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

Abstract:
This paper studies how changes in welfare benefit levels affect welfare recipients' residential choices. Although several empirical studies have stressed that welfare policy may affect residential choices of welfare recipients, few studies have simultaneously taken into account that residential choices of welfare recipients also affect welfare policy. The main contribution of this paper is to address this policy endogeneity by utilizing a policy reform as a natural experiment. The results show that welfare policy exerts a nontrivial effect on residential choices of welfare recipients. Moreover, I show that ignoring the policy endogeneity may give rise to a downward bias in the estimated migration responses.

Keywords: Welfare Benefits, Migration, Policy Endogeneity

JEL classification: I38, H73, H77, R23

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1. Introduction

With closer EU integration some economists and policy makers 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 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.

This relates to a large body of literature in public finance going back to Stigler (1957) and Musgrave (1959) that has warned against the consequences of decentralized responsibility for redistribution. 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, e.g. a decentralized system may be better to tailor (appropriate) benefits to those that are truly in need and in maintaining bureaucratic control.[2] Theoretical models relying on different assumptions on the importance of welfare migration are likely to have different implications for assignment of redistributive policy across different tiers of government. Consequently, it is of interest to evaluate whether welfare migration is important in practice. This is the object of the current paper.

To provide a proper test of the welfare migration hypothesis[3], two key methodological issues must be addressed. First, unobserved omitted variables that are correlated with welfare policy may generate a spurious relationship between welfare policy and residential choices. Second, when policy making is

[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 redistributive activity.

[2] These and other aspects are discussed in detail by Ladd and Doolittle (1982).

[3] Do jurisdictions that offer higher welfare benefits attract poor people who would not otherwise move there and retain poor people who might otherwise have chosen to leave?

purposeful action, responsive to economic and political conditions, it is necessary to identify and control for the forces that lead to these policy changes (Besley and Case, 2000). If for example policy makers take into account the migration responses of welfare recipients when deciding on welfare policy, estimation based on cross-sectional variation in welfare policy will be biased. Utilizing a natural experiment, namely exogenous placement of refugees across Swedish municipalities, Dahlberg and Edmark (2004) find that policy makers respond to increases in the welfare population by reducing welfare benefits. This suggests that estimates based on the assumption that variation in welfare benefits is exogenous may underestimate the impact of welfare policy on net immigration. While the first problem is properly addressed in the more recent studies of welfare migration, the second problem is typically ignored. The current analysis addresses both issues. Besley and Case (2000) provide a general treatment and discussion of bias due to both omitted variables and policy endogeneity.

The existing literature on the welfare migration hypothesis has studied data from the United States. The current analysis employs data from another country with decentralized welfare policy, Norway. The main contribution is to address the potential policy endogeneity by utilizing a centrally implemented policy reform as a natural experiment. More specifically, I use exogenous variation of changes in welfare benefits across Norwegian local governments provided by an announcement of national guidelines taking place in 2001. The econometric analysis applies a difference-in-differences strategy in which I evaluate whether changes in welfare benefits have a positive effect on changes in net inflow of welfare recipients relative to a control group. The results from the econometric analysis confirm the welfare migration hypothesis and suggest that ignoring the policy endogeneity may give rise to a downward bias in the estimated welfare migration effect.

The early studies on the welfare migration hypothesis have provided conflicting results as to whether welfare recipients respond to (changes in) welfare benefit levels by relocating. These studies are however plagued by several methodological problems, discussed by, for example, Bailey (2005), McKinnish (2005), and Meyer (2000). Some of the very recent studies on the welfare migration hypothesis add to the welfare migration literature by applying more sophisticated identification strategies than previous studies and confirm the existence of welfare migration. 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 failed to find any welfare migration effect suffer from omitted variable bias. Applying a more rigorous estimation strategy than many previous analyses, in particular by including state fixed

effects and a control group, he finds evidence of the welfare migration hypothesis. Gelbach (2004) points out that the incentives to migrate for welfare benefits in the United States are highest when a mother's children are young, as there is a longer period of welfare benefit eligibility. He finds that for single women with less education than a college degree, their own state's welfare benefits affect residential choices and that the effects were decreasing in the age of the oldest child. The interaction effect is not present for a comparison group of mothers with college degrees. McKinnish (2005, 2007) introduces another clever 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 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 what McKinnish (2005) finds, utilizing aggregate county level data. Consistent results are found when micro level data on migration decisions are used (McKinnish, 2007).

A related literature has focused on strategic interaction among jurisdictions in the determination of welfare policy, i.e. welfare competition. If a jurisdiction is concerned about becoming a 'welfare magnet', then benefit levels in other (typically close by) jurisdictions will affect the jurisdiction's own benefit choice. Evidence of welfare competition thus provides indirect support to the welfare migration hypothesis. US studies, summarized by Brueckner (2000), and European studies, such as Fiva and Rattsø (2006) studying Norwegian local governments, find results consistent with the welfare competition hypothesis.[4] Jurisdictions seem indeed to be playing a 'welfare game', suggesting that studies of welfare migration should pay close attention to policy endogeneity. Surprisingly, the existing literature does not follow this recommendation. Almost all existing studies rely purely on observed variation in welfare policy to identify welfare migration effects. However, if welfare policy affect residential decisions of the poor, then residential choices of the poor are likely to affect how benefit levels are set (Moffitt, 1992). If politicians respond to increased inflow of welfare recipients by lowering welfare benefits, a downward bias in studies that neglect this problem, such as Bailey (2005), Gelbach (2004), and McKinnish (2007), can be expected.[5]

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[4] Note that it is empirically challenging to separate strategic interaction in welfare policy due to mobility pressure from other sources of strategic interaction (notably yardstick competition). Revelli (2006) exploits an institutional reform taking place in the United Kingdom to address this issue. The results suggest that the spatial pattern observed in welfare policy is (at least partially) driven by yardstick competition. 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" (pp. 460-461).

[5] Policy endogeneity is less likely to be a problem for McKinnish (2005) since welfare migration effects are estimated by comparing welfare caseloads at state borders to state interiors, but this identification strategy only allows identification of short distance welfare migration effects.

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

2. Institutional Structure and Data

The welfare benefit system is the final safety net for those who fall through the gaps of other arrangements of the Norwegian welfare system and is intended to provide temporary 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 local governments. The Social Service Act leaves considerable discretion to 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. Their main responsibilities concern care for the elderly, preschool, primary and lower secondary education. Financing of local governments is highly centralized with around 90 percent of local government revenue coming from regulated income taxation and grants from the central government. Local governments have some discretion related to user charges and property taxation, which are important additional 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.

2.1. Welfare Benefits

The implementation of welfare policies includes guidelines set by the local council and actual payments made by the local welfare office. The politically determined norms are defined as the amounts paid to 'standard users' per month. These are likely to be the most visible measure of welfare policy from the perspective of potential welfare immigrants and consequently appear well suited for a study of the welfare migration hypothesis. In this study I rely on the locally determined norms for a single-person household per month.

