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Do Incentives for Municipalities Reduce the Welfare Caseload? Evaluation of a Welfare Reform in the Netherlands

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Abstract Since 2004, most municipalities in the Netherlands receive lump sum payments from the state for the payment of social assistance allowances. As municipalities had no authority to change the eligibility rules for social assistance, the effects of the welfare reform are solely due to the efforts of municipalities to decrease the number of welfare recipients. Using variation in the timing of policy changes, this paper uses a difference-in-difference approach to assess the effectiveness of the incentive for municipalities. Based on individual panel data from administrative records, we show that the high-powered scheme led to a decline of the welfare caseload of 14% up till 2008. The reform has been most effective for those with the highest welfare

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dependency: single mothers and singles from non-western origin. In line with standard economic predictions, the reform does not give an incentive for cream skimming: the welfare caseload declined as well for easy to place recipients as for difficult to place recipients.

Keywords Decentralisation · Welfare caseload · Financial incentive · Principal agent

JEL Classification H53 · I38 · H75

1 Introduction

There is a substantial body of literature measuring the effects of welfare reforms on the number of welfare recipients. Most of them deal with the effects of reforms that stimulate welfare recipients to increase their search effort to find a job. These reforms typically include financial incentives for welfare recipients to find a job by reducing benefit levels and introducing sanctions for those recipients not searching sufficiently (Gautier and Van Der Klaauw 2012).

In contrast to this little is known on the effectiveness of financial incentives for the agencies that are responsible for the re-employment of those on welfare. Incentives in the public sector are usually low-powered or absent. The argument for this is that performance is often hard to measure and not directly attributable to the efforts of the agency (Heinrich and Marschke 2010; Burgess and Ratto 2003). There is a small body of literature on the effects of financial incentives for re-employment providers contracted by public agencies (Heckman et al. 2002; Koning and Heinrich 2013). As performance pay for providers usually involves a fixed amount bonus for each recipient finding work, it imposes incentives that may lead to cream-skimming practices. That is, these providers may concentrate on individuals with high a priori chances of work, rather than increasing their work opportunities.

That said, the use of financial incentives may be more adequate when they are imposed at the level of public agencies that have no opportunities for cream-skimming. In the current paper, we will study the effectiveness of such financial incentives for Dutch municipalities that are responsible for the provision of welfare benefits. In the Netherlands, a major policy reform took place in 2004, giving large municipalities a high-powered financial incentive to decrease the welfare caseload. With the introduction of the so-called 'Work and Social Assistance Act' in that year, matching grants for welfare expenditures were replaced by block grants. Before the reform, municipalities billed 90% of actual expenses on welfare to the federal government (i.e., a 90% matching grant); the rest had to be financed from a fixed budget based on historical welfare expenses (i.e. a 10% block grant). This provided municipalities virtually no incentive to reduce their welfare caseload. From 2004 on, municipalities received each year a fixed budget that did not vary with respect to the actual caseload in that year (a 100% block grant). For small municipalities, this fixed budget was based on historical welfare expenses, which provided them with minimal incentives to reduce welfare caseloads. For large municipalities however, a substantial part of the block grant no longer depended on their historical expenses. More specifically, the fraction

of lump-sum financing increased to 100% in 2006. Accordingly, large municipalities were fully rewarded with the present value of benefit savings due to work resumption of their unemployed citizens.

Using administrative data on about 900,000 welfare recipients between 1999 and 2008, we exploit the differential and gradual implementation of the new welfare law across municipalities. Our estimation approach can be characterized as a difference-in-difference model, with small municipalities with minor changes in financial incentives as 'control group' and larger municipalities as the 'treatment' group.

The estimation results show that the Dutch welfare reform proofs successful in reducing the number of welfare recipients. The replacement of a matching grant with a block grant that is independent of historical expenses reduced the welfare caseload with 14%. The effect is larger for groups with the largest probability of being on welfare, particularly single parents. The policy reform also reduced the probability of being on welfare for a year or longer, indicating that also hard-to-place recipients are targeted by municipalities. This confirms that cream-skimming did not occur.

Our impact estimates are somewhat lower than those for studies on a similar US welfare reform that took place in 1998. For these, Blank (2007) and Moffitt (2008) find impact estimates of about 20%. Before 1996, US states received a matching grant of 40% of welfare expenses from the federal government. The welfare reform replaced the matching grant by a block grant. The change in financial incentives was thus smaller than in the Netherlands, where a 90% matching grant was replaced by a block grant. However, at the same time nationwide eligibility rules were changed in 1996, including a time limit on the duration of allowances of 5 years and more severe work requirement rules. Moreover, the reform gave states the discretion to pose more severe eligibility rules and lower benefit levels than stipulated by nationwide regulation. This means that empirical studies on the 1996 reform essentially measured the combined impact of financial incentives and changes in benefit conditions. As Grogger (2004) argues, about 6% of the impact of the reform on the welfare caseload can already be explained by the impact of time limits for allowances.

With this in mind, this paper thus adds to the literature by estimating the isolated effect of the replacement of a matching grant by a block grant—and without the possibility for municipalities of changing eligibility rules or the benefit level. In addition, we address the issue of cream skimming in more detail. That is, we investigate whether changes in welfare dependency rates were confined to welfare caseloads in the first year of benefit receipt or whether they extend to long-term benefits as well?

This paper proceeds as follows. The next section describes the theoretical effects of financial incentives from a principal-agent perspective. Section 3 goes into the results of empirical research on effects of financial incentives to reduce the welfare caseload. Section 4 describes the institutional context of the welfare reform in the Netherlands. Sections 5 and 6 describe the data and the empirical results. Section 7 concludes.

2 Financial Incentives: Theoretical Considerations

In most western countries, local municipality offices administer welfare (Adema 2006). These offices can hire welfare-to-work providers to help recipients back to

work. In this context, financial incentives can be given to either providers, or to the local governments themselves. In both cases, a principal-agent problem arises where contracting involves a trade-off between risk and incentives (Gibbons 1996; Burgess and Ratto 2003). Taking welfare caseloads as the relevant measure of output in these contracts, output is not only influenced by the effort of the agent, but also by other factors that cannot be influenced by the agent. This is especially true in re-employment services, with business cycle conditions and unobserved characteristics of clients (like motivation) as key drivers of job finding rates. Financial incentives based on welfare caseloads thus impose a risk on agents.

