whether an economic agent decides to work in another region depends not only on expected benefits. Moving also generates costs: search and transaction costs when selling and buying a house, commuting costs, costs to overcome cultural or language barriers, personal costs when leaving familiar surroundings, etc.

Oswald (1996, 1997a,b,c) emphasizes the negative effects of home ownership on geographical mobility and labour market performance. Oswald (1997c) describes a perfectly competitive economy with two separate locations that are joined by a road. People have to live in one of them, either as owner or as renter. Each location experiences real shocks to labour demand. Tenure choice is made before these shocks are revealed. When their region is hit by a bad shock, renters can move to the other region at no cost. Owners in the bad region either remain unemployed and accept unemployment benefits, commute to the better region at a commuting cost, or pay a fixed (high) transaction cost and move. The commuting cost rises in the number of commuters. At some number of commuters this cost becomes equal to the transaction cost of moving. Due to commuting or moving costs, owners will have a higher reservation wage for jobs in the other location. As a result, labour supply to each location is horizontal at a low level of wages up to the number of owners in that location and the total number of renters in the economy[2]. It then becomes upward sloping as higher wages will be necessary to induce (rising numbers of) owners from the other location to commute. Labour supply becomes horizontal again when commuting costs have risen to the level of the transaction cost of moving. Everyone is willing to work in a good region at a wage that covers both the unemployment benefit and moving costs. In the end the position of the labour demand curve determines equilibrium quantities and wages. Given the competitive nature of the labour market, owners and renters receive the same wage offers. Due to their higher reservation wage at distant jobs, however, owners are more likely to be unemployed. Renters are fully employed. Furthermore, at the aggregate level, higher degrees of home ownership imply a leftward shift of the upward sloping part of the labour supply curve. Lower equilibrium employment, higher unemployment and higher wages are the result.

Oswald’s arguments may be strengthened by a number of complementary considerations. First, if long distance commuting contributes to traffic congestion, overall production costs may rise, which further undermines employment. Hymel (2009) provides empirical proof of congestion damping employment growth in U.S. metropolitan areas. Furthermore, if the overall promotion of home ownership undermines the development of a well-functioning rental market, it will also be more difficult for unemployed renters to move to other regions (Oswald, 1999). Traffic congestion and a tight rental market imply that the disadvantages of home ownership are not necessarily concentrated in the segment of owners.

Nickell (1998) and Nickell and Layard (1999) embed Oswald’s argument in an imperfectly competitive macro model of the labour market. In this model equilibrium (un)employment reconciles competing claims of wage and price setters. Any factor which raises targeted price or wage mark-ups will imply higher equilibrium unemployment. An important determinant of the price mark-up is the degree of product market competition. Wage mark-ups depend on the unemployment benefit system, union power and the characteristics of wage bargaining, labour taxes, etc. Ownership is important in this setup as a determinant of wage pressure. Following Oswald, rising rates of ownership imply reduced mobility and search effectiveness among the unemployed. The employed can then claim a higher wage mark-up. Ownership may also raise the mark-up of prices on wages because non-wage costs may rise: hiring costs (if it becomes more difficult to fill in vacancies), congestion costs, etc. Overall, equilibrium employment will fall.

More recent theoretical work has reconsidered and/or extended Oswald's assumptions and conclusions. Dohmen (2005) basically confirms Oswald's results but emphasizes the role of education and skills. Workers only move to another region in Dohmen's model when the wage in that region exceeds the unemployment benefit and the cost of changing location. Since the latter cost is higher for home owners, owners will be less mobile and face higher unemployment, as in Oswald. Rising ownership rates then go along with inferior labour market performance. Skill differences may however disturb this simple pattern. High skilled workers earn high wages, which exceed the unemployment benefit plus relocation cost. As a consequence, the skilled may both be owner and mobile. Their mobility raises their chances to find a job. The low skilled, however, earn wages below the sum of the unemployment benefit and the cost of changing location. As a consequence, when a low skilled owner loses his job, he will not move, and remain unemployed. Low skilled renters by contrast remain mobile. The implication of Dohmen's model for empirical work is important. When testing the relationship between ownership and labour market outcomes, it is crucial to control for skill levels, i.e. to keep skills constant in the regression. Furthermore, above a certain skill level, there need not be any relationship between ownership and employment.

Munch et al. (2006) raise another argument which may undermine the Oswald hypothesis. Due to high costs of moving, owners will not only have a higher reservation wage for distant jobs, they will also have a lower reservation wage for local jobs. It is therefore possible that rising ownership goes along with higher employment, but then this should be at lower wages. Brunet and Havet (2009) confirm this idea for French workers. Home owners in their study are more wage downgraded (and feel more overeducated) than renters. In line with this, Rouwendal and Nijkamp (2006) find empirical prove for lower geographical mobility of home owners but also for higher exit rates from unemployment. The latter is due to more intensive search activity and faster acceptance of jobs on the local labour market, especially by highly leveraged owners. Munch et al. (2008) add that increased willingness to accept local jobs need not imply lower actual wages. Immobility may cause owners to invest more in their local jobs, increasing firm-specific productivity. The establishment of a long-term employment relationship may also raise the incentive for firms to train their workers-owners.

Coulson and Fisher (2009) discuss the Oswald hypothesis within a model of search and bargaining in the style of Pissarides (1990). Owners face higher unemployment than renters in this model because they search on a smaller scale. Because their search is narrower, owners have less bargaining power, which implies that firms can make them work at lower wages. The latter effect is important because it implies that an aggregate rise in ownership reduces expected wages and raises expected profits for firms. Higher expected profits may cause new firms to enter. Under certain assumptions this favourable entry effect may dominate the unfavourable (standard) composition effect according to which an increase in the number of (immobile) owners undermines overall labour market performance.

Theoretical ambiguity underscores the relevance of empirical work on the Oswald hypothesis. Empirical studies do not settle the issue, however, certainly not when it comes to aggregate effects. Among studies that make use of micro data one can observe some degree of consensus. Most of these studies find home owners to have a better employment status than renters (see e.g. Coulson and Fisher, 2002, 2009, for the US; Robson, 2003, for the UK; van Leuvensteijn and Koning, 2004, for the Netherlands; Munch et al., 2006, 2008, for Denmark). Owners are in general less mobile than private renters (Caldera Sánchez and Andrews, 2011). However, when they lose their job, this does not necessarily imply longer unemployment spells. Battu et al. (2008) observe similar unemployment durations for home owners and private renters in the UK. Munch et al. (2006) find home owners in Denmark to have even shorter unemployment spells due to a lower reservation wage to local jobs.

Empirical studies using macro data cannot confirm the message emanating from the (more or less) micro consensus. Many macro studies confirm the Oswald hypothesis that a rise in the rate of home ownership goes along with inferior labour market results (see Table 1 for an overview). The question is how strong and robust this finding is. On the one hand, a contradiction between micro and macro findings is perfectly possible. As we have mentioned before, rising degrees of home ownership may cause negative effects (congestion, tightening of rental markets, bargained wage pressure,…) beyond the owners themselves. Even if they are not worse off, aggregate labour market performance may be weaker. Clearly, the aggregate story is important for policy makers. On the other hand, many macro studies may be challenged on methodological grounds. Our summary in Table 1 reveals that many studies have only one observation along the time dimension which makes it impossible to control for fixed regional/country effects. This also makes it more difficult to embed the Oswald hypothesis in a broader model explaining (un)employment, where also differences in wages and productivity, skills, demography, etc. have their role. Furthermore, only Barrios García and Rodríguez Hernández (2004), Cochrane and Poot (2007) and Coulson and Fisher (2009) control for endogeneity of home ownership by means of IV methods. Yet, both theoretically and empirically, housing tenure choices have been found to be determined also by one's employment prospects and permanent income (e.g. Rosen and Rosen, 1980; Henley, 1998; van Leuvensteijn and Koning, 2004). Neglecting the possibility of reverse causality could bias the estimates. In the next sections we try to overcome these limitations in an empirical macro study for Belgium.

Table 1. Empirical studies of the Oswald hypothesis using macro data.

