Is There a "Race-to-the-Bottom" in the Setting of Welfare Benefit Levels? Evidence from a Policy Intervention*

Matz Dahlberg† and Karin Edmark‡

First version: November, 2003
This version: March, 2004

Abstract

In this paper we investigate whether local governments react on the welfare benefit levels in neighboring jurisdictions when setting their own benefit levels. We solve the simultaneity problem arising from the welfare game by utilizing a policy intervention; more specifically, we use a centrally geared exogenous placement of a highly welfare prone group (refugees) among Swedish municipalities as an instrument. The IV estimates indicate that there exists a "race-to-the-bottom" and that the effect is economically as well as statistically significant; if the neighboring municipalities decrease their welfare benefit level with 100 SEK, a municipality decreases its benefit level with approximately 65 SEK. This result seems to be robust to several alternative model specifications.

Keywords: Welfare benefit level, Strategic interactions, Race-to-the-bottom, Policy intervention

JEL classifications: C33, D6, H73

*We thank Per-Anders Edin, Peter Fredriksson, Ann-Sofie Kolm, Olof Åslund, and seminar participants at Uppsala University for helpful comments and discussions.

†Department of Economics, Uppsala University, P.O. Box 513, SE-751 20 Uppsala, Sweden. E-mail: matz.dahlberg@nek.uu.se

‡Department of Economics, Uppsala University, P.O. Box 513, SE-751 20 Uppsala, Sweden. E-mail: karin.edmark@nek.uu.se

1 Introduction

A "race-to-the-bottom" in the setting of welfare benefit levels can materialize in two different ways. First, it may materialize in the presence of welfare migration; welfare migration may make jurisdictions reluctant to offer generous benefits because it may increase the number of program participants and thus the total cost of providing a given level of benefits. As a result, welfare benefits may be lower than the socially desirable level and, hence, we have a "race-to-the-bottom".[1] Second, it may materialize if there are strategic interactions among the local jurisdictions, no matter if there really exists any welfare migration or not; all that is required is a *perception* on the part of local government decision makers that generous benefits attract welfare migrants, and, since nobody wants to be the most generous jurisdiction in the region, we once again have a "race-to-the-bottom".

The evidence on welfare migration is mixed. While there are studies that have found large effects (see, e.g., Gramlich (1984) and Enchautegui (1997)), there also exist studies that have found no effects (see, e.g., Walker (1994) and Levine and Zimmerman (1995)).[2] The perhaps most reliable study, Meyer (2000), indicates that there is a statistically significant effect although it seems to be economically small. It can be worth stressing, however, that welfare migration is only a prerequisite for a "race-to-the-bottom" to materialize along the first line of reasoning above. For a real "race-to-the-bottom", the local politicians must also react on the welfare migration by lowering their welfare benefit levels.[3]

A more direct test of the existence of a "race-to-the-bottom" in the setting of welfare benefit levels is to test for strategic interactions among local governments. What one typically estimates is an equation of the form

$$B_i = \gamma \sum_{j \neq i} \omega_{ij} B_j + X_i \beta + \varepsilon_i \qquad (1)$$

where B_i is the benefit level in local government i, B_j is the benefit levels in other local governments j, $j \neq i$, ω_{ij} are weights that indicate the importance attached by local government i to benefits in the other local governments, X_i is a vector of socio-economic and demographic characteristics for local government i with the associated parameter vector β, and ε_i is the error term. The parameter of interest, γ, represents

[1] Even though it is likely to overstate the issue, we will follow the earlier literature and use the phrase "race-to-the-bottom" as a convenient shorthand description of the phenomenon of interest (while the theory only points to a downward bias in welfare benefits caused by a concern about welfare migration, popular usage of the phrase sometimes have the meaning of a much more dire outcome).

[2] Excellent surveys of the earlier research can be found in Bruckner (2000) and Meyer (2000).

[3] This latter step has typically not been examined in the earlier literature. In a companion paper (Dahlberg and Edmark, 2004) we do however show that local politicians seem to react to an inflow of welfare prone individuals by lowering the welfare benefit levels.

the slope of the local government's reaction function. If γ is significantly different from zero, then strategic interaction occurs between a given local government and other local governments that have not been assigned a weight of zero.

The econometric problem in estimating the above equation is that the benefit levels on the right-hand-side are endogenous variables since the benefit levels in all localities are jointly determined when strategic interactions occurs. Earlier empirical work in this area have found a positive and statistically significant estimate of γ (see, e.g., Saavedra (2000) and Figlio, Kolpin and Reid (1999)). The existing empirical work does however suffer from different potential drawbacks, all related to the identification of the interaction parameter.[4]

The aim of this paper is to re-investigate whether local governments react to the welfare benefit levels in neighboring jurisdictions when setting their own benefit levels. Our main contribution is that we propose and use a policy intervention to solve the simultaneity problem arising from the welfare game. More specifically, we utilize an exogenous variation that was provided by a policy intervention in Sweden in the late 1980s and early 1990s as an instrument; a centrally geared exogenous placement of a highly welfare prone group (refugees) among Swedish municipalities. Using a policy intervention to identify endogenous and exogenous interactions is an approach that has never been used before in the literature on welfare competition, and, according to Moffitt (2000) is an approach that is seldomly, if ever, used in general in empirical work on social interactions.

The IV estimates indicate that there exists a "race-to-the-bottom" and that the effect is economically as well as statistically significant; if the neighboring municipalities decrease their welfare benefit level with 100 SEK, a municipality decreases its benefit level with approximately 65 SEK. This result seems to be robust to several alternative model specifications.

The remainder of the paper is organized as follows. The next section discusses potential problems with the earlier empirical work. Section 3 lays out the theoretical framework, section 4 presents the policy intervention, section 5 describes the data and the empirical specification, and section 6 provides the regression results. A detailed sensitivity analysis is given in section 7 and, finally, section 8 concludes.

2 Earlier empirical work

The empirical literature on the interdependencies between a region's welfare benefit level and the benefit levels of its neighbors is rather small. In the wake of the major welfare reforms that took place in the United States in the 1990s, there has however

[4] These drawbacks will be discuss in the next section.

been a growing interest in empirical work on this topic.[5] The existing work, that can be classified into three types of studies, do however suffer from three potential drawbacks. The first type of studies have assumed that there is no exogenous interactions and/or no correlated shocks at work. The second type of studies have assumed that there are no endogenous interactions. The third type of studies have put restrictions on the coefficient that measures strategic interactions.

When estimating models of strategic/social interactions between different types of agents, it is important to consider the distinction between endogenous and exogenous interactions, as discussed by, e.g., Manski (1993) and Moffitt (2000). Endogenous interactions are transmitted through the outcome variable (that is, in our case, the welfare benefit levels) while exogenous interactions are transmitted through the other municiaplities' characteristics other than their welfare benefit levels (that is, if exogenous interactions are present, a municipality's welfare benefit level will be affected by other municipalities' characteristics other than their welfare benefit levels). As is discussed and shown by Moffitt (2000), without a policy intervention (through randomized trials or some non-experimental counterpart), it is impossible to identify endogenous and exogenous interactions separately. According to Moffitt (2000), many studies in the literature on social interactions try to overcome this problem by imposing the restriction that only one form of interaction is at work, thereby obtaining identification. However, if the assumed form of interaction is incorrect, the resulting estimates are either biased or misinterpreted. If, for example, exogenous interactions are assumed to be zero when they are not, and the model is estimated by two-stage least squares in which the characteristics of the other regions are used as instruments, then we get biased estimates of the coefficient on the endogenous social interaction variable (that is, in our case, of the reaction function coefficient). Similar problems arise if the endogenous interactions are assumed to be zero when they are not, implying that the model is estimated by regressing a region's benefit level only on its own characteristics and the characteristics of the other regions. To be able to identify the two channels separately, we must hence control for both the other municipalities' welfare benefit levels and the characteristics of the other municipalities while we at the same time use a policy intervention to solve the simultaneity problem.

An example of the first type of studies is Figlio et al. (1999). They use a panel of U.S. state-level data over the period 1983-1994 to examine the degree to which states simultaneously set their welfare benefit levels. In order to do so, they estimate an equation that is very similar to equation (1), but where they also control for

[5] Apart from the main reform in the U.S. in 1996 (PRWORA), there were several state waivers enacted in the first part of the 1990s. As a consequence of the reforms, there was a highly increased decentralization of responsibilities for the welfare system to the state level, implying an increased probability for strategic interaction among the states to take place.

state-specific fixed effects, time dummies, and the states' characteristics (but not the characteristics of neighboring states). To break the simultaneity, they use an instrumental variables technique. As instruments they use characteristics of the neighboring states (more specifically, they use the neighbor states' female unemployment rate, the neighbor states' ratio of females to employed males, and the neighbor states' average weekly wages in variety stores). They find substantial empirical evidence supporting the notion that states set welfare benefit levels interdependently; their results indicate that a state is expected to change its benefit levels by 90 cents when neighboring states change their benefit levels by one dollar. Their IV estimate is more than five times as large as their OLS estimate (i.e., when the endogeneity of the neighboring states welfare benefit level is neglected). In addition, they find that state responses to neighbor benefit decreases tend to be significantly larger in magnitude than their responses to neighbor benefit increases. As noted above, their choice of instruments are however problematic if there are exogenous interactions, implying that their estimates might be biased.[6] In other words, in the presence of exogenous interactions (and/or correlated shocks), the municipalities' characteristics should be included as control variables, not as instruments.

Assuming that there are no endogenous interactions, Smith (1991) is an example of the other types of studies. Using a single cross-section (1979 U.S. data), she only uses the rival states' characteristics as explanatory variables (and not their benefit levels). By estimating such a model, Manski (1993) has shown that the existence of social interactions is in general identified. If the coefficients for the other regions' characteristics are significant, then either one, or both, of the two types of interactions must be significant (but we cannot disentangle their separate roles). Smith finds that distance matters; it is only close states' (within 750 miles) that engage in a benefit-setting game.

