Does Welfare Policy Affect Residential Choices? Evidence from a Natural Experiment

Jon H. Fiva

Center for Economic Research, Department of Economics,
Norwegian University of Science and Technology, N-7491 Trondheim, Norway.
E-mail: jon.fiva@svt.ntnu.no

ALL RESULTS ARE PRELIMINARY! COMMENTS ARE WELCOME.

Abstract

Although several studies have stressed that welfare policy may affect residential choices of welfare recipients, few studies have taken into account that residential choices of welfare recipients affect welfare policy. If residential choices are endogenous, then cross sectional variation in welfare benefits is as well. It follows that studies that take variation in welfare benefits as exogenous may obtain biased and inconsistent parameter estimates. I address this policy endogeneity by utilizing a centrally implemented reform which changed welfare benefit levels in many Norwegian local governments. I show that ignoring the policy endogeneity may give rise to a downward bias in the estimated welfare migration responses.

JEL Classification: I38, H73, H77, R23
Keywords: Natural experiment, policy endogeneity, migration, welfare benefits, fiscal federalism.

First version: March, 2006
This version: May 22, 2006

* Data used in this analysis are from Statistics Norway's database 'FD-trygd'. I am grateful to Nicolas J. I. Rodriguez at the Norwegian Social Science Data Service for aggregating the data and for funding from the Norwegian Research Council under the FIFOS program. I wish to thank seminar participants at the University of Oslo, Norwegian University of Science and Technology and Statistics Norway for helpful comments and suggestions, in particular Fredrik Carlsen and Jørn Rattsø. The responsibility for the analysis conducted and the conclusions drawn remains with the author.

1. Introduction

A large literature in public finance going back to Stigler (1957) and Musgrave (1959) has warned against the consequences of decentralized responsibility for redistribution due to household mobility. The basic argument is that policies that are redistributive in nature give rise to a phenomenon that resembles adverse selection: net beneficiaries of redistributive policies are attracted to generous jurisdictions, while net contributors are repelled. It follows that jurisdictions have incentives to behave strategically in their welfare policy to avoid becoming 'welfare magnets'. Because the concern about welfare migration limits welfare provision in all jurisdictions, no jurisdiction succeeds in repelling welfare recipients and the equilibrium is characterized by all jurisdictions setting lower benefits than they would in the (hypothetical) no-mobility case (Wildasin, 1991).[1] Such reasoning has lead Stigler and other scholars to the conclusion that "redistribution is intrinsically a national policy" (Stigler, 1957, p. 217). However there are also potential benefits to reap from decentralized responsibility for redistribution. A decentralized system may for example be better able to tailor (appropriate) benefits to those that are truly in need and may be better at maintaining bureaucratic control. These and other aspects are discussed in detail by Ladd and Doolittle (1982). In designing a well functioning public sector one needs to trade these potential benefits of decentralization with the welfare costs related to welfare migration. It follows that theoretical models that rely on different assumptions on household mobility (in response to fiscal parameters) are likely to give different implications for assignment of redistributive policy across different tiers of government. From a normative perspective then, it is interesting to evaluate whether welfare migration is important in practice. This is the object of the current analysis.

I follow a large literature that evaluate whether the residential location of welfare recipients (often proxied for by poor households) depends on welfare generosity. These studies typically analyze either aggregated migration flows or individual level migration choices. Looking for evidence in favor of welfare migration, these studies typically try to answer the question: Does jurisdictions that offer higher welfare benefits attract poor people who would not otherwise move there and by retaining poor people who might otherwise have chosen to leave? I refer to this as the 'welfare migration hypothesis' in the following.

[1] This phenomenon is sometimes referred to as a 'race to the bottom' in welfare policy. This term is somewhat misleading since theoretical models of fiscal competition typically do not suggest an intense race to the bottom, but a general downward pressure on welfare generosity.

There are serious methodological challenges to test the welfare migration hypothesis. Looking for evidence of welfare migration most existing studies rely on cross-sectional variation in welfare benefits to identify welfare migration. However, as accentuated by Moffit (1992, pp. 34-36), if residential choices are endogenous, then cross sectional variation in welfare benefits is as well. It follows that studies that take variation in welfare benefits as exogenous may obtain biased and inconsistent parameter estimates. If welfare benefits affect residential decisions of the poor, then residential choices of the poor is likely to affect how benefit levels are set. Since rational politicians are likely to respond to increased inflow of welfare recipients by lowering welfare benefits, a downward bias in standard Ordinary Least Squares (OLS) estimates can be expected. This problem has not been adequately addressed in the existing literature.

The main contribution of this paper is to address the potential endogeneity problem by utilizing a centrally implemented policy reform as a natural experiment. More specifically I utilize exogenous variation in welfare benefits across Norwegian local governments provided by an announcement of national instructive guidelines taking place in 2001. The econometric analysis applies a conventional difference-in-difference strategy where I evaluate whether changes in welfare benefits is positively correlated with changes in net inflow of welfare recipients relative to a control group. The results are consistent with the welfare migration hypothesis, and the most recent US empirical studies. More importantly, ignoring the policy endogeneity yields a downward bias in estimated welfare migration responses.

The structure of the paper is as follows. The next section discusses the existing literature, and section 3 presents the institutional setting and the data set analyzed. Section 4 presents the empirical strategy and discusses potential problems with earlier work. The results are presented and discussed in section 5. Finally section 6 concludes the paper.

2. Prior Research on Welfare Migration

Broadly, the welfare migration hypothesis has been investigated in two ways. One strand of literature focuses on the strategic interaction among jurisdictions in the determination of welfare benefits. If a jurisdiction is concerned about becoming a ‘welfare magnet’, then benefit levels in other (typically neighboring) jurisdictions will affect the jurisdiction’s own

benefit choice. Evidence of strategic interaction among jurisdictions thus provides indirect evidence that welfare migration affects policy decisions. The empirical findings of this literature suggest that jurisdictions are indeed playing a 'welfare game'. However, it is challenging empirically to separate strategic interaction in welfare generosity due to mobility pressure from other sources of strategic interaction (notably yardstick competition). From a normative perspective, disentangling different sources of strategic interaction is important. One way to do this is would be to evaluate explicitly whether welfare migration is in fact an important phenomenon.[2] This is what the second type of studies do, focusing on aggregated migration flows or individual migration choices of welfare recipients. This literature has more or less exclusively utilized data from the US. The most recent strand of papers in this tradition, notably Gelbach (2004), Bailey (2005) and McKinnish (2005a, 2005b) rely on difference-in-difference methods and confirm the existence of welfare migration. The current paper adds to this literature evaluating aggregate migration flows across local governments in Norway. In section 2.1 and 2.2 I review the two complimentary strands of the literature. The empirical strategy laid out in this paper is deferred to Section 4.

2.1 Strategic Interaction in Welfare Policy

When welfare policy is decentralized to a regional level, each jurisdiction choose benefit levels in strategic fashion taking into account of the mobility of the poor and the choices of other jurisdictions. To identify strategic interaction among sub-central governments, scholars have typically relied on a reduced form equation where the welfare benefit level in one jurisdiction is related to a weighted average of neighboring jurisdiction's benefit levels. Both US studies, summarized by Brueckner (2000), and European studies, such as Fiva and Rattsø (2006) studying Norwegian local governments, have found results consistent with the welfare migration hypothesis. Conditional on other characteristics, an observed spatial pattern (known as spatial auto-correlation) is found. Typically, instrumental variable methods or Maximum Likelihood estimations are used to handle the simultaneity problem related to neighboring jurisdictions welfare benefit levels.

As elaborated by Revelli (2005), a reduced form interjurisdictional interaction function might not by itself allow one to discriminate among competing theories of strategic interaction. An

[2] Jurisdictions may believe in the welfare migration hypothesis, even in the absence of conclusive evidence. This is an argument for looking for strategic interaction in welfare benefits rather than migration flows.

observed spatial pattern in welfare benefits can stem from different sources: First, because politicians take into account (actual or perceived) migration responses from welfare recipients to avoid becoming 'welfare magnets'. Second, because imperfectly informed voters make use of information about political choices in close by governments. This is known as yardstick competition and the understanding of the mechanism was first developed by Salmon (1987) and formalized by Besley and Case (1995). From yardstick competition theory it follows that voters condition their electoral choices on the relative fiscal performance of their own versus neighboring local governments. Rational politicians realize this and have an incentive to mimic neighboring local governments. Third, a spatial pattern may simply be caused by unobserved spatially correlated shocks or omission of a variable which is correlated across space and affect nearby jurisdictions in a similar way. However, with valid instruments, such spatial error dependence will not be driving the estimated strategic interaction.

In order to separate these three competing explanations from each other (welfare competition, yardstick competition and spatially correlated shocks) it is necessary to rely on auxiliary regressions that are directly derived from theory (Revelli, 2005). Studying welfare benefits in the UK, Revelli (2006) follows this approach. To identify yardstick competition, Revelli exploits an institutional reform taking place in the UK which introduced a national system of social service performance rating. It is reasonable to argue that this reform reduced the importance of local information spill-overs by making information on nation-wide practice in social service provision easily available. Since observed spatial auto-correlation decreased substantially in the period after the reform taking place, he concludes that the spatial auto-correlation is likely to be driven (at least partially) by yardstick competition and not by welfare migration. He argues that if welfare migration was driving the spatial pattern, then one would expect more, not less interaction after the reform. Revelli does not explicitly test the welfare migration hypothesis, but concludes based on a survey of 1500 households that "the mobility of the welfare beneficiaries of personal social services is likely to be rather low, virtually ruling out the race to the bottom hypothesis" (2006, pp. 460-461).