Study	Regions or countries (# cross-sections)	Time dimension	Methodology	Oswald?
Oswald (1996)	US states (51) UK regions (13)	1986-95, annual 1973-94, annual	Panel data. Fixed effects OLS with time dummies. Bivariate (+ lags), levels.	yes
	regions within France (22), Italy (20) and Sweden (8)	1990s (1 observation)	Correlation between levels in unemployment and ownership, bivariate	yes
	cross-section of countries (11, 18)	1960s (1 observation), 1990s (1 observation)	Correlation between levels in unemployment and ownership, bivariate	yes
Oswald (1997a)	US states (51)	change between 1970s and 1990s (1 observation)	Correlation between changes in unemployment and ownership, bivariate	yes
Oswald (1997b)	regions within OECD countries	1990s (1 observation)	Correlation between levels in unemployment and ownership, bivariate	yes (a)
Partridge and Rickman (1997)	US states (48)	1972-1991, annual	Panel data. Pooled OLS / Fixed Effects OLS with year dummies, multivariate	yes
Pehkonen (1997)	regions within Finland (13)	1991 (1 observation)	OLS, bivariate, multivariate	yes (b)
Nickell (1998)	OECD countries (20)	Mid 1980s and early 1990s (1 observation per period)	Panel data. Random effects GLS. Multivariate.	yes
Hassink en Kurvers (2000)	regions within the Netherlands (COROP) (40)	1990-1998, annual	Panel data. Pooled OLS / Fixed effects OLS, deterministic time trend. Bivariate (+ lags), levels.	no
Green and Hendershott (2001)	US states (51)	change between 1970 and 1990 (1 observation)	Bivariate regression, WLS, different unemployment rates (age groups)	yes/no (c)
Flatau et al. (2002)	Regions (LGA) within Australia (590)	1986, 1991, 1996, 2001 (4 observations)	Multivariate regression, WLS, separate regressions per year.	no
Glaeser and Shapiro (2003)	US Metropolitan Statistical Areas	1998 (1 observation)	Correlation between levels in unemployment and ownership, bivariate	no
Barrios Garcia and Rodriguez Hernandez (2004)	regions within Spain (46)	1991 (1 observation)	Cross section. Multivariate, Simultaneous equation system explaining unemployment and home ownership, 3SLS	no
Nickell et al. (2005)	OECD countries (19)	1961-1995, annual	Panel data. Fixed Effects GLS with year dummies, multivariate.	yes
Bassanini and Duval (2006)	OECD countries (21)	1982-2003 (1 observation)	Bivariate regression of the fixed country effect in a panel study of unemployment on home ownership.	yes
Cochrane and Poot (2007)	regions within New Zealand (58)	1986, 1991, 1996, 2001 (4 observations)	Panel data. Pooled OLS / Fixed Effects OLS / Hausman Taylor estimator. Multivariate.	yes
Coulson and Fisher (2009)	US Metropolitan Statistical Areas	1990 (1 observation)	OLS and 2SLS, multivariate.	no
Lerbs (2011)	regions within Germany (87)	1998, 2002 and 2006 (3 observations)	Separate regressions per year (OLS), Panel data: pooled OLS, fixed effects OLS.	yes/no (d)

Notes:
(a) except for Belgium, Netherlands and West Germany.
(b) significant only in the bivariate case
(c) no for young and older households, yes for middle aged
(d) no for OLS and pooled OLS, yes for fixed effects

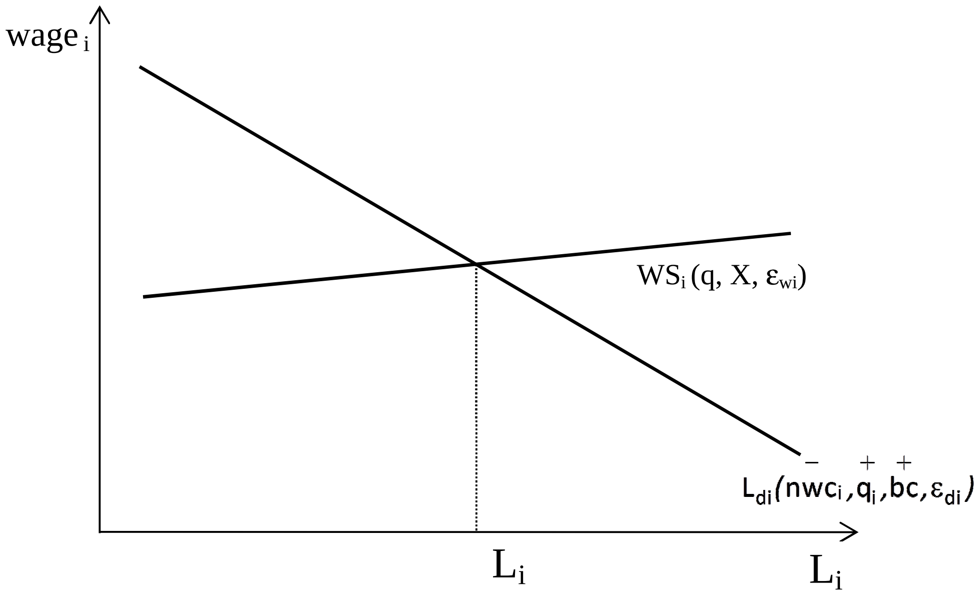
3. Econometric model and methodology

We now discuss our empirical specification for the employment rate and some methodological considerations, which guide our analysis in the next sections. We define the employment rate as the fraction of all people at working age living in a district who have a job. Our setup is mainly inspired by Layard et al. (1991) and Nickell and Layard (1999). Their approach to model the determination of wages and employment corresponds most closely to the Belgian situation. We rely on Oswald (1997c) and some of the literature that we summarized in the previous section when it comes to the effects of changes in ownership on employment.

Empirical specification

Starting point of our discussion is Figure 1, describing the labour market and the determination of the equilibrium number of jobs in district i. The latter is obviously a key determinant of the employment rate among the people at working age living there. It need not be the same, however, since people may commute to work in a different district. The equilibrium number of jobs in the district (L_i) is determined at the intersection of the labour demand curve (L_{di}) and the wage setting curve (WS_i). Labour demand falls in the real wage per worker ($wage_i$), including taxes on labour. For a given real wage, labour demand is negatively affected by real non-wage production costs (nwc_i) and positively by labour productivity (q_i). Business cycle and other aggregate labour demand shocks are captured by bc, district-specific demand shocks by ε_{di}. The wage setting curve (WS_i) indicates bargained real wages. It is flat since wages in a district are only very weakly affected by local employment conditions. Wages in Belgium are mainly bargained at the sectoral level, often within a nationally imposed range. The coverage rate of collective bargaining exceeds 90% (OECD, 2004). Wages will therefore mainly reflect sectoral and national variables, like sectoral or aggregate labour productivity (q) and overall wage push variables (X). The latter include union power, unemployment benefits, the tax wedge, etc. As mentioned before, Nickell (1998), Nickell and Layard (1999) and Nickell et al. (2005) also see a role for aggregate ownership here. The role for local factors shifting the WS-curve (ε_{wi}) will be very small.

Figure 1. Employment (number of jobs) and wages at the district level



Equation (1) puts these theoretical considerations into a workable econometric specification for the employment rate in district i and year t. As we indicate below the equation, our dataset contains observations for 42 districts over 6 years between 1970 and 2005 (see next section).

$$\mathrm{Empl}_{it} = \delta_i + \gamma_1\, \mathrm{OWN}_{it} + \gamma_2\, \mathrm{Schooling}_{it} + \gamma_3 \log(\mathrm{wage}_{it}) + \gamma_4 \log(q_{it}) + \gamma_5\, \mathrm{Age1524}_{it}$$

$$+ \gamma_6\, \mathrm{Age5564}_{it} + \gamma_7\, \mathrm{DFlanders90}_{it} + \lambda_t + \upsilon_{it} \qquad (1)$$

with : $i = 1, \ldots, 42; \quad t = 1970,\ 1977,\ 1981,\ 1991,\ 2001,\ 2005.$

The parameters γ_3 and γ_4 measure the effects on the employment rate in percentage points of a 1 percent increase in the real wage (wage_{it}) and labour productivity (q_{it}) respectively. We expect γ_3 to be negative and γ_4 to be positive. The main reasons to have the rate of ownership (OWN_{it}) in Equation (1) – for given wages and productivity – follow from our discussion in the second section. They are as follows. First, ownership may affect non-wage labour costs for firms in a district (nwc_{it}) due to an increase in traffic and congestion costs. Labour demand may shift to the left. Second, the rate of ownership may affect the reservation wage, search intensity and overall mobility of inhabitants in the district. Owners may have a lower reservation wage, and search more intensively, for local jobs. Given the nature of wage bargaining in Belgium, the influence on wages is likely to be very small. The probability for firms to fill vacancies, however, may rise, which brings down non-wage labour costs (hiring costs), and promotes employment. The third effect of ownership concerns the employment rate (Empl_{it}) for a given number of jobs in the district (L_{it}). As argued by Oswald (1997c), owners also have a higher reservation wage for distant jobs. For a given level of wages in other districts, they may therefore have a higher probability than renters to be unemployed. The aggregate employment rate in their own district will then fall, as a smaller fraction of the population will have a job. Which of all these effects from ownership is dominant, and therefore the sign of γ_1, remains an empirical issue.