Finally, Saavedra (2000) is an example of the third type of studies. Using three cross-sections of U.S. data (for the years 1985, 1990, and 1995), Saavedra tests empirically for strategic interactions among the states. The analysis is conducted on separate cross-sections as well as on pooled data, where in the latter case state-specific fixed effects are controlled for. Saavedra (2000) does not use an instrumental variables approach, but estimates a reduced form using spatial econometric methods. As in Figlio et al. (1999), the results suggest that the states behave strategically when they set their welfare benefit levels; the estimated slope parameter of the reaction function is positive and significant. A drawback with the econometric approach is though that one has to impose restrictions on the values that the slope parameter of the reaction

[6] There estimates would be biased also if there were correlated shocks across regions. The same type of instruments are also used by Hernes Fiva and Rattso (2003) when testing for strategic interactions among Norwegian local governments.

function can take. In Saavedra's model, the absolute value of the slope parameter of the reaction function must be less than one.[7]

3 Theoretical framework

3.1 General setup

To organize our thoughts on the problem, we will use the theoretical setup presented in Bruckner (2000), building on work by Brown and Oates (1987) and Wildasin (1991). While we in this section simply present the model, we will in section 4.3 analyze how the proposed policy intervention affects the welfare game and examine under what circumstances it can be considered as a suitable instrument for solving the simultaneity problem.

The model economy contains two regions: A and B. In each region there are M non-poor consumers, referred to as "rich", who are immobile across states. The economy contains $2\overline{N}$ poor consumers, who work at low-paying jobs as well as receive welfare benefits from the region where they reside. The poor are assumed to be mobile across regions, with zero migration costs. There are N_A poor people in region A and $N_B = 2\overline{N} - N_A$ in region B.

The wages of the poor are determined in a competitive labor market, and thus reflect the marginal productivity of unskilled labor. Suppose that the output of region i depends on the amount N_i of unskilled labor along with other fixed factors (such as land and capital), $f(N_i)$. The wage of a region is hence equal to $w_i = f'(N_i)$ We assume that f is strictly concave, wich implies that the wage falls as the unskilled labor pool grows; $w'(N_i) \equiv f''(N_i) < 0$. Wages in the two regions are then given by $w_A = w(N_A)$ and $w_B = w(N_B)$. Letting B_A and B_B denote the welfare benefits paid to the poor, the total income of a poor resident equals $w(N_A) + B_A$ in region A and $w(N_B) + B_B$ in region B.

Each region's welfare benefit level is chosen by its rich residents, who care about the well-being of the local poor (through interdependent preferences). We assume that the rich in both regions have the same utility function: $U(x_i, w_i + B_i)$, $i = A, B$, where x_i gives consumption expenditure for the rich in region i. For simplicity, the utility function is assumed to be quasi-linear, that is

$$U(x_i, w_i + B_i) = x_i + V(w_i + B_i), \qquad i = A, B \tag{2}$$

where V is increasing and strictly concave in $w_i + B_i$.

[7] Saavedra uses different weighting schemes when estimating the reaction function, also allowing non-neighboring states to have positive weights. Most of her results do however suggest that state choices of benefit levels depend on choices in contiguous states, while they in most cases seem to be independent of the chosen benefit levels in non-contiguous states. Hence, it seems like strategic interactions mainly take place among states that share border.

Letting y denote the income of the rich, which is assumed to be the same in both regions, the budget constraint of a rich resident is given by

$$x_i = y - \frac{N_i B_i}{M}, \qquad i = A, B \tag{3}$$

The benefit level of the region is thus chosen to maximize equation (2) with respect to the benefit level, such that the budget restriction in equation (3) holds.

We will start by briefly looking at the no mobility case, before turning to the more interesting case in which the poor are allowed to move between the regions.

3.2 The no-mobility case

In the no-mobility case, solving the maximization problem for the optimal benefit level of region A yields the following first order condition:

$$MV'(w_A + B_A) = N_A \tag{4}$$

The condition states that the rich of the region set the benefit levels so that the sum of their marginal utilities of the poor's income, is equal to the marginal cost of increasing the poor's incomes through increasing benefits. The first order condition is hence a Samuelsson condition for the provision of a public good.

3.3 The mobility case

If we allow the poor to move between the regions, the analysis becomes sligthly more complicated. Solving the maximization problem in the presence of welfare migration implies that the rich of region A choose the welfare benefit level taking account of the fact that an increase in B_A raises N_A through welfare migration. The regions thus play a Nash welfare game, with the rich in region A viewing region B's welfare benefit level, B_B, as fixed in making their own choice.

In order to derive an internal migration equilibrium, i.e. in order to avoid a situation where all poor individuals move to the region with the marginally higher benefit level, we need to put some constraint on migration. In this model setup, the assumption that wages depend negatively on the number of poor residents provides such a constraint.[8] Migration equilibrium is achieved when the total income of the poor is equalized between the two regions, i.e. when the following expression holds:

$$w(N_A) + B_A = w(N_B) + B_B \tag{5}$$

[8] This assumption is not crucial to the qualitative results, but could be replaced by other assumptions that constrain the migration elasticity of the poor, for example idiosyncratic moving costs or regional preferences (see for example Smith (1991) or Wheaton (2000)). The wage assumption has the advantage of being strighforward and easy to analyze.

By maximizing equation (2) with respect to B_A subject to equation (3), and allowing N_A to vary, we obtain the following first order condition for the benefit level of region A:

$$MV'(w_A + B_A) = \frac{N_A + \frac{\partial N_A}{\partial B_A} B_A}{1 + w'(N_A) \frac{\partial N_A}{\partial B_A}} \tag{6}$$

By comparing equation (6) with the first order condition in the no-mobility case, equation (4), we easily see that the optimal benefit level is lower in the presence of migration. Two effects contribute to this: First, since increases in the benefit level now cause welfare migration, a marginal increase in the benefit level will increase total costs more than in the no mobility case. We call this the "cost effect". Second, benefit increases are less productive when the poor are mobile. The reason is the induced welfare inmigration has a negative effect on the local wage, which partly offsets the increase in the local poor's income. We denote this the "wage effect".

Our main interest, however, lies in the interaction between the benefit levels of the regions, i.e. in the benefit level reaction functions. In order to simplify the derivation of these, following Bruckner (2000), we assume simple quadratic functional forms for utility and production. Specifically, we assume that $U(x_i, w_i + B_i) = x_i + \eta(w_i + B_i) - \frac{1}{2}\theta(w_i + B_i)^2$ with $\eta, \theta > 0$, and that $f(N_i) = \alpha N_i - \frac{1}{2}\beta N_i^2$, with $\alpha, \beta > 0$, which gives $w(N_i) \equiv f'(N_i) = \alpha - \beta N_i$.

By applying these functional forms and by combining the first order condition in equation (6) with the migration equilibrium constraint in equation (5), we can solve for B_A as a function of B_B:

$$B_A = \Psi + \frac{2 - M\beta\theta}{(4 + M\beta\theta)} B_B \tag{7}$$

where Ψ is a constant.

Equation (7) shows the interaction between the benefit levels of neighboring regions. In empirical work, equation (7) is typically estimated through an equation similar to the one given in equation (1).

By using the quadratic functional forms to solve for $B_A(N_A)$ in equation (6), we find that the following holds[9]:

$$\frac{\partial B_A}{\partial B_B} \begin{array}{c} > 0 \\ = 0 \\ < 0 \end{array} \; iff \; \frac{\partial B_A}{\partial N_A} \begin{array}{c} < 0 \\ = 0 \\ > 0 \end{array},$$

We see that, in this theoretical framework, we can expect some interaction between the benefit levels, unless we have the knife-edge case of zero-sloping reaction functions. The sign of the reaction functions depends on whether the "cost effect" or the "wage effect" dominates: if the cost effect is larger than the wage effect, we have positively sloped reaction functions - the race to the bottom case; if they exactly balance, we

[9] The model setup is symmetric, so the corresponding holds for B_B.

have a zero slope; and if the wage effect is larger, the reaction functions have a negative slope.[10]

The model illustrates that the number of poor in a region affects the benefit level, which suggests that an exogenous increase in the number of poor in a region could be used to instrument for the benefit level. The following section argues that the Swedish refugee placement program of the late 1980's and early 1990's provided exactly this; an exogenous increase in the number of welfare prone individuals in the municipalities that can be used as instruments for the municipalities' welfare benefit levels.

4 Solving the simultaneity problem: Exogenous placement of refugees

In order to solve the simultaneity problem, we would, generally speaking, like to have a variable that is exogenously distributed among the municipalities and that affects the setting of welfare benefit levels in a certain municipality but that does not directly affect the corresponding levels in neighboring municipalities. The theoretical analysis in the previous section shows that one variable that is likely to affect the setting of welfare benefits in a municipality is the inflow of welfare prone individuals to the municipality. If we could find a social program or policy intervention that generates an exogenous placement of a welfare prone group among the municipalities, we could hence use that program as an instrument to solve the simultaneity problem arising in equation (1). It turns out that such a policy intervention existed in Sweden in the late 1980s and early 1990s, namely an exogenous placement of refugees. We will use this policy intervention as an instrument to break the simultaneity problem arising from the welfare game. The following section describes the main characteristics of the refugee placement program and discusses the appropriability of using it as an instrument.

4.1 Description of the refugee placement program

The system of non-voluntary placement of refugees was in place between the beginning of 1985 and the first of July 1994. The assignment of refugees to the municipalities was coordinated by The Immigration Board through municipalitywise contracts. The purpose of the program was to achieve a more even distribution of refugees over the country, or more specifically, to break the concentration of immigrants to larger towns.

[10] It is hence the "wage effect", or the assumption that wages depend negatively on the number of poor in the region, that lies behind the possibility of negatively or zero sloped reaction functions in this model setup. This scenario is not unrealistic. We can think of other mechanisms that would yield the same result, for example including housing costs of the poor in the model, and letting these increase in the number of poor in the region.

