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Does welfare policy affect residential choices? An empirical investigation accounting for policy endogeneity ☆

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ABSTRACT

This paper studies how changes in welfare policy 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 are also likely to affect welfare policy. This paper utilizes a policy reform to address this policy endogeneity. The results show that welfare policy exerts a substantial effect on residential choices of welfare recipients.

1. Introduction

Do individuals relocate in response to changes in welfare policy? And do policymakers react to the competitive pressure by holding welfare benefits below what they otherwise would have been? I argue that to obtain a reliable estimate of the extent of welfare migration, one should take into account policymakers' responses. The two related phenomena, 'welfare migration' and 'welfare competition', should be jointly considered.

In this paper I take advantage of a policy reform to address the policy endogeneity problem. More specifically, I suggest that the introduction of a national welfare benefit norm creates variation in changes in welfare policy across Norwegian local governments that can be treated as exogenous. The empirical analysis indicates that a one standard deviation increase in the welfare benefit level increases the welfare participation rate in the population under study with about one standard deviation. The estimated 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.

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The validity of the identification strategy pursued in the current analysis hinges on the assumption that the lagged level of benefits, which are used as instruments for the adjustment to the national instructive welfare benefit norm, are correctly excluded from the second stage difference-in-differences equation. If this assumption is valid, then my strategy eliminates biases due to unobserved local characteristics, not netted out by the comparison group, and policy endogeneity. To investigate the identifying assumption I conduct several sensitivity checks. They all suggest that the identifying assumption is satisfied.

The early studies on the welfare migration hypothesis provide mixed results. These studies are however plagued by several methodological problems (discussed in detail by Meyer, 2000). Recent studies apply more sophisticated identification strategies and confirm the existence of welfare migration (Bailey, 2005; Gelbach, 2004; McKinnish, 2005, 2007). Bailey stresses that many earlier studies risk distorting the effect of welfare on migration decisions by inadequately accounting for attributes of the jurisdictions that affect migration. He applies an estimation strategy based on state fixed effects and a comparison group to address this potential omitted variable problem. Gelbach deals with this issue differently, comparing low-skilled single mothers with young children to low-skilled single mothers with older children. A stronger welfare migration effect is expected for the former group, as they have a longer period of welfare benefit eligibility. McKinnish introduces another convincing identification strategy based on comparison of individuals in border areas of states to individuals in non-border areas of states.

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 jurisdictions will affect the jurisdiction's own benefit choice. Results consistent with the welfare competition hypothesis is found in analysis from the US (e.g. Figlio et al., 1999; Saavedra 2000), Norway (Fiva and Rattsø, 2006), Sweden (Dahlberg and Edmark, 2008) and the UK (Revelli, 2006).[1] Jurisdictions seem indeed to be playing a 'welfare game', suggesting that studies of welfare migration should pay close attention to policy endogeneity.[2] Surprisingly, the existing literature does not follow this recommendation. Most existing studies rely purely on observed variation in welfare policy to identify welfare migration effects.

While 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. It is reasonable to expect larger welfare migration effect in the Norwegian setting in comparison to the US setting for two reasons. First, in the US, welfare benefits are constrained to women with dependent children. In Norway, also groups that are much more geographically mobile, such as single men without dependent children, are eligible for benefits. Second, the Norwegian local governments responsible for welfare benefit provision are much smaller geographic units than the US states.[3]

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. The main results follow in Section 4, and Section 5 discusses and investigates the validity of the instrumental variable approach adopted. Section 6 provides a concluding discussion.

2. Institutional setting 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.

The local governments, responsible for welfare benefits, are democratic institutions led by an elected local council. Their main responsibilities are care for the elderly, preschool, primary and lower secondary education. While spending is decentralized, the financing is strongly centralized. The local governments obtain around 90% of their total revenues from grants from the central government and regulated income tax sharing. The remainder stems from fees from infrastructure services, which are limited by law to cover production costs, and property taxation. Grants are primarily distributed as block grants and are based on objective criteria. The grant system has elements of both spending and tax equalization. Spending equalization is based on population characteristics (in particular, age composition) and local cost factors. The comprehensive tax equalization scheme attempts at raising the revenues of local governments with weak tax bases (Rattsø, 2003).

There is no explicit spending equalization (or matching grants) based on the generosity of the local welfare policy or the number of welfare recipients. In principle, the local governments face the full marginal cost of one welfare recipient migrating to the jurisdiction. However, because socio-economic background characteristics of the population, such as unemployment, marital and (non-western) immigrant status, are criteria in the spending equalization system, there may be some element of compensation implicitly.

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

[1] 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 UK to address this issue. The results suggest that the spatial pattern observed in welfare policy is at least partially driven by yardstick competition.

[2] Besley and Case (2000) provide a general treatment and discussion of bias due to policy endogeneity.

[3] The median Norwegian local government is about 465 km^2, whereas the continental US states, responsible for welfare benefit provision, range from around 4000 km^2 (Rhode Island) to almost 700,000 km^2 (Texas).