Other determinants of the employment rate in Equation (1) are the skill level of the population ($\mathrm{Schooling}_{it}$), two demographic variables ($\mathrm{Age1524}_{it}$, $\mathrm{Age5564}_{it}$) and a separate dummy for all districts in one region (Flanders) since 1990 ($\mathrm{DFlanders90}_{it}$). As we explain below, we measure the skill level by the fraction of people with a tertiary degree. Productivity already being controlled for by including q_{it}, 'Schooling' mainly captures the idea raised by Dohmen (2005). For a given share of owners in a district, overall mobility of the population and expected employment rates will rise when skill levels are higher. High skilled workers are better able to bear commuting costs, for example. As demographic variables we consider the share of two specific age groups among the population. Our selection of groups reflects well-known differences (in all OECD countries) in labour market participation and unemployment rates among young, prime-age and older workers. We expect the employment rate in a district to be lower when the fractions of the youngest people (age 15-24) and the older people at working age (age 55-64) rise, implying negative signs for γ_5 and γ_6 [3].

The introduction of a separate Flemish regional dummy since 1990 (DFlanders90) captures the possible effects from constitutional reform in Belgium. Since the end of the 1980s the Flemish and Walloon regions have gained much more autonomy in the area of economic policy, including important aspects of labour market policy (e.g. public employment services and training of the unemployed). The parameter γ_7 measures differential effects for all districts in the Flemish region. Finally, we control for district-specific fixed effects (δ_i) and common time effects (λ_t). The latter capture the effects of common labour demand shocks (e.g. aggregate business cycle effects, oil shocks). Idiosyncratic shocks will show up in υ_{it}.

Next to its basic specification, we estimate in the fifth section two extended versions of Equation (1). In a first extension, we allow for time variation in the effects of the demographic age groups. Extension of compulsory education from the age of 14 to 18 in Belgium since 1983 for example may induce lower employment for a given demographic structure. Employment rates may also be affected when preference for leisure or non-employment benefit regimes evolve differently across age groups. In this respect, the increased possibility to retire early since the end of the 1970s may explain lower employment rates among older workers in the second half of our period of study. Our second extension aims to shed more light on the determinants of the size of the Oswald effect. To that aim we introduce in Equation (1) a number of interaction terms γ_{11} $VAR*OWN_{it}$, where VAR is a variable that may affect the size of the Oswald effect. This variable may vary along the time dimension or the cross-sectional dimension. Variables that we have in mind are the skill level of the population, population density, the proximity of a country or language border, and the proximity of a major centre of economic activity.

Methodological considerations

Methodologically, it is obvious from Figure 1 and from the literature on the determinants of ownership that instrumental variables techniques will be necessary to estimate Equation (1). Figure 1 reveals the endogeneity of real wages in a district to district-specific shocks in labour demand. Positive shocks will push up wages, and induce correlation between υ_{it} and $wage_{it}$. Given the above mentioned characteristics of wage formation in Belgium, reflected in the flat slope of the WS-curve, this kind of endogeneity is most likely very small, but it will not be zero. Furthermore, any labour demand shock affecting employment and the error term in Equation (1) may also feed through in district-specific productivity q_{it}. As to ownership, its endogeneity is clear from work on tenure choice by e.g. Rosen (1979) and Rosen and Rosen (1980). Micro tenure choice is commonly modelled as a function of the relative cost of living as an owner versus living as a renter, household permanent income, and a number of social and demographic characteristics of the household. The employment situation being a key determinant of permanent income, the proportion of home owners in the population is logically affected by the (un)employment rate (see also Di Salvo and Ermisch, 1997; Barrios García and Rodríguez Hernández, 2004). Finally, also 'Schooling' may be endogenous to shocks in employment. The literature for example provides ample empirical evidence that schooling is counter-cyclical (e.g. DeJong and Ingram, 2001; Heylen and Pozzi, 2007). Positive shocks to employment may pull young people out of education and into work, and vice versa. We discuss our choice of instruments in the fifth section.

Another methodological issue follows from the spatial dimension of our dataset and the possibility of spatial autocorrelation. If significant, we would need to take this into account in our estimation. To test for spatial effects, we computed Geary's C statistic on the dependent variable as well as on the residuals for each single year *t*. We could not reject the null hypothesis of no spatial autocorrelation[4].

4. Data

We use macro data at the level of Belgian districts. Because of some difficulties in data consistency, and because of its different nature, we have omitted the Brussels district. This leaves us with 42 cross-sections, 22 in Flanders and 20 in the Walloon Region (see Appendix A). As to the time dimension, we are limited to the years in which a census or a large-scale survey has taken place. The years in our database are 1970, 1977, 1981, 1991, 2001 and 2005. Since in 2005 the survey only took place in Flanders, we are left with a panel of 232 observations. In this section we describe our data. We summarize the main descriptive statistics of all variables in Table 2.

Table 2. Main descriptive statistics of the variables

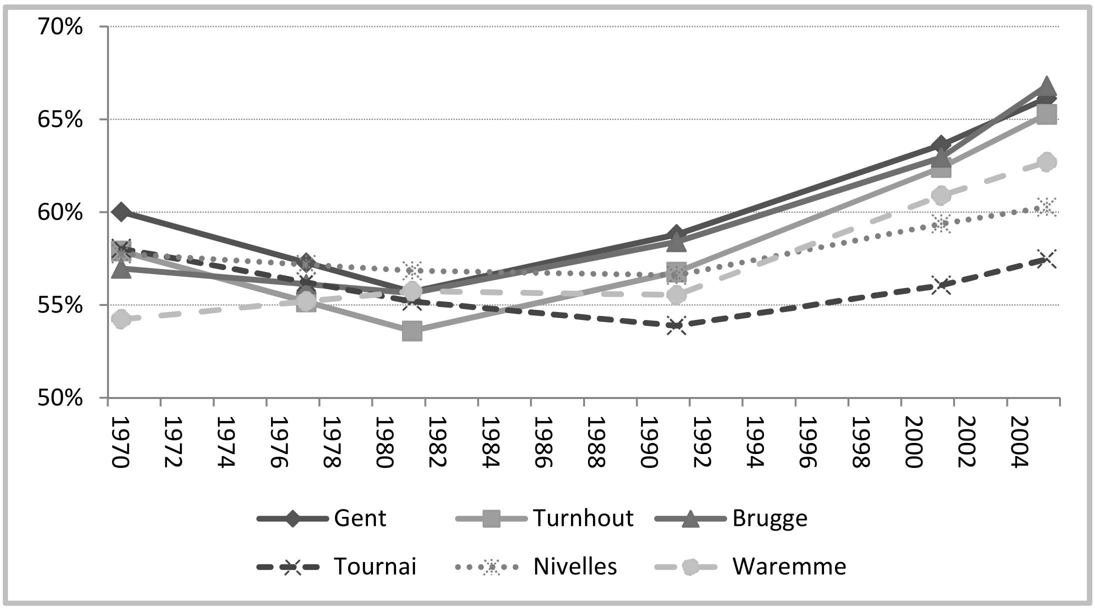
-	EMPL	OWN	SCHOOLING	WAGE	PRODUCTIVITY (q)	WAGE GAP	AGE1524	AGE2554	AGE5564
Overall Mean	57.2%	69.7%	14.5%	1.505	1.286	1.198	14.3%	39.5%	10.8%
Minimum	44.0%	44.3%	5.3%	0.698	0.601	0.891	10.7%	32.7%	7.1%
First quartile	54.0%	64.7%	9.0%	1.279	0.963	1.100	12.6%	37.2%	10.3%
Median	56.8%	70.8%	12.5%	1.582	1.175	1.191	14.5%	39.5%	11.0%
Third quartile	60.0%	75.5%	20.0%	1.766	1.528	1.346	15.7%	41.7%	11.4%
Maximum	70.4%	83.3%	38.7%	2.298	2.424	1.518	20.6%	45.0%	14.9%
Std. Dev.	4.8%	7.8%	6.7%	0.381	0.413	0.160	1.9%	2.9%	1.0%
Between Std. Dev.	3.6%	6.7%	2.5%	0.156	0.214	0.121	1.8%	2.7%	0.7%
Within Std. Dev.	3.2%	4.1%	6.2%	0.348	0.354	0.106	0.7%	1.1%	0.7%
1970 mean	56.6%	63.8%	8.3%	0.882	0.803	1.108	14.0%	39.3%	11.0%
1977 mean	55.2%	68.8%	9.2%	1.320	1.012	1.319	14.5%	39.2%	10.9%
1981 mean	54.4%	68.4%	9.7%	1.433	1.095	1.320	14.7%	39.5%	10.5%
1991 mean	55.7%	71.2%	14.9%	1.663	1.405	1.198	14.4%	39.9%	10.8%
2001 mean	59.4%	72.9%	21.2%	1.857	1.654	1.146	14.0%	40.0%	11.0%
2005 mean	61.9%	75.7%	23.7%	1.876	1.746	1.099	14.4%	39.2%	10.7%
Observations	252	232	252	252	252	252	252	252	252