Data on welfare benefit norms are available from 1993 and onwards. There are quite a few observations missing from the two first years of data collection, and consequently I limit the analysis to the period 1995 to 2004. A total 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 in this analysis. The politically determined norm varies considerably across local

governments (see Appendix Table 1). Since housing costs 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 along other particular dimensions, such as differences between rural and urban local governments.[6] The average welfare benefit norm to a single-person household was NOK 4203 (USD 700) per month in 2004. On average, the welfare benefits declined in real terms in the ten year period under study. The welfare competition mechanism, empirically identified by Fiva and Rattsø (2006), may have contributed to this.

2.2. Migration Rates

Data on received social assistance are available for the entire adult Norwegian population. In the current analysis I analyze migration patterns of single men aged 16 to 66, without dependent children living in the same household, approximately 400,000 individuals each year.[7] This sample is further divided into two groups, welfare recipients and a control group of non-recipients.

Although the basis for the analysis is micro data (described in detail in Appendix Table 3), I rely on net migration flows across local government lines in the econometric analysis. The key variables measure net inflow of welfare recipients and net inflow of nonrecipients respectively, scaled by local government population size in 10,000s. Descriptive statistics is provided in Table 1. Migration flows are measured from January 1st in year t to January 1st in year t+1. A person is defined as a welfare recipient if he received welfare benefits in year t, independent of whether he received welfare benefits in year t+1 (more on this in section 3.1). As a proxy for short distance migration flows I use migrations across local government lines within the same county, the regional level of government in Norway.[8]

The econometric design elaborated in section 3 requires that the treatment and control group are as similar as possible (except for welfare migration incentives). This is the main reason for constraining the analysis to single men, aged 16 to 66, without dependent children. But there are also two other reasons for focusing on this group of people. First, welfare participation is very high among single men aged 16 to 66 without dependent children. This group constitute around one third of the entire

[6] Using data for 1998, Fiva and Rattsø (2006) find no statistically significant association between welfare benefits and population size and settlement pattern, controlling for other factors.

[7] This paper is a revised version of a chapter in my PhD thesis (Fiva, 2006). In Fiva (2006) the inclusion criterion was more generous, including all men that at any point in time between 1992 and 2004, were single, aged 16 to 66 and without dependent children living in the same household. The results were similar.

[8] There are 18 counties in Norway in addition to Oslo which is both a county and a local government.

welfare recipient population, but below ten percent of the entire Norwegian population.[9] Second, residential choices of this sub-population is unlikely to be sensitive to the other main local government services provided, primarily directed to children and elderly people, which may vary inversely with welfare policy. [10]

Table 1. Descriptive statistics on net migration flows, 1996-2004

	Mean	St. Dev.	Min	Max	Obs.
Net inflow of recipients	-1.2	9.8	-172.1	62.0	3870
Net inflow of nonrecipients	-1.3	17.5	-137.4	87.7	3870
Net inflow of recipients - Net inflow of nonrecipients	0.1	19.3	-194.3	155.7	3870
Net inflow of recipients, from local governments within the same county	-0.4	5.9	-61.0	42.9	3870
Net inflow of nonrecipients, from local governments within the same county	-0.1	11.0	-84.7	70.8	3870
Net inflow of recipients - Net inflow of nonrecipients, from local governments within the same county	-0.2	12.3	-76.7	84.7	3870

Note: All variables are scaled by population size in 10,000s.

Although social assistance is intended to be granted in emergency situations and not as long-term support, the micro data show that many recipients are dependent on welfare benefits for longer periods of time. Around 60 percent of the welfare recipients receive welfare benefits for more than three months within a given year and the majority of recipients that receive welfare in year t also received welfare in year t-1 and year t+1.

Around 10 percent of the welfare recipient population moved across local government lines from one year to the next compared to around 5 percent of the control group of nonrecipients (see Appendix Table 3). Levine and Zimmerman, who also utilized US data to study the welfare migration hypothesis, distinguished between poor single mothers and four different control groups (poor single women without children, poor single men, poor married women, and poor married men). They 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" (Levine and Zimmerman (1999, p. 401)).[11] It follows that Norwegian single men without children receiving

[9] The proportion of welfare recipients in the sub-population under study varies from 9.5 to 12.2 percent between 1996 and 2004, while only 3 percent in the general population receive welfare benefits.

[10] The Norwegian Social Science Data Service provided the aggregate migration flows utilized in the current study. I have not had the opportunity to experiment with alternative treatment and control groups.

[11] Levine and Zimmerman (1999) found no evidence in favor of the welfare migration hypothesis.

welfare are close to three times as likely to move across jurisdiction lines with responsibility for welfare benefits as poor women with dependent children in the United States. The treatment group members (never-married high school dropouts with children) in the McKinnish (2007) study are even less mobile: only 5–6 percent of them moved across state lines during a *five*-year period. [12]

3. Empirical Strategy

As stressed in the introduction, one has to address two key methodological challenges if one is interested in the causal effect of changes in welfare policy on residential choices of welfare recipients. First, unobserved factors that are correlated with welfare policy may generate a spurious relationship between welfare policy and residential choices (omitted variable problem). Second, since policy makers are likely to take into account the migration responses of welfare recipients when deciding on welfare policy, cross-sectional variation in changes in welfare benefits is endogenous to the migration responses (policy endogeneity). Both issues are addressed in the current study.

3.1. The Difference-in-Differences Estimator

A naïve test of the welfare migration hypothesis would be to investigate whether there is a positive correlation between welfare generosity and net inflow of recipients. However, since people move for any number of pecuniary or nonpecuniary 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, this would confound the estimates. In order to take account of such problems one can use migration patterns of similar people that are not welfare recipients. Introducing a valid control group allows me to take into account factors unrelated to welfare policy. The central idea is that welfare generosity will affect residential location of welfare recipients while leaving nonrecipients unaffected.

On theoretical grounds, however, one cannot rule out that location decisions of nonrecipients also are affected by local government welfare policy. Welfare policy may have an impact on the residential choices of people in the control group if taxes are increased or if spending on other local services is reduced. The latter effect seems most worrisome in the Norwegian setting where local taxing power is limited. The main local services (child care, primary education and care for the elderly) are however unlikely to be important determinants of the residential choices of households in the control group.

[12] Note that the average local government in Norway is much smaller in area than is the average US state. The average local government jurisdiction in Norway is approximately 700 km^2, whereas the continental US states range from around 4,000 km^2 (Rhode Island) to almost 700,000 km^2 (Texas).

