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Compulsory activation of young welfare recipients: revisiting the trade-off between workfare and welfare generosity

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Abstract

We revisit the trade-off between workfare and welfare through a quasi-natural experiment by exploiting municipal variation from a 2017 Norwegian reform introducing compulsory activation for young welfare recipients. The results show a significant negative effect on social assistance. On the other hand, we find no effect on income adequacy, implying that other transfers mitigate the drop in social assistance. Our results convey therefore that the trade-off between workfare and welfare is binding when focusing exclusively on social assistance: investing in activation policies creates challenges to poverty alleviation channeled through the last social safety net. However, the trade-off is mitigated through other transfers, allowing us to conclude that social investment does not need to be inimical to the poor.

Key words: causal mechanisms; public administration; social policy; welfare state.

JEL classification: I32, Measurement and Analysis of Poverty; I38, Government Policy, Provision and Effects of Welfare Programs; J08, Labor Economics Policies

1. Introduction

Over the past decades, labor market activation measures have occupied a central place in both academic and policy debates. A key line of argument holds that activation policies should be paired with reduced welfare benefit levels to enhance work incentives, thereby

promoting higher employment rates and income levels (Besley and Coate 1992; Konle-Seidl and Eichhorst, 2008; van den Berg, Uhlenborff, and Wolff, 2022).

Besley and Coate (1992) also advocate reducing the size of cash benefits to limit the potential *crowding out* effect on private-sector work from activation programs. Other scholars have argued that activation schemes are formulated as conditional systems designed to restrict access and push receivers back into the labor market, who often fill the least paid and protected jobs (Rueda 2015). Overall, these theoretical contributions highlight the complex and intertwined relationship between welfare generosity, the economic incentives to seek employment, and the emphasis on activation.

In our view, the relationship between (compulsory) activation and lower social assistance payments can be understood by focusing on (a) *incentive constraints* and (b) *expenditure constraints*. Incentive constraints imply that a reduction in benefit levels enhances the economic incentives for recipients to participate in activation measures. In other words, less generous welfare benefits function as a *stick* that encourages beneficiaries to seek employment. Additionally, a trade-off between extensive activation measures and benefit generosity may arise due to expenditure constraints. In Norway, municipalities bear both the administrative and financial responsibilities for social assistance. When the state imposes activation requirements on all recipients, the costs of implementing and managing these activation measures rise. Consequently, municipalities may be compelled to reduce allocations for direct cash transfers.

This article revisits the trade-off between activation policies and benefit generosity within the national context of Norway. It investigates whether a heightened focus on activation measures is associated with a reduction in the generosity of benefits provided to specific target groups. In particular, the study analyzes how this relationship influences the capacity of minimum income schemes to alleviate poverty among young social assistance recipients aged 18 to 29 years. In this study, we define the income adequacy of social assistance benefits as suggested by Nelson (2013) by comparing benefit levels and household income with the EU-poverty threshold. Empirically, the comparative literature on European welfare states identifies a puzzle: some studies document a negative relationship between public expenditure on active labor market policies and institutional levels of social assistance (Nelson 2013). Note that institutional levels refer to the benefit levels set by politicians as advisory or “norm” levels used in means-testing processes. They do not necessarily reflect real levels of social assistance received. On the other hand, there is a set of studies that do not find strong evidence of this trade-off (Iacono 2017; Noel 2020).

Nelson (2013) hypothesizes that the introduction of compulsory activation could pave the way toward less generous benefit levels. First, he provides descriptive evidence showing that the income adequacy of minimum income schemes has decreased since the 1990s. Second, he documents the correlational association between increased spending on activation policies and the decreased adequacy of minimum income in Europe 1990 to 2007. On the other hand, using approximately the same data sources, Iacono (2017) extends the analysis to 2013 and finds no evidence of a binding trade-off between minimum income spending and expenditure on activation policies. Noel (2020) assesses the trade-off with time-series cross-sectional models of the determinants of ALMPs expenditures, childcare spending and the adequacy of minimum income protection (MIP), for 18 OECD countries between 1990 and 2009, and find that social investments do not develop at the expense of income protection.