Table 1
Descriptive statistics on welfare benefit levels across local governments

	1995	1996	1997	1998	1999	2000	2001
Mean	3620	3710	3808	3969	4044	4119	4119
Mean in constant 1995 NOK	3620	3667	3668	3739	3724	3678	3570
Standard deviation	524	525	556	605	613	624	543
Coefficient of variation	0.14	0.14	0.15	0.15	0.15	0.15	0.13
Minimum	1900	1900	2102	2258	2484	2600	2760
Median	3660	3697	3800	3935	4005	4068	3950
Maximum	5281	5520	5722	6441	5964	6969	7291
National instructive norm, in NOK							3880
Number of local governments above instructive norm						265 *	220
Number of local governments at instructive norm						0 *	119
Number of local governments below instructive norm						165 *	91
Observations	430	430	430	430	430	430	430

Notes: Welfare benefits are measured as the politically determined norm for single-person households without children, per month in nominal NOK (unless otherwise noted).
* Relative to the norm announced in 2001.

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. The current analysis uses data from the period 1995 to 2001.[4] The politically determined norm varies considerably across local governments, as illustrated in Table 1. In 2001, the average welfare benefit norm to a single-person household was NOK 4119 (USD 650) per month, ranging from NOK 2760 to NOK 7291.

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 well explained by differences along other particular dimensions, such as differences between rural and urban local governments (Fiva and Rattsø, 2006).

2.2. Migration rates

Data on received social assistance are available for the entire adult Norwegian population (4.5 million). 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. This sample is further divided into two groups, welfare recipients and a comparison group of non-recipients.[5] The welfare participation rate is particularly high for this type of households. Approximately 10% of single men aged 16 to 66 without dependent children receive welfare benefits within a given year, in comparison to approximately 3% in the general population.[6]

Although the basis for the analysis is micro data, I rely on net migration flows across local government lines in the econometric analysis. The key variables measure net inflow of individuals of type j [j=recipients (r), nonrecipients (n)] to local government i in year t, denoted M^j_{it}. Descriptive statistics are provided in Table 2. M^j_{it} are measured from January 1st in year t to January 1st in year $t+1$ and scaled by local government population size in 10,000s (at beginning of year t). An individual 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.[7]

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. Above 50% of the welfare recipients that receive welfare benefits in year t also received welfare benefits in year $t-1$ and year $t+1$.

Around 10% of the welfare recipient population moved across local government lines from one year to the next compared to around 5% of the comparison group of nonrecipients (see Appendix Table 1). In comparison to the migration rates reported in US

[4] A total of 435 Norwegian local governments existed between 1995 and 2001. Due to a few missing variables and local government mergers I analyze a balanced panel of 430 local governments here.
[5] 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 comparison groups.
[6] According to official statistics from Statistics Norway, 127,914 individuals received welfare benefits in 2001 in total. This implies that the sub-population under study constituted 31% of all welfare recipients. The remaining 69% consist of: single men younger than 16 or older than 66 (8%), single women (22%), single men or women with dependent children (16%), couples without children (12%) and couples with children (10%).
[7] There are 18 counties in Norway in addition to the capital Oslo, which is both a county and a local government. Thus, for the within county estimations, presented below, I analyze a balanced sample of 429 local governments.

Table 2
Descriptive statistics

	Mean	St. dev.	Min	Max
Local government population: total	10450	29838	335	508726
Local government population: single men 16–66, without dep. children	941	4282	27	81788
Relative size of sub-population	0.073	0.018	0.036	0.161
Welfare participation	0.096	0.035	0.011	0.222
b, welfare benefits in NOK 1000	4.119	0.543	2.760	7.291
Δb	0.001	0.375	−2.163	1.280
Net migration flows, all moves				
M_{it}^r (net inflow of recipients)	0.00	11.38	−75.08	50.46
M_{it}^n, (net inflow of nonrecipients) unstandardized	0.00	18.49	−90.97	67.14
M_{it}^n (net inflow of nonrecipients)	0.00	11.38	−55.98	41.31
ΔM_{it}^r (net inflow of recipients)	−1.36	13.11	−58.89	69.29
ΔM_{it}^n (net inflow of nonrecipients)	−1.30	15.19	−75.06	66.20
$\Delta M_{it}^r - \Delta M_{it}^n$	−0.06	19.50	−109.12	76.94
Net migration flows, within county moves				
M_{it}^r (net inflow of recipients)	0.00	6.96	−46.96	28.89
M_{it}^n, (net inflow of nonrecipients) unstandardized	0.00	11.63	−50.52	36.71
M_{it}^n (net inflow of nonrecipients)	0.00	6.96	−30.23	21.97
ΔM_{it}^r (net inflow of recipients)	−0.71	8.80	−42.92	29.18
ΔM_{it}^n (net inflow of nonrecipients)	−0.47	9.44	−45.87	48.61
$\Delta M_{it}^r - \Delta M_{it}^n$	−0.24	12.85	−57.62	54.87

Notes: All migration variables are scaled by population size in 10,000s. Data are from 2001, 430 observations. Migration flows are computed based on all 435 local governments. The distribution of all M_{it}^j is adjusted such that the mean is zero.

studies, these migration rates are large. Only around 5% of the main treatment group members in the Gelbach (2004) and McKinnish (2007) studies (never-married high school dropouts with children) moved across state lines during a five-year period.