To capture unobserved, time invariant local government effects I first-difference the model. Technically this yields a difference-in-differences approach, where the dependent variable measures changes in migration flows of welfare recipients (the treatment group) adjusted for changes in migration flows of nonrecipients (the control group). The main regression, where Δ denotes time differences, is given by:

$$Y_{it} = \delta_1 + \delta_2 \Delta \text{Benefits}_{\text{it}} + u_{it} \quad (1)$$

where Y_{it} = Δnet inflow of recipients$_{it}$ - Δnet inflow of nonrecipients$_{it}$. ΔBenefits$_{it}$ is the change in the politically determined norm granted to a single person per month in NOK 1000 from year t–1 to year t.[13] Migration rates are measured as net inflows from January 1st in year t to January 1st in year t+1. The dependent variable is scaled by local government population size (at beginning of year t) in 10,000s. δ_2 can then be interpreted as an estimate of how many welfare recipients a local government with a population size of 10,000 (i.e. an average local government) receives if it increases welfare benefits with NOK 1000. The welfare migration hypothesis suggests that $\delta_2 > 0$.

To reduce the possibility that endogenous welfare participation (correlation of welfare participation and welfare benefits) creates a spurious relationship between welfare policy and residential location I condition on welfare receipt in year t.[14] I have also estimated the model, conditioning on welfare recipient status in year t+1, and the results are similar. This is comforting and suggests that endogenous welfare participation is not confounding the estimates.

3.2. Policy Endogeneity

If welfare-induced migration is a concern for policy makers, then ΔBenefits is endogenous to the left-hand side variable in Eq. (1). If policy makers respond to an increasing welfare population by reducing welfare benefit levels, this results in a negative bias in δ_2 when estimating Eq. (1) with standard OLS. This problem is not properly addressed in the existing literature. While most studies simply ignore policy endogeneity, Peterson and Rom (1989) and Berry et al. (2003) aim to break the simultaneity

[13] I have also experimented with utilizing the percentage change in welfare benefits as an independent variable. The results are then similar.

[14] If some people do not receive welfare payments in low-benefit states but would if they were in a high-benefit state, conditioning on welfare receipt in year t+1 is likely to exaggerate the flow of welfare recipients from low- to high-benefit jurisdictions. Conditioning on welfare receipt in period t would reduce the problem, but bias could still exist and would most likely go against finding evidence of welfare migration (Meyer, 2000). Conditioning on welfare receipt in period t is also likely to exhibit a downward bias if a substantial number of poor people who are not on welfare in period t (and are consequently assigned to the control group) migrate to other local governments to receive welfare benefits.

problem by estimating the mutual effects of welfare benefits and poverty rates, but the exclusion restrictions that they impose to obtain identification are questionable.[15] Moreover, using poverty rates to estimate welfare migration effects is highly problematic since poverty rates within jurisdictions may change as a function of welfare policy without any migration taking place (due to disincentive effects or because welfare programs lift people out of poverty).

Without the ability to experimentally vary the relevant variable (welfare generosity) this paper follows the recommendation of Meyer and seeks "variation that is driven by factors that are clearly identified and understood" (1995, p. 153). I suggest that the introduction of a national instructive welfare benefit norm in February 2001 can act as a natural experiment that allows me to obtain exogenous variation in ΔBenefits. The instructive norm was not a minimum standard, but aimed to "contribute to a more homogenous practice across local governments and to provide more similar support for equal recipients" (Circular I-13/2001 from the central government, my translation). Although it was not mandatory for the local governments to implement the national instructive welfare benefit norm, this reform had a large impact on local government priorities. In a survey conducted in August 2001, 104 out of 336 local governments[16] claimed that they had altered the welfare benefit levels after the national guidelines were announced. Of the local governments, 78 (19) 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 governments chose to exactly implement the national instructive norm in 2001. 62 of these local governments were initially below the instructive national norm and 57 were initially above. The coefficient of variation in locally determined norms decreased from around 0.15 prior to the announcement to around 0.10 thereafter (see Appendix Table 1). The national guidelines seem to have succeeded in making the locally determined norms more homogenous.

Utilizing the exogenous variation in ΔBenefits provided by this reform, a credible test of the welfare migration hypothesis can be conducted and potential bias on estimates that neglect policy endogeneity can be investigated.[17] Because the policy reform was implemented at one point in time, this identification strategy reduces the data set from a panel data set of nine years of observations to a (first

[15] Both studies exclude measures of government ideology, political competition, tax capacity and tax effort from the poverty rate equation. Berry et al. also exclude the federal share of costs of assisting particular welfare recipients. These studies provide conflicting evidence on the importance of policy endogeneity when estimating welfare migration effects. Peterson and Rom (1989) find that when welfare benefit levels increase, the size of the poverty population increases. But simultaneously, when poverty rates increase, benefit levels are cut. Berry et al. (2003) find only weak support for the welfare migration hypothesis and poverty rates do not seem to affect welfare policy.

[16] A total of 98 local governments did not respond.

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

differenced) cross-section for 2001.[18] Panel data prior to the policy reform are used to discuss possible bias caused by policy endogeneity.

Since local governments chose whether or not to respond to the national guidelines, a two-stage least squares (2SLS) approach is warranted. To capture the impact of the national guidelines I rely on information on local governments' welfare policy existing prior to the national guidelines. In my first stage regression, changes in welfare benefit levels from 2000 to 2001 (**$\Delta benefits_{2001}$**) are regressed on variables indicating the distance from the central guidelines (to be launched in 2001) in 2000:

$$\Delta \text{Benefits}_{i2001} = \alpha_0 + \alpha_1 Below_{i2000} + \alpha_2 Benefits_{i2000} + \alpha_3 Below_{i2000} * Benefits_{i2000} + \varepsilon_i \,, \quad (2)$$

where ***below*$_{2000}$** is a dummy variable turned on if the local government had welfare benefits in 2000 below the national instructive norm given in 2001 and ***benefits*$_{2000}$** is the benefit level in 2000. The interaction term (***below*$_{2000}$**benefits*$_{2000}$**) captures an asymmetric impact of the guidelines for local governments initially below rather than above the national instructive norm. The validity of this identification strategy rests on the assumption that local governments' welfare benefit levels in 2000 had no 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.

4. Results

Looking into the Norwegian data on welfare policy and residential choices I start by providing a descriptive analysis of how the national guidelines may have affected overall migration rates. I then move on to the direct test of the welfare migration hypothesis, drawing on the identification strategy laid out in Section 3, before I discuss whether the estimated welfare migration effects are likely to be an important concern for policy makers.

4.1. Descriptive Analysis

The introduction of the national guidelines in 2001 was associated with a drop in the correlation of welfare benefit levels between two consecutive years (see Appendix Table 1). Based on the welfare migration hypothesis, average welfare migration incentives increase when several local governments

[18] Appendix Table 2 provides descriptive statistics for the dependent variable for the 2001 cross section (corresponding to Table 1).

change their welfare benefits (for welfare recipients). This effect remains until the welfare recipient population has adjusted to the new welfare benefit levels.