We aim at addressing this puzzle by advancing methodologically beyond previous comparative studies in the following way: while cross-country research primarily examines institutional benefit levels, this study focuses on the actual benefits received at the individual level. Furthermore, the introduction of compulsory activation for social assistance recipients reveals multiple mechanisms that shape the interaction between activation measures and minimum income schemes—many of which operate at the municipal, local, and individual levels. As a result, a significant local dimension of this relationship may not be captured by nationally aggregated data. We argue that an empirical approach centered on sub-national variations will provide more robust, evidence-based policy insights. Specifically, leveraging local variation in the implementation of compulsory activation—within a system where municipalities both administer social assistance and enforce activation policies—offers a valuable opportunity for causal estimation. This setting enables a rigorous assessment of the trade-off between activation measures and individual economic outcomes under the compulsory activation policy reform. By exploiting the municipal variation given by the staggered adoption of compulsory activation in a sample of Norwegian municipalities, we can strengthen our understanding of the relationship between activation and the poverty-alleviating features of minimum income schemes, as there is still limited causal evidence of this relationship. Notice that many municipalities that self-imposed compulsory activation at unknown times before the national activation reform are not included in this study.

Social assistance and similar minimum income schemes primarily function as residual safety nets, providing crucial protection against poverty in both Europe (Nelson 2013) and the United States (Meyer and Wu 2018; Parolin 2021). In Norway, social assistance is a means-tested benefit of last resort, where municipalities and ultimately caseworkers exercise significant discretion in determining benefit amounts (Dokken 2016; Vilhena 2021). In 2017, the Norwegian government implemented a national reform requiring municipalities to enforce compulsory activation for social assistance recipients under the age of 30. This study examines whether the introduction of compulsory activation for young recipients affects benefit adequacy for the targeted group. To our knowledge, the 2017 reform has been analyzed causally only by Dahl and Hernæs (2023), who found no effects on benefit receipt, employment rates, or education within the first year of implementation. Earlier studies have examined a “first wave” of compulsory activation measures introduced voluntarily by municipalities in the early 2000s. These studies document significant effects, including reduced benefit duration and increased earnings (Hernæs 2020), higher school completion rates (Hernæs, Markussen and Røed 2017), and lower youth crime (Bratsberg et al., 2019). However, Dahl and Hernæs (2023) and Smedsvik and Iacono (2025) found limited effects of the 2017 reform when comparing municipalities that had implemented activation policies earlier with those subject to the national policy change. In this study, we focus exclusively on municipalities that implemented due to the national reform, utilizing the staggered roll-out and excluding earlier treated municipalities not subject to the 2017 reform. Furthermore, we restrict our analysis to social assistance recipients, rather than all individuals at risk of receiving such support. Importantly, we extend the observation period by three additional years, covering outcomes through 2019. Our findings indicate negative effects on benefit adequacy—and overall benefit levels—emerging in the third and final post-reform year (2019) for the first cohort of treated municipalities (those implementing activation in 2016). Card et al. (2010, 2018) find that significant effects of active labor

market policy (ALMP) programs often become apparent in the second or third year following implementation.

In our opinion, this research contributes to answering the puzzle highlighted by the comparative literature, on the strength of the trade-off between activation expenditure and poverty alleviation. We show a significant negative effect on social assistance. However, we find no substantial effect on income adequacy, implying that other transfers compensate for the drop in social assistance. Our results convey therefore that the trade-off between workfare and welfare bind only when focusing on social assistance: investing in activation policies creates challenges to poverty alleviation through the last social safety net. However, the trade-off is mitigated through other transfers, allowing us to conclude that social investment does not need to be inimical to the poor.

Section 2 provides data and institutional details. Section 3 introduces the identification strategy. Section 4 discusses the analytical approach used and presents the results. Finally, the findings are critically examined in Section 5 before concluding remarks are offered in Section 6.