Initially, only a fraction of the municipalities were contracted, but as the number of refugees soared in the late 1980s and early 1990s, so did the number of receiving municipalities. In 1991, 277 out of 286 municipalities had agreed to participate.

The original ambition was to direct the flow of immigrants toward municipalities with good future prospects in terms of labor market conditions and education possibilities. The increasing inflow of immigrants combined with the shortage of housing during the second half of the 1980s and the early 1990s meant, however, that these ambitions had to give way to the more immediate concern of availible housing.

The municipalities received financial compensation, paid out by the Immigration Board, for the refugee placement. Compensation was paid out gradually, during the year of placement and the 3 following years, to compensate for the running expenses of the receiving municipality. The larger part of the expenditures consisted of welfare benefit payments. In 1991, the system was replaced by one where the municipalities were given a lump sum grant for each refugee. The grant was paid out during the year of the placement, and was estimated to cover the expenses of the municipality for about 3,5 years. In addition to the grant, the municipalities had the possibility to apply for compensation for "extraordinary costs" for the refugee placement, for example for old or disabled refugees that were in need of special care.[11]

The refugees were allowed to move immediately after the placement. Under the system with running expenses, the compensation was tied to the refugee, i.e. was provided to the new municipality in case of migration. This was not the case under the lump sum system, where the municipality of placement received the entire sum, no matter how long the refugee stayed in the municipality. If the refugee did move within two years after placement, the new municipality also received some compensation.

One previous study uses the refugee placement program as a natural experiment, Edin, Fredriksson and Åslund (2003). They study the consequences of the program placement for the labor market participation of the refugees and use data for 1987-91. The paper provides a detailed description of "the handling of a typical asylum seeker from the border to the final placement".

4.2 Using the refugee placement program as an instrument for the benefit level

We will use the policy intervention defined by the refugee placement program between 1986 and 1991 to instrument for the rival municipalities' welfare benefit levels in 1990-1994.[12] In order to motivate that the refugee placement program is an appropriate

[11] This system was in place until 1996. From 1996 the compensation is in the form of a lump sum, but it is paid out gradually during a 2-year period. (The Immigration Board (1997, pp22f))

[12] According to Edin et. al. (2003) the refugee placement program was more strictly implemented during this initial period of the program, than during the later years.

instrument, we need to discuss the exogeneity of the refugee placement program. In addition, we need to show that it is reasonable to believe that the placement of refugees in a municipality could affect the benefit level of that municipality, but not directly the benefit levels of the rival municipalities.

Regarding the exogeneity of the program placement, what is important for our analysis, is that the refugee placement was exogenous from the point of view of the municipalities. The period we study is characterized by a number of circumstances that we argue support this claim.

First, during the time period we will use, the number of refugees arriving to Sweden increased dramatically. During 1986-91 on average over 16,000 refugees arrived each year (peaking in 1989 at 24,879), compared to a yearly average of just above 5,000 during the previous six years. This probably made it harder for the municipalities to refuse to accept the Immigration Board's refugee placement proposals. The refugees had to be placed somewhere, and with the increasing inflow, all municipalities had to share the responsibility for this.[13]

Second, we see that refusals to accept refugee placement were in fact very rare. Only 5 out of the 281 municipalities in our data refused to receive any refugees at all during the period we study. We believe it likely that the decision to refuse refugee placement was connected to municipality-specific parameters that stay relatively fixed over time, such as ideology.

Third, the period under study is characterized by a very tight housing market. This means that if any factor, except for fixed municipality-specific characteristics, did influence the refugee placement, it was probably the availability of housing.[14]

We argue that the refugee placement can be viewed as exogenous from the point of view of the municipalities, *conditional on housing vacancies and on municipality-specific fixed effects.*

Another criteria for using the refugee placement program as an instrument for the benefit levels, is that the program actually affects the benefit levels. A first prerequisite for this to happen is that the program leads to an increase in the number of welfare prone individuals and that this increase, in turn, implies increased costs for the municipalities.

When placed in a municipality, the refugee was supported by welfare benefits during a period of introductory Swedish courses, and after that until he/she had found other maintenance. It is therefore reasonable to expect that, during an initial

[13] This notion is supported by the fact that almost all municipalities agreed to receive refugees. Furthermore, some of the municipalities that did refuse, received a lot of negative publicity for this.

[14] This claim is supported by various studies that argue that the high unemployment rates among immigrants from 1980 and onwards are partially due to the fact that housing, instead of factors such as labor market prospects, has been determining the refugee placement (see for example Edin et al., 2003).

period, the larger part of the refugees received social assistance. The municipalities were compensated for this, during the first part of the period we study through the compensation for runnning expenses during three years, and from 1991 on, through a lump sum grant that was to cover the expenses for a corresponding period. The question is whether this compensation was enough, i.e. whether the refugees had moved out of welfare when the compensation ended or not.

Looking at the data, we find it likely that the refugee placement did increase the pool of welfare dependent inhabitants in a municipality. Refugees, as well as foreign citizens in general, are overrepresented in the data on welfare recipients. Over the period 1990-1994, refugee households made up on average 11 percent, and non-Swedish citizens in general (including refugees) 26 percent, of the welfare-receiving households, while the fraction of refugees and the fraction of foreign citizens (including refugees) in the population during the same period roughly equalled 1 and 6 percent respectively.[15]

These figures may however merely represent the fact that the refugees are supported by welfare during an initial period in the country, for example during the period of mandatory introductory Swedish courses. For us to be able to use the refugee placement as an instrument, i.e. for the refugee placement to affect the costs of the receiving municipalities, we need a significant number of the refugees to stay on welfare also after the termination of the financial compensation scheme.

Franzén (2004) analyzes welfare dependency among immigrants, based on interviews conducted in 1996 with refugee immigrants that arrived in Sweden between 1980 and 1989. Of the immigrants in the sample, 24 percent are recipients of welfare benefits after 7-16 years in Sweden. In comparison, the share of welfare benefit recipients in the population in general in 1996 was below 10 percent (8.4 percent, SCB).

Hansen and Lofstrom (1999) also show that refugees as a group are less likely to move out of welfare than the native population; still after 20 years in Sweden both refugee and non-refugee immigrants show higher social assistance participation rates than statistically similar natives.

Our descriptive data shows that the average size of the annual refugee placement to a municipality, during the period we study, was equal to five percent of the pool of welfare recipients. Provided that many of the refugees stayed on welfare also after the compensating financial grant had run out, as suggested by the studies cited above, we can conclude that the effect on the welfare costs must have been quite substantial,

[15] Based on data from Statistics Sweden and the Migration Board. A person is definied as a refugee during the year of receiving a residence permit and the three following years. After that he/she is defined broadly as a foreign citizen or as a Swedish citizen if a Swedish citizenship is obtained (a refugee can obtain a Swedish citizenship at the earliest after four years).

at least for the municipalities in the upper part of the distribution.

This suggests that even though the municipalities were to some extent compensated for the refugee placement, we can expect some of the costs to remain after the compensation period. The fact that the municipalities were provided compensation for the first three to four years of the placement, furthermore suggests that the effect is probably lagged.[16]

It hence seems like the refugee placement program lead to an increase in the pool of welfare dependent inhabitants and to increased costs in the municipalities. The next important step is that the program affects the reaction function of the receiving municipality but not the reaction functions of its neighbors. This will next be analyzed within the theoretical framework laid out in section 3.

4.3 The policy intervention and the theoretical setup

In this section, we will analyze the policy intervention within the theoretical framework presented in section 3. In order to keep the section short and simple, we will assume positively sloping reaction functions, i.e. the race-to-the-bottom case.[17]

The refugee placement program can be described as a 3-stage game between regions A and B. We assume that only region B receives refugees, in order to derive how this affects the benefit levels of the two regions. (That is, we view region B as the "neighbor" and investigate how a change in its benefit level affects the benefit level in region A.) We furthermore assume that the refugees are poor (i.e., that they are welfare recipients). Unlike the native poor, however, we assume that the refugees do not work. This implies that the migration constraint of the refugees differs from that of the native (working) poor. We do not explicitly model any migration constraint mechanism for the refugees, but start by assuming that the refugees are immobile between the regions, and then analyse what happens if this assumption is relaxed.

In order to be able to separate between the arriving refugees and the "native poor", we change the notation of the native poor of region i to $\hat{N}_i$, and use $\mathring{N}_i$ to denote the refugees of region i.

[16] It can be noted that several of the Swedish municipalities that found themselves in financial troubles during the 1990s, claim that one of the main explanations to the financial situation in their municipality was due to increased costs in the wake of the refugee placement program. Also, several of the municipalities claim that they had been undercompensated in the first place, indicating that the inflow of refugees might have lead to a real cost for the municipalities earlier than three to four years after the placement.

[17] The points to be made in this section do not rest on this assumption, but hold for the model in general.