In the context of this paper, performance pay usually takes the form of a linear contract with a fixed amount A and a bonus B for every client who finds a job:

$$W = A + By$$

where W is the income of the agent, y is the output of the agent, A is a fixed amount, independent of output, and B is the reward per output. In the case of welfare benefits, the principal (the government) wants to decrease the welfare caseload. Therefore output (y) is defined as the decrease of the welfare caseload.

In high powered incentive schemes, A is low or zero and B is high, which implies a high risk for the provider. As agents are usually risk averse, a high risk might induce the agent not to accept the scheme. On the other hand, if the agent gets paid a fixed amount ($B = 0$), independent of the number of recipients who get back to work, there is no incentive for the agent to reduce the welfare caseload and the principal bears all risks. The higher the risk and the more risk averse the agent is, the higher A has to be to induce agents to accept the scheme.

With a matching grant every allowance is paid by the principal, so there is no reward for a decrease of the welfare caseload ($B = 0$). Performance pay for providers of re-employment services usually involves a fixed amount bonus for each recipient finding work. In a "no cure no pay" contract $A = 0$ and B is a fixed amount. In a no cure less pay contract A is a positive amount. A block grant that is independent of expenses on welfare implies that the agent is rewarded for each recipient finding work with the present value of the benefits saved. In this case B is thus not a fixed amount, but differs with respect to the characteristics of the client. For hard-to-place clients the reward is higher than for easy-to-place clients. A block grant based on historical expenses gives a reward which will be close to zero. More specifically, the reward is equal to the net present value of the benefits saved in period t minus the present value of the benefits saved three periods later. Thus, as soon as the municipality stops making an effort, the financial gain in the first periods will be lost: expenses go up, while the budget increases three periods later. If municipalities anticipate this to occur, the incentive to reduce the welfare caseload will be close to zero. This is commonly referred to as the ratchet effect (Weitzman 1980).

Cream skimming may occur if the client group is heterogeneous in the probability of finding a job, while B is fixed. In that case, for those for whom the effort to resume work is smaller than B are likely to be served first. In addition, 'parking' occurs for those clients for whom the costs of getting them back to work is larger than B: they have an intake but no further activities take place, so the provider is rewarded with A.

With this in mind, theory predicts that efforts to place clients in a job as well as cream skimming is higher in high-powered incentive schemes (high B and low A), while parking occurs more often in low-powered incentive schemes (high A and low B). As a block grant does not reward the agent with a fixed amount, but with an amount which exactly matches future expenses, a block grant will not induce cream skimming, even though it is a high-powered scheme (A is low or zero).

3 Literature Review

Empirical literature on the effects of financial incentives for private re-employment providers, contracted by public agencies, focuses on intended and unintended effects of performance pay, like cream skimming and parking.

The empirical literature shows cream skimming and parking occurs, but not on a large scale (Heckman et al. 2002; Koning and Heinrich 2013). Koning and Heinrich (2013) evaluate the effects of high powered incentive schemes for private re-employment agencies contracted by public agencies in the Netherlands. They find that high powered schemes for ready to place clients have positive intended effects—the job-placement rate increased by three percentage point—and hardly any unintended effects. This is probably because these groups entail little risk for the provider, even in a high powered incentive scheme. However, cream skimming did occur in high powered incentives schemes for difficult to place clients with impairments and the intended effect of an overall higher job-placement rate did not occur. These clients implied considerable risk for the private agency, as costs to re-employ them are high and the probability of success low (Koning and Heinrich 2013). Aligning the payment scheme with the degree of risk the client group entails for the provider thus appears to be crucial for the overall success of the payment scheme.

An incentive on the level of the municipality in the form of a block grant does exactly this: aligning the payment scheme with the degree of risk the client group entails for the agent. Moreover, a block grant independent of expenses does this in an automatically optimal way: the agent (the municipality) is rewarded by the present value of benefit savings due to outflow. A likewise incentive might be given at the level of a provider contracted by the municipality (or any other public agent), but it will be more difficult to assign the right payment scheme per client group.

Several studies attempt to estimate the effect of the introduction of a 100% block grant as part of the welfare reform in the US in 1996. The reform entailed a nationwide change in eligibility requirements. The duration of benefit spells was maximized at 5 years. Moreover, more severe work requirements and sanctions for not complying with these rules were introduced (Blank 2007; Moffitt 2008). The welfare reform replaced a federal matching grant of 40% of expenses by a block grant and gave states more discretion about eligibility rules and the benefit level. This provided states with an incentive to cut expenses, either by decreasing the number of recipients or by decreasing the benefit level. Grogger and Karoly (2005) estimated a downward impact of the welfare reform on the welfare caseload of around 20%. The effect of the introduction of time limits alone already amounted to a decrease of 6–7% of the welfare caseload (Grogger 2004). This means the effect of a change in financial

incentives will be no more than 14% (20% minus 6%). Attempts to identify the effect of the replacement of the matching grant by a block grant try to isolate the change in incentives from the nationwide changes in eligibility rules (Chernick 1998; Ribar and Wilhelm 1999; Baicker 2005). Estimates vary widely depending on the estimation strategy. Baicker (2005) estimates the effect of a change from a matching grant to a block grant at a downward effect on welfare spending of 40%. However, it is not clear from the research of Baicker (2005), which part of this effect is due to a lower welfare caseload and which part is due to lower benefits levels.

4 Institutional Context

The Work and Social Assistance Act (WWB) grants a minimum income to anyone legally residing in the Netherlands who has insufficient means to support himself/herself and does not qualify for any other benefit (like an unemployment insurance benefit or a disability benefit). Welfare allowances are supplied at the level of households: individuals without income who live together with a partner with adequate income, or households with sufficient assets, are not eligible. The amount of welfare benefits is related to the statutory minimum wage. A family with two adults gets 100% of the net minimum wage (in 2014 €1350 per month), while single living persons receive 70% of minimum wage and single parents 90% of minimum wage. People who receive welfare are required to accept generally suitable jobs.