Interestingly, as Figure 1 shows, the proportion of welfare recipients that moved across local government lines peaked in 2001. The same finding holds when controlling for the migration rates of nonrecipients which should be unaffected by the 'shock' to welfare benefit levels. If welfare migrations typically are short distance moves one would expect that migration rates across local government lines *within counties* to be particularly sensitive to the centrally implemented policy reform. Figure 2, which emphasizes within-county migration flows, exhibits the same pattern as Figure 1.

Figure 1. Share of households moving across local government borders

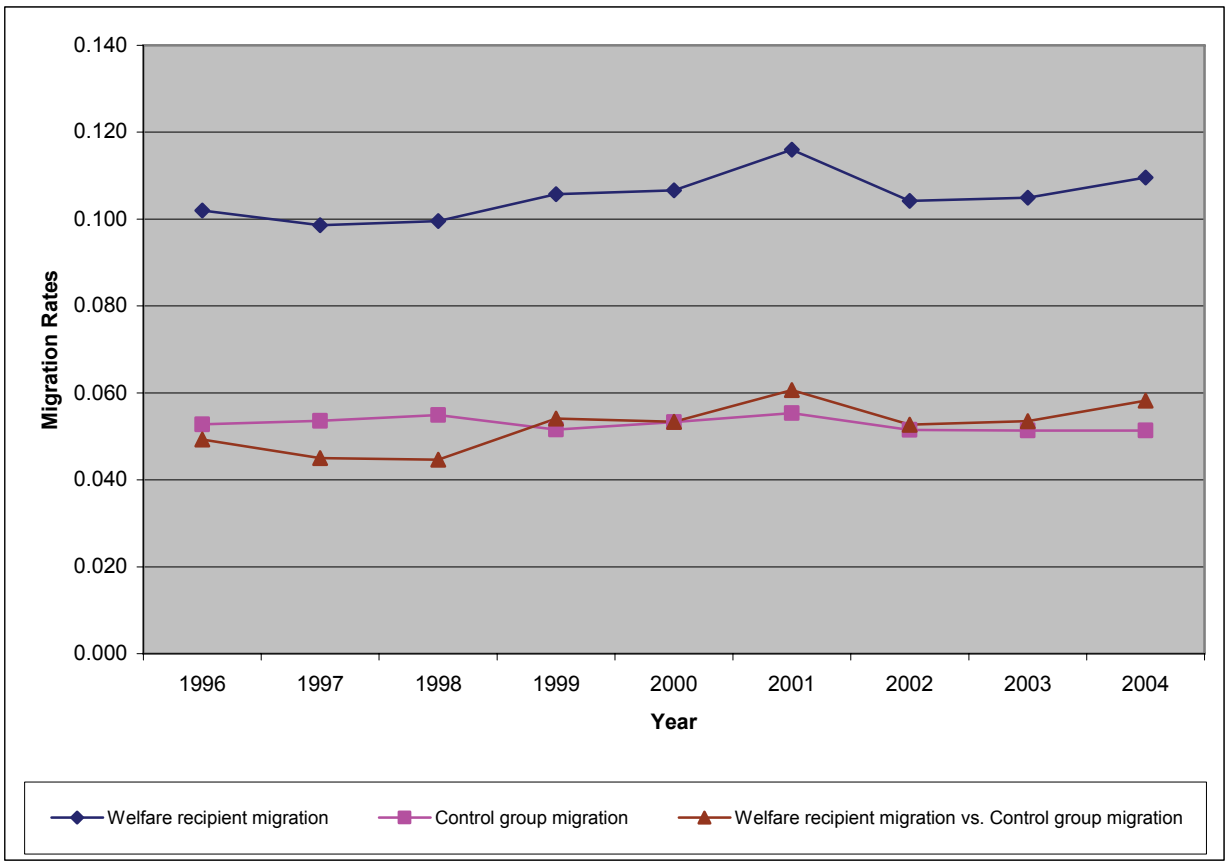
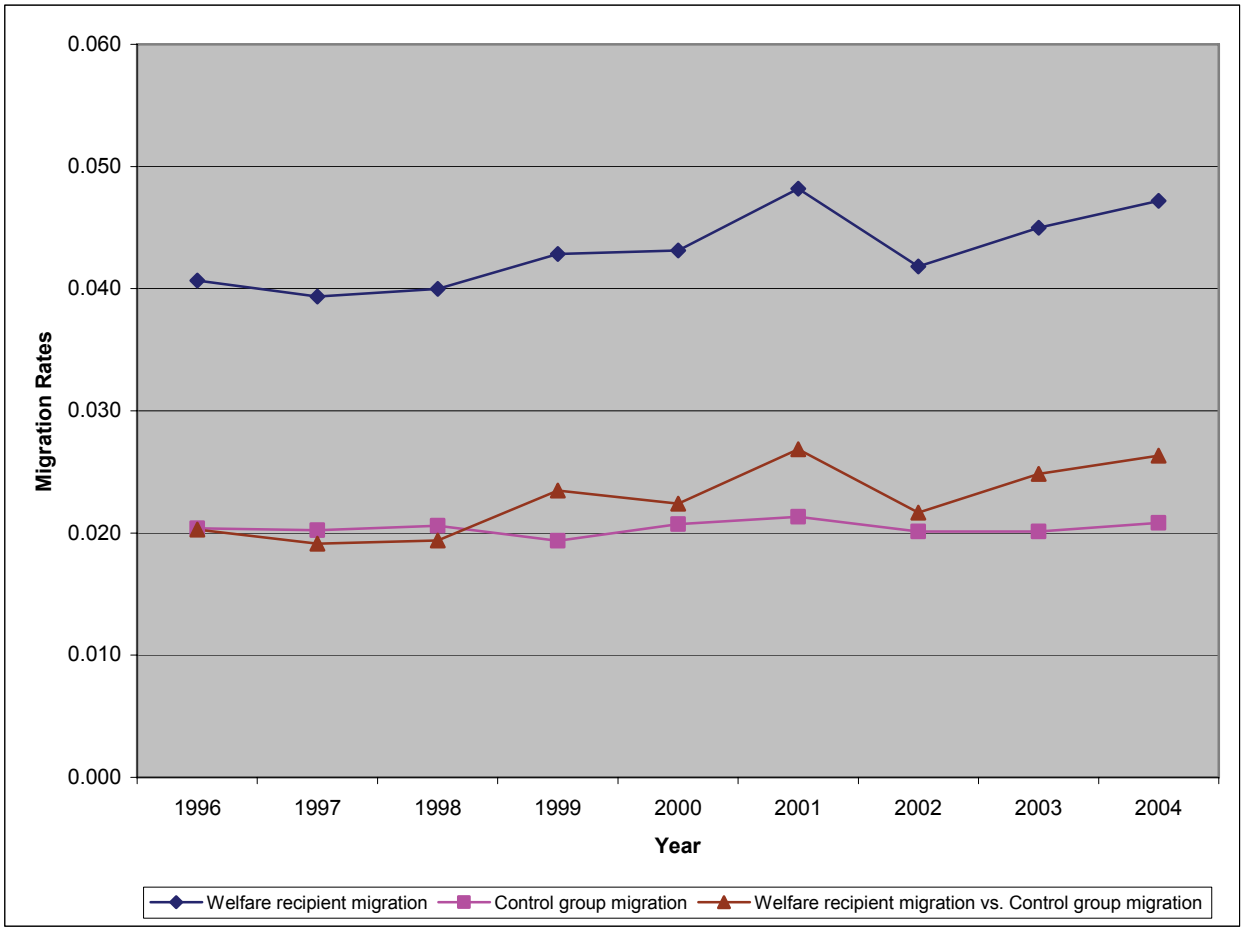