2. Data and institutional details

2.1 Street-level bureaucrats as policymakers in social assistance

Before we dig into the data and the specific features of social assistance in Norway, we provide here some institutional analysis of the dynamics of social assistance allocation. It is primarily national and local authorities that influence institutional (or recommended) benefit levels. The actual benefit amounts, however, are determined through means testing. Local welfare administrations are granted discretion in setting the benefit levels, which are partly based on the institutional benefit levels as well as factors such as the applicant's necessary expenditures, personal situation, living conditions, age, and health (Dokken 2016). This discretion empowers local administrations and case workers, enabling them to significantly influence the allocation of social assistance to clients. Research on caseworkers' behavior indicates that their assessments of clients are influenced by perceptions of personal responsibility. As a result, the specific circumstances of an individual's situation play a crucial role in caseworker decisions (Torsvik, Molander, and Terum 2022). Additionally, Fiva and Rattso (2006) demonstrate that case workers are more responsive to individual income levels than politicians. Furthermore, changes in municipal priorities and professional discretion can also affect benefit levels at the local level (Brandtzæg et al., 2006).

The institutional structure of the social assistance scheme positions case workers as key actors in the implementation of the compulsory activation reform. Consequently, case worker discretion is a crucial policy element. We define discretion as the extent of freedom a case worker can make judgments in a specific context. In the Norwegian social assistance scheme, this discretion is particularly evident in the Social Services Act (Sosiltjenesteloven 2009), which permits the use of discretion to develop flexible solutions tailored to the needs of individual applicants (NAV 2012). According to Lipsky (2010), caseworkers can influence policy in two primary ways. First, caseworkers are granted significant discretion in making decisions about the claimants they interact with (Lipsky 2010). Second, caseworkers' individual actions collectively contribute to the agency's overall behavior. As a result, the social assistance scheme can enable caseworkers to play a role in policymaking,

given their high degree of discretion and autonomy from organizational authority in determining benefit allocation.

Lipsky (2010) also highlights the critical role of caseworkers in rationing services, a key aspect of the social assistance scheme. In this capacity, caseworkers are entrusted with both the mandate and the authority to determine the size of benefits allocated to claimants. This practice reflects society's approach to empowering frontline workers, allowing them to respond flexibly to unique situations and to treat individuals in accordance with their specific circumstances (Lipsky 2010, p. 105). While case workers aim to improve the lives of claimants, they must also make distinctions between recipients. For instance, Brandtzæg et al. (2006) find that benefit amounts may vary, even when the need for assistance is identical. Means testing explicitly acknowledges that not all individuals are equally entitled to the same level of support. In contrast to other income replacement schemes, such as earnings-related unemployment benefits, where both entitlement and benefit size are determined by more explicit criteria, the discretion exercised by caseworkers in social assistance is more flexible and subjective. Therefore, one outcome of the institutional arrangement is that caseworker discretion allows them to both "*intervene on behalf of clients as well as discriminate among them*" (Lipsky 2010, p. 22).

2.2 Social assistance in the Norwegian welfare state

Social assistance in Norway is designed as a principal minimum income scheme and offers long-term unemployed individuals and families a social and economic safety net (once all other transfers have been exhausted). Social assistance is therefore a last-resort benefit. It is means-tested, and it is only intended to cover necessities. There is no formal limit to the length of the reciprocity. The actors involved in the Norwegian social assistance scheme can be divided into three levels. First, the State has the legislative responsibility for designing and regulating the social assistance scheme, including the compulsory activation requirement. The central authorities also indicate a norm for the benefit levels that should be followed by the municipalities. Second, the municipalities and local politicians decide how much to allocate to the social assistance scheme. Municipalities therefore have the power to adjust the benefit levels. These adjustments can be driven by political motivation in the local council, by the municipality's economic situation or by more practical adjustments to the variations in local living costs (e.g. those driven by local housing prices). Fiva and Rattsø (2006) find, for example, that both the institutional and actual benefit levels in a municipality adapt to those of neighboring municipalities to mitigate the risk of welfare migration.