2.2 Welfare Generosity and Residential Choices

Early studies of the welfare migration hypothesis have provided conflicting and inconclusive results as to whether welfare recipients respond to (changes in) welfare benefit levels. Gramlich and Laren (1984) follow the most common approach in the migration literature and

examine changes in location from period t-1 to period t, conditioning on welfare receipt in period t. Gramlich and Laren (1984) find that welfare recipients are more likely to have migrated to high-benefit states from low-benefit states, rather than vice versa. This evidence is suggestive, but if welfare participation is endogenous, in the sense that welfare participation is correlated with welfare benefits, then these results may be biased. If some people who do not receive welfare payments in low benefit states, would if they were in a high benefit state, then conditioning on welfare receipt in period t, is likely to exaggerate the flow of welfare recipients from low to high benefit jurisdictions (Meyer, 2000). Conditioning on welfare receipt in period t-1, would reduce the problem, but bias could still exist and most likely go against finding evidence of welfare migration. A possible solution is to rely on at-risk groups rather than actual welfare participants per se but this approach also has its' limitations, and is further discussed below.

One of the main difficulties with econometric models that estimate the effect of welfare benefits on migration decisions is that welfare benefits and economic conditions tend to be positively correlated. The more recent studies of welfare migration have relied on micro-data on migration, comparing the migration responses of potential or actual welfare recipients (the treatment group) to comparison groups that are similar to, but more sophisticated than the ones used by Gramlich and Laren. Walker (1994) analyzes migration patterns at border counties of three contiguous US states with considerable welfare benefit differentials. Comparing inter-state migration rates of poor young women relative to migration rates of two control groups (nonpoor young women and poor young men) he fails to find any statistically significant effect of benefit-differentials. Brueckner (2000) argues that this study can be criticized on several accounts; in particular by ignoring important migration flows to and from major cities and by using a too broad treatment group. In another study, Levine and Zimmerman (1999) also find no evidence in favor of the welfare migration hypothesis. They compare inter-state migration decisions of poor single mothers in the US to four different control groups: poor single women without children, poor single men, poor married women, and poor married men.

Although the poverty rate may be the best indicator of people susceptible to changes in welfare policy, it can be highly problematic. Meyer (2000) points out that conditioning on poverty status, both Walker (1994) and Levine and Zimmerman (1999) understate the welfare migration effects: "It seems likely that there are many people who would be poor in low

benefit state but not in a high benefit state due to the higher benefits and wages" (Meyer, 2000, footnote 10). In the widely cited work of Peterson and Rom (1989, 1990), variation in a state's poverty rate is used to test the welfare migration hypothesis. This can be highly problematic since increases in benefit levels may also increase the size of the poverty population, due to disincentive effects. It follows that poverty rates may change due to changes in benefit policies even without any migration taking place. Peterson and Rom (1989, pp. 716-717) acknowledge that the poverty rate is likely to change for other reasons than interstate migration, but still use it in their empirical analysis. They find that a state's poverty population increases when its benefit level is high. The same empirical analysis is the basis for their 1990 book (Peterson and Rom, 1990). [3]

Some of the very recent studies on the welfare migration hypothesis add to the literature by applying more sophisticated identification strategies than what previous studies has done. Four studies are particularly well conducted and deserve to be mentioned. First, Bailey (2005) stresses that many studies risk distorting the effect of welfare on migration decisions by inadequately accounting for attributes of the jurisdictions that affect migration. He demonstrates that some previous studies that have failed to find any welfare migration effect suffers from specification errors. Applying a more rigorous estimation strategy than many previous analyses, in particular by including state fixed effects and a control group which helps to soak up (unobserved) confounding factors, he finds evidence in favor of the welfare migration hypothesis. Second, Gelbach (2004) points out that the incentives to migrate for welfare benefits are highest when a mother's children are young, as there is a longer period of welfare benefit eligibility in the US. He finds that for single women with less than a college degree, own state's welfare benefits affect residential decisions and that the effects are decreasing in the age of the oldest child. The interaction effect is not present for a comparison group of mothers with college degrees. Finally, McKinnish (2005a, 2005b) introduces another clever and convincing identification strategy. She compares welfare program size in border counties to interior counties within US states. The key assumption made is that costs of between-state migration are lower for individuals located close to state borders. It follows that it is reasonable to expect that at state borders with large cross-border benefit differential, the border counties on the high (low) benefit side should have higher (lower) welfare participation relative to the interior counties of the high (low) benefit state. This is exactly

[3] Berry et al. (2003) pursue a similar strategy as Peterson and Rom (1989, 1990). Using a more complex specification they find a negative (but small) impact of welfare benefits on poverty rates.

what McKinnish (2005a) utilizing aggregate county level data finds. Consistent results are found when micro level data on migration decisions are used (McKinnish, 2005b).

3. Institutional Structure and Data

Local governments in Norway are responsible for primary and lower secondary education, in addition to care for the elderly, preschool education, and some other local services, such as infrastructure. They are also responsible for the welfare benefit system. The welfare benefit system is the final safety net for those who fall through other arrangements of the Norwegian welfare system and is intended to provide temporarily support to people in need. The social service act represents the regulations in force and states criteria and guidelines for the welfare benefits granted by the local governments. The social service act leaves considerable discretion to the local governments concerning the generosity of the system, regarding both eligibility and the level of the benefits.

The local governments are democratic institutions led by an elected local council. The financing of the local governments is highly centralized with around 90% of the local government's revenue stemming from regulated income taxation and grants from the central government. The local governments have some discretion related to user charges and property taxation, which are important revenue sources on the margin. The grants are distributed as block grants and are based on objective criteria, partly as income tax equalization and partly as spending equalization. It follows that local governments do not face the full economic consequences of welfare migration.

3.1 Welfare Benefits

The implementation of welfare policies includes guidelines set by the local council and actual payments made by the welfare bureaucracy. The politically determined norms are specified as an amount paid to a 'standard user' per month. As accentuated by Fiva and Rattsø (2006), the link between politically determined norms and actual welfare payments is not trivial. The unique data set utilized by Fiva and Rattsø (2006) on actual payments for a 'standardized household' is only available for one year (1998). In this analysis I need to rely on the political determined norms to reflect welfare generosity. The politically determined norms are likely to

be the most visible measure of welfare policy from the perspective of potential welfare immigrants.

Data on welfare benefit norms are available from 1993 onwards. However there are quite a few missing variable the two first years of data collection and consequently I constrain the analysis to the period 1995 to 2004. Table 1 shows the distribution of the locally determined norms for a single household per month, from 1995 through 2004.[4] The politically determined norm varies considerably across local governments. Since housing costs generally are excluded from the politically determined norms, the observed variation in welfare benefits can hardly be attributed to differences in living costs. Nor can the variation in welfare benefits be explained by differences across other particular dimensions, such as differences between rural and urban local governments.[5] The average welfare benefit norm was NOK 4203 (USD 700) per month in 2004.

Table 1 about here.

No national standard concerning how much welfare recipients should be granted in welfare benefits existed until 2001. But in February 2001 the central government announced a national instructive welfare benefit norm, which can be understood as a response to the differences observed among local governments prior to 2001. The rational for the instructive norm was according to a circular from the central government to "contribute to a more homogenous practice across local governments and to provide more similar support for equal recipients" (Rundskriv I-13/2001, my translation).[6] The coefficient of variation reported in Table 1 has decreased from around 0.15 prior to the announcements to around 0.10 after the announcement. It is evident that the instructive norms have succeeded in making the locally determined norms more homogenous.

Interestingly, the local government in Norway with the most generous benefits in 2004, Finnøy, severely cut them in 2005, according to the deputy mayor because the benefits were too generous compared to their neighbors. This decision received national attention because

[4] A total number of 435 Norwegian local governments existed between 1995 and 2004. Due to a few missing variables and local government mergers I analyze a balanced panel of 430 local governments.
[5] Fiva and Rattsø (2006) find no statistically significant association between welfare benefits and population size and settlement pattern, controlling for other factors.
[6] Rundskriv I-13/2001 : "Formålet med retningslinjene er å bidra til en mer ensartet praksis ved utmåling av økonomisk stønad i kommunene og større likhet i utmålt ståndad for like stønadstilfeller".

Finnøy is being lead by politicians representing the same parties as the then recently elected national government, which had promised to work for the "eradication of poverty in Norway" in their inaugural address a couple of weeks earlier (September 19th, 2005).

3.2 Migration Rates

Data on received social assistance are available for the entire Norwegian grown up population. To provide a clean test of the welfare migration hypothesis I constrain the data set to men aged 16 to 66, without dependent children living in the same household. In each year this corresponds to approximately 810 000 people. It follows that about 1/6th of the total Norwegian population is included in the data set (the total Norwegian population is approximately 4.5 millions). A welfare recipient is defined as a person receiving welfare benefits within a given year. Table 2 shows the descriptive statistics on welfare recipient status and migration rates. In the econometric analysis to be conducted I take a bird's eye perspective and evaluate overall migration flows to and from each local government. The key variable is then the net inflow of welfare recipients to each local government. The migration rates are measured as the share of people moving from one local government to another from January 1st in year t-1 to January 1st in year t.

Table 2 about here.

The share of welfare recipients varies from 6.9% to 8.3% between 1996 and 2004. Single men between 16 and 66 without dependent children are overrepresented as welfare recipients. In the general population only around 3% received welfare benefits during a given year. Since the welfare recipient population is quite heterogeneous it may be useful to distinguish between short and long term welfare recipients. Recipients that are in the welfare system for longer periods have stronger incentives to respond to changes in welfare generosity than the general population of welfare recipients. It is not clear where the line between 'short term recipient' and 'long term recipient' should be drawn. I suggest that it may be drawn at 3 months of welfare receipt. Although social assistance is intended to be given in emergency situations and not as long term support, the data indicate that several recipients have long

welfare spells.[7] Figure 1 shows the distribution of the number of months welfare recipients received welfare benefits within a given year. The figures are based on averages across 1996 to 2004, but the distributions for each year are close to identical to the average distribution. Around 20% of the welfare recipients received welfare benefits for only one month, another 22% received welfare benefits for two or three months. This yields on average 42% short term recipients and 58% long term recipients according to my definition.

Figure 1 about here.