Figure 2. Share of households moving across local government borders *within counties*



To further investigate whether welfare recipients actually on average migrated more frequently following the implementation of the national instructive norms I evaluate within-county mobility in Norway's 18 counties. Equation (3) shows the simple regression to be estimated:

$$(3) \qquad (\text{Mobility\_Recipients}_{it} - \text{Mobility\_NonRecipients}_{it}) = f_i + \beta d2001_t + \eta_{it},$$

where the dependent variable is the difference in within-county migration rates of welfare recipients and non-recipients, in county i at time t. On the right-hand side I include county specific fixed effects, f_i, and a time specific effect for year 2001 (d2001). Estimation of this specification shows that in comparison to other years, the difference in migration rates between recipients and nonrecipients within counties were on average 0.5 percentage points higher in 2001. This effect is statistically significant at the 1 percent level and holds when studying either the entire period available (specification 1) or only the period up to the reform (specification 2), and when the lagged

unemployment rate is included as an explanatory variable (specification 3 and 4).[19] This is suggestive evidence in favor of the welfare migration hypothesis.

Table 2. A 2001-effect on within county migration rates? The dependent variable is (Mobility_Recipients$_{it}$ -Mobility_NonRecipients$_{it}$)

	1		2		3		4	
	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error
D2001	0.0048	0.0016	0.0055	0.0015	0.0042	0.0016	0.0045	0.0015
Unemployment (–1)					-0.2030	0.1138	-0.3728	0.1185
R^2	0.58		0.62		0.59		0.66	
Number of observations	162		108		162		108	
County fixed effects	Yes		Yes		Yes		Yes	
Sample	1996-2004		1996-2001		1996-2004		1996-2001	

4.2. The Direct Test of the Welfare Migration Hypothesis

As discussed above, one needs variation in welfare policy that reasonably can be treated as exogenous to identify welfare migration effects. The empirical strategy followed here is to utilize variation in changes in welfare benefits generated by the introduction of national guidelines launched in 2001. The first-stage regression is reported in Table 3.

Table 3. First-stage regression: The dependent variable is ΔBenefits

	5	
	Coeff.	St. error
Constant	0.719	0.194
Below	1.192	0.371
Benefits (2000)	-0.189	0.043
Benefits (2000) * Below	-0.296	0.099
R^2	0.275	
Number of observations	430	
Sample	2001	
Estimation method	OLS	

The first stage F-statistic for the joint null hypothesis of zero coefficients on all excluded instruments shows that the instruments are strong (with an F-statistic of 53.75). As expected, local governments

[19] Time dummies for other years all come out statistically insignificant, except the dummy for 2004 (which has a positive sign), when including them one at a time.

with welfare benefits below the central guidelines for 2001 are predicted to have increased their welfare benefits from 2000 to 2001, and to a greater extent the further they were from the central guidelines. The local governments above the central guidelines were predicted to reduce their welfare benefits and to a greater extent 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 those below the central guidelines. A local government NOK 500 below the national guidelines is predicted to increase their welfare benefits with NOK 270, while a local government NOK 500 above the national guidelines is predicted to reduce their welfare benefits with NOK 109.

Figure 3 and Figure 4 plots respectively *predicted* and *actual* changes in welfare benefits from 2000 to 2001 against welfare benefit levels in 2000. The downward sloping line that can be spotted in Figure 4 corresponds to the 119 local governments that chose to exactly implement the national instructive norm. For comparison, Appendix Figure 1 shows corresponding figures for 1996-2000.

Figure 3. ***Predicted changes*** **in welfare benefit levels from the first-stage regression (vertical axis) against welfare benefit levels in 2000 (horizontal axis). Welfare benefit levels are measured in 1000 NOK**