Source: see Appendix B. Note that the data for OWN in 2005 only include Flemish districts.

Figures 2 to 5 show the evolution of important variables graphically. To bring some structure - it is not practical to show data for all 42 districts - we select those Flemish and Walloon districts that are at the 20th, the 50th and the 80th percentile when ranked from low to high according to the change in the employment rate since 1970. So, these are relatively weak, median and relatively strong performers when it comes to change in the employment rate. In Flanders these districts are respectively Gent, Turnhout and Brugge, in Wallonia Tournai, Nivelles and Waremme. For a detailed description of the construction of our data and their sources, we refer to Appendix B.

Figure 2 shows the evolution of the employment rate. We observe a fall in about all districts during the 1970s. In Wallonia employment continues to decline on average during the 1980s, whereas in Flanders it then recovers. During the 1990s and 2000s most Belgian districts show rising employment rates.

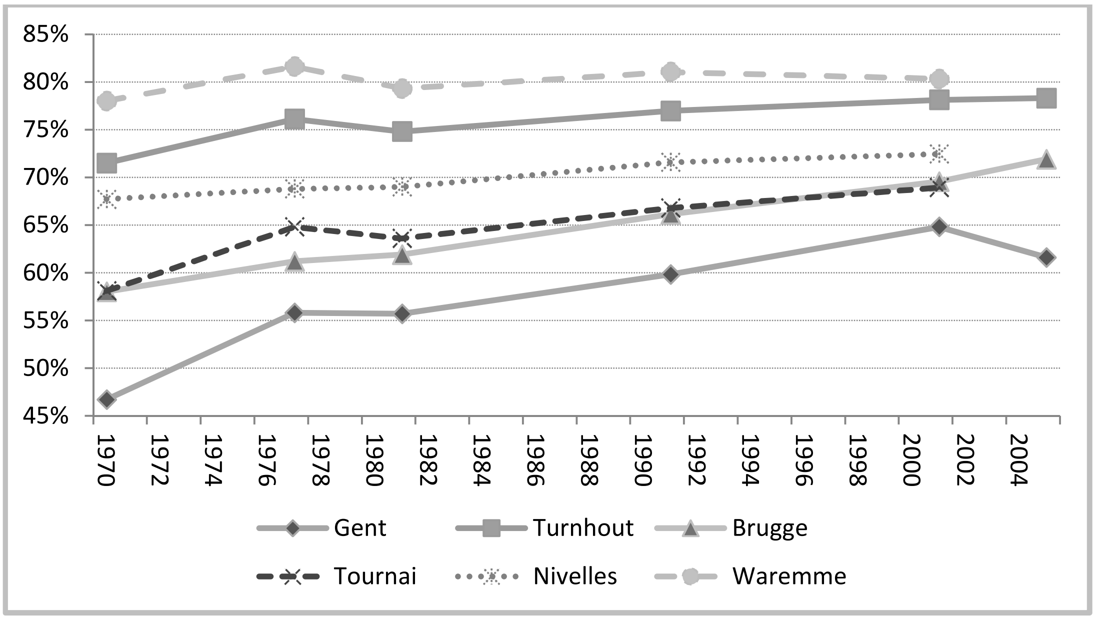
Figure2. Employment rate among 15-64 year olds living in the district (Empl)



Source: Appendix B.

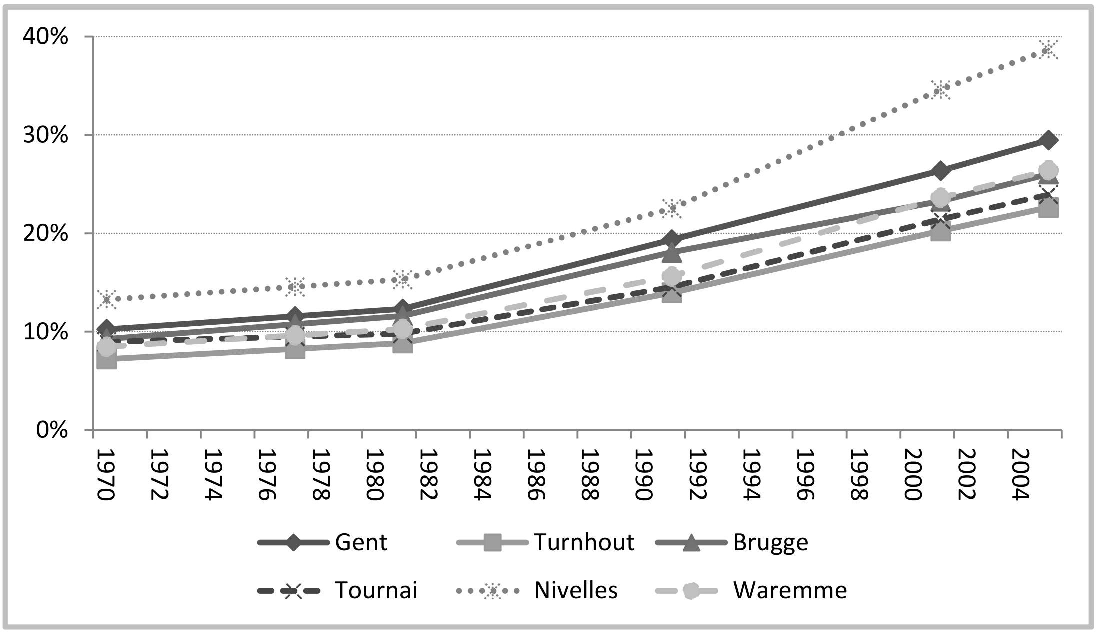
Figure 3 depicts the evolution of the rate of home ownership. This rate represents the fraction of houses that are occupied by their owner. The remaining fraction is occupied by renters. We observe a gradual increase in ownership in about all districts, although the size of this increase clearly differs across districts. As to skill levels (Schooling) we were able to detract from the censuses the population of age 14 and older that has terminated school, sorted by their highest diploma. For our regressions we use the number of highly skilled people, i.e. people with tertiary education, in percent of the population of 14 and older. Figure 4 shows the data for the six districts that we focus on. We observe a rise in each of them. Compared to other variables, differences across districts are quite small for this variable. The data that we report in Table 2 show that, relative to the within standard deviation, the between standard deviation is the smallest for Schooling.

Figure 3. Home ownership rates (OWN)



Source: Appendix B.