It is often claimed that household mobility is higher in the US than in European countries. However, the share of people moving across local government lines in Norway is much higher than the corresponding share of people moving across US states lines. This follows from the fact that the average local governments in Norway are much smaller in size than the average US state. The average local government size in Norway is approximately 700 km^2. While the continental US states range in area from around 4000 km^2 (Rhode Island) to almost 700 000 km^2 (Texas). It follows that the 'race to the bottom' in welfare benefits is more likely to occur in Norway than in the US.

Table 2 shows that around 13% of the welfare recipient population moved across local government lines from one year to the next compared to around 6-7% of the control group of non-recipients.[8] In line with several others, for example Peterson and Rom (1990, p. 16), I find that welfare recipients are considerably more mobile than non-recipients. Labor economists have generally explained the greater mobility of the poor as a function of the relatively low opportunity costs of moving for those with low incomes (Peterson and Rom, 1990, p. 16). Levine and Zimmerman (1999, p. 401) who utilize US data and distinguish between poor single mothers and four different control groups (poor single women without children, poor single men, poor married women, and poor married men) find that "roughly 5-7.5% of the control group members are observed moving across state lines between one year and the next, compared to just under 4% of the treatment group members". It follows that Norwegian single men on welfare receipt without children are around 3 times as likely to

[7] Note also that around two out of three welfare recipients that received welfare benefits in year t also received welfare benefits in year t-1. Around half the welfare recipients that received welfare benefits in year t also received welfare benefits in year t-1 and year t+1.

[8] A person is a welfare recipient if he received welfare benefits in year t-1, independent of whether he received welfare benefits in year t.

move across jurisdiction lines as poor women with dependent children in the US. The treatment group members (never-married high school dropouts with children) in the McKinnish (2005b) study is even less mobile, only 5-6% of them moved across state lines during a *five* year period.

4. Empirical Strategy

4.1 The DiD estimator

The most straightforward way to test the welfare migration hypothesis would be to check if there is a positive correlation between changes in welfare generosity and net inflow of recipients from one year to the next. However, since people move for any number of pecuniary or non-pecuniary reasons, any sensible model of welfare migration cannot rely on changes in welfare benefits as the sole determinant of migration. If for example regional economic shocks altered both migration patterns and welfare generosity, then this would confound the estimates. In order to take account of such problems one can use a group of similar people as control group. A valid control group allows me to take into account factors unrelated to changes in welfare benefits. Technically this Difference-in-Difference (DiD) approach facilitates this by adjusting changes in migration flows of welfare recipients (the treatment group) by changes in migration flows of non-recipients (the control group). The central idea is that increases in welfare generosity will affect residential location of welfare recipients while leaving other people unaffected. In this analysis I divide the sample into two groups, welfare recipients (the treatment group) and non-recipients (the control group). I also check how excluding short term welfare recipients from the treatment group affect the estimates.

As discussed in section 2.1, one may obtain biased estimates of welfare migration if welfare participation is endogenous. If some people who do not receive welfare payments in low benefit states, would if they were in a high benefit state, then conditioning on welfare receipt in period t is likely to overstate the importance of welfare magnets (Meyer, 2000). Conditioning on welfare receipt in period t-1, would reduce the problem, but bias could still exist and most likely go against finding evidence of welfare migration. Conditioning on welfare receipt in period t-1 is also likely to exhibit a downward bias if a substantial number of poor people who is not on welfare in period t-1 (and is consequently assigned to the control

group) migrate to other local governments to receive welfare benefits. I choose the most conservative strategy and condition on welfare receipt status in year t-1. However, I have also estimated the model, conditioning on welfare recipient status in period t, and the results are very similar. This is comforting and suggests that endogenous welfare participation is not confounding the estimates.

The main regression to be estimated is given by:

$$\left(\frac{\Delta\text{net\_inflow\_recipients-}\Delta\text{net\_inflow\_control}}{\text{Population in 10 000s}}\right)_{it} = \delta_1 + \delta_2 \Delta\text{Benefits}_{\text{it-1}} + \delta_3 \Delta\text{Unemployment}_{\text{it-1}} + u_{it}, \quad (1)$$

where

$$\Delta\text{net\_inflow\_recipients}_{it} = (\text{inflows of recipients}_{it} - \text{outflows of recipients}_{it}) - (\text{inflows of recipients}_{it-1} - \text{outflows of recipients}_{it-1}),$$

and

$$\Delta\text{net\_inflow\_control}_{it} = (\text{inflows of non-recipients}_{it} - \text{outflows of non-recipients}_{it}) - (\text{inflows of non-recipients}_{it-1} - \text{outflows of non-recipients}_{it-1}).$$

ΔBenefits$_{it-1}$ is the change in the politically determined norm granted to a single person per month in 1000 NOK from year t-2 to year t-1.[9] ΔBenefits is lagged one period because change in benefits from year t-1 to year t cannot technically have an impact on the migration flows observed from t-1 to t, since migration rates are measured as net inflows from January 1st in year t-1 to January 1st in year t. The welfare migration hypothesis suggests that $\delta_2 > 0$.

As discussed above is the central idea in the DiD estimator that the control group face many of the same migration incentives facing welfare recipients. But changes in economic conditions do not necessarily influence treatment and comparison groups in the same way. To take into account the possibility that the treatment and control group may respond differently to changes over time in economic conditions, I have included ***Δunemployment*** as a control

[9] I've also experimented with utilizing the percentage change in welfare benefits as an independent variable. The results are then similar, but in most specification the fit is worse than when I use absolute changes.

variable. Unemployment is defined as the share of the male population that is unemployed (yearly average). δ_3 captures then the differential effect of changes in unemployment rates on the treatment and control population. Utilizing US data, Bailey (2005) finds that the welfare population is less repelled by high unemployment, suggesting that $\delta_3 > 0$.

4.2 Handling the Policy Endogeneity

Estimating eq. (1) with Ordinary Least Squares (OLS) may be problematic. The reason is that welfare benefits and migration patterns may affect each other simultaneously. If welfare induced migration is a concern for policy makers then ΔBenefits is endogenous to the left hand side variable in eq. (1). It is reasonable to argue that if the welfare population is increasing, policy makers will be inclined to reduce benefit levels.[10] It follows that such policy endogeneity is likely to result in a negative bias in δ_2 when estimating eq. (1) with standard OLS.[11] This problem is as far as I know not properly addressed in the existing literature. Peterson and Rom (1989, 1990) and Berry et al. (2003) acknowledge the policy endogeneity and try to handle this by simultaneously estimating the mutual effects of welfare benefits and poverty rates, but they have no convincing identification strategy. Without the ability to experimentally vary the relevant variable (welfare generosity) this paper follows the recommendation of Meyer (1995: 153) and look for "variation that is driven by factors that are clearly identified and understood". Rather than relying on a simultaneous equation model with questionable exclusion restrictions I suggest that a centrally implemented policy reform taking place in Norway can act as a natural experiment.

The central government national instructive welfare norm, launched in February 2001, made several local governments change their welfare policy. In a survey conducted in August 2001, 104 out of 336 local governments[12] claimed that they had altered the welfare benefit levels after February 2001 when the national instructive guidelines were announced. 78 (19) out of the local governments claimed that they had changed their welfare benefits exclusively (partially) due to the announcement. In the current data set 119 out of the 430 local

[10] Gelbach's (2004) simulation results on US data suggest that welfare migration in fact depresses optimal state benefit levels. The simulated effects are substantial, in particular for lower-benefit states.
[11] Another aspect that may give rise to endogeneity is that changes in welfare benefits may go together with changes in other local government services that affect migration patterns of welfare recipients and control group members differently.
[12] 98 local governments did not respond.

governments had chosen to implement the national guidelines for 2001. The policy reform can be understood as a natural experiment which enables me to identify exogenous variation in $\mathbf{\Delta Benefits_{i2000}}$, which facilitates an improved test of the welfare migration hypothesis.[13]

This identification strategy reduces the data set from a panel data set with 8 years of observations to a simple cross section. The identification strategy rests on the assumption that local governments that had welfare benefits below 3880 NOK in 2000 to be more inclined to increase their welfare benefits relative to local governments that had welfare benefits above 3880 NOK and I expect this effect to be increasing in the distance to the central guidelines. To capture these two aspects I divide the local governments into five groups according to their distance from the central guidelines (launched the year after):

- More than 500 NOK below the guidelines (Group1, 11% of all local governments).
- Below the guidelines, but less than 500 NOK (Group2, 27% of all local governments).
- Above the guidelines, but less than 500 NOK (Group3, 31% of all local governments).
- More than 500 NOK above the guidelines, but less than 1000 NOK (Group4, 18% of all local governments).
- More than 1000 NOK above the guidelines (Group 5, 13% of all local governments).

I introduce one dummy variable for each of these groups, which I regress on $\Delta \text{Benefits}_{i2001}$. In addition I include interaction terms with each group dummy and the absolute distance from the central guidelines (distance). The right hand side of the first stage regression then consists of these 10 variables and $\Delta Unemployment_{i2001}$. The validity of this identification strategy rests on the assumption that local governments' welfare benefit levels in 2000 did not have any impact on the change in net inflows of welfare recipients (relative to the control group) in the following year, except through the impact on the change in welfare benefits.

5. Results

I present two tests of the welfare migration hypothesis. In section 5.1 I present an implicit test of this hypothesis by comparing aggregate migration rates of recipients to non-recipients over time. The idea is that the exogenous introduction of the national guidelines would have an

[13] Meyer (1995) provides an insightful discussion of the issues surrounding natural experiments.

impact on the mobility of the recipients, but not on the mobility of non-recipients (at least not to the same extent). In section 5.2 I move on to the explicit test of the welfare migration hypothesis drawing on the identification strategy discussed in section 4.