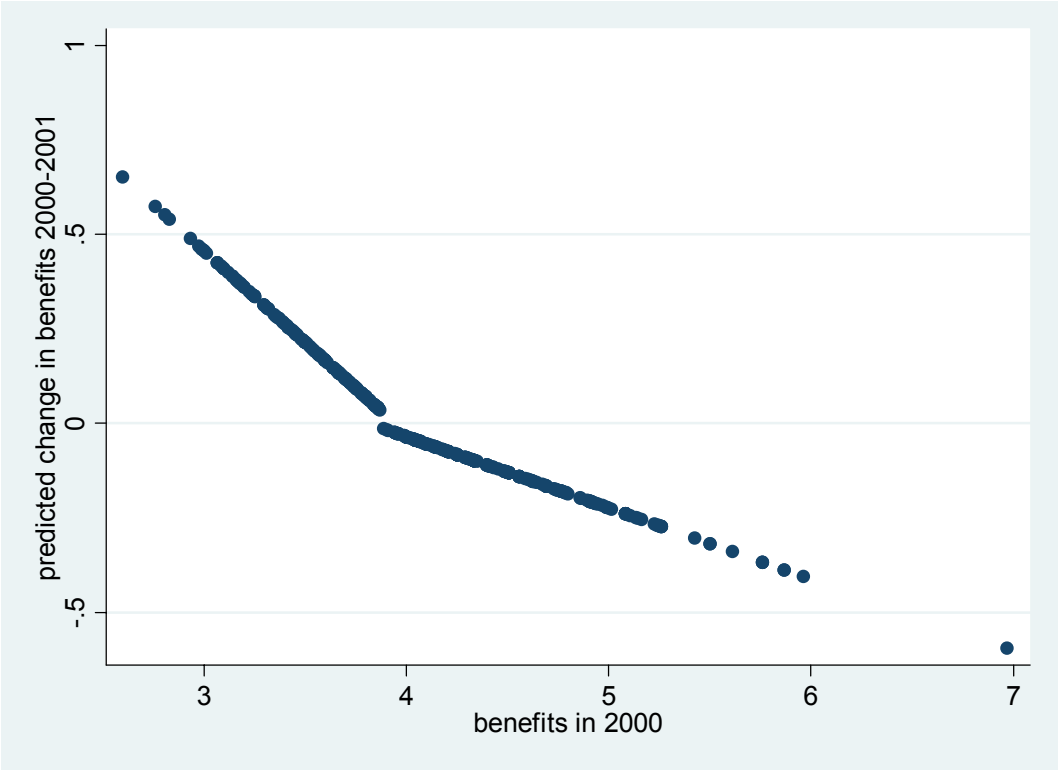
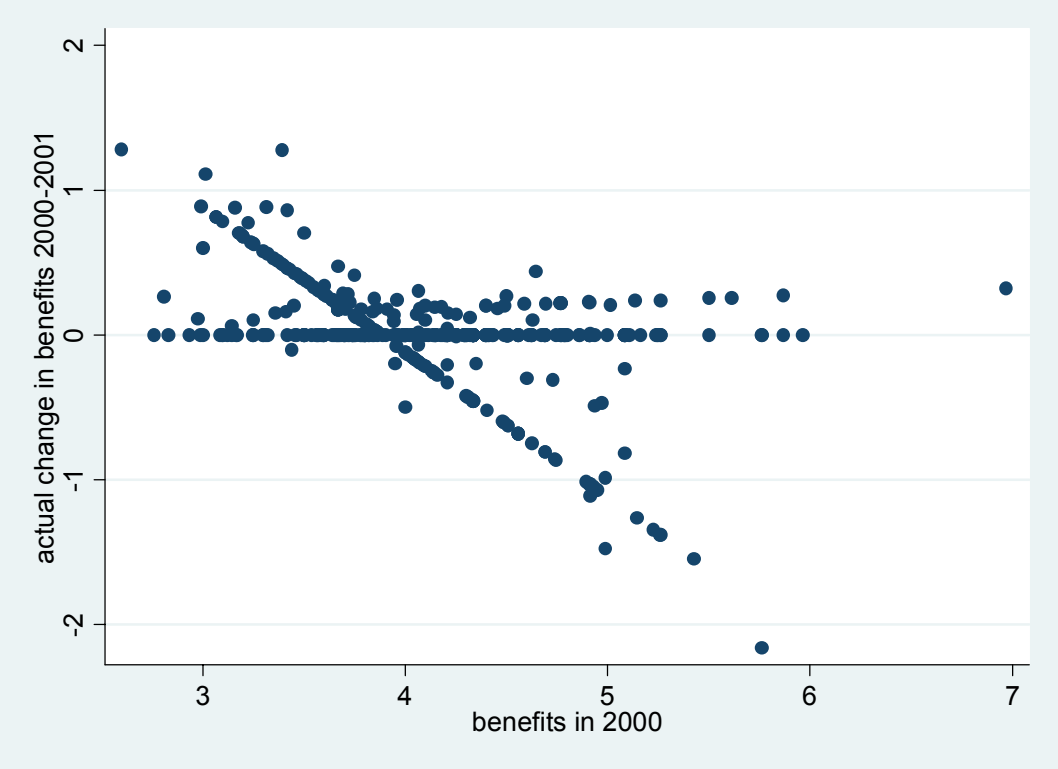


Figure 4. ***Actual changes*** **in welfare benefit levels from 2000 to 2001 (vertical axis) against welfare benefit levels in 2000 (horizontal axis). Welfare benefit levels are measured in 1000 NOK**



The main results are presented in Table 4. As a benchmark for evaluating the potential policy endogeneity problem I include results from OLS regressions on Eq. (1) based on panel data for the period prior to the introduction of the central guidelines (1996–2000). In this sample no welfare migration effect can be found, evaluating overall migration flows (specification 6) or only short distance migration flows (specification 7). Short distance moves are defined as migrations across local government lines *within counties*.

When constraining the sample to the (first differenced) cross- section when the central guidelines were launched and replacing actual values for ΔBenefits with fitted values from the first-stage regression, I find economically important welfare migration effects. A NOK 1000 increase in welfare benefits is estimated to lead to a net inflow of around 10 welfare recipients from the population under study the following year (for a local government with a population of 10,000).

Even though long distance migrations constitute around 60 percent of all migrations, I find similar point estimates when evaluating all migration flows and only within county migration flows. While

the latter estimate is close to statistically significant at the 1 percent level, the former is only borderline significant at the 10 percent level.[20] This suggests that most, if not the entire, welfare migration effect is driven by welfare recipients migrating between jurisdictions that are geographically close. Moreover, a comparison of OLS and 2SLS specifications suggests that there seems to be a downward bias in estimates that ignore policy endogeneity.

Table 4. Second-stage regressions: The dependent variable is (Δnet inflow of recipients$_{it}$ - Δnet inflow of nonrecipients$_{it}$)

	6		7		8		9	
	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error
Δbenefits	0.436	1.763	1.223	1.255	9.250	5.899	11.344	4.458
Number of observations	2150		2150		430		430	
Time fixed effects	Yes		Yes		No		No	
Sample	1996–2000		1996–2000		2001		2001	
	All		Within county		All		Within county	
Estimation method	OLS		OLS		2SLS		2SLS	

Note: A constant term is included in all regressions. Standard errors are robust to unknown forms of heteroscedasticity and clustered at the local government level (specification 6 and 7).

The central idea in the difference-in-differences estimator is that, except for welfare migration incentives, do control and treatment group members face the same migration incentives. However, to capture that changes in economic conditions do not necessarily influence treatment and comparison groups in the same way, and minimize the risk of getting an omitted variable bias, I have included ***Δunemployment*** as a control variable. Unemployment is defined as the share of the male population that is unemployed (yearly average). This variable captures 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. In the current data set I do not find an impact of the lagged unemployment rate and the effect of changes in welfare benefits is unaltered when this variable is included, reported in Table 5.

[20] A more parsimonious first-stage specification where the only IV is benefits(2000) yields a somewhat lower fit of the first-stage regression (R^2 of 0.253). The point estimates are similar: 10.993 (6.319) and 12.157 (4.746) corresponding to specification 8 and 9 (standard errors within brackets). If only 'below' is used as an instrument the first stage regression yields an R^2 of 0.192 and corresponding point estimates of 6.763 (7.835) and 10.164 (5.255).

Table 5. Second-stage regressions: The dependent variable is (Δnet inflow of recipients$_{it}$ - Δnet inflow of nonrecipients$_{it}$), change in unemployment included as a control variable

	10		11		12		13	
	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error
Δbenefits	1.580	2.355	1.264	1.257	9.255	5.920	11.352	4.464
Δunemployment	0.399	1.772	-0.892	1.083	0.676	5.662	-0.167	3.752
Number of observations	2150		2150		430		430	
Time fixed effects	Yes		Yes		No		No	
Sample	1996–2000		1996–2000		2001		2001	
	All		Within county		All		Within county	
Estimation method	OLS		OLS		2SLS		2SLS	

Note: A constant term is included in all regressions. Standard errors are robust to unknown forms of heteroscedasticity and clustered at the local government level (specification 10 and 11).