4.3.1 Case 1: Refugees immobile between the regions

Stage 0: We start in a stable equilibrium, where the benefit levels of the regions satisfy the first order conditions in the mobility case, and the migration equilibrium of the native poor, equation (8), is fulfilled. The benefit level of region i hence satisfies[18]:

$$MV'(w_i + B_i) = \frac{\hat{N}_i + \frac{\partial \hat{N}_i}{\partial B_i} B_i}{1 + w'(\hat{N}_i)\frac{\partial \hat{N}_i}{\partial B_i}}$$

and

$$w(\hat{N}_i) + B_i = w(2\overline{\hat{N}} - \hat{N}_i) + B_j \tag{8}$$

Stage 1: At stage one, the refugees, $\mathring{N}_B$, are placed in region B. The increase in the number of poor of the region increases the total benefit costs of the rich of region B. The inflow of refugees hence changes the budget constraint of the rich (see equation (3)) to also include the cost of the immigrants.

$$x = y - \frac{\hat{N}_B B_B}{M} - \frac{\mathring{N}_B B_B}{M} \tag{9}$$

The wage level is, however, unaffected by the refugee placement, since the refugees do not work. This also implies that the migration constraint of the native poor, equation (8), is unchanged. Assuming that the rich of a region only care about the native poor, we can rewrite the utility function of the rich in region B as[19]:

$$U(x_B, w_B + B_B) = y - \frac{\hat{N}_B B_B}{M} - \frac{\mathring{N}_B B_B}{M} + V(w(\hat{N}_B) + B_B) \tag{10}$$

How does the refugee placement affect the benefit levels of the regions? This depends on our assumption regarding the migration elasticity of the refugees with respect to the benefit levels of the regions. The assumption that the refugees are immobile between the regions introduces an asymmetry in the model, since the refugees are placed only in region B. The optimal benefit level of region B after the refugee placement is derived by maximizing equation (10) with respect to the benefit level and such that equation (8) holds. The resulting first order condition for region B is given by:

$$-\frac{(\hat{N}_B + \mathring{N}_B)}{M} - \frac{B_B}{M}\frac{\partial \hat{N}_B}{\partial B_B} + V'(w(\hat{N}_B) + B_B)\left[w'(\hat{N}_B)\frac{\partial \hat{N}_B}{\partial B_B} + 1\right] = 0 = \Omega^1 \tag{11}$$

By differentiating equation (11) with respect to B_B and $\mathring{N}_B$, we obtain the effect of

[18] We have a symmetric model, so the first order conditions for the regions are similar, and wages and the benefit levels for the two regions are equal in optimum.

[19] This assumption implies that the refugees only enter as a cost in the utility of the rich. This, together with the assumption that the poor do not work, ensures a negative effect on the benefit level in region B of the refugee placement.

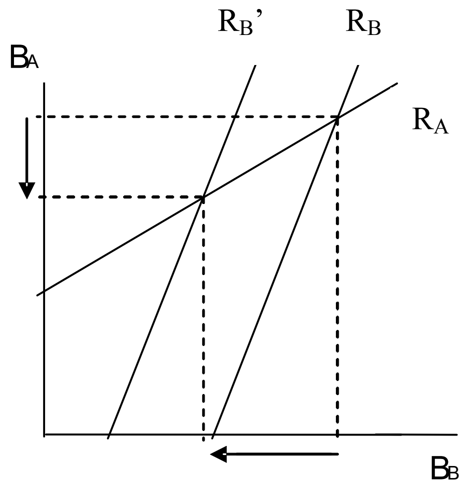
the refugee placement on the benefit level of region B[20]:

$$\frac{\partial B_B}{\partial \mathring{N}_B} = -\frac{\Omega^1_{\mathring{N}_B}}{\Omega^1_{B_B}} < 0$$

We see that the optimal benefit level of region B, given the benefit level of region A, is lower after the refugee placement. This implies a downward shift in the reaction curve of region B:s benefit level (see Figure 1).

Stage 2: At stage 2 region A responds to the decrease in B_B. Since no refugees have been placed in region A, the first order condition for the benefit provision of region A is equal to that of Stage 0. However, the benefit decrease of region B affects region A, since it makes some working poor from region B move to region A. Assuming linear reaction functions (as in section 2.3), with a positive slope (the "race to the bottom" case), the effects of the refugee placement in region B on the benefit levels of the two regions, can be illustrated by the reaction functions in Figure 1.

Figure 1: Reaction functions, No refugee mobility



In Figure 1 we see that the benefit levels of both regions are lower after the refugee placement. The mechanisms are the following: In the first stage, the reaction function of region B, R_B, shifts, because the regions' optimal level of benefit is now lower, *given* the benefit level of region A. In the second stage, region A reacts to the benefit decrease of its neighbor, by lowering its benefit level. The effect on B_A is hence channelled through B_B and corresponds to a movement along the reaction function of A, R_A, in the figure. As Figure 1 shows, the decrease is larger for the benefit level of region B.

[20] We know that $\Omega^1_{B_B} < 0$ by the assumption of strict concavity in $V(w(\hat{N}_B) + B_B)$. $\Omega^1_{\mathring{N}_B} = -\frac{1}{M} < 0$ is easily seen from equation (11).

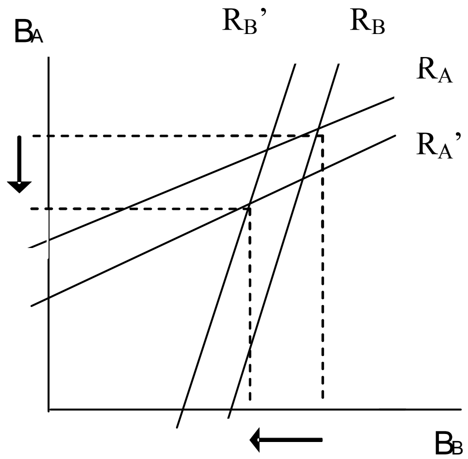
4.3.2 Case 2: Allowing the refugees to move

Figure 1 describes the case of zero migration elasticity of the refugees. What happens if we relax this assumption?

The assumption of immobile refugees introduced an asymmetry in our otherwise symmetric model. This asymmetry results in different optimal benefit levels of the regions. In addition, and more importantly for the empirics of this paper, the assumption of immobile refugees assures that all the effect of the refugee placement in region B on the benefit level in region A, is transmitted through the change in the benefit level in region B. This is important for the validity of the refugee placement as an instrument for the benefit level.

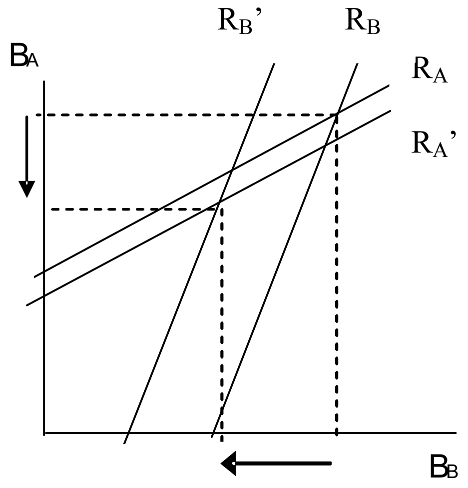
If we instead assume that the refugees are mobile with infinite migration elasticity with respect to the benefit level, we are back in a symmetric model. The intuition behind this is the following: If the migration of the refugees is perfectly elastic, it does not matter in which region they are initially placed, but they will "immediately" move to the region with the higher benefit level. The increase in the total benefit costs will be shared equally between the regions, and we will hence see equal shifts in the reaction functions, as shown in Figure 2.

Figure 2: Reaction functions, Perfect refugee mobility



We have now illustrated the interaction between the benefit levels in the cases of infinite and zero migration elasticity of the refugees with respect to the benefit levels. However, a more realistic assumption is that the migration elasticity is neither zero nor infinite, but somewhere in between these extemes. In this case the refugees do not react perfectly to differing benefit levels between the regions. This implies that even though some of the increase in the total benefit costs will spill over to region A, the costs will not be shared equally between the regions, but region B will bear the largest cost. In terms of the reaction functions, we will see shifts in the reaction functions of both regions, but that of region B will shift more. This scenario is illustrated in Figure 3.

Figure 3: Reaction functions, Limited refugee mobility



The limited migration elasticity of the refugees has introduced some asymmetry in the model. In Figure 3 we see that this results in an equilibrium that is characterized by lower benefits in region B than in region A.

4.3.3 Implications for the use of the refugee placement as an instrument

In terms of the appropriateness of using the refugee placement program as an intrument for the benefit level, we ideally wish that the refugees stay in the region in which they were initially placed, as in Figure 1. In this case the cost effect of the refugees is confined to region B, and we can be sure that any effect on region A:s benefit level is transmitted through B_B, i.e. is a result of interactions in the benefit levels.

If this is not the case, some of the cost effect may however "spill over" directly, through the migration of the refugees, which implies that some of the effect on B_A of the refugee placement in region B is a direct cost effect, and not a result of interactions on benefit levels.

5 Data and econometric considerations

5.1 Data

The reaction function derived in the theoretical model is estimated using data on the 281 municipalities' generosity in providing welfare benefits over the years 1989-94.[21]

[21] Seven municipalities (Gnesta, Trosa, Nyköping, Bollebygd, Borås, Lekeberg och Örebro) were excluded since they had been involved in either secessions or mergers of municipalities in the time period 1989-1994.

The reason for starting in 1989 is that we have to use the number of refugees in earlier periods as instruments (the longest lag is $t-4$; see below). And since we only have information on refugee placement from 1986, the first year in which we can use the welfare benefit levels is 1989.

There are several potential candidates for measuring the benefit generosity of a municipality. One is the norm that regulates the amount of benefits that a person is eligible for, another is the actual benefit expenditures. We choose to focus on the benefit expenditures normalized by the number of benefit recipients. There are a couple of reasons for this. First and foremost, it enables us to use a longer panel (data on expenditures is available for several years, while data on the benefit norm only is available for the years 1991, 1992 and 1994). In addition, by using benefit expenditures rather than the benefit norm, we avoid the risk of distortions based on imperfect implementation of the norm.[22] We believe that our definition of the welfare benefit level takes us closer to the "true generosity" of the municipalities. In the following, "benefit expenditures per benefit recipient" will therefore be intended, when we talk about the "benefit level".[23]

The drawback of our definition is that it is a rough measure, in the sense that it does not pick up variations in the time that a person spends on welfare - i.e. a person that is on welfare at some point during a year counts as a benefit recipient, regardless of the number of months he or she receives benefits. An alternative, and in this respect more precise, measure is to normalize the benefit expenditures by the total number of benefit months. We have chosen to normalize by the number of benefit recipients, since this strikes us as a more straightforward and intuitive measure, but will in the sensitivity analysis check that the results obtained in the baseline estimations are robust to the alternative normalizing factor.