5.1 An Implicit Test of the Welfare Magnet Hypothesis

The national instructive guidelines had basically two effects on local government priorities. First, the guidelines made the locally determined norms more homogenous. The coefficient of variation decreased from approximately 0.15 in the period prior to the guidelines to around 0.1 in the period after the guidelines (see Table 1). Second, the introduction of the guidelines in 2001 was associated with a drop in the raw correlation of welfare benefits between two consecutive years. Looking for evidence in favor of the welfare migration hypothesis I start out by evaluating whether the centrally implemented reform is associated with any changes in overall migration rates. If welfare migration is important in practice, then it is reasonable to expect that the introduction of the centrally given guidelines would affect the migration decisions of welfare prone households. In the short run should the average incentives to move increase because several local governments radically altered their welfare benefit norms, and in the longer run by reducing the incentives to move due to more homogenous welfare benefits.

Figure 2 about here.

Interestingly, as Figure 2 shows, the share of welfare recipients that moved across local government borders peaked in 2001-2002. The same holds controlling for the migration rates of non-recipients, but the extent is then smaller. If welfare migrations typically are short distance moves, which McKinnish (2005b) finds to be the case in the US, then one would expect that migration rates across local government lines *within counties* to be particularly sensitive to the centrally implemented policy reform. Figure 3 which emphasize within county migration flows exhibit the same pattern as Figure 2.

Figure 3 about here.

Since welfare benefits became more homogenous after the introduction of the central guidelines, one would expect welfare migration to decline after the initial ‘shock’ taking place

in 2001-2002. In 2002-2003 this argument is indeed supported by the data illustrated in Figure 2 and 3. However, somewhat puzzling the migration rates then increases in 2003-2004. To further investigate whether welfare recipients actually moved more following the national instructive norms I evaluate within county mobility in Norway's 18 counties. Equation (2) shows the estimated simple regression:

$$(\text{Mobility\_Recipients}_{it}\text{-Mobility\_NonRecipients}_{it}) = \alpha + \beta_1 d1997 + ... + \beta_{12} d2004 + unemployment_{it-2} + u_{it}, \quad (2)$$

where the dependent variable is the difference in within county migration rates of welfare recipients and other people, in county i at time t. On the right hand side I include a set of year dummies and county fixed effects. Since regional economic shocks may affect welfare recipients and non-recipients differently, I include the lagged unemployment rate as an explanatory variable.

The results are presented in Table 3. The coefficient of interest is related to the 2002 dummy, which, all else equal, is expected to be positive according to the welfare migration hypothesis. In specification (1) I fail to find any statistically significant effect of any of the year dummies, including the dummy for 2002. As a further test, I impose the restriction that the impact of all other dummies except one are set to be zero. This is reported in specification (2) to (9). None of the dummies for the period prior to the policy reform turns out statistically significant, while the 2002, 2003 and 2004 dummies come out statistically significant.

Table 3 about here.

As discussed above, there are expected to be two effects of the policy reform on overall migration rates of welfare recipients. One short term effect (positive) and one long term effect (negative). The cleanest test of the welfare migration hypothesis is therefore to evaluate only the 2002 dummy where only the former effect can be present. The positive point estimate of 0.003 is consistent with the welfare migration hypothesis. Relative to other years, migration rates were on average 0.3% higher, relative to the control group. This effect also holds when only the period 1997-2002 are considered (specification (10)). This is indicative evidence in favor of the welfare migration hypothesis.

5.2 An Explicit Test of the Welfare Migration Hypothesis

Moving on to the more direct test of the welfare migration hypothesis I run an OLS regression on eq. (1). The results are reported in Table 4. Utilizing panel data for the entire period (1997-2004) changes in welfare benefit levels are positively associated with net inflow of welfare recipients (relative to the control group). But the effect is small and not statistically significant. The parameter estimate 2.58 suggests that an increase in welfare benefit levels of 1000 NOK (all else equal) increases an average local government (with a general population of 10 000) with 2-3 new welfare recipients from the sub-population that is under study here. Lagged unemployment rates have a positive, but not statistically significant impact on the difference in difference estimate of migration flows.

Table 4 about here.

As discussed in Section 4, it may be problematic to rely on simple cross sectional variation in changes in welfare benefits to identify welfare migration. Since changes in welfare benefits affect migration flows, then migration flows are likely to affect how benefits are set. This suggests that changes in welfare benefits are endogenous to the right hand side variable in eq. (1). Because many local governments that altered their welfare benefits from 2000 till 2001 did so in response to the central guidelines, it is reasonable to argue that the change in welfare benefits is "more exogenous" for the 2002 cross section. In this sense it can be interesting to evaluate how the estimated effect for the 2002 cross section relates to other years. As a benchmark for evaluating the potential endogeneity problem, consider specification (2) in Table 4. This is panel data for the period prior to the central government lines (1997-2001). Here no effect of welfare migration can be found. The point estimate even suggests a very weak negative effect. However, when constraining the sample to the cross section when the central guidelines were launched, specification (8), the point estimate is positive and statistically significant at the 1% level. The point estimate of 11.77 suggests that an increase of 1000 NOK is associated with an inflow of approximately 12 welfare recipients for an average local government. A local government with 10 000 inhabitants will have around 1700 men without children, between 16-66 years of age. On average will around 7% of them be welfare recipients. It follows that this sub-population of welfare recipients will increase with around 10%. If this is a correct estimate, then the welfare migration effect is non-trivial. For completeness, I report in specification (3) - (7) and (9) - (10) cross section regressions for all

other years. The point estimates vary substantially from year to year. Specification (8) even reports a strong and positive statistically significant effect (p-value of 0.056).

Increasing welfare benefits have two effects on migration flows – attracting people from other local governments and retaining the welfare population that already are living in the local government. Reductions in welfare benefits will work exactly oppositely – repelling both people from other local government and the welfare population already living in the local government. The repelling and attracting forces may not necessarily be symmetric. To test this I have run additional regressions where I have included an interaction term between $\mathbf{\Delta Benefits_{it-1}}$ and a dummy variable turned on if $\mathbf{\Delta Benefits_{it-1}}>0$. Results are reported in Appendix Table 1. In the most reliable specification (8a) the repelling effect is stronger than the attracting effect, but the asymmetric impact is not statistically significant and is therefore not pursued further.

Since welfare recipients that are more dependent on welfare benefits may respond more to differences in welfare benefits, I run another set of regression where I exclude short term recipients from the sample. The treatment group then only consists of welfare recipients that received welfare benefits in at least 3 months in year t-1. Table 5 reports the results. The results from Table 4 are basically reproduced. The welfare migration effect is statistically significant at the 5% level for the 2002 cross section and basically insignificant (or even negative) in all other specifications, the exception being (again) the cross section for 2000. Interestingly the lagged unemployment rate is now higher and typically statistically significant. This suggests that long term welfare recipients are less repelled by high unemployment than both short term recipients and non-recipients (this becomes clear when comparing specification (1) and (11)). [14]

Table 5 about here.

Table 6 and Table 7 reproduce Table 4 and Table 5, but consider only short distance moves. Short distance moves are defined as migrations across local government lines *within*

[14] Note that since the lagged change in unemployment may be endogenous to the left hand side and therefore problematic as a control variable, I have run all specifications without this variable and this basically does not alter the impact of $\mathbf{\Delta Benefits_{it-1}}$.

counties.[15] Again the 2002 cross sections find that increases in welfare benefits are associated with an inflow of welfare recipients, but the effects are not statistically significant (p-values of 0.11 and 0.18).

Table 6 and Table 7 about here.

5.3 Handling the Policy Endogeneity

Although it is reasonable to argue that many local governments that altered their welfare benefits from 2000 till 2001 did so as a response to the central government guidelines, one cannot conclude that the cross sectional variation this year is exogenous. Bias may for example exist if generous local governments that faced severe inflow pressure of welfare recipients were more likely to conform to the national standard. To avoid these problems, an instrumental variable approach is warranted. Following the discussion in section 4.2 I estimate a first stage regression where changes in welfare benefit levels from 2000 to 2001 are regressed on variables indicating the distance from the central guidelines (launched in 2001) in 2000. The first stage regression is reported in Table 8.

Table 8 about here.

The F-test for joint significance of the excluded instruments suggests that the instruments do not suffer from the problems related to weak instruments (with an F-value of 19.86). As expected, local governments with welfare benefits below the central guidelines for 2001 are predicted to increase their welfare benefits from 2000 to 2001 and more so the further away they were from the central guidelines. The local governments above the central guidelines were also predicted to reduce their welfare benefits and to some extent they were more likely to do this the further they were from the central guidelines. However the effect is not symmetric for local governments above and below the guidelines. Local governments above the central guidelines seem to have been less inclined to conform to the national guidelines than the ones below the central guidelines. The discrepancy is best illustrated graphically. In Figure 4 actual welfare benefit levels in 2000 are on the horizontal axis, while predicted values for the change in welfare benefits from 2000 to 2001 are on the vertical axis. For

[15] I acknowledge that this is a crude definition of short distance moves. In future work I am planning to implement more sophisticated measures of short distance moves.

comparison, Figure 5 shows the actual changes in welfare benefits relative to welfare benefit levels in 2000. The linear curve that can be observed in Figure 5 corresponds to the 119 local governments that conformed to the central guidelines.

Figure 4 and Figure 5 about here.

Table 9 reports the results from the second stage regression where the actual values for ΔBenefits are replaced with the fitted values from the first stage regression. The welfare migration effect is now stronger than in any other specification, taking a value of 18.69, and statistically significant at the 5% level (specification (40)). Excluding the short term recipients reduces the coefficient to 13.73 and this effect is only close to being statistically significant at the 10% level. In Table 10 the second stage regression is reproduced considering migration flows only across local government lines within counties. In these specifications the point estimates again increases compared to the OLS results (in Table 6 and Table 7) and the estimates are borderline statistically significant (p-values of 0.072 and 0.101). [16]

Table 9 and Table 10 about here.