In 2014 about 4% of the population between 18 and 65 received a welfare allowance (Statistics Netherlands, www.cbs.nl). Welfare dependency is lower among native Dutch people than among migrants. Especially refugees show a high welfare dependency, for some groups even amounting up to 70%. In addition, single parents also have a high share of welfare recipients, amounting to 22% in 2008. Although people on welfare have to search for a job, municipalities are free to exempt single parents with children under 5 years of age from this obligation.

The Dutch Ministry of Social affairs and Employment determines the eligibility rules and the levels of welfare benefits. Municipalities are responsible for the administration of the Work and Social Assistance Act. Municipalities may influence the number of persons on welfare by deterring inflow and stimulating outflow by active labour market policies. Municipalities may deter inflow by adopting work first policies or use so-called search periods. If individuals do not find a job within four weeks, they are entitled for an allowance starting from the day of their first application. As Bolhaar et al. (2016) show, this discourages some claimants to show up again after four weeks. Stimulation of outflow from welfare is possible by an active policy on sanctioning insufficient search behaviour or fraud. Also, re-employment policies are used to stimulate outflow. Since the introduction of the policy reform, activities to reduce the welfare caseload differ substantially between municipalities (Bosselaar et al. 2007). For instance, policies to reduce the caseload of single parents: some municipalities use the freedom to exempt single parents from the obligation to search a job, while others do not and help them actively to find a job (Knoef and Van Ours 2016).

The WWB has been introduced gradually. Up until 2001, municipalities could claim 90% of welfare expenses from the Ministry (i.e., a matching grant). For the

other 10% they received a block grant based on historical expenses (in year t − 3). As a consequence, municipalities only had a very limited financial incentive to reduce the inflow into social security and stimulate the outflow from social security. In 2001, the part of the budget financed by a matching grant was lowered to 75%, while the block grant was increased to 25%. With the introduction of the WWB in 2004, the share of the block grant was further increased to 100% of the budget. For large municipalities, with more than 60,000 inhabitants, part of the block grant was not based on historical expenses but was completely independent of welfare expenses. The latter part increased from 12.5% of the budget in 2002, 25% in 2003, 40% in 2004 to 100% in 2006. For small municipalities, with less than 25,000 inhabitants, the block grant was based on historical expenses over the whole period up to 2008. For medium-size municipalities the part of the block grant independent of historical expenses was in between large and small municipalities (see Table 1).

Figure 1 illustrates changes in the degree of regression based block grant financing across municipalities. Large municipalities (with 60,000 or more inhabitants) were fully financed with a regression based block grant from 2006 onwards and medium-size municipalities with 40,000 or 50,000 inhabitants from 2007 onwards. Medium-size municipalities with 30,000 inhabitants were financed only partly (33%) with a regression based block grant in 2008 (Fig. 1).

The block grant independent of expenses was based on characteristics that were supposed to be exogenous, like the percentage of low educated inhabitants, the percentage of single parents, the percentage of non-western immigrants and some characteristics of the labour market. As this part of the block grant is driven by exogenous variables, it thus gives substantial incentives to reduce the welfare caseloads. In principle, it rewards municipalities for each recipient finding work with the present value of the benefits saved. The block grant based on historical expenses on the other hand gives a reward which will be close to zero, because municipalities will anticipate that the gain of their efforts will be lost three periods later (see also Toolsema and Allers 2014).

Arguably, the regression based block grant gives an optimal incentive from the perspective of the municipality: they have an incentive to invest in the outflow of recipients, up to an amount equal where a marginal increase in the present value of expected benefit savings is equal to the marginal costs of preventative and reintegration activities. Following this line of reasoning, easy-to-place clients (i.e., with short expected benefit durations) will receive less services than hard-to-place clients (i.e., with long expected benefit durations). On the other hand, if services for easy-to-place clients are more effective than those for hard-to-place clients, municipalities may have an incentive to concentrate on easy to-place clients. Still, this behaviour cannot be interpreted as cream-skimming, as there is no direct incentive for this. Neither do municipalities have an incentive for parking, because they pay the allowances for recipients while parked.

Although financial incentives are optimal from this theoretical perspective, Toolsema and Allers (2014) argue that, if municipalities operate at different levels of efficiency, actual welfare expenditures are a biased indicator of spending needs. Based on a theoretical model, they expect this bias to be small.

Both the regression based block grant and the block grant based on historical expenses, are calculated as a share of the total budget for welfare. So if the total

Table 1 Budget shares of welfare expenditures: matching grants, block grants based on historical expenses and based on regressions

Year	Which municipalities	Matching grant (%)	Block grant based on historical expenses (%)	Regression based block grant (%)	Total budget (%)
1999	All	90	10	0	100
2000	All	90	10	0	100
2001	All	75	25	0	100
2002	60,000 or more inhabitants	75	12.5	12.5	100
	40,000—60,000 inhabitants[a]	75	0–12.5	0–12.5	100
	Less than 40,000 inhabitants	75	25	0	100
2003	60,000 or more inhabitants	75	0	25	100
	40,000–60,000 inhabitants[a]	75	0–25	0–25	100
	Less than 40,000 inhabitants	75	25	0	100
2004	60,000 or more inhabitants	0	60	40	100
	40.000–60.000 inhabitants[a]	0	60–100	0–40	100
	Less than 40,000 inhabitants	0	100	0	100
2005	60,000 or more inhabitants	0	27	73	100
	40,000–60,000 inhabitants[a]	0	27–100	0–73	100
	Less than 40,000 inhabitants	0	100	0	100
2006	60,000 or more inhabitants	0	0	100	100
	30,000–60,000 inhabitants[a]	0	0–100	0–100	100
	Less than 30,000 inhabitants	0	100	0	100
2007	40,000 or more inhabitants	0	0	100	100
	30,000–40,000 inhabitants[a]	0	0–100	0–100	100
	Less than 30,000 inhabitants	0	100	0	100
2008	40,000 or more inhabitants	0	0	100	100
	25,000–40,000 inhabitants[a]	0	0–100	0–100	100
	Less than 25,000 inhabitants	0	100	0	100

Source: Van Es (2010)

[a] This scale is linear. For instance: the budget of a municipality with 35,000 inhabitants in 2007 is determined for 50% by a regression based block grant and 50% by a block grant on historical expenses

budget declines, the block grants of municipalities decrease even if their shares in the total budget stay the same. For instance, a municipality receiving a block grant based on historical expenses receives less budget in year t than the expenses in year $t - 3$ if the total budget declined in this period.