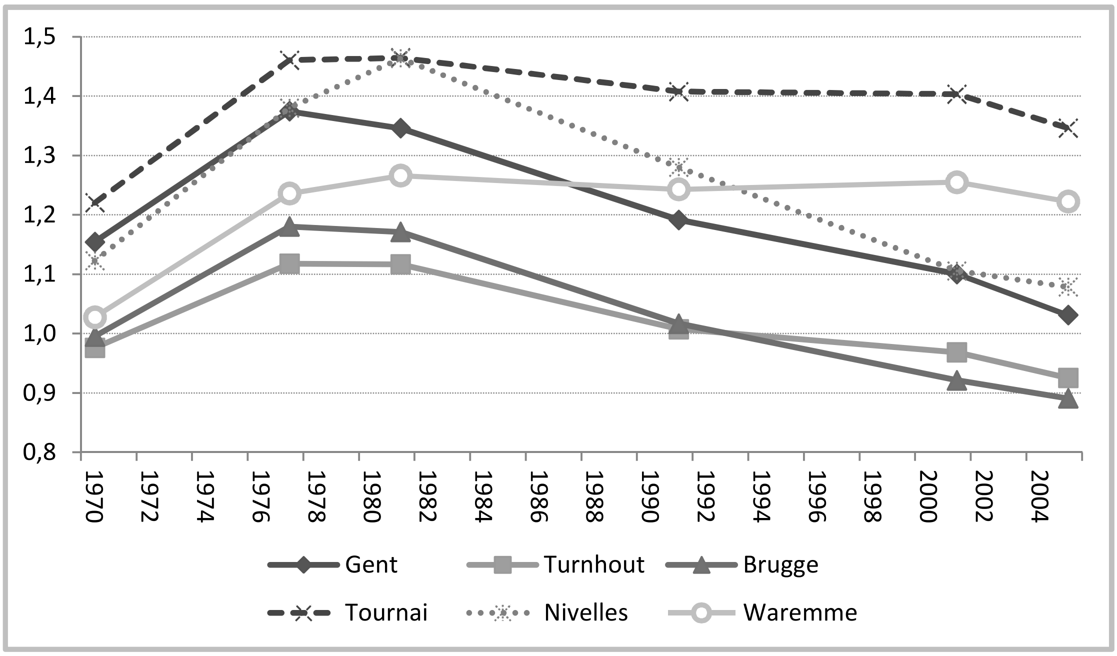
Figure 4. The percentage of highly skilled people (14-... years old) (Schooling)



Source: Appendix B.

The wage gap in Figure 5 reflects the evolution of wage costs relative to productivity, i.e. $wage_{it}/q_{it}$. More precisely, it has been computed as the ratio of real compensation per employee (including taxes on labour) to a proxy for real productivity per employee[5]. Our proxy is real GDP per capita. We prefer this variable above output per employee. The latter is highly endogenous, which may disturb appropriate measurement of the wage gap. A simple example can be illuminating. If wage increases are excessive, pushing up the wage gap, firms may respond by laying off the least productive workers and by substituting capital for labour. As a result, output per (remaining) worker may rise, and the wage gap may fall again. In the end, even if there is a serious problem of job losses, the wage gap may reveal nothing. Employing GDP per capita as a productivity measure makes the wage gap much less vulnerable to this perverse mechanism. Our data in Table 2 and Figure 5 are to be interpreted as an index, compared to a benchmark wage gap. As benchmark we chose the wage gap in the whole of Belgium in 1970. The data clearly show a derailment of wage costs in the seventies. During the eighties the wage gap is strongly reduced in Flanders, mainly thanks to higher productivity growth, with comparable wage growth. The wage gap remains much higher in Walloon districts. Wages have not followed (downwards) the weaker evolution of productivity. The data in Table 2 confirm that relative to the within standard deviation, the variation across districts (between standard deviation) is much smaller for the wage level than for productivity. A final series of variables in Table 2 are demographic. We report the share of three age groups in total population: the fraction of people aged 15 to 24, people aged 25 to 54, and people aged 55 to 64.

Figure 5. Wage gap (index, Belgium 1970 = 1)



Source: Appendix B.

5. Econometric analysis and results

This section contains our main estimation results. In line with earlier arguments, we estimate Equation (1) using the 2SLS estimation method. Endogenous variables to be instrumented are the rate of ownership, schooling, the wage level and productivity. As a result of the endogeneity of wages and productivity, also the wage gap is endogenous. Since the coefficients γ_3 and γ_4 of the log wage level and log productivity are never significantly different from each other in absolute value, we concentrate in this section on results using the log wage gap[6]. The latter comes down to imposing in Equation (1) the restriction that $\gamma_3 = -\gamma_4$.

Instrumental variables

Good instruments should have explanatory power for the variable to be instrumented and be uncorrelated to shocks υ_{it} in the employment rate in the individual district.

When it comes to instrumenting real wages and productivity in individual districts, and therefore the wage gap, it is our hypothesis that the 'aggregate' regional counterparts of these variables contain key information on exogenous drivers, without being affected to any important extent by district-specific shocks.

For Flemish districts these aggregate regional variables are averages over all 22 districts in Flanders. For Walloon districts we use averages over all 20 districts in Wallonia. Although the cross-sectional variation of these aggregate regional variables is very small, they are fully time-varying. Aggregate real wages for example will reflect changes in wage push variables like union power in key sectors in the region, taxation, and (aggregate regional) labour market policies. Aggregate real wages should not seriously be affected by idiosyncratic labour demand and employment shocks in individual districts, which constitute no more than one twentieth of the regional aggregate. Aggregate variables may of course reflect common shocks across all districts, but due to the use of time dummies these common shocks will not show up in the error term.

Next to the relevant regional aggregate, we include the length of the highway and county road network (in kilometres per km²) as an additional instrument for productivity in a district, and therefore the wage gap[7]. We call this instrument 'infrastructure'. Highways and main roads being major elements of the infrastructure in an area, the causal link with productivity is obvious.

For schooling in a district we define population density in that district as instrument. We rely on Boucekkine et al. (2007) who have shown that high(er) population density in an area promotes the enlargement of education facilities. More schools being nearby will then open the possibility to reach higher education levels for more people. Highly populated areas may also attract more public transport connecting people to higher education facilities at larger distance

Finally, we specify four instruments for the rate of ownership in a district. A first one is the fraction of the population older than 35 (age35+). The literature has shown the explanatory power of various demographic variables for the rate of ownership (e.g. Rosen and Rosen, 1980; Barrios García and Rodríguez Hernández, 2004; Gwin and Ong, 2008). The fraction of the population older than 35 is expected to have a significant positive effect. People in this age group have generally more resources and higher preference to enter into long-term commitments than younger people. Our second and our third instrument are population density and its square. Population density acts as a proxy for urbanization. It has been shown in the literature that differences in urbanization contribute significantly to explain variation in the rate of home ownership. The relationship is negative (e.g. Fisher and Jaffe, 2003). The reason for also including squared population density, is to allow for non-linearity in this relationship. Coulson and Fisher (2009) provide one explanation for this negative relationship when they point to the fact that owner-occupied dwellings tend to be single-family detached units, whereas rentals are more often in multifamily dwellings. The latter are much more frequent in urban areas with high population density. Our fourth instrument for the rate of ownership is a common time trend for the six districts to which the major Belgian cities (next to Brussels) belong. These districts are Antwerp, Ghent, Bruges, Charleroi, Liege and Namur. Corresponding cities all house more than 100.000 people. This common time trend captures the differential positive effect on the rate of ownership in the biggest cities and their suburbs from various structural developments since the 1970s. These developments include rising land prices in less urbanized areas, increasing traffic and more frequent traffic jams on the main axes around big cities, rising rental costs in percent of disposable income, a fall in the age at which people buy their first own house combined with the relative preference of young people to live in bigger cities, and government policies (so called 'grootstedenbeleid' since 1999) raising the attractiveness of living as owner in big cities (Vanneste et al., 2007; Vastmans and Buyst, 2011). These structural developments did not all take place simultaneously, but they contribute to explaining the stronger trend rise in the rate of ownership in the biggest cities and their suburbs over time[8].

To test the quality of our instruments, we first assessed their explanatory power in the first stage regression for the endogenous variable that they are expected to explain. All but one instruments show up statistically significant at 2%. All have the correct sign in these regressions[9]. Table 3 summarizes for the endogenous real wage gap, schooling, and rate of ownership in a first row the list of their instruments and the corresponding first stage F-statistics. These test the null hypothesis that the instruments do *not* significantly enter the first stage regression. The values for the F-statistic that we obtain are always far above Staiger and Stock (1997)'s rule of thumb value of 10, supporting our instruments' joint significance. A second F-statistic in the 'all six instruments' row is the F-value for the null that all instruments are irrelevant, including those that we selected for other endogenous variables. Again we obtain values above 10. Next to their explanatory power, we tested the instruments' exogeneity. We report overidentification test statistics (J-statistics) for their validity at the bottom of Table 4. We can never reject the null hypothesis that the instruments are valid.