3. Empirical strategy

3.1. The welfare migration mechanism

Economic models typically assume migration decisions to be the result of a rational cost-benefit analysis where individuals choose to reside in the jurisdiction that maximizes utility. In the welfare migration literature, utility is typically assumed to be a function of individual characteristics, jurisdiction characteristics, including welfare benefits, and migration costs (both monetary and nonmonetary costs) (e.g. Blank, 1988; Walker, 1994; Enchautegui, 1997; O'Keefe, 2004). To fix ideas, consider the simple exposition of welfare migration provided by Walker (1994). In this model, individuals care about private income and a location-specific amenity. Individuals are homogenous, but differ in their valuation of the amenity. Each period individuals are 'born' and 'die'. Some individuals will be 'born' in their preferred region, while others must move to their preferred region. If one jurisdiction increases its welfare benefits levels, and everything else is held constant, welfare induced migration will occur. This effect can be decomposed into an attractive and a retentive force (Walker, 1994). And the welfare migration hypothesis can be stated as: do local governments that offer higher welfare benefits attract people who would not otherwise move there and retain people who might otherwise have chosen to leave?

3.2. The difference-in-differences estimator

A simple test of the welfare migration hypothesis would be to investigate whether there is a positive association between the welfare benefit level in local government i at time t (b_{it}) and net inflows of welfare recipients (M_{it}^r). However, since individuals have many non-welfare related motives for migrating, one should not rely on welfare benefits as the sole determinant of M_{it}^r. The standard approach in the empirical welfare migration literature has been to rely on a comparison group of nonrecipients to avoid omitted variable bias. This requires a comparison group that doesn't respond to welfare benefits, but does respond to other unobserved local characteristics in the same way as the welfare-responsive ('treatment') group. More formally, let net inflows of recipients (r) and nonrecipients (n) to local government i in year t be determined by:

$$M_{it}^r = \delta_i^r + \theta_{it} + \beta b_{it} + \varepsilon_{it}^r \tag{1}$$

$$M_{it}^n = \delta_i^n + \theta_{it} + \varepsilon_{it}^n \tag{2}$$

where β is the coefficient of interest, capturing the extent of welfare migration. The underlying relationship between net migration flows and changes in welfare policy is assumed to be linear, β consequently captures the average partial effect. δ_i^j captures time

invariant characteristics that affects migration flows of households of type j. First-differencing will remove these effects. θ_{it} captures time varying, potentially unobserved characteristics, assumed to affect migration flows of both types of household similarly. Given this, the welfare migration effect can be estimated utilizing a difference-in-differences framework. The key specification of interest, where Δ denotes time differences and u is an error term, is given by:[8]

$$\left(\Delta M_{it}^{r} - \Delta M_{it}^{n}\right) = \beta \Delta b_{it} + u_{it} \tag{3}$$

To avoid that an estimate of β would suffer from omitted variable bias one has to assume that changes in migration rates of comparison group members is an appropriate counterfactual for changes in the migration rates among welfare recipients in the absence of changes in welfare benefits. Note that an exogenous policy shock can be used to correct for omitted variable bias, not corrected for by the difference-in-differences approach, as long as the shock is uncorrelated with local unobserved characteristics (more on this below).

In the empirical implementation, M_{it}^{j} are measured as net inflows from January 1st in year t to January 1st in year $t+1$, scaled by local government population size in 10,000s. Before differencing, M_{it}^{n} is standardized to have the same standard deviation as M_{it}^{r}. Δb_{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. β should be interpreted as how many welfare recipients a local government with a population size of 10,000 receives if it increases welfare benefits with NOK 1000. General population growth do not influence the dependent variable, as only households that fulfill the inclusion criteria in both year t and year $t+1$ are included in the sample.

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. If some individuals that do not receive welfare payments in low-benefit states, would if they were in a high-benefit state, conditioning on welfare receipt in year $t+1$ is likely to exaggerate the welfare migration effect. 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).[9]

3.3. Policy endogeneity

An extensive theoretical literature discusses local level determinants of welfare policy. Most authors treat welfare policy as a public good, where the median voter, assumed to be nonpoor, cares about his own income and also altruistically, about the income of the poor (e.g. Orr, 1976; Brown and Oates, 1987). When the cost of redistribution is taken into account, poor households living in localities where they are a small fraction of the population is expected to receive higher welfare benefits, than in localities where they are a large fraction of the population (Orr, 1976). Brown and Oates (1987) extend this understanding to include a migration function explicitly, which shows the elasticity of the number of poor with respect to the welfare benefit level. It follows, that the greater the potential or expected extent of welfare migration, the lower will the jurisdictions' level of support be. This model is further developed by Wildasin (1991).

If welfare-induced migration is a concern for policy makers, then Δb_{it} is endogenous to the left-hand side variable in Eq. (3). If policy makers respond to an increasing welfare population by reducing welfare benefit levels, this results in a negative bias in β when estimating Eq. (3) with standard OLS ($Cov(\Delta b_{it}, u_{it}) < 0$), i.e. if residential choices are endogenous, cross-sectional variation in welfare benefits is as well (Moffitt, 1992). Dahlberg and Edmark (2008) investigate empirically Swedish politicians' response to an exogenous increase in the welfare population. They find that policy makers respond to increases in the welfare population by reducing welfare benefit levels.