5 Data

In our analysis, we employ data at the level of the individuals. Data at the level of individuals can more accurately control for differences in the characteristics of the caseload of municipalities (Haider and Klerman 2005). Moreover, using individual

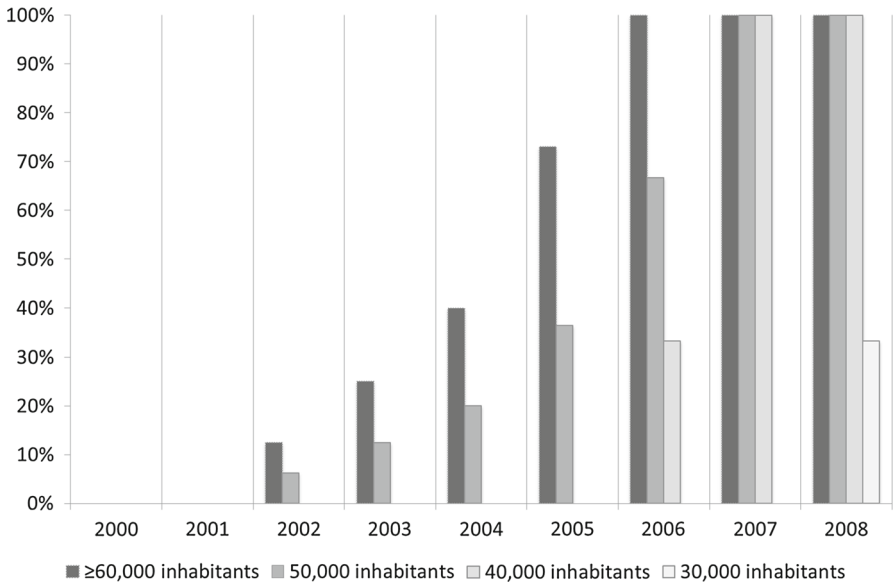

Fig. 1 Increase of regression based block grant as part of total budget

data enables us to evaluate heterogeneous effects between subgroups of welfare recipients. The dataset contains almost 900,000 individuals, who are followed from 1999 to 2008.

We use two datasets with administrative data that are extracted from Statistics Netherlands. The first dataset contains demographic information, whereas the second dataset contains information about starting and ending dates of welfare benefit durations. The combined dataset contains high quality, detailed information on demographics and beginning—and end date of benefit payments for all individuals living in the Netherlands. For our analysis, we only select individuals aged 18–65, as only they can be eligible for welfare. In both data files, each person has a unique registration number, as well as a municipality code depending on the municipality in which they reside. We use registration numbers to merge data files.

For each quarter of a particular year, each individual in the dataset was assigned a 1 if receiving welfare at the start of that quarter, and a 0 if not. The population at risk (18–65 years old) consists of 10 million persons, which appear in the dataset on average 32 times over 36 quarters (because people appear in the dataset when they reach the age of 18 and disappear when they get 65). As computing capacity fell short to estimate the model with 10 million persons observed in 32 quarters, we drew a random sample of 900,000 individuals. On average, we have about 800,000 individuals in each quarter in our sample. The dataset is enriched with local business cycle indicators and characteristics of municipalities—these are also obtained from Statistics Netherlands.

Table 2 contains summary statistics of the sample for different municipality size classes. As explained in Sect. 4, small municipalities with less than 25,000 inhabitants do not face a financial incentive from the regression based block grant. Medium-sized

Table 2 Descriptive statistics

Inhabitants municipality	<25,000		25,000–60,000		>60,000		Total	
Quarter	2000Q1	2008Q1	2000Q1	2008Q1	2000Q1	2008Q1	2000Q1	2008Q1
Observations	173,010	109,714	193,847	204,831	335,211	438,406	702,068	752,951
Receiving welfare (%)	1.4	1.2	2.3	1.7	5.8	3.6	3.7	2.7
Receiving welfare 4 quarters in a row (%)	1.1	1.0	1.9	1.4	4.9	3.1	3.1	2.3
Receiving welfare 8 quarters in a row (%)	0.9	0.8	1.5	1.2	4.1	2.6	2.5	2.0
Demographic variables as of recipients welfare								
Female (%)	60	60	60	63	55	58	56	59
Average age	43.1	45.0	41.8	43.9	40.8	44.2	41.2	44.2
Single, DWO (%)	34	33	31	34	29	29	30	30
Single, NWO (%)	3	6	5	7	12	15	10	13
Single parent, DWO (%)	29	27	27	26	18	17	21	19
Single parent, NWO (%)	4	6	6	9	13	16	11	14
Couple, DWO (%)	24	19	21	15	13	10	15	11
Couple, NWO (%)	7	10	9	9	15	13	13	12
Characteristics of municipalities								
Degree of urbanisation (scale from 1 to 5)	1.8	1.8	2.7	2.5	4.2	3.7	3.2	3.1
Average population	16,774	17,303	36,758	38,988	246,724	229,749	132,084	146,898
Policy variable, average (%)	0	0	0	69	0	100	0	77

Source: Statistics Netherlands, own calculations
DWO Dutch or other western origin, *NWO* non-western origin

municipalities receive a budget which is partially based on a regression based block grant. From 2006 onwards, the budget of large municipalities is completely determined with a regression based block grant. The table shows there are large differences in the probability of being on welfare between municipalities of different sizes. In the first quarter of 2000, only 1.4% of the population in small municipalities (with less than 25,000 inhabitants) were on welfare, whereas in large municipalities 5.8% of the inhabitants were enrolled. Both in small and large municipalities the probability of being on welfare declined in the period 2000–2008, but at a faster pace in large municipalities.