The main lesson from this analysis is that ignoring policy endogeneity can give rise to biased estimates. Assuming that cross sectional variation in changes in welfare benefits are exogenous I fail to find any evidence in favor of the welfare migration hypothesis. The welfare migration response to changes in welfare benefits is estimated to be around zero in these specifications (2, 12, 21 and 30). However when evaluating migration responses following a policy reform implemented at the central level, quite substantial effects are found (specifications 8, 18, 27 and 36). Utilizing instrumental variables related to the same policy reform, the welfare migration responses are confirmed (specifications 40, 41, 42 and 43) and the point estimates increase. Hence, there seems to be a downward bias in the general OLS estimates for the years prior to the reform.

6. Conclusion

[16] In preliminary work I have also experimented with a more parsimonious first stage specification where the distance to the central guidelines are linear below and above 3880 NOK. This specification provides a somewhat lower fit of the first stage regression (R2 adj of 0.271). With the more parsimonious first stage, the parameter estimates in the second stage is lower (around 10) and the standard errors are larger. There is no longer any statistically significant of $\mathbf{\Delta Benefits_{it-1}}$ (with p-values of around 0.2).

With closer EU integration some scholars and policymakers are worried that increased mobility of people, goods and factors of production may release competitive forces leading to a roll back of social standards and welfare state arrangements. Countries have incentives to aim to improve their relative position through successive undercutting of tax rates and welfare state arrangements. Thereby attracting productive mobile production factors and deterring immigrants that impose a negative fiscal impact on the government budget. Hans-Werner Sinn, among others, has been concerned about this development for the European welfare states (see for example Sinn, 2003).[17] How important migration of welfare prone households is in Europe is not obvious, both because cross country mobility of households is fairly low and because EU rules have been designed to prevent this form of 'social mobility' by making free mobility contingent on employment (Andersen, 2003). This being said, one should note that half of the immigration into Germany during the 1990s was immigration into unemployment and welfare receipt (Sinn, 2003: 23).

The current paper deals with the welfare migration hypothesis in a setting where household mobility is much higher than across European country lines and where there exists no rules to prevent this form of migration. Mobility across jurisdiction lines with responsibility for welfare policy in the country under study, Norway, is considerably higher than in the US. This follows from the fact that the average Norwegian local government is much smaller than the average US state. Consequently, if welfare migration is important, it should become obvious in Norway. This paper tries to empirically evaluate whether generous local governments in fact attract and retain people. The econometric analysis exploits a natural experiment to investigate the welfare migration hypothesis and finds supportive evidence. In particular, there seems to be a downward bias in regular OLS estimates that ignores the inherent policy endogeneity. Given the strong strategic interaction in welfare benefit determination established in Fiva and Rattsø (2006), it should come as no surprise that ignoring the policy endogeneity can give rise to substantial bias.

[17] Ireland is often highlighted as an example of a country that has succeeded in attracting companies by lowering their corporate taxes.

References

Andersen, Torben M. (2003): "International Integration and the Welfare State" in Torben M. Andersen and Per Molander (eds.) Alternatives for Welfare Policy, Cambridge University Press, Cambridge, UK, pp. 23-48.

Bailey, Michael A. (2005): "Welfare and the Multifaceted Decision to Move", American Political Science Review 99, 125-135.

Berry, William D. and Richard C. Fording and Russell L. Hanson (2003): "Reassessing the 'Race to the Bottom' in State Welfare Policy", Journal of Politics 65, 327-349.

Besley, Tim and Anne Case (1995): "Incumbent Behavior: Vote seeking, Tax setting and Yardstick Competition", American Economic Review 85, 25-45.

Brueckner, Jan K. (2000): "Welfare Reform and the Race to the Bottom: Theory and Evidence", Southern Economic Journal 66, 505-525.

Fiva, Jon H. and Jørn Rattsø (2006): "Welfare Competition in Norway: Norms and Expenditures", European Journal of Political Economy 22, 202-222.

Gelbach, Jonah (2004): "Migration, the Lifecycle and State Benefits: How Low is the Bottom?" Journal of Political Economy 112, 1091-1130.

Gramlich, Edward M. and Deborah S. Laren (1984): "Migration and Income Redistribution Responsibilities", Journal of Human Resources 19, 489-511.

Ladd, Helen F. and Fred C. Doolittle (1982): "Which Level of Government Should Assist the Poor", National Tax Journal 35, 323-336.

Levine, Phillip and David Zimmerman (1999): "An Empirical Analysis of the Welfare Debate Using the NLSY", Journal of Population Economics 12, 391-409.

McKinnish, Terra (2005a): "Importing the Poor: Welfare Magnetism and Cross-Border Welfare Migration", Journal of Human Resources 40, 57-76.

McKinnish, Terra (2005b): "Welfare-Induced Migration at State Borders: New Evidence from Micro-Data", Institute for Research on Poverty, Discussion Paper no. 1306-05.

Meyer, Bruce D. (1995): "Natural and Quasi-experiments in Economics", Journal of Business and Economics Statistics 13, 151-161.

Meyer, Bruce D. (2000): "Do the Poor Move to Receive Higher Welfare Benefits?", unpublished paper, Department of Economics, Northwestern University.

Moffit, Robert (1992): "Incentive Effects of the U.S. Welfare System: A Review", Journal of Economic Literature 30, 1-61.

Musgrave, Richard (1959): *The Theory of Public Finance*, McGraw, New York.

Peterson, Paul E. and Mark C. Rom (1989): "American Federalism, Welfare Policy, and Residential Choices", American Political Science Review 83, 711-728.

Peterson, Paul E. and Mark C. Rom (1990): *Welfare Magnets: A New Case for a National Welfare Standard*. The Brookings Institution, Washington.

Revelli, Federico (2005): "On Spatial Public Finance Empirics", International Tax and Public Finance 12, 475-492.

Revelli, Federico (2006): "Performance Rating and Yardstick Competition in Social Service Provision", Journal of Public Economics 90, 459-475.

Salmon, Pierre (1987): "Decentralization as an Incentive Scheme", Oxford Review of Economic Policy 3, 24-43.

Sinn, Hans-Werner (2003): *The New Systems Competition*, Blackwell Publishing, Oxford.

Stigler, George J. (1957): "The Tenable Range of Functions of Local Government", in U.S. Congress Joint Economic Committee (ed.), *Federal Expenditure Policy for Economic Growth and Stability*, 213-219, Washington D.C., also reprinted in Wallace E. Oates (ed.), 1998 *The Economics of Fiscal Federalism and Local Finance*, Cheltenham, Edward Elgar, pp 3-9.

Walker, James (1994): "Migration among Low-income Households: Helping the Witch Doctors Reach Consensus", Discussion Paper 94-1031, Institute for Research on Poverty, University of Wisconsin.

Wildasin, David E., (1991): "Income Redistribution in a Common Labor Market", American Economic Review 81, 757-774.

Figure 1
The Distribution of Number of Months Receiving Welfare Benefits, 1996 – 2004 averages.

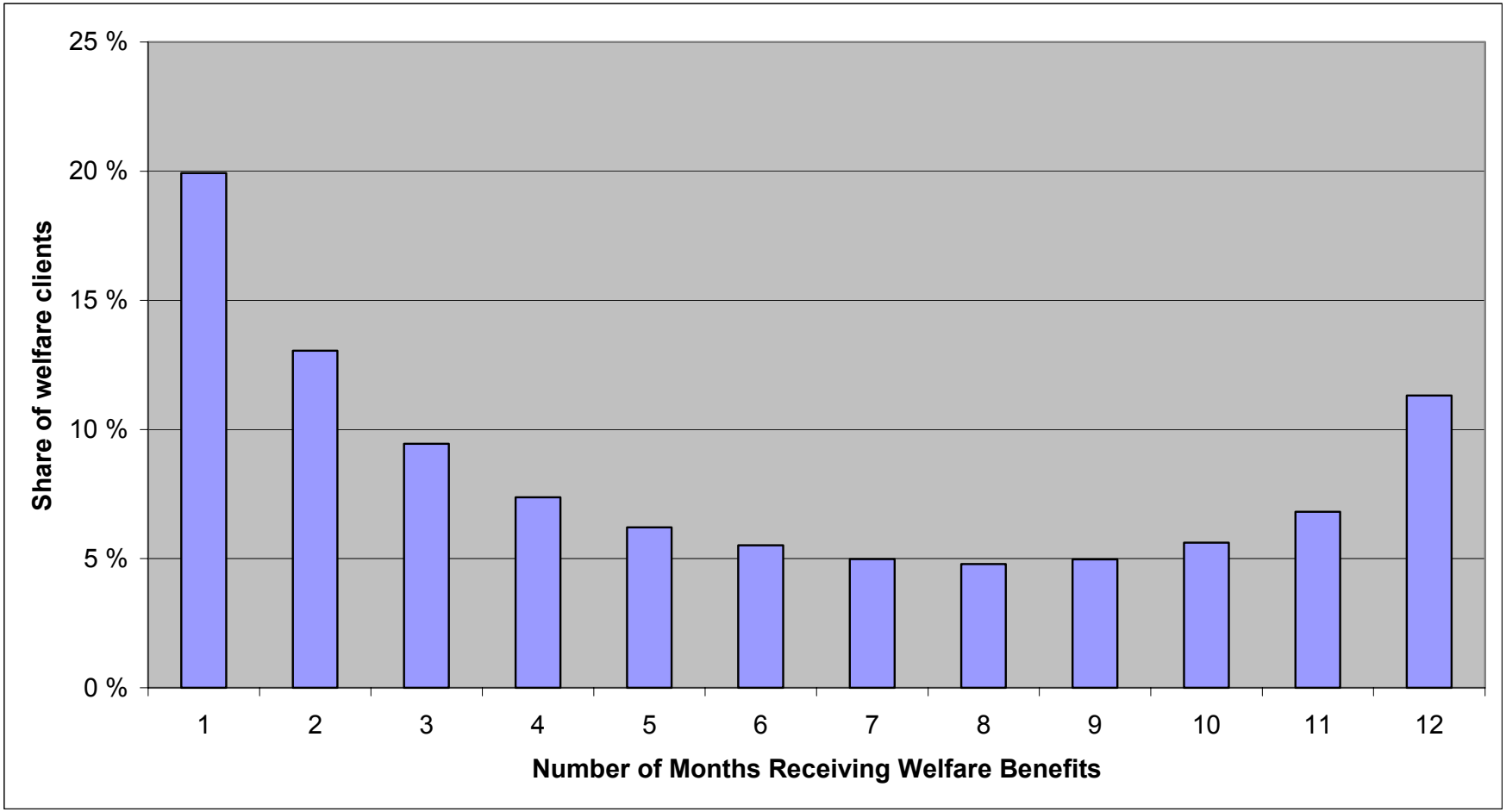

Figure 2
People moving across local government borders from one year to the next, according to welfare recipient status (short, long, non-recipient).