The policy endogeneity problem is not properly addressed in the existing literature. While most studies simply ignore it, Peterson and Rom (1989) and Berry et al. (2003) aim to break the simultaneity problem by estimating the mutual effects of welfare benefits and poverty rates, but the exclusion restrictions that they impose to obtain identification are questionable.[10] 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).

To address the policy endogeneity problem, I take advantage of a policy shock, namely the introduction of a national instructive welfare benefit norm. The instructive norm, introduced in February 2001 by the central government, 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

[8] I also include a constant term when estimating Eq. (3).

[9] Conditioning on welfare receipt in period t is also likely to exhibit a negative bias if individuals who are not on welfare in period t (and are consequently assigned to the comparison group) migrate to other local governments to receive welfare benefits. I have experimented with conditioning on welfare receipt in year $t+1$, and the results are similar. If endogenous welfare participation is important, then conditioning on welfare receipt in year t or $t+1$ is expected to give different estimates. When this does not seem to be the case, it suggests that endogenous welfare participation is unlikely to be a problem in the current setting.

[10] 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. Shroder (1995) also applies a simultaneous equation framework, based on recipiency rates rather than poverty rates, but with similar questionable identification strategy as Peterson and Rom (1989) and Berry et al. (2003). Furthermore, Shroder let neighboring state's welfare benefit levels enter exogenously in the recipiency ratio equation, and exclude it from the welfare benefit equation, an approach which seems problematic.

Table 3
First-stage regression

	1	
	Coeff.	St. error
Constant	0.719**	0.347
$Below_{2000}$	1.192**	0.501
b_{2000}	−0.189**	0.081
b_{2000}* $Below_{2000}$	−0.296**	0.129
R^2	0.275	
Number of observations	430	
Year	2001	
Estimation method	OLS	

Notes: The dependent variable is Δb. Standard errors are robust to unknown forms of heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

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 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.[11] The share of local governments implementing the national standard increased further in 2002 to 2004. As the policy reform was implemented at one point in time, this identification strategy can only be applied at the first differenced cross-section for 2001. First differenced cross-sections prior to 2001 can be used to discuss possible biases.

A simple OLS regression on the first differenced 2001 cross-section may produce biased results since local governments chose whether, and how, to respond to the national guidelines. Local governments that adjusted their welfare policy in response to the national policy recommendation may be systematically different, also with respect to the perceived threat of welfare migration, from local governments that did not adjust their welfare policy. To address this problem I rely on a two-stage least squares (2SLS) approach. To isolate exogenous variation created by the national guidelines I rely on information on local governments' welfare policy existing prior to 2001. The first-stage regression is given by:

$$\Delta b_{i,2001} = \alpha_0 + \alpha_1 \text{below}_{i,2000} + \alpha_2 b_{i,2000} + \alpha_3 \text{below}_{i,2000} * b_{i,2000} + v_{i,2001} \quad (4)$$

where $below_{i,2000}$ is a dummy variable turned on if the local government had welfare benefits in 2000 below the national instructive norm given in 2001. The interaction term ($below_{i,2000}*b_{i,2000}$) 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 two assumptions. First, the instruments must be relevant, i.e. $\alpha_1 \neq 0$ and/or $\alpha_2 \neq 0$ and/or $\alpha_3 \neq 0$. Second, the instruments must be excludable from Eq. (3), i.e. $\text{Cov}(below_{i,2000}, u_{i,2001}) = \text{Cov}(b_{i,2000}, u_{i,2001}) = \text{Cov}(below_{i,2000}*b_{i,2000}, u_{i,2001}) = 0$. This implies that local governments' welfare benefit levels in 2000 only affect $(\Delta M_{it}^r - \Delta M_{it}^n)$ through $\Delta b_{i,2001}$. The exclusion restriction is extensively discussed and empirically investigated in Section 5.

4. Results

The results from the first-stage regression are reported in Table 3. The first-stage *F*-statistic for the joint null hypothesis of zero coefficients on all excluded instruments shows that the instruments are relevant (with an *F*-statistic of 51.84). As expected, local governments with welfare benefits below the central guidelines for 2001 are predicted to have increased their welfare benefits from 2000 to 2001, while local governments initially above the central guidelines were predicted to reduce their welfare benefits. 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. The fitted values from the first-stage are graphically illustrated in Fig. 1.[12]

The main results of this paper are presented in Table 4. The second-stage regression establishes economically important welfare migration effects, evaluating overall migration flows (specification 1) and short distance migration flows, defined as migrations across local government lines within counties (specification 2).

A NOK 500 increase in welfare benefits is estimated to a net inflow of around 4 welfare recipients (corresponding to about one standard deviation increase in the welfare participation rate) from the population under study the following year for an average

[11] In a survey conducted in August 2001, 104 out of 336 local governments (A total of 98 local governments did not respond) 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.

[12] For comparison, Appendix Fig. 1 provides corresponding scatter plots also for other years.