Table 3. Instruments and instrument relevance

Endogenous variable	Set of instrumental variables	First stage F - statistic testing the relevance of the instruments
log real wage gap	regional aggregate log real wage gap infrastructure	21.5
	all six instruments	10.3
schooling	population density	20.9
	all six instruments	27.1
ownership	fraction of population older than 35 (age35+) time trend for districts of 6 major cities population density population density squared	17.5
	all six instruments	13.4

Data sources and summary of descriptive statistics for the instruments: see Appendix B and Table B1.

Basic estimation results

To estimate our equations we use the fixed effects estimator A Hausman test rejects the null hypothesis that the unobserved effects are uncorrelated with the explanatory variables. Column (1) in Table 4 contains estimation results for our basic Equation (1), still allowing unrestricted γ_3 and γ_4 [10]. As we have mentioned before, γ_3 emerges highly insignificant. Column (2) introduces the log real wage gap, and therefore imposes the restriction that γ_3=-γ_4. Our estimation results in columns (1) and (2) show significant negative effects from the rate of ownership and highly insignificant effects from the fraction of highly educated (schooling). The effects from the real wage gap in column (2) are significantly negative. Furthermore, we find negative effects on the employment rate from the share of young and older people in the population, which confirms expectations, but these negative effects are not (or only weakly) significant. In column (3) we allow variation over time in the effects of these two demographic variables. In line with expectations formulated earlier, we observe that the negative effects are larger in the second part of the period that we study (1990-05) than in the first part (1970-89), but nothing is statistically significant here[11]. Finally, our results reveal a significant positive differential Flemish policy effect on the employment rate of a little more than 2 percentage points since 1990. The far right column (4) in Table 4 re-estimates column (2) by the OLS method.

Table 4. Estimation results for the employment rate (Equation 1)

EMPLOYMENT	**1 – 2SLS**	**2 – 2SLS**	**3 – 2SLS**	**2 - OLS**
Home ownership rate (OWN)	-0.285(**) *(0.17)*	-0.358(***) *(0.08)*	-0.327(***) *(0.08)*	-0.257(***) *(0.04)*
Schooling	-0.005 *(0.14)*	0.038 *(0.12)*	0.066 *(0.13)*	0.115 *(0.07)*
100*Log (wage)	-0.114 *(0.22)*	-	-	-
100*Log (productivity)	0.249(***) *(0.08)*	-	-	-
100*Log (wage gap)	-	-0.225(***) *(0.06)*	-0.235(***) *(0.06)*	-0.130(***) *(0.02)*
Fraction age 15-24	-0.220 *(0.20)*	-0.185 *(0.19)*	-	-0.177 *(0.18)*
Fraction age 15-24 X Dummy1970-1989	–	–	-0.132 *(0.19)*	–
Fraction age 15-24 X Dummy1990-2005	–	–	-0.302 *(0.21)*	–
Fraction age 55-64	-0.278(*) *(0.17)*	-0.276 *(0.17)*	-	-0.249 *(0.16)*
Fraction age 55-64 X Dummy1970-1989	–	–	-0.210 *(0.19)*	–
Fraction age 55-64 X Dummy1990-2005	–	–	-0.367 *(0.23)*	–
Dummy Flanders 1990-2005	2.53(***) *(0.72)*	2.37(***) *(0.70)*	2.47(***) *(0.71)*	3.05(***) *(0.43)*
R-squared within	0.87	0.87	0.87	0.88
R-squared between	0.25	0.16	0.18	0.19
R-squared overall	0.43	0.37	0.40	0.44
J-statistic (p-value) [(a)]	0.19	0.20	0.33	-
Time dummies	yes	yes	yes	yes
District dummies	yes	yes	yes	yes
Number of observations	232	232	232	232

Note: * (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors.
(a) Sargan-Hansen J-test of overidentifying restrictions. The null hypothesis is that the overidentifying restrictions are correct.

Our results confirm the Oswald hypothesis. We find in columns (1) – (3) that a 1 percentage point rise in the rate of ownership in a district implies a significant fall in the employment rate in that district by about 0.3 to 0.35 percentage points. This effect is not only statistically significant, it is also important economically. Using the estimated coefficients in column (3) and the data in Table 2, one can compute for example that a one standard deviation rise in the wage gap implies a fall in the employment rate by about 3 percentage points. A one standard deviation rise in the rate of ownership may cause a fall in the employment rate by no less than 2.6 percentage points. These findings underscore the importance of housing and the arguments underlying the Oswald hypothesis for employment in Belgium. In this respect, our results are in line with earlier work by Estevão (2002) and OECD (2011). Investigating regional labour market disparities in Belgium, Estevão finds low labour migration, and concludes that “Belgians move too little”. He points at linguistic and cultural factors, a compressed wage structure and generous unemployment benefits to explain low mobility. Although our study is not about mobility, it would suggest a high rate of home ownership as another potential explanatory variable. OECD (2011) confirms the negative effect of home ownership on mobility. This OECD study also indicates Belgium as a country with very high transaction costs of buying and selling houses.

Our results also underscore the importance of the estimation method and of controlling for cross-sectional fixed effects and common time effects, when testing the Oswald hypothesis. We observe in the far right column in Table 4 the bias that may follow from OLS estimation. Given the expected positive effect of the employment rate (as a determinant of permanent income) on ownership, it should be no surprise to observe a weaker Oswald effect when we do not control for endogeneity. Its estimated size falls by about 30 percent. Note, however, that this reduced Oswald effect is still significantly different from zero. This result demonstrates that our main conclusion in this paper is invariant to the estimation technique (IV, OLS).

Table 5 contains estimation results where we do not control for cross-sectional fixed effects and/or common time effects. The estimation errors that occur here, are much more serious. As one can see, anything goes. If common time effects are not controlled for in column (2_b), the Oswald coefficient falls to a little more than 1/2 of its estimated value in Table 4[12]. Not controlling for district fixed effects in column (2_c) yields an estimated Oswald coefficient which is even slightly positive, although statistically insignificant. If we control neither for district fixed effects nor for common time effects in column (2_d), a positive and statistically significant coefficient of 0.096 shows up.

Table 5. Additional estimation results for the employment rate

EMPLOYMENT	**2_b - 2SLS**	**2_c – 2SLS**	**2_d – 2SLS**
Home ownership rate (OWN)	-0.188(**) *(0.09)*	0.045 *(0.07)*	0.096(*) *(0.05)*
Schooling	0.246(***) *(0.06)*	-0.823(*) *(0.44)*	0.078 *(0.08)*
100*Log (wage gap)	-0.146(***) *(0.03)*	-0.022 *(0.07)*	-0.191(***) *(0.02)*
Fraction age 15-24	-0.148 *(0.26)*	0.705(***) *(0.17)*	0.650(***) *(0.13)*
Fraction age 55-64	-0.296 *(0.24)*	0.443 *(0.29)*	0.239 *(0.22)*
Dummy Flanders 1990-2005	2.17(***) *(0.65)*	4.55(***) *(1.53)*	1.59 *(1.00)*
R-squared within R-squared between R-squared overall	0.74 0.23 0.43	- - 0.40	- - 0.62
J-statistic (p-value) [(a)]	0.00	0.98	0.00
Time dummies District dummies	no yes	yes no	no no
Number of observations	232	232	232

Note: * (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors.
(a) Sargan-Hansen J-test of overidentifying restrictions. The null hypothesis is that the overidentifying restrictions are correct.

Our results in Table 5 may also shed light on the (somewhat surprising) insignificance of schooling in Table 4. One explanation is that (in contrast to other variables) schooling shows a highly similar evolution over time in all districts. Even if this evolution is important for employment, its effects may at least partly be picked up by the common time dummies[13]. Dropping these time dummies in Table 5, but controlling for district fixed effects, yields a positive and highly significant schooling effect (see column 2_b).

Additional results: size of the Oswald effect, robustness.