In the time period under investigation, the composition of the population on welfare clearly differed between small and large municipalities. In general, the population on welfare consisted of individuals that were predominantly single or single parents. Single parents constituted 33% of the population on welfare in 2008. This differs not much between municipality class sizes, although in large municipalities single parents are more often from non-western origin. In 2008 44% of recipients in large municipalities are non-western immigrants, mainly from Surinam, the Caribbean, Turkey and Morocco, whereas in small municipalities only 22% of recipients are non-western immigrants. On average, the share of non-western immigrants in the population on welfare has increased in the period 2000–2008, from 34 to 39%. The group with the largest welfare dependency are single parents of non-western origin. In 2008 they constitute 13% of the population on welfare, while their share in the population is small, although growing. Of all single parents from non-western origin 37% were on welfare in 2000, declining to 26% in 2008. Of single parents from Dutch or other western origin 18% were on welfare in 2000 declining to 10% in 2008. Single households from non-western origin also had a high welfare dependency: 20% in 2000 declining to 15% in 2008. The other groups had much lower probabilities of being on welfare, less than 10% and couples of Dutch or other non-western origin only 0,4%.

Figure 2 shows that the welfare caseload in the Netherlands declined from 376,000 in 2000 to 267,000 in 2008. Before the introduction of the policy reform in 2002, the number of welfare recipients declined, probably due to favourable business cycle conditions. In particular, we observe a strong rise in the vacancy–unemployment ratio ("lnvu": the natural logarithm of the vacancy–unemployment ratio) in this period. As the economy slowed down, the number of recipients rose up to 2005. After 2005, there has been a substantial decline in the number of recipients, after a once more booming economy. At the same time, the question rises to what extent the policy reform contributed to this decline.

Figure 3 shows that the probability of being on welfare shows the same pattern as the total welfare caseload in the period 1999–2008. It seems the decline in the probability of being on welfare is larger in the large municipalities in the period 1999–2002 and the period 2005–2008.

However, when the probabilities are calculated as indexes, another pattern emerges. Figure 4 shows that, up to 2002, the probability of being on welfare followed the same pattern in municipalities of different size classes. This suggests a common trend in welfare dependency rates in the period before the introduction of the regression based block grant. From 2003 to 2006 the probability of being on welfare increased rapidly in small municipalities and decreased afterwards. In large municipalities, there was

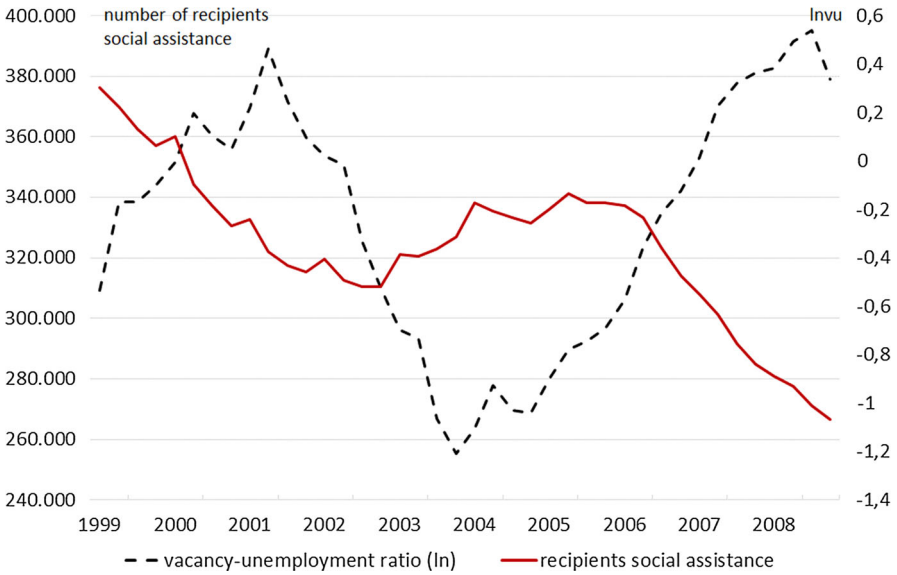

Fig. 2 Welfare caseload and the business cycle. *Source*: Statistics Netherlands, own calculations

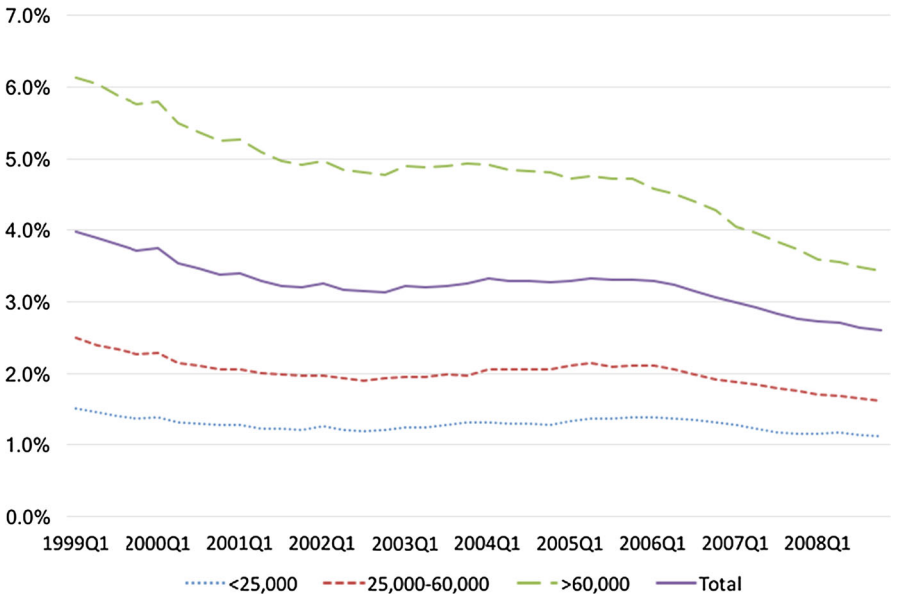

Fig. 3 Development of welfare caseload in municipalities of different size classes, 1999–2008

a small increase in 2003, but afterwards the trend has been steadily downwards. The development in medium size municipalities was in between small and large municipalities. After 2006, we observe a downward trend in welfare dependency across all size classed of municipalities. It seems the effect of the reform was especially strong