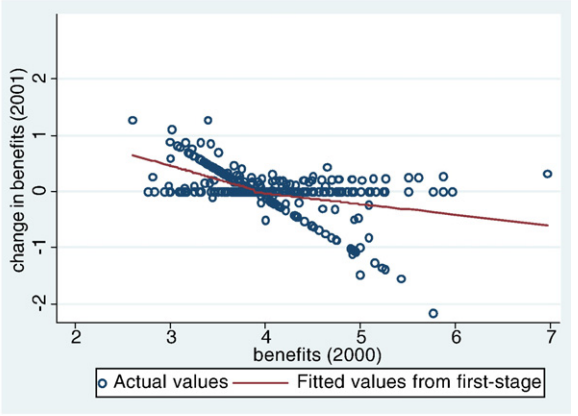


Fig. 1. Changes in welfare benefit levels against lagged welfare benefit levels.

local government. Even though long distance migrations (across county lines) constitute around 60% of all migrations, I find welfare migration effects of a similar magnitude when evaluating all migration flows and only within county migration flows. This suggests that most, if not the entire, welfare migration effect is driven by welfare recipients migrating between jurisdictions that are geographically close. The estimates are not very precise. The within county estimate is however statistically significant at the 1% level. The general estimate is only statistically significant at the 10% level.

The estimated welfare migration effect is large, in particular when one takes into account that this is not necessarily the full long term effect, but only the short term year-to-year effect. To understand the quick mobility response, it is important to keep in mind that the welfare recipient population under study is a very mobile group. They do not have children and typically have weak attachment to the labor market. In a given year, around 10% move across local government lines. It is reasonable to expect this group to react fast when incentives to migrate change.

In Table 5 I report OLS (first differenced) cross-section regressions for each year 1996 to 2001 based on short distance migration flows. 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. One possible

Table 4
The effect of welfare generosity on migration flows

	1		2	
	Coeff.	St. error	Coeff.	St. error
Δb	7.02*	4.01	7.96***	3.07
Number of observations	430		429	
Year	2001		2001	
Moves	All		Within county	
F statistic from first-stage	51.84		52.10	
Estimation method	2SLS		2SLS	

Notes: The dependent variable is $(\Delta M_{it}^{r}-\Delta M_{it}^{n})$. A constant term is included in all regressions. Standard errors are robust to unknown forms of heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5
The effect of welfare generosity on migration flows, all years, OLS estimates

	1		2		3		4		5		6	
	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error
Δb	4.82***	1.85	1.19	1.19	2.44	1.99	−0.42	1.89	1.93	2.29	−0.18	2.08
Number of observations	429		429		429		429		429		429	
Year	2001		2000		1999		1998		1997		1996	
Moves	Within county		Within county		Within county		Within county		Within county		Within county	
Estimation method	OLS		OLS		OLS		OLS		OLS		OLS	

Notes: The dependent variable is $(\Delta M_{it}^{r}-\Delta M_{it}^{n})$. A constant term is included in all regression. Standard errors are robust to unknown form of heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

interpretation of this finding is that the bias in the OLS estimates is small 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.[13] In other cross-sections the OLS estimates seem to be downward biased. These estimates may partially capture that when the welfare population is increasing, policy makers will be inclined to reduce benefit levels. Another interpretation is that the policy reform corrects for omitted variable bias, not eliminated by the difference-in-differences approach.

An increase in welfare benefits potentially 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. Reductions in welfare benefits will according to the welfare migration hypothesis work oppositely. The inflow and outflow effects may not necessarily be symmetric and may manifest over different time spans, I have however not found support of an asymmetric effect in the data.

5. Sensitivity analysis

A proper instrumental variable strategy requires that the instruments must be both relevant and excludable from the second-stage regression (i.e. exogenous). The *F*-test of the excluded instruments, show that the instruments clearly are relevant. To examine whether the instruments are correctly excluded from the second stage, I conduct four sensitivity checks: (i) adding covariates to Eq. (3), (ii) looking closer around the discontinuity of the recommended policy, (iii) running placebo 2SLS regressions in years without any release of national guidelines, and (iv) applying alternative first-stage specifications. Since the main welfare migration effect seems to be driven by short distance, within county, migrations, I conduct the sensitivity analysis based on within county migration flows.

5.1. Control variables

The central idea in the difference-in-differences estimator is that, except for welfare migration incentives, comparison and treatment group members face the same migration incentives. However, one may be concerned that unobserved time varying local attributes, such as changes in local labor market conditions, do not necessarily influence treatment and comparison groups in the same way.

Welfare recipients and non-recipients may respond differently to changing local labor market conditions. One may even question to what extent they operate in the same labor markets (i.e. high vs. low-skilled labor). This would however, not bias the estimates presented above, as long as the exclusion restriction from the 2SLS approach holds. The identification strategy applied in this paper effectively correct for both omitted variable bias, due to different migration incentives for treatment and comparison group members, and policy endogeneity, given that the policy shock is truly exogenous.

However, if some variable are changing over time at a different rate in low benefit jurisdictions than in high benefit jurisdictions this would be problematic for the 2SLS approach, based on the lagged levels of welfare benefits. In particular, one may worry that changing local economic conditions are correlated with welfare benefit levels and also has a direct differential impact on the treatment and comparison group. To address this concern I include variables capturing changes in local labor market conditions, namely $\Delta unemployment$ and $\Delta wage$, for three different education groups (lower secondary, upper secondary and higher education). All labor market variables vary only at the labor market regional level, and both sets of variables are computed for the population of individuals living in the region in both year t and year $t-1$.[14] The labor market regions, 90 in total, are defined by Statistics Norway on the basis of information about commuting flows across municipal borders.