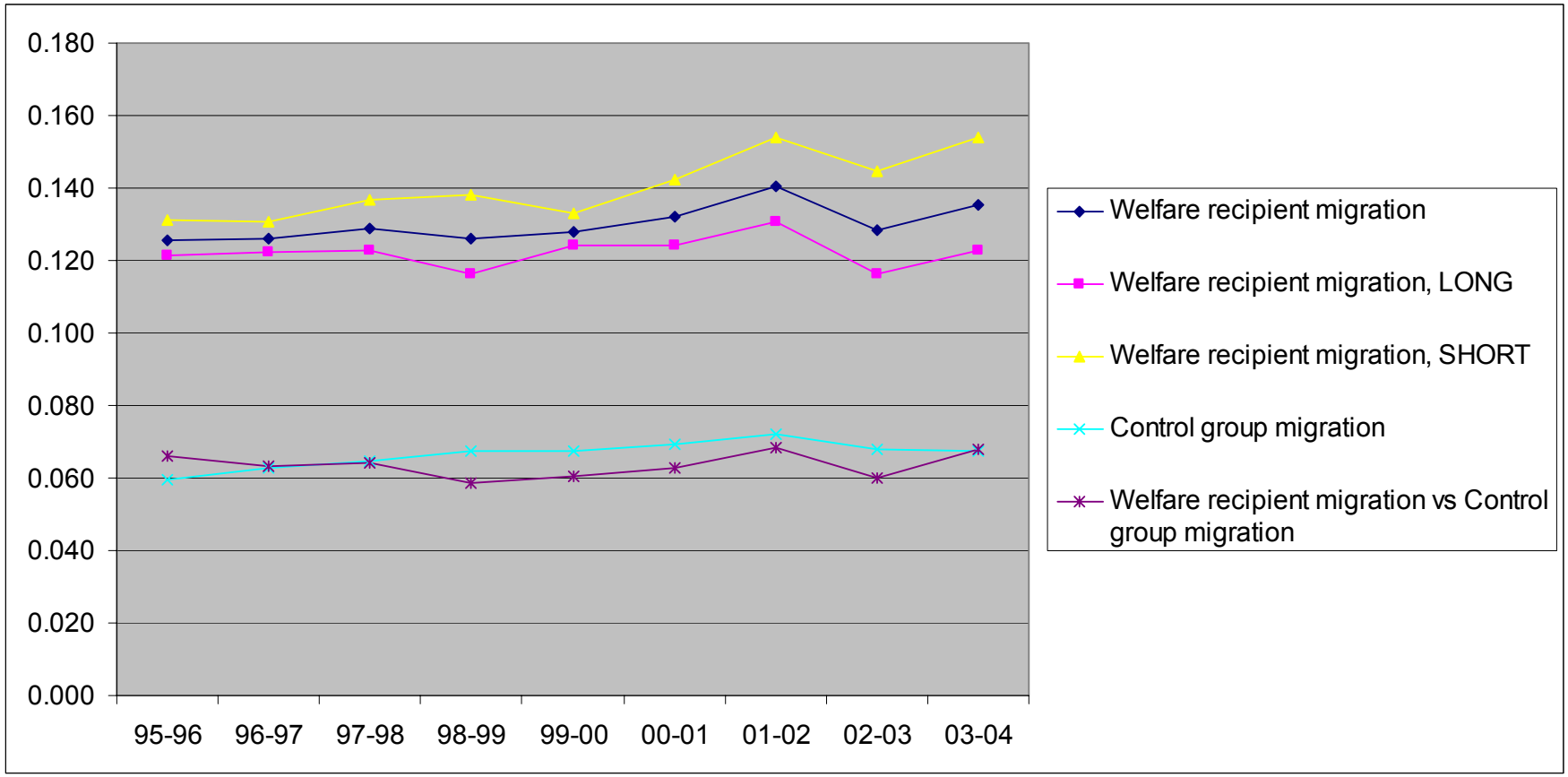

Figure 3
People moving across local government borders ***within counties*** from one year to the next, according to welfare recipient status (short, long, non-recipient).

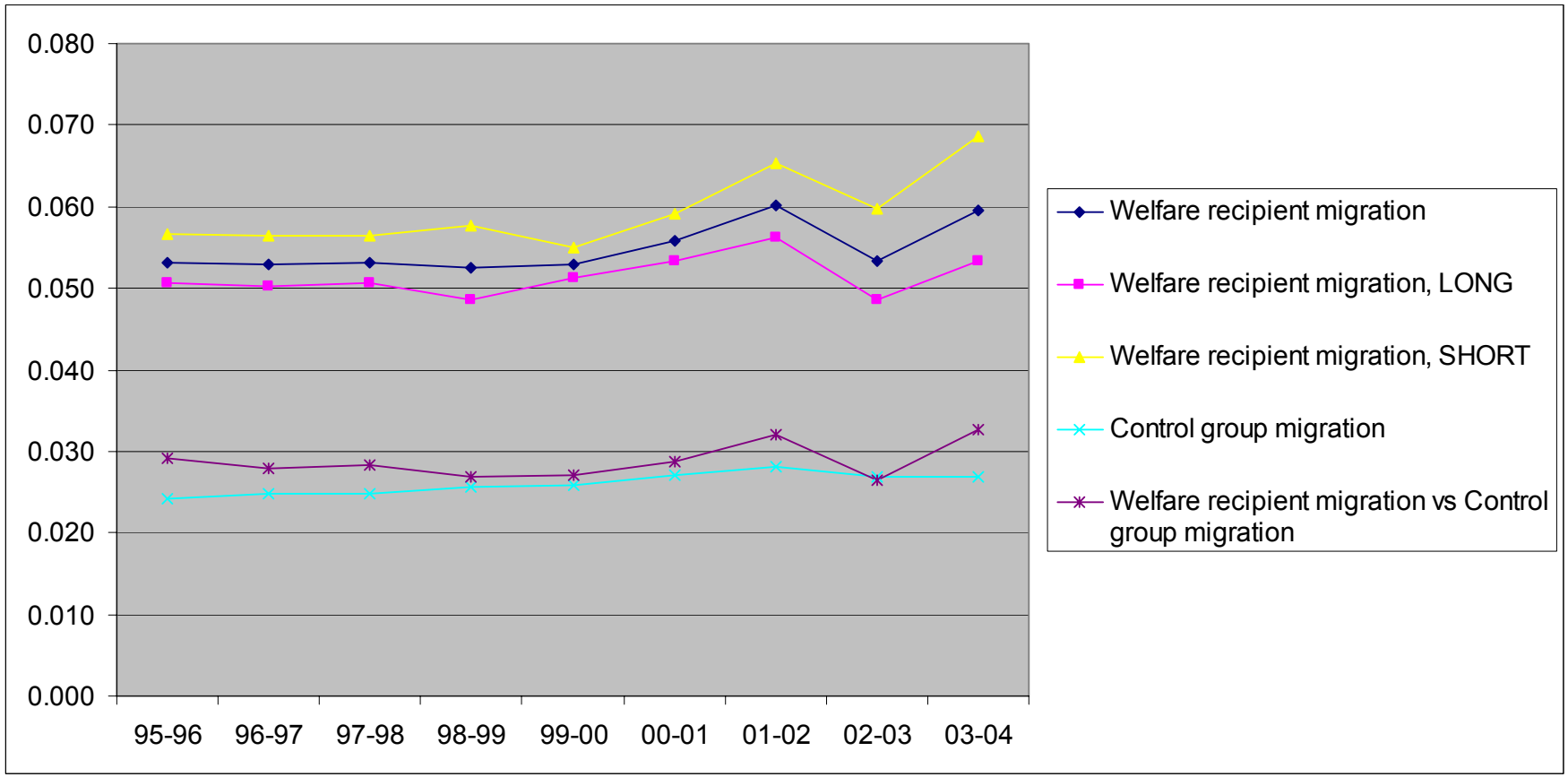

Figure 4
Welfare benefit levels in 2000 and *predicted* values on ΔBenefits from the First Stage (Table 8).