Increasing welfare benefits have two effects on migration flows: attracting people from other local governments and retaining the welfare population that is already living in the local government area. Reductions in welfare benefits will work oppositely. The repelling and attracting forces may not necessarily be symmetric and may also manifest over different time spans. I have investigated whether the repelling effect is stronger than the attracting effect (or vice versa) but I have not found such an asymmetric welfare migration effect in the data. Moreover, since I only evaluate migration flows from one year to the next, it is reasonable to believe that I only capture the short term and not the full welfare migration effect.

In Appendix Table 4 and 5 I report OLS (first differenced) cross-section regressions for each year. The estimated welfare migration effects exhibit considerable variation from year to year. Interestingly, the cross-section regression for 2001 is the only regression with a positive and statistically significant welfare migration effect for estimates based on both overall and short distance migration flows.[21] One possible interpretation of this finding is that the bias in the OLS estimates due to policy endogeneity is smaller in the 2001 cross- section because many local governments mechanically adjusted their welfare benefit levels in line with the national instructive guidelines from 2000 to 2001.

4.3. Is Welfare Migration Likely to be a Concern for Policy Makers?

The results above show that changes in welfare benefits exert a nontrivial effect on residential choices of welfare recipients. However, one may question whether this migratory response is perceived by

[21] The welfare migration estimate is positive and statistically significant for the 1999 cross- section when overall migration flows are evaluated.

local policy makers and whether policy makers react by holding benefit levels below what they otherwise would be. Gelbach (2004) and McKinnish (2007) argue that this is probably not the case. Although they find that differences in welfare generosity generate substantial changes in the migration rates of welfare-prone households, they argue that even sizeable differences in migration rates do not generate particularly large migration flows because welfare-prone individuals in the United States are a relatively immobile group. They conclude that welfare migration is unlikely to be a large concern of policy makers. The main findings of the current analysis stand in contrast to this conclusion. If welfare migration had no impact on the political decision making process, the standard OLS estimates presented above would not be biased.

To see why policy makers are likely to worry about welfare magnetism, consider the following stylized example based on the results presented above. An average local government consisting of 10,000 inhabitants and 3 percent welfare recipients considers increasing its welfare benefits from NOK 4000 to NOK 4500 for all types of welfare recipients. In the no-mobility case, the cost of such a policy would simply be the increase in welfare benefits times the number of recipients living in the jurisdiction (NOK500*300 = NOK150,000). However when welfare recipients respond to changes in welfare benefits by relocating, the cost of welfare generosity increases because the jurisdictions' welfare recipient population increases. According to specification 9, a NOK 500 increase would lead to a net inflow of approximately six welfare recipients from the subpopulation under study the following year (0.5*11.34), corresponding to around 6 percent increase in the welfare recipient population under study.

In the data set (constrained to migration flows of single men aged 16-66 without dependent children) I cannot test whether the welfare migration responses are equally strong for other welfare recipient groups (single women, families, etc.). Assuming that such households react in similar fashion to changes in welfare benefit levels as the welfare recipient population studied here, the cost of increasing welfare benefits with NOK 500 become: NOK 500*300 current recipients + NOK 4500*18 new recipients = NOK 231,000 per month. The cost is 54 percent higher than if welfare recipients do not respond to changes in welfare benefits. This back-of-the-envelope calculation exaggerates the welfare migration cost if, for example, households with children respond less to changes in welfare benefits than single-person households. However, assuming that the migration response of all other households except single men, 16 to 66 years old, without dependent children is equal to zero still suggests that the cost of increasing welfare benefits from NOK 4000 to NOK 4500 is 18 percent higher than it would be in the absence of welfare migration.

Although the confidence interval of the estimate driving this result admittedly is quite large, the results in this analysis suggest that welfare migration is likely to be a concern for policy makers. This is consistent with the strong strategic interaction in welfare policy found among Norwegian local governments by Fiva and Rattsø (2006).

5. Conclusion

The current 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 estimates that ignores policy endogeneity. The welfare migration responses are of a magnitude that suggests that policy makers are likely to worry about 'welfare magnetism' and fiscal competition in welfare policy is likely to prevail. As is well known from the theoretical literature, this may result in an equilibrium characterized by suboptimal levels of redistribution. Although this is not necessarily the case in Norway (due to the centralized financing of the local governments), this paper suggests that the main argument against decentralized responsibility for redistribution is not merely a theoretical possibility. Rather this argument seems highly relevant in settings similar to the one analyzed by the current paper. The importance of welfare migration in settings where household mobility is lower than across local government borders in Norway, such as between European countries, is a topic that should be addressed in future research.

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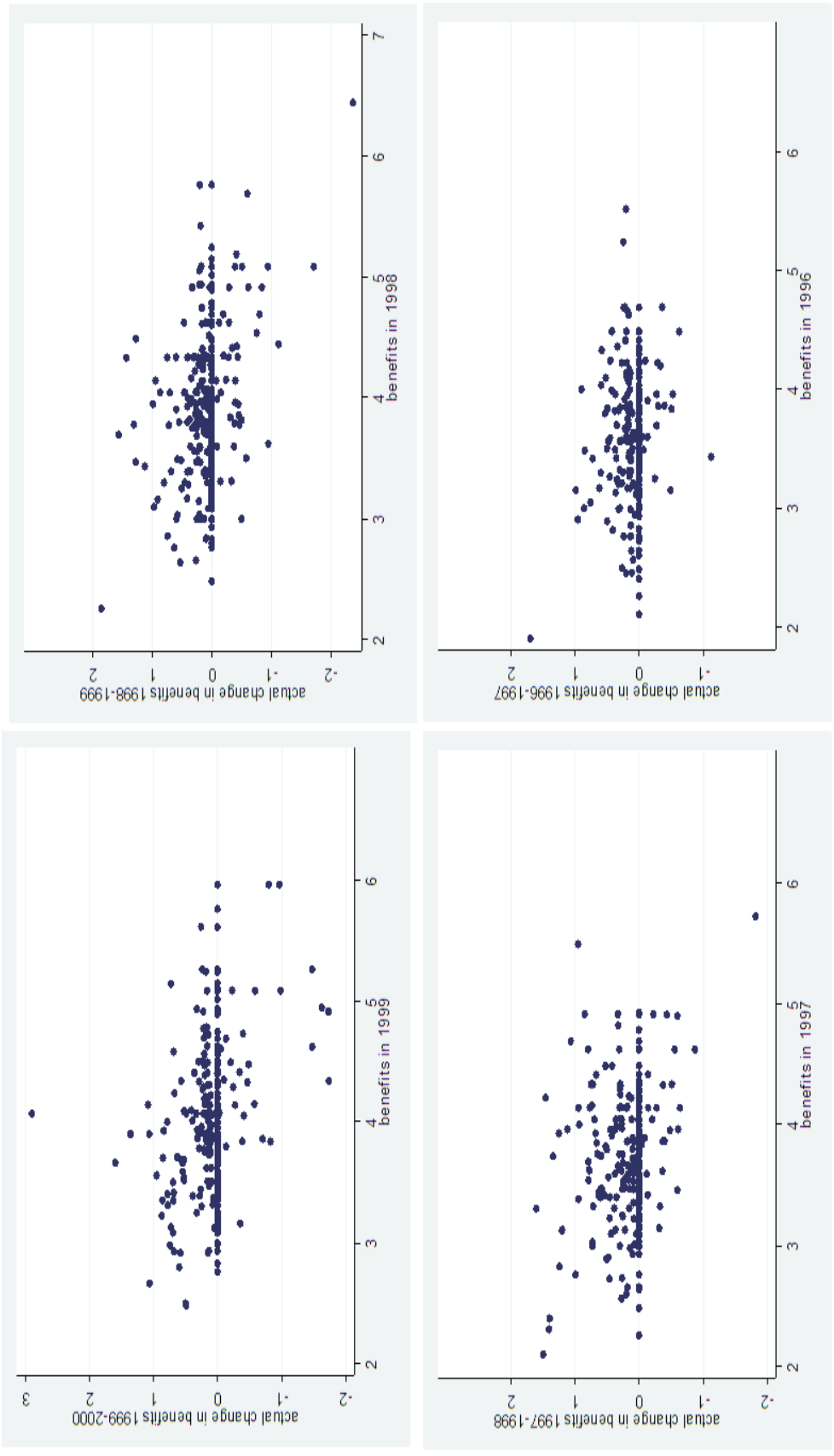