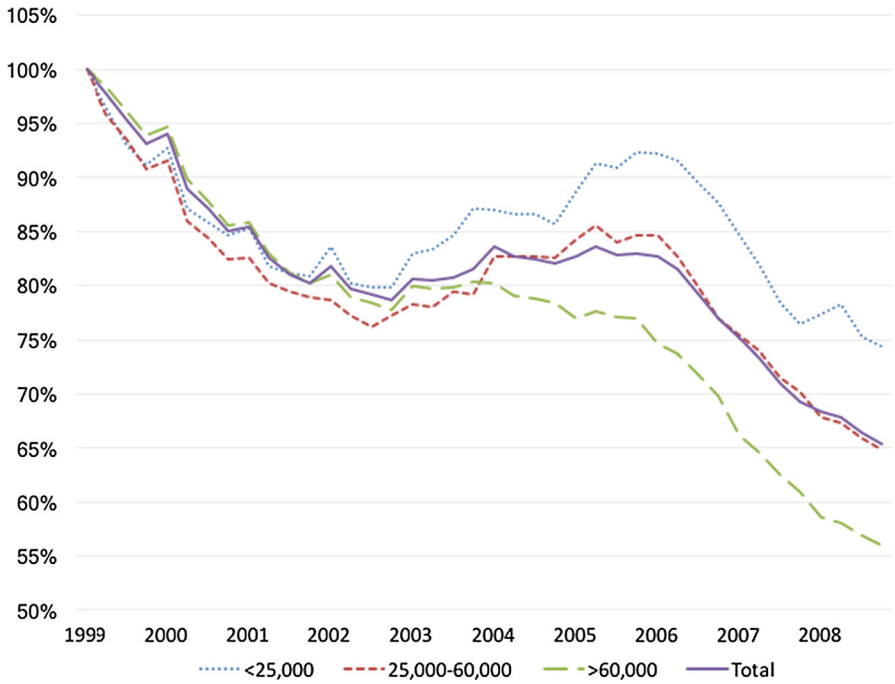

Fig. 4 Index welfare caseload in municipalities of different size classes, 1999–2008 (1999Q1 = 100%). *Note* Shown is the index of the number of recipients on welfare (1999Q1 = 100) for municipalities of different size classes. *Source*: Statistics Netherlands, own calculations

in large municipalities directly after the reform in 2004 up to 2006, when they faced the maximum incentive. After 2006, the decline in the probability of being on welfare seems to follow a similar pattern for municipalities of all size-classes.

6 Empirical Implementation and Estimation Results

6.1 Model Specification

In our analysis, we exploit variation in the incentive scheme over time and between municipalities, so as to estimate the effect of the policy reform. As the budget that is based on historical expenses hardly provides municipalities with incentives to reduce the welfare caseload, the incentive variable for municipalities is proxied by the percentage of their budget that consists of a regression based block grant. This percentage depends on the size of the municipality and the year (see Table 1). Only medium-sized and large municipalities fully face the strong incentives of a regression based block grant. The part of the budget which is financed as a regression based block grant has increased over time for both medium-sized and large municipalities, but in a different pace for medium-size and large municipalities (see Fig. 1).

To estimate the impact of the reform, we employ a difference-in-difference approach. The treatment group consists of all municipalities of which the budget from a certain point in time has been (partly) financed as a regression based block grant. The control group consists of small municipalities of which the budget still inhibited virtually no financial incentives to reduce welfare caseloads. Table 2 shows that the probability of being on welfare is smaller in small municipalities. Moreover, the composition of the caseload in small municipalities is different from that in large municipalities. This raises the question if small municipalities are a valid control group. However, a difference-in-difference strategy does not require that the treatment and the control group are the same, only that they are expected to have the same development of welfare dependency in the absence of the policy reform. Differences in the development of welfare dependency between municipalities most notably stem from differences in the effect of the business cycle and differences in the demographic composition of the population. Both are controlled for in our model specification. Moreover, the model does not only use the differences between small and larger municipalities to identify the effect of the reform, but also uses the gradual increase in the share of financing with a regression based block grant within the treatment group.

A difference-in-difference approach assumes a common trend in the development of the welfare caseload in the treatment group and the control group before the introduction of the reform. Figure 4 suggests there is such a common trend prior to 2002, the year the first incentives have been introduced. Second, we assume that municipalities did not anticipate the reform; in the literature, this is often referred to as the Ashenfelter dip, in which an observed pretrend goes in the opposite direction as the post-implementation effects of treatment (Ashenfelter 1978; Heckman and Smith 1999). In the current context, an Ashenfelter dip would occur if municipalities would postpone effort prior to the reform, in order to maximise the reward for effort after introduction of the policy change. In that case, the effect of the reform would be overestimated. However, municipalities have no incentive to postpone effort. They might even have an incentive to increase effort before the policy reform, because it decreases the welfare caseload after the policy reform. This would lead to an underestimation of the effect of the reform. As financial incentives have been introduced gradually, we expect this anticipation effect to be small. This is confirmed by Fig. 3, which does not show discontinuities around 2004.

We use an individual fixed effect panel data model to calculate the effect of the policy reform. A fixed effect panel data analysis incorporates duration dependence in the individual specific fixed effect. As serial correlation biases the standard errors, we use robust covariance structure to obtain standard errors of our coefficients. As Angrist and Pischke (2008) show that with a vast amount of data a linear model performs equally well as a logit model, we estimated a model with a linear fixed effect estimator:

$$y_{it} = \beta_1 p_{it} + \beta_2 x_{it} + \beta_3 m_{it} + \beta_4 u_{it} + \beta_5 Y_t + a_i + \varepsilon_{it} \quad (1)$$

- y_{it} indicates if individual i receives welfare at the start of quarter t. It is a binary variable: 1 if receiving welfare and 0 if not.