Table 6 summarizes the results of a series of additional regressions that we have run, and where we include not just OWN_{it} in the employment regression, but also one or more interaction terms OWN_{it}*VAR_i, where VAR_i stands for a structural variable at the district level which may affect the size of the Oswald effect. Included structural variables are: a dummy for districts situated at a national or linguistic border[14], a dummy for districts close to one of the major cores of economic activity in Belgium (Brussels, Antwerp, Ghent, Liege and Charleroi), the log of average population density in the district, and the log of the average share of highly educated inhabitants[15]. Another interaction term that we include is a dummy common to all districts for the more recent period 1990-2005. Including this dummy (times OWN_{it}) allows to test whether the Oswald coefficient has changed over time.

The data in Table 6 indicate the change in the estimated effect from the rate of ownership on the employment rate brought about by the interaction variable.

Table 6. Influence of structural variables on the estimated Oswald coefficient

Determinants of the Oswald effect. **Different effect...**	**Change in estimated Oswald coefficient** [(a)]	
	Interaction term included separately	Interaction terms included together (and *p*-value ≤ 30%)
for districts at a national or linguistic border? District where at least 30% of the municipalities are situated at a national or linguistic border (versus other districts)	- 0.383 (°)	-
for districts close to an economic centre / major city (b)**?** Districts close to an economic centre (versus other districts)	+ 0.222 (°)	-
for densely populated areas? Effect of a rise in population density by one standard deviation (= +226 persons per square kilometre) (c)	+ 0.323	-
depending on the share of highly educated people ? Effect of a rise in the fraction of highly educated by one standard deviation (= +2.45 percentage points in 'schooling') (c)	+ 0.132 (°)	+ 0.160(*)
in the past versus more recent periods? Change in estimated Oswald coefficient for 1990-2005 (versus 1970-1989)	+ 0.144 (°)	- 0.047(°)

Note: (*) (°) statistically significant at less than 10% (20%).
(a) A negative change points at a stronger Oswald effect.
(b) Our interaction term is a dummy which equals 1 in districts neighbouring the districts of the major cores of economic activity (i.e. Brussels, Antwerp, Ghent, Liege and Charleroi).
(c) Standard deviations are determined over the 42 district averages for population density/schooling over 1970-2005. The log of these district averages are also the data for VAR_i that we use in the interaction term OWN_{it}*VAR_i by which we extend Equation (1).

One column shows the results from including each interaction term separately to the regression reported in Table 4, column (2). The other column follows from including all interaction terms together but dropping those with *p*-values above 30%. Only two interaction terms survive here. Only one of these is statistically significant at 10%. Our results reveal a weaker Oswald effect in districts with a higher share of highly educated people, thereby confirming Dohmen (2005). As to other interaction terms, we see a stronger Oswald effect in districts closer to a linguistic or country border. All other things equal, proximity of a border may imply higher costs to be mobile (e.g. personal costs due to a change of language, or transaction costs due to a shift of legal regime). Furthermore, the Oswald effect would seem to be weaker in densely populated districts and in districts closer to major cores of economic activity (i.e. districts close the major cities). However, none of these differences are statistically significant. Neither do we observe significant differences in the Oswald effect over time. Additional tests with different time periods than those reported in Table 6 did not yield any interesting results.

Table 7 includes the main results of a number of robustness checks on our findings in column (2) in Table 4. In particular the tested the robustness of the estimated coefficient on ownership (γ_1) for changes in the functional form that we impose on the relationship between ownership and the employment rate, and for changes in the included years.

Table 7. Robustness checks to the regression result in column (2) in Table 4

Robustness checks: estimated coefficients in case we...	γ_1
Include log(OWN) instead of OWN as explanatory variable [(a)]	-0.189(***) *(0.04)*
Include log(Empl) instead of Empl as dependent variable [(b)]	-0.667(***) *(0.14)*
Compute the employment rate as the ratio of the number of jobs in a district to population at working age	-0.345(**) *(0.15)*
Drop the year 1977 (for which many data were missing and had to be computed by interpolation, see Appendix 2)	-0.351(***) *(0.09)*
Drop the year 2005 (for which ownership data were missing for Walloon districts, and some other sources had to be explored for other variables, see Appendix 2).	-0.469(***) *(0.10)*
Drop the years 1977 and 2005	-0.461(***) *(0.11)*
Replace the employment rate as dependent variable by the unemployment rate	0.302(***) *(0.11)*

Note: (**), (***) statistically significant at less than 5% (1%).

(a) The Oswald effect as we report it in this paper (i.e. dEmpl/dOWN) can be derived as the estimated coefficient (-0.189) divided by the level of OWN. Evaluated at the overall sample mean (70%), this implies an Oswald effect equal to -0.27.

(b) The Oswald effect as we report it in this paper (i.e. dEmpl/dOWN) can be derived as the estimated coefficient multiplied by the level of Empl. Evaluated at the overall sample mean (57%), this implies an Oswald effect equal to -0.38.

Furthermore, in one regression we introduced the number of jobs located in a district as the dependent variable, rather than the employment rate among the people living there. None of these changes have important effects on our results. The last row of Table 7 shows the estimated Oswald effect when we introduce a somewhat more fundamental change. Here we estimate our model with the unemployment rate as dependent variable. A first reason for introducing this change is that Oswald's thesis mainly concerns the unemployment rate. A second one is that movements in home ownership may also induce changes in labour force participation, implying a difference between the response of unemployment and the response of employment. For example, due to a positive wealth effect, home owners may retire earlier than renters. Also, the need for both man and wife to work may be smaller when they are outright home owners. We show detailed estimation results with the unemployment rate as dependent variable in Appendix C. The results are highly similar to those explaining the employment rate. The estimated Oswald coefficient is 0.30, and statistically very significant. Again we observe a strong fall in this coefficient when we disregard endogeneity and estimate by means of OLS.

6. Conclusions

In a number of papers A.J. Oswald argues that high rates of home ownership may imply inferior labour market outcomes, both for the individual and in the aggregate. This paper tests Oswald's hypothesis in a macro panel of 42 Belgian districts since the 1970s. The use of data going back to 1970 allows us to embed the Oswald hypothesis in a broader model including important other determinants of employment like labour costs and productivity, the skill level of the population, and a number of demographic variables. Considering that ownership may be endogenous to (shocks in) employment, we mainly use IV estimation methods.

Overall, we find evidence in favour of Oswald's hypothesis. We observe that a 1 percentage point rise in the rate of home ownership in a district implies a statistically significant fall in the employment rate by about 0.35 percentage points. The size of this effect is economically important. Additional estimation reveals that the Oswald effect is smaller in districts with higher fractions of high skilled. Our results underscore the importance of controlling for unobserved cross-sectional fixed effects and common time effects, and of appropriately dealing with endogeneity. Disregarding one or more of these issues, as is generally done in the macro labour literature, may imply very different estimation results. (We then observe a weaker Oswald effect, or no Oswald effect at all). Our main result in favour of Oswald's hypothesis survives various robustness checks. These include changes in the dependent variable. Changing our focus to unemployment rather than employment, does not affect our main conclusion.

The literature on the effect of home ownership on employment shows a remarkable contradiction. Micro studies generally reject Oswald's hypothesis, whereas most macro studies support it. One explanation for this contradiction may the methodological weakness of many macro studies. We avoid these weaknesses in this paper but still confirm Oswald's hypothesis, at least for Belgium. This leaves a very interesting avenue for further research, where we shall use micro data for Belgium to estimate the influence of housing status on unemployment duration, like in Munch et al. (2006) and Battu et al. (2008). Is Belgium different, and does the adverse employment effect from home ownership in macro data also exist at the individual level? Factors that might make Belgium different can for example be very high transaction costs of buying and selling houses, relatively generous unemployment benefits (long benefit duration), or the specific linguistic situation restraining mobility. Or, alternatively, is the adverse macro relationship rather due to negative effects from (high) ownership beyond the owners themselves, like traffic congestion, or a tightening of rental markets?

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Appendix A: Belgian provinces and districts

Figure A1. Belgian provinces and districts

Average size of a district: 723 km²

Average number of inhabitants per district: 214.000

Appendix B: Data description and sources

Most our data have been taken from the national censuses held in Belgium. Because we only had censuses in the years 1970, 1981 and 1991, we had to supplement these with data from extensive surveys (Social-Economic Survey, Woonsurvey). These surveys have been tested and confirmed to be representative by the statistical authorities (Nationaal Instituut voor de Statistiek, NIS). Not every variable is available in the censuses and surveys, so for some data we had to rely on other sources. We now consider every variable of our model and give a short description. We mention data sources and possible data shortages or adjustments. Table B1 contains the main descriptive statistics for those variables (instruments) that are not yet included in Table 2.