During the period we study, the municipalities were free to set their own benefit norms.[24] From Table 1 it is clear that this decentralized decision-making in the setting of welfare generosity led to a large variation in the benefits paid out; the mean benefits paid out to each beneficiary was 8,300 SEK per year with a standard deviation of 2,200 SEK (and with a minimum of 2,000 and a maximum of 17,000 SEK).[25] In fact, it was the great variation between the municipalities, in particular

[22]There has been a discussion in Sweden that there is an heterogenous implementation of the benefit norm, both within and between municipalities.

[23]In the sensitivity analysis, we will however examine whether the results are sensitive to this by re-estimating the model using the norm instead of expenditures per beneficiary.

[24]There was no mandatory rule for the benefit levels, but general guidelines was provided by The National Board of Health and Welfare (Socialstyrelsen).

[25]All monetary variables are deflated to 1991 year values. There was also a significant variation in the benefit norms set by the municipalities. During the years 1991, 1992 and 1994 (which are the years for which we have information about the norms), the benefit norm averaged 112, with minimum and maximum levels at 80 and 145 and with a standard deviation of 7.8. (The norm is

the tendency to set the levels below the recommendations of the Board, that finally led to the introduction of a mandatory minimum level in 1998 (The National Board of Health and Welfare (1999)).

Descriptive statistics on the covariates are also given in Table 1. The covariates are unemployment, tax base, grants from the central government, population 19-29, and housing vacancies. Benefit level, tax base and grants are measured in 1000 SEK, while the rest of the covariates are given in percent. These are variables that have been included in similar studies and/or that we, based on Swedish welfare data, believe likely to affect the benefit expenditures.

Table 1: Descriptive statistics

Variable	Obs	Mean	Std.Dev	Min	Max
Benefit Level	1398	8.3	2.2	2	17
Unemployment	1405	4.5	2.6	0.2	12.2
Tax Base	1405	659	98	463	1366
Grants	1405	50	104	-1496	1531
Population 19-29	1405	13.9	1.8	10	23
Vacant Rentals	1382	3.9	4.2	0	31

As should be clear by now, we will use the number of refugees received by the municipalities to instrument for the benefit level. Since we have to consider lagged effects, we will use refugee data for the years 1986-1994. The overall mean number of refugees received by the municipalities over the period 1986-1994 was 84. In the analysis we will use the number of refugees normalized by the population in the municipality. The annual refugee placement to a municipality during the period equalled 0.3 percent of the population. Normalizing with the number of welfare recipients in the municipality, the corresponding number is 5.5 percent.

5.2 Do local governments respond to an exogenous increase in welfare prone individuals?

A first prerequisite for our story to have any bearing, i.e. for the policy intervention to be a relevant instrument, is that the number of refugees received by a municipality significantly affects the municipality's generosity in providing welfare benefits. As was discussed in the previous section, the real costs of a municipality of receiving refugees arises after three to four years. A first prerequisite for our story to have any bearing is then that the number of refugees received by a municipality in $t-3$ and/or $t-4$ significantly affects the municipality's generosity in providing welfare benefits. Using the same policy intervention as in this paper, this issue has been examined in detail by Dahlberg and Edmark (2004). When running a regression of the type

defined as the percentage of the basic amount and we have used the unadjusted levels for a single individual.).

$$B_{it} = \alpha_i + \lambda_t + \phi R_{i,t-3} + \delta R_{i,t-4} + X_{it}\beta + \varepsilon_{it} \quad (12)$$

where B_{it} is the welfare benefit level in municipality i in time period t, α_i is a municipality-specific fixed effect, λ_t is a time specific effect, $R_{i,t-3}$ ($R_{i,t-4}$) is the number of refugees per capita received in $t-3$ ($t-4$), X_{it} is a vector of time varying municipality-specific characteristics supposed to affect the benefit level, and ε_{it} is an error term, they find that the number of refugees enters significantly and with a negative sign; the more refugees a municipality received three to four years earlier, the less generous they are in providing welfare benefits today. This is true both when the number of refugees in the different time periods are used on their own and when they are used simultaneously (c.f. Dahlberg and Edmark, 2004).

The results in Dahlberg and Edmark (2004) indicate that the number of refugees received in both $t-3$ and $t-4$ might provide relevant instruments, and we will therefore use them as instruments in the baseline analysis.[26] For the policy intervention to provide a relevant instrument in this paper, we need, however, the average number of refugees in a municipality's reference group to be correlated with the average benefit level in the reference group. This will be examined in the section presenting the results.

5.3 Econometric considerations

Before turning to the results, we will discuss how the econometric model shall be specified, what we must control for and why, and how the reference group shall be specified.

The equation we aim at investigating will be of the type given in equation (1), and hence closely related to the reaction function as derived in equation (7). For the empirical work to be trustworthy, there are however some additional aspects that must be considered.

One such aspect is the distinction between endogenous and exogenous interactions. As discussed in section 2, to be able to identify the two channels separately, we must control both for the other municipalities' welfare benefit levels and for the characteristics of the other municipalities, while we at the same time use the suggested policy intervention to solve the simultaneity problem arising from the welfare game.

Another important aspect to be considered is confounding effects such as heterogeneity and correlated shocks that might induce a spurious correlation between a municipality's benefit level and the benefit levels of its reference group. To control for

[26] However, in the sensistivity analysis, we will examine how sensitive the baseline results are to different lags on the instruments. Since 1994 is the last year of the panel, all the observations on refugees are from the period under which the implementation of the refugee placement program was the strictest (i.e., up until 1991). This is an advantage, considering the exogeneity of the instruments.

unobserved heterogeneity, we control for both municipality-specific and time-specific fixed effects. The municipality-specific fixed effects will pick up any unobserved and unchanging characteristics of a municipality that are both related to its benefit level and the benefit levels in the municipalities that form its reference group. The time specific effects will pick up any unobserved macro effects that affect all local governments in the same way.

There may also be idiosyncratic shocks that could induce a spurious correlation. To control for such shocks, we include time-varying municipality-specific measures as, for example, the unemployment rate, income variables, and parts of the demographic structure.

As discussed by Manski (1993), we must control for correlated effects in order to properly identify the social interaction effect. Since the reference group is fixed, and since we control for fixed municipality effects, any unobserved time invariant characteristics of the municipality's reference group will not be part of the identification of the strategic interaction effect. We will also include reference group characteristics, i.e., group means of the set of covariates described earlier. These reference group specific characteristics thus control for correlated shocks within reference groups.[27]

In our case, as discussed earlier, we must also control for the number of vacant housing. If the number of vacant h ousing in a municipality affects the amount of refugees the municipality will receive, and if the number of vacant rentals is correlated with the welfare benefit level in the municipality, then omitting to control for the number of vacant rentals might induce a spurious correlation between the number of refugees and the benefit level.[28]

Next we turn to the question of how the reference group shall be specified. That is, which municipalities play welfare games with each other? It seems reasonable to assume that the municipalities' fear of welfare immigration from other municipalities is stronger the closer these municipalities are. Since the welfare recipients may have better information about the welfare generosity in nearby municipalities and since migration costs increases with distance, it also seems more likely that welfare recipients have a stronger migration response to differences in nearby municipalities than to differences in municipalities further away.[29] Furthermore, it seems reasonable to use a geographical definition of reference group since this captures the idea that geographical neighbors belong to the same media market and that they therefore have

[27] There are hence two reasons why the neighboring municipalities' characteristics should be included as regressors in the model: First, they must be included since we might have exogenous as well as endogeouns interactions. Omitting any one of them might hence yield biased estimates. Second, they must be included to control for any correlated shocks within reference groups.

[28] It can however be noted that the number of vacant rentals is close to a municipality-specific fixed effect, implying that this might already be controlled for by the fixed effect specification.

[29] These arguments are put forward by Saavedra (2000). As noted earlier, also Figlio et al. (1999) adopt a state's neighbors as the relevant reference group.

good information about the generosity of neighboring states.[30]

The richest model specification to be estimated is then given by

$$B_{it} = \alpha_i + \lambda_t + \gamma \overline{B}_{(-i)t} + X_{it}\beta + \overline{X_{it}}\delta + \varepsilon_{it} \tag{13}$$

where B_{it} is the welfare benefit level in municipality i in time period t, α_i is a municipality-specific fixed effect, λ_t is a time specific effect, $\overline{B}_{(-i)t}$ is the average welfare benefit level among municipality i's neighbors, X_{it} is a vector of time varying municipality-specific characteristics, $\overline{X_{it}}$ is a vector of average time varying characteristics of the geographical neighbors, and ε_{it} is an error term. In the empirical part, we will estimate four different specifications of equation (13). These specifications will be discussed in the next section.

5.4 Does refugee migration constitute a real problem?

As noted in section 4, the appropriateness of using the refugee placement program as an intrument for the neighboring municipalities' benefit level hinges on the migration elasticity of the refugees. Ideally, we wish that the refugees stay in the municipality in which they were initially placed (c.f. Figure 1). Otherwise, some of the cost effect may "spill over" directly through the migration of the refugees, which implies that some of the effect on B_A of the refugee placement in region B is a direct cost effect, and not a result of interactions on benefit levels.

How large is the risk of direct cost spill-overs between municipalities from refugee migration? This can be evaluted by looking at the migration pattern of refugees during our sample period. This information is unfortunately not directly available, but we can obtain an approximation by using sample data on the total immigration (refugees and non-refugees) to Sweden. The sample consists of approximately 20 percent of the immigrants to Sweden during 1987-89. Following Åslund (2000) and Edin et al. (2003), we exclude observations of immmigrants originating from OECD-countries and a number of additional western European countries from the sample, in an attempt to remove the non-refugee immigrants from the sample. In addition, immigration of relatives of the refugees has been removed from the sample. We are left with a sample of 9,283 observations, which is indeed roughly equal to 20 percent of the total number of refugees during the period.[31]

In the data we can observe the municipality of residence for the refugees during the year of arrival, and four years after arrival. The sample hence informs us of the migration pattern of the refugees between these time periods.