In addition to controls for local labor market conditions I include variables capturing changes in the supply of educational services (manyears of people working in upper secondary and higher education, per 10,000 inhabitants), changes in publicly provided primary health care (manyears of physicians per 10,000 inhabitants) and publicly provided care for the elderly (number of employees working with care for the elderly per 10,000 inhabitants). One may worry that these variables affect the welfare recipient and comparison group differently. Since individuals included in this analysis do not have children, they are unlikely to base their residential choices on changes in the quality of two of the other main services local governments provide namely child care and compulsory schooling.

Table 6 presents the results from specifications including covariates.[15] Covariates capturing labor market conditions are included in specification 1 and 2. In specification 3, I control for changes in the supply of educational services. In specification 4, I include controls for the provision of health care. In specification 5, I include all variables simultaneously. Finally, in specification 6 and 7 I include labor market region fixed effects, where inference only comes from within labor market region variation in changes in welfare policy. The welfare migration effect is very stable across all these specifications, and statistically significant at the 5% level, or higher, in all specifications.[16]

[13] The OLS point estimate for 2001 is smaller in magnitude than the 2SLS point estimate, but the difference is not statistically significant.

[14] These data were kindly provided by Fredrik Carlsen.

[15] The first-stage regressions are very similar to the one reported in Table 2, and is therefore not reported. Table 6 does however show *F*-tests for the excluded instruments from the first-stage.

[16] Changes in housing costs is another potentially relevant control variable, but such data are not available at the local government level on a yearly basis. However, the specification with region fixed effects will handle this potential problem as long as changes in housing costs are highly correlated within labor market regions, an assumption which seems plausible.

Table 6
The effect of welfare generosity on migration flows when control variables are included

	1		2		3		4		5		6		7	
	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error
Δb	7.52**	3.07	8.10***	3.03	7.65**	3.05	8.33***	3.20	7.61**	3.14	8.62***	3.27	8.52***	3.31
ΔUnemployment (lower secondary)	0.16	0.95							0.17	0.99				
ΔUnemployment (upper secondary)	2.45	1.56							2.61	1.65				
ΔUnemployment (higher education)	0.86	1.73							1.17	1.84				
ΔWage (lower secondary)			44.74	100.20					58.10	111.96				
ΔWage (upper secondary)			−32.04	158.71					−6.28	164.85				
ΔWage (higher education)			0.40	128.11					−0.97	127.25				
Δmanyears (upper secondary)					−0.02	0.02			−0.01	0.02			−0.01	0.02
Δmanyears (higher education)					−0.04	0.05			−0.04	0.06			−0.04	0.05
Δmanyears (physician)							0.67**	0.28	0.62**	0.29			0.54*	0.33
Δmanyears (care)							−0.01	0.04	−0.01	0.04			0.00	0.04
Regional fixed effects.		NO		NO		NO		NO		NO		YES		YES
Number of observations		429		429		424		429		424		429		424
Year		2001		2001		2001		2001		2001		2001		2001
Moves		Within county		Within county		Within county		Within county		Within county		Within county		Within county
F statistic from first-stage		51.27		50.52		51.82		51.59		48.65		31.66		31.88
Estimation method		2SLS		2SLS		2SLS		2SLS		2SLS		2SLS		2SLS

Notes: The dependent variable is $(\Delta M_{it}^{r} - \Delta M_{it}^{n})$. A constant term is included in all regressions. Standard errors are robust to unknown forms of heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

5.2. Looking closer around the discontinuity

An alternative approach to investigate whether there may be some omitted variable that is changing over time and correlated with the welfare benefit level, is to investigate more similar jurisdictions in terms of welfare benefit levels. Two local governments with fairly similar benefit levels may experience quite different changes in welfare policy if they are above and below the nationally recommended standard. Clearly, there is a trade-off when cutting closer to the discontinuity. One may obtain a sample of more comparable local governments, but it comes at the cost of less variation in the data.

In Table 7, the result from this exercise is reported. For comparison, the benchmark results from Table 4 are reproduced in specification 1. Specifications 2 to 4 cut closer to the discontinuity, by leaving out local governments with initial welfare benefit levels more than NOK 1000, NOK 750 and NOK 500 from the national guidelines, respectively. Clearly, the results are robust to leaving out welfare benefit levels that deviate considerable from the national policy recommendation to be launched the next year.

Table 7
Welfare migration estimates when cutting the sample closer to the discontinuity

	1		2		3		4	
	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error
Δb	7.96***	3.07	8.21**	3.97	10.63**	4.87	10.05	6.57
Number of observations	429		370		326		248	
Cut off	No cutoff		±1 k		±0.75 k		±0.5 k	
Year	2001		2001		2001		2001	
Moves	Within county		Within county		Within county		Within county	
F statistic from first-stage	52.10		46.07		41.94		27.41	
Estimation method	2SLS		2SLS		2SLS		2SLS	

Notes: The dependent variable is $(\Delta M_{it}^{r} - \Delta M_{it}^{n})$, A constant term is included in all regressions. Standard errors are robust to unknown forms of heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8
Placebo welfare migration estimates from years without any release of national guidelines