OWN (Home ownership rate)
The fraction of houses that are occupied by their owner.
Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 1977 and 2001 by NIS; Woonsurvey 2005 by the Flemish Government. Data for 2005 are not available for the Walloon districts.

Empl (Labour market performance, employment rate)
Employment rate among households living in the district, i.e. the number of people with a job in the district in percent of the population at working age. Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 2001 by NIS; 'Steunpunt Werk en Sociale Economie' for 2005. The data for 1977 have been interpolated from the years 1970 and 1981.

Unemployment (Labour market performance, unemployment rate)
Unemployment rate among households living in the district, i.e. the number of people without a job but seeking employment in percent of the labour force. Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 2001 by NIS; 'Steunpunt Werk en Sociale Economie' for 2005. The data for 1977 have been interpolated from the years 1970 and 1981.

Schooling
The number of highly skilled people (tertiary education) in percent of the population of age 14 or older. Source: Census 1970, 1981 and 1991; Eurostat for 2001. 1977 has been interpolated from 1970 and 1981. 2005 has been extrapolated from 2001 based on the data of 2001 and the evolution of the national mean according to NIS Labour Force Survey.

Wage
Real compensation per employee. Source: own calculations based on Cambridge Econometrics data. Data for 1970 and 1977 have been extrapolated based on NIS Social Statistics. Due to data limitations, wages have been computed at the level of the provinces. Provinces include about 4 districts on average.

Productivity (q)
Real GDP per capita. Source: own calculations based on Cambridge Econometrics data. Per capita GDP in 1970 and 1977 have been extrapolated based on Cambridge Econometrics and OECD Economic Outlook data. Due to data limitations, productivity has been computed at the level of the provinces. Provinces include about 4 districts on average.

Wage gap

Ratio of wage level to productivity, index with Belgium in 1970 = 1.

Demographic variables

- **Age xx-yy**: People of age between xx and yy in percent of the total population. Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 2001 by NIS for 2001, and 'FOD Economie, KMO, Middenstand en Energie' for 2005 (Rijksregister). The data for 1977 have been interpolated from the years 1970 and 1981.

Population Density

Population density, number of people per square kilometre.

Sources: district area: Eurostat; district population: Census 1970, 1981 and 1991, Social-Economic Survey 2001, 1977 has been interpolated from 1970 and 1981. 2005: Ecodata, 'FOD Economie, KMO, Middenstand en Energie' (Rijksregister).

Infrastructure

The length of highways and county roads per square kilometre.

Source: 1970, 1977, 1981 and 1991: NIS, Statistical yearbooks for Belgium; 2001 and 2005: FOD Mobiliteit en Vervoer (Processing: FOD Economie (Afdeling Statistiek)).

Table B1. Main descriptive statistics for the instrumental variables

	Infrastructure	Population density	Age 35+
Overall Mean	0.443	333	52.0%
Minimum	0.249	34	41.1%
First quartile	0.390	154	49.6%
Median	0.464	268	52.2%
Third quartile	0.502	506	54.6%
Maximum	0.544	955	59.3%
Std. Dev.	0.074	226	3.5%
Between Std. Dev.	0.067	228	1.7%
Within Std. Dev.	0.041	16	1.8%
1970 mean	0.051	321	50.4%
1977 mean	0.054	326	50.6%
1981 mean	0.057	328	50.6%
1991 mean	0.026	333	52.2%
2001 mean	0.025	342	54.3%
2005 mean	0.026	347	53.9%
Observations	252	252	252

Appendix C: Estimation results for the unemployment rate

Table C1: Estimation results for the unemployment rate

UNEMPLOYMENT	**2 – 2SLS**	**3 – 2SLS**	**2 - OLS**
Home ownership rate (OWN)	0.302(***) *(0.11)*	0.299(***) *(0.11)*	0.118 (**) *(0.05)*
Schooling	0.004 *(0.18)*	0.005 *(0.17)*	-0.119 *(0.09)*
100*Log (wage gap)	0.363(***) *(0.08)*	0.362(***) *(0.08)*	0.129(***) *(0.03)*
Fraction age 15-24	0.004 *(0.25)*	-	-0.013 *(0.21)*
Fraction age 15-24 X Dummy1970-1989	–	-0.013 *(0.26)*	–
Fraction age 15-24 X Dummy1990-2005	–	-0.010 *(0.29)*	–
Fraction age 55-64	0.363 *(0.24)*	-	0.302 *(0.19)*
Fraction age 55-64 X Dummy1970-1989	–	0.329 *(0.25)*	–
Fraction age 55-64 X Dummy1990-2005	–	0.445 *(0.31)*	–
Dummy Flanders 1990-2005	-4.00(***) *(1.01)*	-3.95(***) *(1.01)*	-5.87(***) *(0.51)*
R-squared within R-squared between R-squared overall	0.84 0.16 0.44	0.84 0.16 0.44	0.89 0.25 0.66
J-statistic (p-value) [(a)]	0.88	0.89	-
Time dummies District dummies	yes yes	yes yes	yes yes
Number of observations	232	232	232

Note: The estimation results in this table correspond to those in Table 4, but have the unemployment rate as dependent variable. The set of instruments used in the 2SLS regressions includes the aggregate regional log real wage gap, population density, population density squared, a time trend for the districts of the 6 major cities, and the fraction of the population older than 35.
* (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors.
(a) Sargan-Hansen J-test of overidentifying restrictions. The null hypothesis is that the overidentifying restrictions are correct.

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NOTES

[1] Appendix A contains a map and some more information on these districts.

[2] This low level equals the level of the unemployment benefit (or the value of leisure).

[3] Note though that these expectations are unconditional. Controlling for (tertiary) schooling, and wages and productivity, expected signs may be less straightforward.

[4] None of the values for Geary's C that we obtain, differ significantly from 1 (p-values are always above 0.25). In the same spirit, we also tested for temporal autocorrelation in the residuals. Since our panel data are unequally spaced along the time dimension, we relied on the non-parametric Runs test. Here also, test results could never reject the null hypothesis of no autocorrelation. Details on all these tests are available upon request.

[5] Due to lack of data at the level of individual districts in the 1970s, our wage and productivity data have been computed at the provincial level. Provinces include about 4 districts on average (see Appendix A).

[6] Estimating γ_3 and γ_4 separately always yields a value for γ_4 close to 0.25 and highly significant, while γ_3 is always negative but very imprecisely estimated (estimated t-value < 1). The value of γ_3 is never significantly different from -0.25.

[7] Productivity data being available only at the provincial level (see footnote 4), our data for the road network also concern the province to which a district belongs.

[8] Note that the structural developments underlying this 'major city time trend' bear no relationship to the employment rate. Extending Equation (1) with this time trend yields a highly insignificant coefficient (t-value < 0.7 in absolute value). This conclusion of insignificance holds for all instruments when added to our estimated employment equations. They do not matter for employment significantly beyond their influence on the endogenous variables in the regression.

[9] Detailed results are available upon request. The exception is the fraction of people older than 35 in the first stage regression for ownership. It is significant at 11%.

[10] Instead of the aggregate regional real wage gap, we introduce aggregate regional productivity and aggregate regional wages as two separate instruments (next to infrastructure) to estimate this equation.

[11] Also including the fraction of prime age workers in the employment equation implied coefficients which were highly insignificant and almost zero for this age group.

[12] Imagine for example business cycle shocks. Positive shocks may raise both employment, aggregate wages, and household confidence and resources, and the ambition to become owner. If not controlled for in the regressions, such a shock will induce positive correlation between ownership and the error term, and bias upwards the estimated Oswald coefficient.

[13] The estimated time dummies in Table 4, column (2), are respectively 4.3%, 3.3%, 1.9%, 5.0% en 7.3% in 1977, 1981, 1991, 2001 and 2005.

[14] This also includes districts at the border between Dutch-speaking Flanders and French-speaking Wallonia.

[15] These averages are computed per district over all years in our sample (1970, 1977, 1981,…).

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