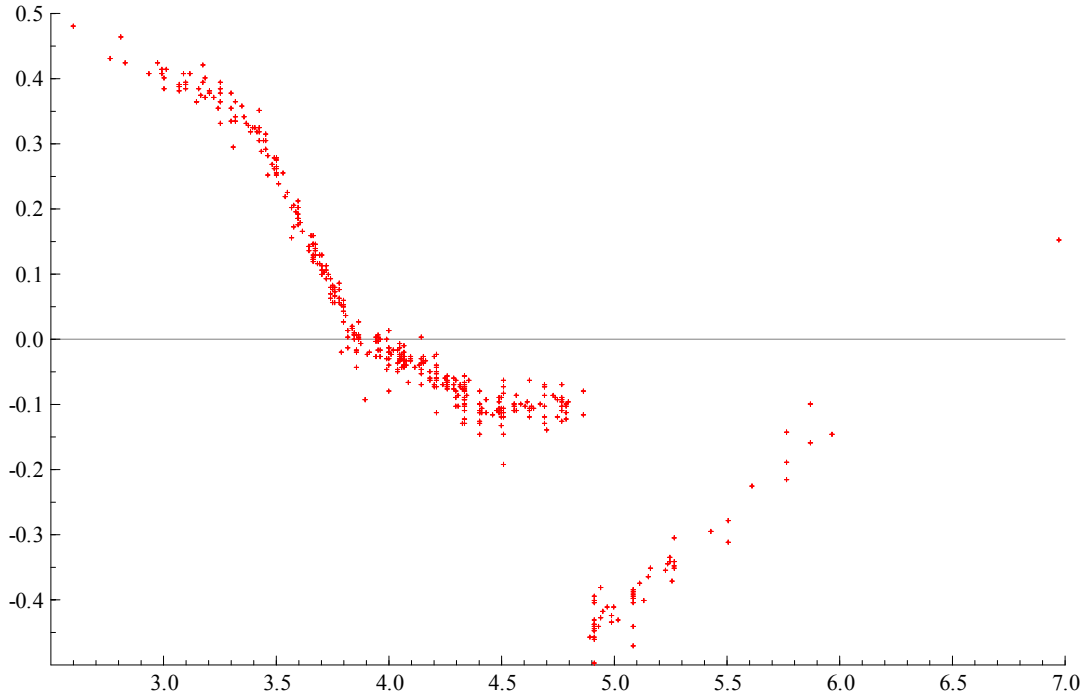

Figure 5
Welfare benefit levels in 2000 and actual values on ΔBenefits.

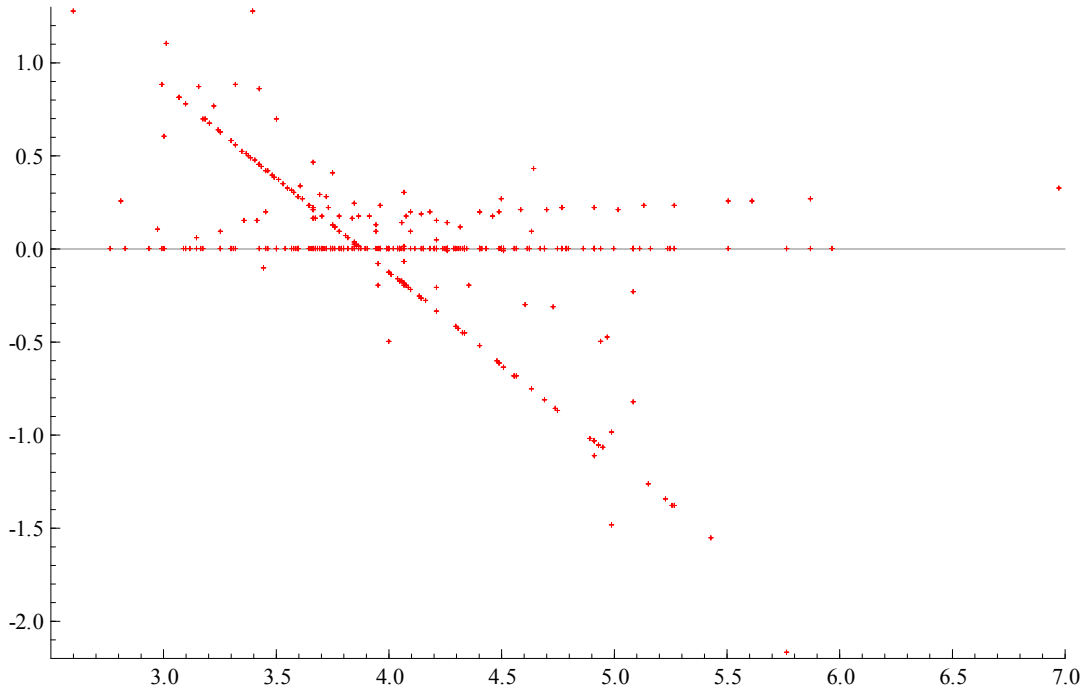

Table 1

Descriptive statistics on welfare benefit levels across local governments. Welfare benefits are measured as the politically determined norm to a single household without children, per month in nominal NOK (unless otherwise noted).

	1995	**1996**	**1997**	**1998**	**1999**	**2000**	**2001**	**2002**	**2003**	**2004**
Mean	3620	3710	3808	3969	4044	4119	4119	4175	4163	4203
Mean in constant 1995 NOK	3620	3667	3668	3739	3724	3678	3570	3572	3477	3494
Standard deviation	524	525	556	605	613	624	543	466	434	386
Coefficient of variation	0.14	0.14	0.15	0.15	0.15	0.15	0.13	0.11	0.10	0.09
Minimum	1900	1900	2102	2258	2484	2600	2760	2760	3000	3000
Median	3660	3697	3800	3935	4005	4068	3950	4000	4000	4140
Maximum	5281	5520	5722	6441	5964	6969	7291	6140	5948	6120
Share of local governments **increasing** the nominal politically determined norm from Year t-1 to year t with more than 600NOK		12	9	52	22	25	21	20	5	5
Share of local governments **decreasing** the nominal politically determined norm from Year t-1 to year t with more than 600NOK		5	2	4	9	10	30	10	15	11
Correlation between politically determined norm in year t and year t-1		0.890	0.927	0.819	0.845	0.842	0.801	0.798	0.846	0.879
National guidelines, in NOK							3880	4000	4000	4140
Above						265*	220	175	165	127
At						0*	119	149	175	175
Below						165*	91	106	90	128
Observations	430	430	430	430	430	430	430	430	430	430

* relative to the norm in 2001.

Table 2

Descriptive Statistics on Welfare Recipient Status and Migration rates.

	1996	1997	1998	1999	2000	2001	2002	2003	2004
Total number of individuals	810926	809279	808099	809120	809824	809945	807811	805871	806224
Total number of welfare recipients	67473	63447	58310	56024	56682	56705	56568	58664	57644
Total number of long term recipients	39533	36698	33136	31506	31885	32105	32465	34161	34663
Total number of short term recipients	27940	26749	25174	24518	24797	24600	24103	24503	22981
Total number of people in the control group	743453	745832	749789	753096	753142	753240	751243	747207	748580
Share of welfare recipients	0.083	0.078	0.072	0.069	0.070	0.070	0.070	0.073	0.071
Share of long term welfare recipients of all recipients	0.586	0.578	0.568	0.562	0.563	0.566	0.574	0.582	0.601
Moving across local government lines	**95-96**	**96-97**	**97-98**	**98-99**	**99-00**	**00-01**	**01-02**	**02-03**	**03-04**
Total number of people moving	52737	54724	56043	57828	57948	59778	62216	58351	58113
Total number of welfare recipients moving	8466	7987	7512	7061	7255	7486	7952	7523	7797
Total number of long term welfare recipients moving	4802	4491	4071	3670	3954	3988	4243	3976	4255
Total number of short term welfare recipients moving	3664	3496	3441	3391	3301	3498	3709	3547	3542
Total number of people in the control group moving	44271	46737	48531	50767	50693	52292	54264	50828	50316
Migration rates	**95-96**	**96-97**	**97-98**	**98-99**	**99-00**	**00-01**	**01-02**	**02-03**	**03-04**
Welfare recipient migration	0.125	0.126	0.129	0.126	0.128	0.132	0.141	0.128	0.135
Long term welfare recipients migration	0.121	0.122	0.123	0.116	0.124	0.124	0.131	0.116	0.123
Short term welfare recipients migration	0.131	0.131	0.137	0.138	0.133	0.142	0.154	0.145	0.154
Control group migration	0.060	0.063	0.065	0.067	0.067	0.069	0.072	0.068	0.067
Welfare recipient migration vs. Control group migration	0.066	0.063	0.064	0.059	0.061	0.063	0.068	0.060	0.068
Moving across local government lines within counties	**95-96**	**96-97**	**97-98**	**98-99**	**99-00**	**00-01**	**01-02**	**02-03**	**03-04**
Total number of people moving	21493	21898	21716	22303	22473	23612	24533	23175	23501
Total number of welfare recipients moving	3585	3352	3099	2945	2996	3167	3400	3125	3427
Total number of long term welfare recipients moving	2004	1840	1679	1529	1634	1711	1828	1663	1849
Total number of short term welfare recipients moving	1581	1512	1420	1416	1362	1456	1572	1462	1578
Total number of people in the control group moving	17908	18546	18617	19358	19477	20445	21133	20050	20074
Migration rates within counties	**95-96**	**96-97**	**97-98**	**98-99**	**99-00**	**00-01**	**01-02**	**02-03**	**03-04**
Welfare recipient migration	0.053	0.053	0.053	0.053	0.053	0.056	0.060	0.053	0.059
Long term welfare recipients migration	0.051	0.050	0.051	0.049	0.051	0.053	0.056	0.049	0.053
Short term welfare recipients migration	0.057	0.057	0.056	0.058	0.055	0.059	0.065	0.060	0.069
Control group migration	0.024	0.025	0.025	0.026	0.026	0.027	0.028	0.027	0.027
Welfare recipient migration vs. Control group migration	0.029	0.028	0.028	0.027	0.027	0.029	0.032	0.026	0.033

Table 3

The dependent variable is (Mobility_Recipients$_{it}$-Mobility_NonRecipients$_{it}$)

Specification	1		2		3		4		5		6		7		8		9		9	
	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error
D1997			-0.0011	0.023																
D1998	-0.0004	0.0023			0.001	0.0018														
D1999	-0.0048	0.0036					-0.0025	0.0016												
D2000	-0.0053	0.0049							-0.0015	0.0018										
D2001	-0.0037	0.0043									-0.0002	0.0017								
D2002 (Year following policy reform)	-0.0005	0.0039											0.0032	0.0016					0.0034	0.00155
D2003	-0.0069	0.0042													-0.0041	0.0017				
D2004	0.0016	0.0035															0.0047	0.0016		
Unemployment (-2)	-0.31	0.34	0.119	0.178	0.043	0.137	0.052	0.124	-0.31	0.34	0.054	0.129	0.097	0.125	-0.01	0.125	0.063	0.121	0.065	0.121
R2 adj	0.605		0.557		0.557		0.564		0.559		0.557		0.569		0.578		0.586		0.608	
Number of observations	144		144		144		144		144		144		144		144		144		108	
County fixed effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Sample	1997-2004		1997-2004		1997-2004		1997-2004		1997-2004		1997-2004		1997-2004		1997-2004		1997-2004		1997-2002	

Table 4

Dependent variable is $\left(\frac{\Delta net\_inflow\_recipients-\Delta net\_inflow\_control}{Population\ in\ 10\ 000s}\right)$, all recipients are included.