[30] For example, Besley and Case (1995) also use this definition of reference group in their empirical analysis of social interaction effects in the form of tax competition between neighboring U.S. States.

[31] The number of granted residence permits over the period 1987-89 for refugees were 55046 (The Immigration Board).

Looking at some descriptive statistics, we see that 9,080 of the 9,283 refugees were still living in Sweden four years after the initial placement (i.e., 203 of the refugees had either migrated from Sweden or died). Out of the 9,080, 60.5 percent were still living in the municipality in which they were initially placed. This means that 3,589 refugees had changed municipality after four years. Where had they moved?

It turns out that it is the three big towns in Sweden (Stockholm, Malmö, and Göteborg) and their surrounding areas that are the main magnets. Out of the refugees that had changed municipality, the majority (68 percent) had moved to or within one of the counties of these three towns; the Stockholm, Malmö and Västra Götaland counties (roughly 60 percent of them had moved from counties other than these three, and approximately 40 percent had moved within or between these counties). We can conclude that the main migration flows are to these counties.

Since we use neighboring municipalities as each municipality's reference group, our instrument is especially sensitive to refugee migration between neigboring municipalities. In our sample, such migration is rare; only 624 of the refugees have moved to a neighboring municipality after four years. This is equal to 6.7 percent of the total sample, or 17 percent of those that have moved. 367 of them had moved to or within the Stockholm, Malmö and Västra Götaland counties.

Three conclusions can be drawn from these descriptive statistics. First, even after four years, the majority of the refugees (60.5 percent) is still living in the municipality in which they were initially placed. Second, only a small fraction of the total number of refugees that arrived four years earlier, had moved to a neighboring municipality. This implies that the risk that our results suffer from bias stemming from direct "cost spillovers" between the municipalities is small. Third, out of those that after four years had migrated within Sweden, the great majority had moved to or within one of the three big city counties in Sweden: the Stockholm, Malmö and Västra Götaland counties. This means that if there is any "cost shifting" going on, it is mainly the three big cities and the surrounding areas that are bearing these costs.

The fact that the migration between neighboring municipalities seems to be small suggests that the risk of direct cost spill-overs is probably small. Furthermore, the majority of the refugees remain in the municipality of placement four years after arrival. We will therefore initially conduct the analysis without taking account of municipality-wise refugee migration. We will, however, test for the robustness of the results to secondary migration by presenting estimation results when the three migration-magnet counties are excluded.

6 Results

In this section we present our results. First, we present, for comparative reasons, the OLS results. Second, we present the reduced form estimates (i.e., the first stage estimates in the two-stage procedure and the estimates when we regress our dependent variable on the instruments). Finally, we present our IV-estimates that are supposed to measure the causal effect of the welfare generosity of neighboring municipalities on the welfare generosity of a certain municipality. In all cases, we will consider four different specifications. In specification I, we only control for time dummies and municipality-specific fixed effects. In specification II, we also control for the number of housing vacancies in the municipalities.[32] In specification III, we add the observable characteristics of a municipality. In specification IV, finally, we add all observable characteristics of a municipality and its neighbors.

6.1 OLS results

For comparative reasons, we initially neglect simultaneity and start by estimating the model without using any instruments. As can be seen from Table 2, neighbors' benefit level enters significantly and with a positive sign; if neighbors increase their welfare benefit level with 100 SEK per beneficiary, a municipality increases its benefit level with approximately 55 SEK per beneficiary. Also, in line with our argumentation, the number of refugees in $t-3$ and $t-4$ significantly affects the welfare generosity of the municipalities; the more refugees per capita that a municipality received three to four years earlier, the lower is the welfare benefit level today.

6.2 Reduced form estimates

In this section, we present the reduced form estimates. We begin with the first stage regression in the IV approach, i.e. where we run the average welfare benefit level in neighboring municipalities (the endogenous variable) on the instruments (the average number of refugees that was placed in the neighboring municipalities, measured as share of the population, in $t-3$ and $t-4$). The reduced form estimates of the endogenous variable on the instruments provide information about the relevance of the instruments. These results are presented in Table 3. As can be seen from the results in the first two rows, there is a significant and negative association between the number of refugees and the generosity in the setting of welfare benefits; the

[32] The number of housing vacancies is used as control variables among the neighbors' characteristics not to disturb the validity of the instruments (as discussed in section 4). Using the number of housing vacancies as a control variable in each municipality as well do not change the results at all. As a matter of fact, the housing vacancies in each municipality always enter insignificantly when included in the specification. This is probably due to the fact that, as noted in section 4, the variable is close to a fixed effect.

Table 2: Estimating the model without instrumenting neighbors' benefit level.

	(1)	(2)	(3)	(4)
Neighbors' benefit level	.333***	.632***	.567***	.546***
	(.047)	(.052)	(.048)	(.05)
Refugees ($t-3$)			-1.185***	-1.13***
			(.328)	(.326)
Refugees ($t-4$)			-1.622***	-1.563***
			(.318)	(.322)
Unemployment			-.003	.046
			(.052)	(.054)
Tax base			.0005	.0008
			(.002)	(.003)
Grants			.0008***	.0007**
			(.0003)	(.0003)
Population 19-29			.123	.15
			(.103)	(.107)
Neighbors' characteristics				
Unemployment				-.227**
				(.089)
Tax base				.0005
				(.004)
Grants				-.00009
				(.0004)
Population 19-29				-.19
				(.178)
Vacant rentals ($t-3$)		-.033	-.029	-.032
		(.045)	(.046)	(.047)
Vacant rentals ($t-4$)		-.034	-.067	-.077*
		(.05)	(.047)	(.048)
Fixed effects	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Obs.	2225	1383	1369	1369
R^2	.717	.811	.823	.824

higher the average placement of refugees was among the neighboring municipalities in $t-3$ and $t-4$, the lower is the average welfare benefit level among the neighboring municipalities in t. The instruments hence seem to be relevant.

The reduced form OLS regression of the dependent variable (welfare benefit level in a municipality) on the instruments (average number of refugees in neighboring municipalities in $t-3$ and $t-4$) and the other covariates also provide valuable information if the instruments are weak since OLS is unbiased in a sample of any size, regardless of the power of the instruments, whereas the IV estimates can be noticeably biased. In other words, the existence of a causal relationship between the dependent variable (welfare benefit level in a municipality) and the endogenous regressor (average welfare benefit level in neighboring municipalities) can be examined through the reduced form without fear of finite sample bias of the IV estimator even if the instrument is weak.[33] Therefore, we next turn to the reduced form estimates

[33] We do however not have any indications that our instruments are weak. Staiger and Stock (1997)

Table 3: First stage estimates. Dependent variable: Neighbors' benefit level. Excluded instrument: Refugees per capita received by neighboring municipalities in t - 3 and t - 4.

	(1)	(2)	(3)	(4)
Neighbors' refugees $(t-3)$	-2.998***	-2.959***	-2.737***	-2.922***
	(.393)	(.386)	(.404)	(.395)
Neighbors' refugees $(t-4)$	-3.115***	-3.186***	-3.183***	-3.218***
	(.374)	(.383)	(.38)	(.382)
Refugees $(t-3)$			-.45**	-.318*
			(.179)	(.175)
Refugees $(t-4)$			-.212	-.045
			(.165)	(.164)
Unemployment			-.136***	-.025
			(.032)	(.033)
Tax base			-.0005	.001
			(.002)	(.002)
Grants			.00002	.00006
			(.0002)	(.0002)
Population 19-29			-.011	.003
			(.066)	(.068)
Neighbors' characteristics				
Unemployment				-.446***
				(.056)
Tax base				-.002
				(.003)
Grants				.0008***
				(.0003)
Population 19-29				.039
				(.116)
Vacant rentals $(t-3)$		.002	.013	.015
		(.035)	(.034)	(.034)
Vacant rentals $(t-4)$		-.131***	-.15***	-.175***
		(.03)	(.03)	(.031)
Fixed effects	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Obs.	1395	1390	1376	1376
R^2	.865	.869	.872	.88

obtained by regressing the dependent variable on the instruments. As is clear from the first row in Table 4, there is a significant and negative association between the average number of placed refugees in the neighboring municipalities three to four years earlier and the generosity in the setting of welfare benefits in a certain municipality today. Hence, there are indications of a causal relationship between the welfare benefit levels in neighboring municipalities and the welfare benefit level in a specific municipality.

suggest using the F-statistic for the joint significance of the excluded instruments in the first-stage equation as a diagnostic of the power of the instruments. They argue that if the F-statistic is larger than 10, there should be no problem associated with weak instruments. Conducting partial F-tests on the excluded instruments in the first-stage regression, we get F-statistics that are in the interval 72-94 for the four specifications.

Table 4: Reduced form estimates: Dependent variable (each municipality's benefit level) on the instruments

	(1)	(2)	(3)	(4)
Neighbors' Refugees ($t-3$)	-2.913***	-2.943***	-2.409***	-2.545***
	(.637)	(.643)	(.56)	(.563)
Neighbors' Refugees ($t-4$)	-2.267***	-2.251***	-1.54***	-1.6***
	(.591)	(.605)	(.566)	(.586)
Refugees ($t-3$)			-1.377***	-1.222***
			(.354)	(.347)
Refugees ($t-4$)			-1.688***	-1.523***
			(.35)	(.353)
Unemployment			-.073	.04
			(.055)	(.059)
Tax base			.0002	.002
			(.002)	(.003)
Grants			.0008***	.0008***
			(.0002)	(.0003)
Population 19-29			.117	.154
			(.11)	(.114)
Neighbors' characteristics				
Unemployment				-.479***
				(.095)
Tax base				0
				(.004)
Grants				.0002
				(.0005)
Population 19-29				-.138
				(.186)
Vacant rentals ($t-3$)		-.034	-.029	-.035
		(.049)	(.05)	(.051)
Vacant rentals ($t-4$)		-.118**	-.149***	-.17***
		(.052)	(.049)	(.05)
Fixed effects	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Obs.	1398	1391	1377	1369
R^2	.785	.788	.801	.806

6.3 IV estimates

The two-stage least squares estimates are presented in Table 5. As can be seen from the first row, there is a significant and positive effect from the setting of welfare benefit levels in neighboring municipalities on the setting of the welfare benefit level in a given municipality. The point estimates indicate that if the neighboring municipalities decrease their welfare benefit level with 100 SEK, a municipality decreases its benefit level with approximately 66 SEK (c.f. specification (4)). The estimates for neighbors' benefit level hence provide indications of strategic interactions among the local governments in the setting of welfare benefit levels, implying that there exists a "race-to-the-bottom".