	1		2		3		4		5	
	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error	Coeff.	St. error
Δb	−2.66	5.58	5.34	7.28	11.17	9.17	5.49	14.31	2.02	10.01
Number of observations	429		429		429		429		429	
Year	2000		1999		1998		1997		1996	
Moves	Within county		Within county		Within county		Within county		Within county	
F statistic from first-stage	9.54		4.09		3.51		2.42		3.50	
Estimation method	2SLS		2SLS		2SLS		2SLS		2SLS	

Notes: The dependent variable is $(\Delta M_{it}^{r} - \Delta M_{it}^{n})$. A constant term is included in all regressions. Standard errors are robust to unknown forms of heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9
The effect of welfare generosity on migration flows, alternative first-stage specification

	1		1	
	Coeff.	St. error	Coeff.	St. error
Δb	8.25**	3.31	7.23**	3.69
Number of observations	429		429	
Year	2001		2001	
Moves	Within county		Within county	
Instruments	b_{2000}		$below_{2000}$	
F statistic from first-stage	47.22		113.42	
Estimation method	2SLS		2SLS	

Notes: The dependent variable is $(\Delta M_{it}^{r} - \Delta M_{it}^{n})$. A constant term is included in all regressions. Standard errors are robust to unknown forms of heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

The point estimate is of a similar magnitude in specification 1 through 4, and statistically significant at the 5% level, in specification 2 and 3.[17] Cutting the sample even closer (e.g. ±NOK 250) the estimates become very imprecise.

5.3. Placebo 2SLS estimates

National guidelines on welfare policy were introduced in 2001. In previous years, local policymakers experienced no policy reform. Consequently, if I find similar results in 2SLS regressions based on artificial policy shocks, it would suggest that some omitted variable, trend or wrong functional form led to the results presented above. Table 8 presents results from such placebo 2SLS estimates. Comfortably, the placebo welfare migration estimates are never close to being statistically significant.

5.4. Alternative first-stage specification

As a final sensitivity analysis, I have checked the robustness of the baseline estimates to alternative first-stage specifications. Table 9 reports results when only b_{2000} or $below_{2000}$ are utilized separately as instruments. This yields a slightly lower fit in the first-stage regressions and the standard errors in the second stage consequently increase slightly, but the welfare migration effect is similar and statistically significant at the 5% level in both specifications.

6. Concluding discussion

The results presented in Sections 4 and 5 suggest that a one standard deviation increase in welfare benefit levels yields about one standard deviation increase in the welfare participation rate. Does this suggest that policymakers will react to the competitive pressure by holding welfare benefits below what they otherwise would have been? To answer this question I draw on Gelbach (2004). Gelbach calculates the extent of policy endogeneity in the US by modeling the game between states in setting benefit levels and then calculating the predicted decrease in benefit levels as a function of estimated welfare migration flows. Assuming that policy makers have log utilities and maximize a social welfare function subject to a budget constraint he argues that optimal state benefits in the US are probably not much affected by a concern for welfare migration. In the case of Norway, the results from the current analysis point to a different conclusion.

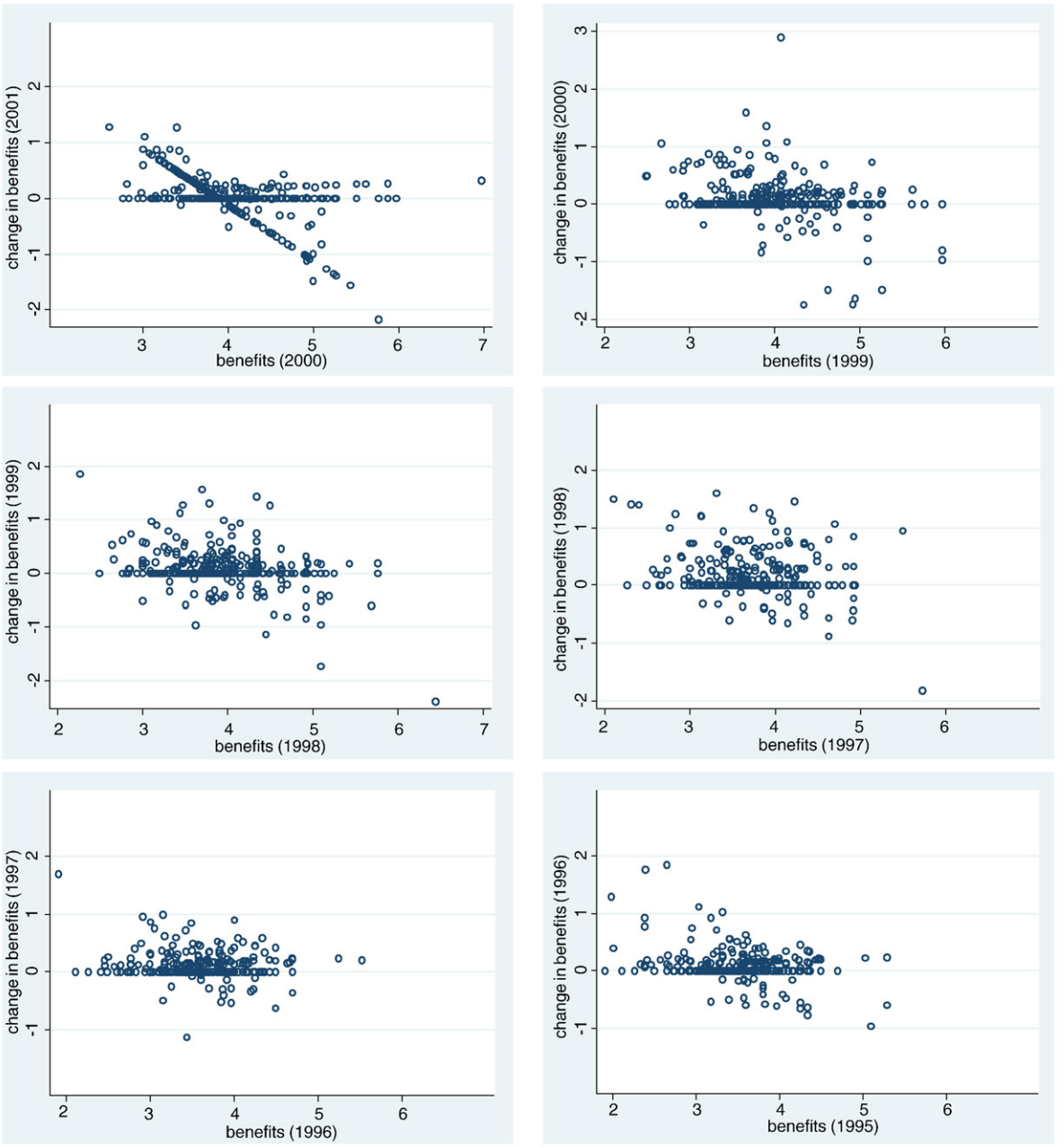
To see why policy makers in Norway are likely to worry about welfare magnetism, consider the following stylized example. An average local government considers increasing the welfare benefit levels from NOK 4000 to NOK 4500 for all types of welfare recipients. In the hypothetical no-mobility case, the cost of an increase in welfare benefit levels would simply be the increase in welfare benefits times the number of recipients living in the jurisdiction. However when welfare migration occurs, the cost of welfare generosity increases because the jurisdictions' welfare recipient population increases. Erring on the side of caution and assuming a zero welfare migration response of