Specification	1		2		3		4		5		6		7		**8**		9		10	
	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	**Coeff.**	**St.error**	Coeff.	St.error	Coeff.	St.error
Δbenefits (-1)	2.58	2.54	-0.53	3.54	1.96	8.12	7.25	8.25	-20.39	7.88	16.75	8.74	-1.64	4.48	**11.77**	**4.68**	-2.16	5.07	13.56	9.35
Δunemployment (-1)	3.82	2.43	2.73	3.54	8.59	4.63	0.09	7.81	10.99	8.82	-14.19	12.47	5.86	5.1	**3.41**	**8.12**	10.04	4.57	3.69	4.04
R2 adj	0.004		0		0.009		0.003		0.028		0.023		0.003		**0.008**		0.006		0.006	
Number of observations	3440		2150		430		430		430		430		430		**430**		430		430	
Time fixed effects	Yes		Yes		No		No		No		No		No		**No**		No		No	
Sample	1997-2004		1997-2001		1997		1998		1999		2000		2001		**2002**		2003		2004	
Treatment group	All recipients		All recipients		All recipients		All recipients		All recipients		All recipients		All recipients		**All recipients**		All recipients		All recipients	
Estimation method	OLS		OLS		OLS		OLS		OLS		OLS		OLS		**OLS**		OLS		OLS	

Note: Standard errors are robust to unknown form of heteroscedasticity.

Table 5

Dependent variable is $\left(\frac{\Delta net\_inflow\_long\_recipients-\Delta net\_inflow\_control}{Population\ in\ 10\ 000s}\right)$, only long term recipients are included.

Specification	11		12		13		14		15		16		17		**18**		19		20	
	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	**Coeff.**	**St.error**	Coeff.	St.error	Coeff.	St.error
Δbenefits (-1)	2.43	2.4	0.07	3.37	3.32	7.79	7.6	7.54	-19.64	7.83	15.14	8.11	0.82	4.18	**9.66**	**4.75**	0.19	4.35	8.06	8.93
Δunemployment (-1)	6.14	2.46	5.82	3.62	12.1	5.81	3.23	7.83	12.2	8.19	-9.89	11.39	8.39	4.9	**4.06**	**7.74**	12.09	4.36	4.37	3.76
R2 adj	0.008		0.004		0.025		0.003		0.028		0.013		0.001		**0.005**		0.011		0.003	
Number of observations	3440		2150		430		430		430		430		430		**430**		430		430	
Time fixed effects	Yes		Yes		No		No		No		No		No		**No**		No		No	
Sample	1997-2004		1997-2001		1997		1998		1999		2000		2001		**2002**		2003		2004	
Treatment group	Long term rec.		Long term rec.		Long term rec.		Long term rec.		Long term rec.		Long term rec.		Long term rec.		**Long term rec.**		Long term rec.		Long term rec.	
Estimation method	OLS		OLS		OLS		OLS		OLS		OLS		OLS		**OLS**		OLS		OLS	

Note: Standard errors are robust to unknown form of heteroscedasticity.

Table 6

Dependent variable is $\left(\frac{\Delta\text{net\_inflow\_recipients}-\Delta\text{net\_inflow\_control}}{\text{Population in 10 000s}}\right)$, only within county migration, all recipients are included.

Specification	20		21		22		23		24		25		26		**27**		28		29	
	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	**Coeff.**	**St.error**	Coeff.	St.error	Coeff.	St.error
Δbenefits (-1)	1.58	1.66	0.56	2.17											**5.55**	**3.46**				
Δunemployment (-1)	-0.35	1.53	-1.72	2.12											**1.21**	**5.83**				
R2 adj	0		0												**0**					
Number of observations	3440		2150		430		430		430		430		430		**430**		430		430	
Time fixed effects	Yes		Yes		No		No		No		No		No		**No**		No		No	
Sample	1997-2004		1997-2001		1997		1998		1999		2000		2001		**2002**		2003		2004	
Treatment group	All recipients		All recipients		All recipients		All recipients		All recipients		All recipients		All recipients		**All recipients**		All recipients		All recipients	
Estimation method	OLS		OLS		OLS		OLS		OLS		OLS		OLS		**OLS**		OLS		OLS	

Note: Standard errors are robust to unknown form of heteroscedasticity.

Table 7

Dependent variable is $\left(\frac{\Delta\text{net\_inflow\_long\_recipients}-\Delta\text{net\_inflow\_control}}{\text{Population in 10 000s}}\right)$, only within county migration, only long term recipients are included.

Specification	29		30		31		32		33		34		35		**36**		37		38	
	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	Coeff.	St.error	**Coeff.**	**St.error**	Coeff.	St.error	Coeff.	St.error
Δbenefits (-1)	1.3	1.59	0.59	2.17											**4.75**	**3.5**				
Δunemployment (-1)	0.72	1.64	-0.39	2.29											**1.58**	**5.43**				
R2 adj	0		0.004												**0**					
Number of observations	3440		2150		430		430		430		430		430		**430**		430		430	
Time fixed effects	Yes		Yes		No		No		No		No		No		**No**		No		No	
Sample	1997-2004		1997-2001		1997		1998		1999		2000		2001		**2002**		2003		2004	
Treatment group	Long term rec.		Long term rec.		Long term rec.		Long term rec.		Long term rec.		Long term rec.		Long term rec.		**Long term rec.**		Long term rec.		Long term rec.	
Estimation method	OLS		OLS		OLS		OLS		OLS		OLS		OLS		**OLS**		OLS		OLS	

Note: Standard errors are robust to unknown form of heteroscedasticity.

Table 8

First stage regression, dependent variable is ΔBenefits (-1)

Specification		39	
		Coeff.	St.error
Group1	(Benefits<3380)	0.27	0.20
Group2	(3380<Benefits<3880)	-0.01	0.05
Group3	(3880<Benefits<4380)	0.00	0.06
Group4	(4380<Benefits<4880)	-0.15	0.19
Group5	(Benefits>4880)	-0.73	0.15
Group1*distance		0.16	0.27
Group2*distance		0.71	0.21
Group3*distance		-0.18	0.21
Group4*distance		0.07	0.27
Group5*distance		0.29	0.11
ΔUnemployment (-1)		0.06	0.05
R2 adj		0.324	
Number of observations		430	
Sample		2002	
Estimation method		OLS	

Table 9

Second stage regression, dependent variable is $\left(\frac{\Delta net\_inflow\_recipients - \Delta net\_inflow\_control}{Population\ in\ 10\ 000s}\right)$

Specification	40		41	
	Coeff.	St. error	Coeff.	St. error
ΔBenefits (-1)	18.69	8.68	13.73	8.63
ΔUnemployment (-1)	2.91	7.99	3.77	7.65
R2 adj	0.008		0.005	
Number of observations	430		430	
Time fixed effects	Yes		Yes	
Sample	2002		2002	
Treatment group	All recipients		Long term recipients	
Estimation method	IV		IV	

Note: Standard errors are robust to unknown form of heteroscedasticity.

Table 10

Second stage regression, dependent variable is $\left(\frac{\Delta net\_inflow\_recipients - \Delta net\_inflow\_control}{Population\ in\ 10\ 000s}\right)$, only within county migration

Specification	42		43	
	Coeff.	St. error	Coeff.	St. error
ΔBenefits (-1)	14.02	7.79	12.44	7.59
ΔUnemployment (-1)	0.61	5.86	1.02	5.49
R2 adj	0.000		0.000	
Number of observations	430		430	
Time fixed effects	Yes		Yes	
Sample	2002		2002	
Treatment group	All recipients		Long term recipients	
Estimation method	IV		IV	

Note: Standard errors are robust to unknown form of heteroscedasticity.

Appendix Table 1

Dependent variable is $\left(\frac{\Delta\text{net\_inflow\_recipients}-\Delta\text{net\_inflow\_control}}{\text{Population in 10 000s}}\right)$, all recipients are included, allowing for an asymmetric impact of increases/reductions in welfare benefits.

Specification	1A		2A		3A	4A	5A	6A	7A	**8A**		9A	10A
	Coeff.	St.error	Coeff.	St.error	Coeff. St.error	Coeff. St.error	Coeff. St.error	Coeff. St.error	Coeff. St.error	**Coeff.**	**St.error**	Coeff. St.error	Coeff. St.error
Δbenefits (-1)	6.18	4.15	1.48	7.26						**13.60**	**6.24**		
Δunemployment (-1)	0.68	1.80	-0.38	2.43						**3.33**	**8.13**		
Δbenefits (-1) * INCREASE	-6.41	5.70	-2.54	8.82						**-4.85**	**11.55**		
R2 adj	0.000		0.000							**0.006**			
Number of observations	3440		2150		430	430	430	430	430	**430**		430	430
Time fixed effects	Yes		Yes		No	No	No	No	No	**No**		No	No
Sample	1997-2004		1997-2001		1997	1998	1999	2000	2001	**2002**		2003	2004
Treatment group	All recipients		All recipients		All recipients	All recipients	All recipients	All recipients	All recipients	**All recipients**		All recipients	All recipients
Estimation method	OLS		OLS		OLS	OLS	OLS	OLS	OLS	**OLS**		OLS	OLS

Note: Standard errors are robust to unknown form of heteroscedasticity.