An interesting thing to note is that the IV-estimates are only slightly higher than the OLS estimates (c.f. Table 2), and they are not significantly different from the OLS estimates. This indicates that the bias resulting from OLS estimation of models of welfare competition can be seen as rather negligble, at least when using the definition of welfare generosity used here.

Turning to the other explanatory variables, it is clear from specifications (3) and (4) that the number of refugees received by a municipality in $t-3$ and $t-4$ both have a significant and negative effect on the municipality's welfare benefit level in t, which is in line with the main argument of this paper, and that the intergovernmental grants received from the central level has a significant and positive effect on the municipality's benefit level. The municipality's tax base, unemployment rate and population aged 19-29 do however not seem to have any significant impacts on the municipality's welfare generosity.

Turning to the neighboring municipalities' characteristics, there seems, at a first glance, to be some exogenous interaction at work since the neighboring municipalities' unemployment rate enters significantly (although marginally so). However, when conducting a F-test, we cannot reject the null hypothesis that the neighboring municipalities' characteristics (unemployment rate, tax base, grants, and population aged 19-29) are jointly zero. This indicates that there is possibly no exogenous interaction at work.

Table 5: IV estimates for baseline specification

	(1)	(2)	(3)	(4)
Neighbors' benefit level	.834***	.834***	.648***	.661***
	(.118)	(.118)	(.116)	(.117)
Refugees $(t-3)$			-1.108***	-1.034***
			(.344)	(.337)
Refugees $(t-4)$			-1.548***	-1.479***
			(.333)	(.33)
Unemployment			.011	.056
			(.055)	(.055)
Tax base			.0005	.0009
			(.002)	(.003)
Grants			.0008***	.0007**
			(.0003)	(.0003)
Population 19-29			.137	.16
			(.105)	(.108)
Neighbors' characteristics				
Unemployment				-.182*
				(.097)
Tax base				.0001
				(.004)
Grants				-.0003
				(.0005)
Population 19-29				-.163
				(.181)
Vacant rentals $(t-3)$		-.027	-.028	-.033
		(.046)	(.047)	(.047)
Vacant rentals $(t-4)$		-.013	-.057	-.059
		(.051)	(.049)	(.05)
Fixed effects	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Obs.	1388	1383	1369	1369
R^2	.805	.808	.823	.823
Hansen J-statistic	0.754	0.985	2.103	2.103
(p-value)	(0.385)	(0.321)	(0.147)	(0.147)

7 Sensitivity analysis

In this section we will conduct several sensitivity analyses to check the robustness of the baseline results obtained in the previous section. In particular, we will examine how sensitive the results are to (i) migration among refugees, (ii) different lag structures on the instrument (i.e., different lag lengths on the refugee variable), (iii) different definitions of the welfare generosity variable, and (iv) the use of neighbors' characteristics as instruments (as used by, e.g., Figlio et al., 1999).

7.1 Migration among refugees

As suggested by the theoretical model, the policy intervention may not provide a valid instrument if the secondary migration of refugees (i.e., any migration that takes place after the initial placement) among municipalities is large. Our choice of instruments is motivated by the fact that refugees that arrive to a municipality are statistically likely to become recipients of welfare, and hence increase the welfare benefit costs of the municipality. This, of course, hinges on the assumption that the refugees stay in the municipality that they were initially assigned. It is hence of great importance to test the robustness of the results to such migration.

The descriptive statistics of section 4.4 showed that the migration flows of the refugees during the time period we study first and foremost were directed towards the counties of the three largest towns, the Stockholm, Malmö and Västra Götaland counties. Furthermore, out of the 624 persons in the sample that had migrated to a neighboring municipality, more than half (367) had moved to municipalities in these counties. A straighforward sensitivity analysis is to re-estimate the model without these counties. If the baseline results are biased because of secondary refugee migration, excluding these observations will provide a model with more valid instruments.[34] The results of the IV-estimation, excluding the municipalities of the three "big city counties" are presented in Table 6.

Table 6 shows that excluding the counties that are the main "migration magnets" does not substantially change the results. The coefficients of the neighbors' benefit level are significant in all model specifications and are very close to those obtained in the baseine specification (c.f. Table 5). As expected, the Hansen J-statistic is smaller when the three migration magnets are excluded. We conclude that the baseline results do not seem to be driven by the migration of the refugees.

7.2 Different lag structures on the instrument

Next, we will investigate how sensitive the baseline results are to different lag structures on the instrument (i.e., on refugees). In the baseline estimations we used the average number of refugees in $t-3$ and $t-4$ as instruments. What happens if we have other lags or combinations of different lags? The results when we use different sets of instruments are presented in Table 7. In Table 7 we only report the coefficient for the neighboring municipalities benefit level, implying that each cell corresponds to a separate regression. The covariates used in each regression are the same as those used earlier for specifications (1)-(4).

The first row in Table 7 simply replicates the baseline estimates (i.e., it shows the results when we use the number of refugees in $t-3$ and $t-4$ as instruments). When we use the instruments lagged $t-2$, $t-3$, and $t-4$, we get significant estimates in the same order of magnitude as in the baseline estimations (c.f. the second row). When we also use the instrument lagged only one period, we get significant estimates

[34] It shall be noted that the municipalities are only dropped as dependent variables, i.e. they are kept when we compute the neighbors' benefit levels and characteristics. We do this to minimize the distorsion of the exclusion of the observations of the big city counties.

Table 6: Sensitivity analysis: IV estimates when the counties of Stockholm, Malmö and Västra Götaland are excluded

	(1)	(2)	(3)	(4)
Neighbors' benefit level	.841***	.834***	.672***	.71***
	(.141)	(.147)	(.14)	(.141)
Refugees ($t-3$)			-1.136***	-1.014***
			(.384)	(.373)
Refugees ($t-4$)			-1.492***	-1.334***
			(.385)	(.382)
Unemployment			-.048	-.002
			(.063)	(.064)
Tax base			-.0003	.005
			(.003)	(.004)
Grants			.003***	.003***
			(.001)	(.001)
Population 19-29			.125	.113
			(.156)	(.159)
Neighbors' characteristics				
Unemployment				-.025
				(.114)
Tax base				-.017***
				(.007)
Grants				-.001
				(.002)
Population 19-29				-.097
				(.266)
Vacant rentals ($t-3$)		.032	.023	.01
		(.068)	(.067)	(.067)
Vacant rentals ($t-4$)		-.058	-.079	-.064
		(.058)	(.054)	(.055)
Fixed effects	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Obs.	869	864	858	858
R^2	.774	.78	.804	.805
Hansen J-statistic	0.562	0.456	1.758	0.923
(p-value)	(0.453)	(0.499)	(0.185)	(0.337)

when all instruments are used simultaneously (c.f. the third row) but insignificant estimates when the instrument lagged one period is used on its own (c.f. the fourth row). The latter result is in line with expectations given how the program was set up. From the last three rows, we note that when the instrument lagged $t-2$, $t-3$, and $t-4$ are used one at a time, we get significant estimates in the same order of magnitude as in the baseline estimations.

It can also be worth mentioning that when we test the validity of the instruments, using the Hansen J-test for overidentifying restrictions, the results suggest that any combination of the lags $t-2$, $t-3$, and $t-4$ produces exogenous intruments. When the first lag is included, the null hypothesis of exogenous variation is, however, rejected.[35]

[35] This is consistent with the fact that the implementation of the program, and hence the exogeneity of refugee placement, was the strictest during the earlier years.

Table 7: Sensitivity analysis: Different lag lengths on the instruments. Only the coefficient of the neighboring municipalities benefit level is presented.

	(1)	(2)	(3)	(4)
$t-3, t-4$	.834***	.834***	.648***	.661***
	(.118)	(.118)	(.116)	(.117)
$t-2, t-3, t-4$	.857***	.869***	.722***	.723***
	(.119)	(.125)	(.115)	(.114)
$t-1, t-2, t-3, t-4$	.772***	.765***	.608***	.619***
	(.103)	(.111)	(.101)	(.1)
$t-1$	.186	.14	.257	.272
	(.165)	(.174)	(.164)	(.167)
$t-2$	.827**	.883***	1.056***	1.027***
	(.346)	(.335)	(.282)	(.278)
$t-3$	.804***	.811***	.787***	.798***
	(.166)	(.168)	(.153)	(.152)
$t-4$	.78***	.784***	.565***	.565***
	(.138)	(.138)	(.141)	(.147)

7.3 Different definitions of welfare generosity

Next, we will examine how sensitive the baseline results are to alternative definitions of the welfare generosity variable. In particular, we will examine what happens if we use the benefit norm or welfare expenditures per benefit month instead of welfare expenditures per beneficiary. Starting by looking at simple correlations between the three measures of welfare generosity, it is obvious that the benefit norm is only slightly correlated with the two expenditures measures, while the correlation between the two expenditures measures are much higher (c.f. Table 8). This indicates that the norm is not perfectly implemented, as discussed earlier.[36] The IV estimates for the three alternative definitions of the municipalities' welfare generosity is presented in Table 9. In Table 9 we only report the coefficient for the neighboring municipalities benefit level, implying that each cell corresponds to a separate regression. The covariates used in each regression are the same as those used earlier for specifications (1)-(4).[37] When using the welfare expenditures per benefit month, we note from the last row that we get very similar results as those in the baseline estimation (c.f. the middle row). When using the benefit norm, we get point estimates that are much higher than in the baseline estimations, even though they are not significantly different from each other. In all cases, the estimated effects are however significant, indicating that the baseline conclusion of strategic interaction among the municipalities in the setting of welfare benefit levels does not hinge on the way the welfare benefit level was defined.