- p_{it} is the variable of interest. It is the percentage of the budget which is financed with a regression based block grant. This variable varies over time and between municipalities of different sizes (see Fig. 1).
- x_{it} contains demographic variables, including age, living with a partner and having children.
- m_{it} is a vector of characteristics of municipalities, including size, province and degree of urbanisation (on a scale of 1–5).
- u_t contains business cycle variables, including quarters within years and the unemployment rate at the level of the region.
- Y_t contains the year dummies.
- a_i is a time invariant individual specific effect.
- ε_{it} is the error term that is assumed to be identically and independently distributed.

6.2 Estimation Results

Table 3 shows the fixed effect estimation results of Eq. (1). The dependent variable is 1 if someone is on welfare in a quarter and 0 if he or she is not on welfare. It contains year dummies and dummies that control for the size of the municipality. Table 3 also shows model outcomes with the probability of being on welfare for four and eight consecutive quarters as outcome measures, respectively. Accordingly, these models measure whether the policy reform reduces long term welfare dependency. In all models, the unemployment rate is lagged with 1 year. This is done because people enter welfare after their unemployment benefit expires.

In all model variants, the demographic variables have the expected sign. The probability of receiving welfare increases with age. Being single increases the probability of being on welfare. Single parents have the largest probability of receiving welfare. The degree of urbanisation (the number of persons living on a square kilometre) increases the risk of receiving welfare allowances, as does the number of inhabitants of the municipality. The unemployment rate has a positive sign, as expected.

Model 1 shows that the policy reform reduced the probability of being on welfare. Predicting the probability of being on welfare with and without the reform implies that, according to model 1, the probability of being on welfare in 2008 decreased with 14% because of the policy change; this corresponds to a reduction of about 0.4 percentage points of the population at risk, or about 43,000 persons in 2008. In the period 2000–2008 the welfare caseload declined from 376,000 recipients to 267,000 recipients (see Fig. 1). Thus, 39% of the total decline in this period can be attributed to the policy change. Also, predicting the welfare caseload without a change in the unemployment rate from 1999 to 2008 shows that 27% of the decline of the welfare caseload can be attributed to the decrease in the unemployment rate.

Models 2 and 3 show that the policy reform reduces also long term welfare dependency (4 or 8 quarters of a year or longer). The probability of being on welfare 4 and 8 consecutive quarters is decreased by respectively 13% and 12% due to the reform. The policy reform thus not only induces municipalities to aim at 'quick wins' by deterring potential entry or stimulate exit of easy-to-place welfare recipients. As expected, the policy reform also induces municipalities to put effort in difficult to place recipi-

Table 3 The impact of the reform on three different outcome measures

	Model 1	Model 2	Model 3
Linear fixed effect models (robust clustered standard errors)			
Outcome measure	On/off welfare	4Q on/off welfare	8Q on/off welfare
Policy variable, not lagged	−0.005*** (0.000)	−0.004*** (0.000)	−0.003*** (0.000)
Unemployment rate lagged 1 year	0.238*** (0.019)	0.177*** (0.017)	0.096*** (0.016)
Ln age	0.056*** (0.002)	0.067*** (0.002)	0.058*** (0.002)
Couple (ref)			
Single	0.019*** (0.000)	0.017*** (0.000)	0.012*** (0.000)
Single parents	0.029*** (0.001)	0.026*** (0.001)	0.017*** (0.001)
Quarter dummy	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes
Characteristics municipality			
Population <15,000 (ref)			
15,000–25,000 inhabitants	0.000 (0.001)	0.000 (0.000)	0.000 (0.000)
25,000–40,000 inhabitants	0.002*** (0.001)	0.001** (0.000)	0.000 (0.000)
40,000–60,000 inhabitants	0.004*** (0.001)	0.003*** (0.000)	0.002*** (0.000)
60,000–100,000 inhabitants	0.005*** (0.001)	0.004*** (0.000)	0.003*** (0.000)
100,000–200,000 inhabitants	0.007*** (0.001)	0.005*** (0.001)	0.003*** (0.000)
>200,000 inhabitants	0.007*** (0.001)	0.005*** (0.001)	0.003*** (0.001)
Degree of urbanisation	Yes	Yes	Yes
Regional dummy (12 regions)	Yes	Yes	Yes
Constant	−0.178*** (0.009)	−0.221*** (0.008)	−0.191*** (0.007)
Number of observations	29,127,632	29,127,632	29,127,632
Number of individuals	894,552	894,552	894,552

Source: Statistics Netherlands, own calculations
Corrected for heteroscedasticity and serial correlation of residuals
* Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level

ents, because they are rewarded with the expected value of all future payments to the recipient.

To test the sensitivity of our model outcomes, we conducted various robustness checks. First, we used values of the policy variable that were lagged with 1 year (see Table 4); the underlying idea is that municipalities may have needed time to adjust to the reform. The outcomes show that lagging the policy variable hardly influences the outcomes of the model. In particular, the effect is slightly smaller than the non-lagged variable. It thus seems municipalities reacted promptly to the reform. Second, we adopted a non-linear specification of the policy variable. We tested a quadratic form and a square root. As the policy variable ranges from 0 to 1, the quadratic form is convex, while the square root form is concave. With a quadratic specification, we still observe a monotonously decreasing effect of the policy variable on the probability of being on welfare. Likewise, welfare dependency still decreases with respect to the

Table 4 The sensitivity of the results to different specifications of the policy variable