Figure A.1. Actual changes in welfare benefit levels from year t to year t+1 (vertical axis) against welfare benefit levels in year t (horizontal axis). Welfare benefit levels are measured in 1000 NOK

Table A.1. Descriptive statistics on welfare benefit levels across local governments. Welfare benefits are measured as the politically determined norm for a single-person 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
Proportion of local governments **increasing** the nominal politically determined norm from year t–1 to year t with more than NOK 600		12	9	52	22	25	21	20	5	5
Proportion of local governments **decreasing** the nominal politically determined norm from year t–1 to year t with more than NOK 600		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 instructive norm, 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 A.2. Descriptive statistics on net migration flows, 2001

	Mean	St. Dev.	Min	Max	Obs.
Net inflow of recipients	-2.4	11.4	-77.5	48.0	430
Net inflow of nonrecipients	-2.4	18.5	-93.5	64.7	430
Net inflow of recipients - Net inflow of nonrecipients	0.0	19.9	-65.6	74.8	430
Net inflow of recipients, from local governments within the same county	-0.7	7.0	-47.7	28.1	430
Net inflow of nonrecipients, from local governments within the same county	-0.7	11.6	-51.2	36.0	430
Net inflow of recipients - Net inflow of nonrecipients, from local governments within the same county	-0.1	13.3	-45.2	58.5	430

Note: All variables are scaled by population size in 10,000s.

Table A.3. Descriptive statistics on welfare recipient status and migration rates

	1996	1997	1998	1999	2000	2001	2002	2003	2004
Total number of individuals	352263	361371	371104	383470	393380	405658	412660	423126	431944
Total number of welfare recipients	42964	41101	38062	37378	38142	39240	39322	41595	42055
Total number of nonrecipients	309299	320270	333042	346092	355238	366418	373338	381531	389889
Proportion of welfare recipients	0.122	0.114	0.103	0.097	0.097	0.097	0.095	0.098	0.097
Proportion of welfare recipients receiving more than 3 months of support	0.630	0.618	0.607	0.594	0.594	0.595	0.601	0.603	0.621
Moving across local government lines									
Total number of people moving	20697	21215	22075	21813	22993	24838	23331	23962	24642
Total number of welfare recipients moving	4383	4053	3790	3951	4068	4552	4097	4363	4609
Total number of nonrecipients moving	16314	17162	18285	17862	18925	20286	19234	19599	20033
Migration rates									
Welfare recipient migration rates	0.102	0.099	0.100	0.106	0.107	0.116	0.104	0.105	0.110
Nonrecipient migration rates (control group)	0.053	0.054	0.055	0.052	0.053	0.055	0.052	0.051	0.051
Welfare recipient migration vs. Control group migration	0.049	0.045	0.045	0.054	0.053	0.061	0.053	0.054	0.058
Moving across local government lines within counties									
Total number of people moving	8048	8097	8379	8303	9009	9710	9160	9552	10110
Total number of welfare recipients moving	1747	1618	1522	1601	1645	1891	1644	1871	1984
Total number of nonrecipients moving	6301	6479	6857	6702	7364	7819	7516	7681	8126
Migration rates within counties									
Welfare recipient migration rates	0.041	0.039	0.040	0.043	0.043	0.048	0.042	0.045	0.047
Nonrecipient migration rates (control group)	0.020	0.020	0.021	0.019	0.021	0.021	0.020	0.020	0.021
Welfare recipient migration vs. Control group migration	0.020	0.019	0.019	0.023	0.022	0.027	0.022	0.025	0.026

Table A.4. Year-by-year estimations. The dependent variable is (Δnet inflow of recipients$_{it}$ - Δnet inflow of nonrecipients$_{it}$)

	(1a)		(2a)		(3a)		(4a)		(5a)		(6a)		(7a)		(8a)		(9a)	
	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	-1.985	3.989	4.619	5.546	-8.004	4.195	11.643	5.717	2.612	2.580	8.910	3.210	-2.705	3.156	4.184	6.437	-0.989	4.723
Number of observations	430		430		430		430		430		430		430		430		430	
Time fixed effects	No		No		No		No		No		No		No		No		No	
Sample	1996		1997		1998		1999		2000		2001		2002		2003		2004	
	All		All		All		All		All		All		All		All		All	
Estimation method	OLS		OLS		OLS		OLS		OLS		OLS		OLS		OLS		OLS	

Note: A constant term is included in all regression. Standard errors are robust to unknown form of heteroscedasticity.

Table A.5. Year-by-year estimations, only within county migration. The dependent variable is (Δnet inflow of recipients$_{it}$ - Δnet inflow of nonrecipients$_{it}$)

	(10a)		(11a)		(12a)		(13a)		(14a)		(15a)		(16a)		(17a)		(18a)	
	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	0.117	2.716	1.641	3.345	-0.925	2.587	3.414	2.788	1.750	1.609	6.757	2.699	-2.309	2.217	-0.370	2.428	-3.820	3.451
Number of observations	430		430		430		430		430		430		430		430		430	
Time fixed effects	No		No		No		No		No		No		No		No		No	
Sample	1996		1997		1998		1999		2000		2001		2002		2003		2004	
	Within county		Within County		Within county		Within County		Within county		Within County		Within county		Within county		Within County	
Estimation method	OLS		OLS		OLS		OLS		OLS		OLS		OLS		OLS		OLS	

Note: A constant term is included in all regression. Standard errors are robust to unknown form of heteroscedasticity.