[36] This pattern is also observed by Hernes Fiva and Rattso (2003) on Norwegian data.

[37] A difference is that we only have information about the benefit norm for the years 1991, 1992 and 1994, implying that we have fewer observations in those estimations (from 768 in specifications (3) and (4) to 777 in specification (1)).

Table 8: Correlations between the three measures of welfare generosity

	Benefit norm	Exp./beneficiary	Exp./benefit month
Benefit norm	1		
Expenditures/beneficiary	-0.147	1	
Expenditures/benefit month	-0.090	0.791	1

Table 9: Sensitivity analysis: Alternative definitions of the welfare benefit level. Only the coefficient of the neighboring municipalities benefit level is presented.

	(1)	(2)	(3)	(4)
Benefit norm	1.519**	1.388**	1.704**	1.825*
	(.65)	(.624)	(.815)	(.995)
Expenditures per beneficiary	.834***	.834***	.648***	.661***
	(.118)	(.118)	(.116)	(.117)
Expenditures per benefit month	.858***	.829***	.485***	.561***
	(.21)	(.206)	(.16)	(.157)

7.4 Neighbors' characteristics as instruments

As discussed earlier, there are two "theoretical" reasons why the neighboring municipalities' characteristics should be included as regressors in the model: First, they must be included since we might have exogenous as well as endogeouns interactions. Omitting any one of them might hence yield biased estimates. Second, they must be included to control for any correlated shocks within reference groups. From the baseline estimations, there was however only little, if any, evidence of exogenous interactions among the municipalities. Furthermore, if there are no correlated shocks on the observables, it might be the case that the neighboring municipalities' characteristics can be used as instruments. This is interesting to examine, since this type of instruments have been used earlier in the literature (e.g., by Figlio et al., 1999).

The results when we use the neighboring municipalities' characteristics as instruments are presented in Table 10.[38] There are two things to note from these results. First, the tests for overidentifying restrictions indicate that the instruments are valid. Second, the point estimates for the neighbors' benefit level are somewhat higher, even though they are in the same ballpark, as the baseline estimates. Also, the results are closer to those obtained by Figlio et al. (1999).

7.5 Some additional sensitivity analyses

In addition to the above discussed sensitivity analyses, we have checked the robustness of the baseline estimates to: (i) another functional form (using a semi-logarithmic specification), (ii) the inclusion of a lagged dependent variable on the right-hand

[38] The instruments are the neighbors' unemployment rate, tax base, grants, and share of the population in the age interval 19-29.

Table 10: Sensitivity analysis: Neighbors' characteristics as instruments

	(1)	(2)	(3)
Neighbors' benefit level	.943***	.96***	.932***
	(.182)	(.218)	(.186)
Refugees ($t-3$)			-.815**
			(.365)
Refugees ($t-4$)			-1.26***
			(.35)
Unemployment		.071	.062
		(.053)	(.058)
Tax base		.003	.0008
		(.002)	(.002)
Grants		.001***	.0008**
		(.0004)	(.0003)
Population 19-29		-.054	.188*
		(.081)	(.111)
Fixed effects	yes	yes	yes
Time dummies	yes	yes	yes
Obs.	2225	2225	1374
R^2	.682	.681	.808
Hansen J-statistic	4.013	3.551	4.856
(p-value)	(0.260)	(0.314)	(0.183)

side (estimating the model in first differences), (iii) the inclusion of lagged covariates (but no lag on the dependent varianble). In none of these three cases do we reach other conclusions than those reached in the baseline analysis. We have also examined how sensitive the baseline results are to an alternative normalization of the refugees variable: Normalizing the number of refugees with the number of individuals on welfare in the municipality instead of normalizing it with the municipality's entire population yields almost identical results as in the baseline case.

8 Conclusions

In this paper we re-investigate whether local governments react to the welfare benefit levels in neighboring jurisdictions when setting their own benefit levels. The main contribution of the paper is that we suggest and use a specific policy intervention as an instrument to solve the simultaneity problem that arises from the welfare game that the local governments play; a centrally geared placement of a highly welfare prone group (refugees) among Swedish municipalities.

We argue in the paper that given that one controls for the number of vacant apartments in the municipality and for municipality-specific fixed effects, the refugee placement can be considered as exogenous. Furthermore, we show, in a theoretical framework following the work of Bruckner (2000), Brown and Oates (1987), and Wildasin (1991), that, given certain assumptions, it is appropriate to use the refugee placement program as an instrument since the refugee placement program shifts the neighboring local governments' reaction function while holding the reaction function of my local government fixed (implying that the effect on my benefit level is only chanelled trough the benefit level of the neighboring jurisdictions).

In the application we use panel data for Swedish municipalities. In addition to controlling for observable characteristics of the municipalities, we control for both municipality-specific and time-specific fixed effects. In the baseline analysis, we find a significant and positive effect from the setting of welfare benefit levels in neighboring municipalities on the setting of the welfare benefit level in a given municipality. The point estimates indicate that if the neighboring municipalities decrease their welfare benefit level with 100 SEK, a municipality decreases its benefit level with approximately 65 SEK. The estimates for neighbors' benefit level hence provide indications of strategic interactions among the local governments in the setting of welfare benefit levels, implying that there exists a "race-to-the-bottom". These results seem to be robust to several alternative model specifications.

In addition, we find that the IV estimates are only slightly higher than the OLS estimates (and they are not significantly different from them), indicating that the possible bias from using OLS estimation is negligable. We also find that exogenous interactions seem to be negligible, indicating that neighboring jurisdictions' characteristics (other than their welfare benefit levels) might be used as instruments when trying to solve the simultaneity problem arising from the welfare game. However, this result might be data-specific and should be tested in each case.

We do not believe that the policy intervention that we suggest and use as an instrument in this paper is unique for Sweden. Similar programs exist in other countries (for example in Norway), and we believe that the use of such programs can be a fruitful way of approaching the problem encountered in models of welfare competition.

References

Åslund, O.: 2000, Immigrant settlement policies and subsequent migration. Working Paper 2000:23, Department of Economics, Uppsala University.

Besley, T. and Case, A.: 1995, Incumbent behavior: Vote seeking, tax setting and yardstick competition, *American Economic Review* pp. 25–45.

Brown, C. and Oates, W.: 1987, Assistance to the poor in a federal system, *Journal of Public Economics* pp. 307–330.

Bruckner, J.: 2000, Welfare reform and the race to the bottom: Theory and evidence, *Southern Economic Journal* pp. 505–525.

Dahlberg, M. and Edmark, K.: 2004, Do local governments respond to an exogenous increase in welfare prone individuals? Unpublished Paper, Department of Economics, Uppsala University.

Edin, P.-A., Fredriksson, P. and Åslund, O.: 2003, Ethnic enclaves and the economic success of immigrants - evidence from a natural experiment, *The Quarterly Journal of Economics* pp. 329–357.

Enchautegui, M.: 1997, Welfare payments and other determinants of female migration, *Journal of Labor Economics* pp. 529–554.

Figlio, D., Kolpin, V. and Reid, W.: 1999, Do states play welfare games?, *Journal of Urban Economics* pp. 437–454.

Franzén, E.: 2004, *Egenförsörjning eller bidragsförsörjning? Invandrarna, arbetsmarknaden och välfärdsstaten*, SOU 2004:21, chapter Invandrare och socialbidragstagare - ett liv i ofärd, pp. 103–131.

Gramlich, E.: 1984, Migration and income distribution responsibilities, *The Journal of Human Resources* pp. 489–511.

Hansen, J. and Lofstrom, M.: 1999, Immigrant assimilation and welfare participation: Do immigrants assimilate into or out-of welfare? Discussion Paper No 100, IZA.

Hernes Fiva, J. and Rattso, J.: 2003, Welfare competition in norway. Department of Economics, Norwegian University of Science and Technology, Trondheim.

Levine, P. and Zimmerman, D.: 1995, An empirical analysis of the welfare magnet debate using the NLSY. NBER Working Paper No 5264.

Manski, C.: 1993, Identification of endogenous social effects: The reflection problem, *Review of Economic Studies* pp. 531–542.

Meyer, B.: 2000, Do the poor move to receive higher welfare benefits? Unpublished Paper, Department of Economics, Northwestern University.

Moffitt, R.: 2000, Policy interventions, low-level equilibria and social interactions. Department of Economics, Johns Hopkins University.

Saavedra, L.: 2000, A model of welfare competition with evidence from AFCD, *Journal of Urban Economics* pp. 248–279.

Smith, P.: 1991, An empirical investigation of interstate AFDC benefit competition, *Public Choice* pp. 217–233.

Staiger, D. and Stock, J.: 1997, Instrumental variables regressions when the instruments are weak, *Econometrica* pp. 557–586.

The Immigration Board: 1997, *Individuall mångfald: Invandrarverkets utvärdering och analys av det samordnade flyktingmottagandet 1991-96*, The Immigration Board.

The National Board of Health and Welfare: 1999, Ändrad Socialtjänstlag - konsekvenser under de 18 första månaderna.

Walker, J.: 1994, Migration among low-income households: Helping the witch doctors reach consensus. Working Paper.

Wheaton, W.: 2000, Decentralized welfare provision: Will there be underprovision?, *Journal of Urban Economics* pp. 536–555.

Wildasin, D.: 1991, Income redistribution in a common labor market, *American Economic Review* pp. 757–754.