[17] The point estimate does not change much if only local governments initially below (or initially above) the central guidelines are included. The fairly stable point estimates across all these specifications indicates that the assumption of linearity between net migration flows and changes in welfare policy is not unreasonable.

other welfare recipients, the cost of increasing the welfare benefit level is approximately 12% higher than it would be in the absence of welfare migration.[18] Assuming policy makers with log utilities this implies that optimal benefits would be about 12% higher in the absence of welfare induced migration. This suggests that policy makers are likely to worry about 'welfare magnetism', consistent with the strong strategic interaction in welfare policy established empirically in several other studies.

Appendix A

Appendix Fig. 1. Changes in welfare benefit levels against lagged welfare benefit levels, all years.



Note: Welfare benefit levels are measured in NOK 1000.

[18] Cost with constant population: (10 450*0.028)*NOK 500 = NOK 146 300. Additional welfare migration cost (four new welfare recipients from the subpopulation under study): 4* NOK 4500=NOK 18 000. The 95 percent confidence interval for the benchmark estimate is [3 percent, 21 percent]. If migration responses are equally strong for other groups (single women, lone parents, couples), the welfare migration cost would be about three times larger.

Appendix Table 1. Descriptive statistics on welfare recipient status and migration rates

	1996	1997	1998	1999	2000	2001
Total number of individuals	352,263	361,371	371,104	383,470	393,380	405,658
Proportion of welfare recipients (national average)	0.122	0.114	0.103	0.097	0.097	0.097
Migration rates (moving across local government lines, all moves)						
Welfare recipient migration rates	0.102	0.099	0.100	0.106	0.107	0.116
Nonrecipient migration rates	0.053	0.054	0.055	0.052	0.053	0.055
Migration rates (moving across local government lines, within county moves)						
Welfare recipient migration rates	0.041	0.039	0.040	0.043	0.043	0.048
Nonrecipient migration rates	0.020	0.020	0.021	0.019	0.021	0.021

Appendix Table 2. Descriptive statistics, control variables

	Mean	St. dev.
Labor market		
Δunemployment (lower secondary), percentage points	−0.34	1.24
Δunemployment (upper secondary), percentage points	−0.33	0.93
Δunemployment (higher education), percentage points	−0.09	0.53
Δwage (lower secondary), NOK 100,000	0.06	0.02
Δwage (upper secondary), NOK 100,000	0.06	0.03
Δwage (higher education), NOK 100,000	0.07	0.03
Educational opportunities		
Δmanyears (upper secondary), per 10,000 inhabitants	−0.75	12.73
Δmanyears (higher education), per 10,000 inhabitants	0.15	4.04
Medical care, care for the elderly		
Δmanyears (physician), per 10,000 inhabitants	0.14	2.30
Δmanyears (care), per 10,000 inhabitants	8.91	26.33

Note: *Δwage* are computed by running hedonic regressions on micro level data (all inhabitants 25 to 60 years, working over 30 h per week, omitting extreme observations). *Δunemployment* is based on the share of the workforce that is registered as unemployed within a given year.

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