Outcome measure	On/off welfare	On/off welfare	On/off welfare
Linear fixed effect models (robust clustered standard errors)			
Policy variable, lagged 1 year	−0.004*** (0.000)		
Policy variable squared, not lagged		−0.004*** (0.000)	
Policy variable square root, not lagged			−0.005*** (0.000)
Unemployment rate lagged 1 year	0.246*** (0.020)	0.234*** (0.018)	0.242*** (0.018)
Ln age	0.062*** (0.003)	0.055*** (0.002)	0.056*** (0.002)
Couple (ref)			
Single	0.019*** (0.000)	0.019*** (0.000)	0.019*** (0.000)
Single parents	0.028*** (0.000)	0.029*** (0.001)	0.029*** (0.001)
Quarter dummy	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes
Characteristics municipality			
Population <15,000 (ref)			
15,000–25,000 inhabitants	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
25,000–40,000 inhabitants	0.001** (0.001)	0.001** (0.001)	0.002*** (0.001)
40,000–60,000 inhabitants	0.002*** (0.001)	0.003*** (0.001)	0.004*** (0.001)
60,000-100,000 inhabitants	0.003*** (0.001)	0.005*** (0.001)	0.005*** (0.001)
100,000–200,000 inhabitants	0.005*** (0.000)	0.006*** (0.001)	0.007*** (0.001)
>200,000 inhabitants	0.005*** (0.001)	0.007*** (0.001)	0.007*** (0.001)
Degree of urbanisation	Yes	Yes	Yes
Regional dummy (12 regions)	Yes	Yes	Yes
Constant	−0.201*** (0.010)	−0.177** (0.008)	−0.178*** (0.008)
Number of observations	25,544,107	29,127,632	29,127,632
Number of individuals	878,437	894,552	894,552

Source: Statistics Netherlands, own calculations
Corrected for heteroscedasticity and serial correlation of residuals
* Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level

policy variable if we allow for a concave function. However, the fit of the model is not improved by either of these models.

To shed more light on heterogeneity in policy effects, model 1 has been estimated on various demographic groups: couples, singles and single parents, and individuals stratified by origin. We estimated the models for migrants of non-western origin on the full population, as this group consist of around 10% of the Dutch population. The estimations show the reform has been most effective for those with the highest welfare dependency: single mothers and singles from non-western origin. The reform led to a reduction of the probability of being on welfare in 2008 with 4.8%-point (24%) for singles of non-western origin, with 4.4%-point (18%) for single parents of non-western origin, and with 1.8%-point (22%) for single parents of Dutch or other western origin. The decrease of the welfare caseload of other groups ranged from 0.8%-point (12%) for couples of non-western origin, 0.6%-point (12%) for singles from Dutch origin to 0.1%-point (15%) for Dutch couples (Table 5).

Table 5 The impact of the reform on being on/off welfare per demographic group

Linear fixed effect models (robust clustered standard errors)

	Single DWO	Single NWO	Single parent DWO	Single parent NWO	Couple NWO	Couple DWO
Outcome measure	On/off welfare	On/off welfare	On/off welfare	On/off welfare	On/off welfare	On/off welfare
Policy variable	−0.008*** (0.001)	−0.051*** (0.002)	−0.023*** (0.003)	−0.047*** (0.004)	−0.001*** (0.000)	−0.009*** (0.001)
Unemployment rate lagged 1 year	0.295*** (0.058)	1.051*** (0.072)	1.172*** (0.146)	1.619*** (0.113)	0.067*** (0.011)	0.503*** (0.033)
Ln age	0.087*** (0.008)	−0.006 (0.009)	0.270*** (0.018)	0.465*** (0.013)	0.014*** (0.001)	0.033*** (0.004)
Quarter dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Characteristics municip.						
Population <15,000 (ref)						
15,000–25,000 inhabitants	0.001 (0.002)	0.012** (0.005)	0.002 (0.006)	0.011 (0.009)	0.000 (0.000)	0.003* (0.002)
25,000–40,000 inhabitants	0.004* (0.002)	0.028*** (0.005)	0.003 (0.006)	0.019** (0.009)	0.000 (0.000)	0.004** (0.002)
40,000–60,000 inhabitants	0.008*** (0.002)	0.062*** (0.005)	0.010* (0.006)	0.037*** (0.009)	0.001** (0.000)	0.007*** (0.002)
60,000–100,000 inhabitants	0.009*** (0.002)	0.073*** (0.005)	0.018*** (0.006)	0.056*** (0.008)	0.001*** (0.000)	0.012*** (0.002)
100,000–200,000 inhabitants	0.009*** (0.002)	0.078*** (0.005)	0.025*** (0.007)	0.065*** (0.009)	0.002*** (0.000)	0.014*** (0.002)
>200,000 inhabitants	0.007*** (0.002)	0.076*** (0.005)	0.029*** (0.008)	0.050*** (0.009)	0.002*** (0.000)	0.016*** (0.002)
Degree of urbanisation	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummy	Yes	Yes	Yes	Yes	Yes	Yes
Constant	−0.238*** (0.030)	0.101*** (0.030)	−0.797*** (0.063)	−1.312*** (0.043)	−0.045*** (0.005)	−0.070*** (0.013)
Number of observations	4,050,627	8,474,651	1,391,218	5,131,706	20,733,393	24,685,932
Number of individuals	238,001	609,161	88,944	310,984	716,771	1,094,867

Source: Statistics Netherlands, own calculations
Corrected for heteroscedasticity and serial correlation of residuals
DWO Dutch or other western origin, *NWO* non-western origin
* Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level

7 Conclusion and Discussion

The picture that emerges from our analysis is that the Dutch welfare reform proofs successful in reducing the number of welfare recipient. The regression based block grant reduced the welfare caseload with on average 14%. The effect is larger for groups with the largest probability of being on welfare (persons from non-western origin and single parents). At the same time, however, it should be stressed that small municipalities still had no incentive to reduce their caseloads, as their budget was determined on historical expenses.

Our findings also suggest that financial incentives of the budgeting system did not only reduce the number of easy-to-place individuals, but also hard-to-place welfare recipients. This in turn indicates that municipalities succeeded in translating these incentives to the level of their caseworkers. Thus, although Dutch municipalities could not change welfare eligibility rules as US states can since the 1996 reform, they succeeded in reducing the welfare caseload. One way of doing so might have been by active labour market policies, another by increasing monitoring and screening activities. Municipalities indeed intensified enforcement policies after the reform (Kok et al. 2007).

Although the welfare reform provides optimal incentives to reduce the welfare caseload from the perspective of the municipality, this does not necessarily mean the incentives are optimal from a societal perspective. From a societal perspective the effects of the reform on employment should be taken into account. The decline of the welfare caseload does not only stem from increased employment among former potential recipients: not everyone that flows out (or does not flow in) will find a job. Therefore, it would be interesting to address future research to the employment effects of the policy reform.

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