Table 1 displays the development in institutional (or norm) benefit levels in the period 2015–2019, in 2015 Norwegian kroner. As shown, benefit adequacy falls by roughly 1% within the period of analysis.

The third and final actors are local welfare agencies which are represented by caseworkers. The application and allocation of social assistance usually requires a meeting with a caseworker, who further assesses the economic needs and potential activation measures. Furthermore, caseworkers are responsible for assessing claims and follow-up, as described in Vilhena (2021).

2.2.1 Adequacy and poverty in Norwegian social assistance

We apply a relative measure of social assistance in terms of minimum income adequacy, which has been used mainly in the comparative literature (Nelson 2013; Iacono 2017). We

Table 1. Institutional social assistance (SA) levels in Norwegian municipalities, 2015–2019.

	2015	2016	2017	2018	2019	Average
Institutional SA benefit (2015 NOK)	5,742	5,698	5,692	5,649	5,617	5,681
Institutional SA benefit adequacy (%)	33	33	33	32	32	32

Note: The first line in this table shows the monthly average institutional SA benefit levels for a single-person household across all municipalities included in this study. The amounts are weighted by the number of youth recipients in each municipality. The second line shows the adequacy of the institutional benefit levels, which are measured as a % of the national poverty line. The data plotted in this table are retrieved from Statistics Norway: <https://www.ssb.no/statbank/table/12131/>.

argue that this measure provides a better understanding of the impact of benefits on poverty alleviation. Additionally, the results become more comparable to evidence from other countries. However, we will also estimate the model in terms of absolute levels of social assistance, for the sake of completeness.

We define social assistance adequacy as the level of social assistance benefit divided by the equivalized relative poverty line in Norway. The applied poverty threshold follows the EU definition; hence, the poverty line for equivalized household income is 60% of the median income after taxes. One could also use non-equivalized individual income after tax. This would produce somewhat lower median income, and thus higher adequacy. However, this is not common practice in the existing literature. Notice that we focus on the amount received for each recipient, rather than on institutional or norm benefit levels. We believe that this provides a more accurate picture of how social assistance is administered.

Table 2 shows that the average social assistance adequacy in the period of analysis (2010–2019) varies between 37% and 40% for single-person households under the age of 30, and between 37% and 39% for the sample of young social assistance recipients, including all household typologies.

Most interestingly, Table 2 indicates a negative trend in benefit adequacy and the overall household income adequacy (% of national median) of the recipients. These estimations are the net of the means-tested support for housing costs. Although housing support is traditionally seen as a part of social assistance, it is administered as a separate, national scheme, and its design is not subject to the 2017 compulsory activation reform. In our measures of poverty and income adequacy, we include all employment incomes and transfers by the government.

Let us now turn to some stylized facts related to poverty for our sample. Table 3 indicates an increase in poverty during the period under analysis. More precisely, we observe a jump of 13 percentage points in the mass of recipients living in poverty, which is a relative increase of 33% with respect to the initial year of 2010. Although there has been an increase in poverty for all household types, poverty is mostly concentrated in childless, single-person households, which is also a feature found in US poverty (Brady and Parolin 2020).

Table 2. Actual SA benefits, income adequacy, and poverty among young SA recipients.

	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	Avg.
SA adequacy (% , single-person hh.)	40	39	38	38	39	39	39	37	37	38	38
SA adequacy (% , all)	39	38	37	38	38	38	38	37	37	37	37
Household income adequacy (%)	66	65	63	63	61	61	60	60	60	60	62
(%) Living in poverty	45	47	51	52	54	55	57	58	58	58	54
Obs.	14,558	14,143	13,878	14,514	15,142	15,619	15,501	15,748	15,053	14,105	148,258

Note: The average SA adequacy level is the monthly benefit as a share of the EU-poverty threshold (<60% of median monthly household income). The numbers are for all social assistance recipients below the age of 30 years in the municipalities included in the study. Household income adequacy is the yearly weighted household income (sum of all income and transfers after tax) for the recipients as a share of the national median. (%) Living in poverty refers to the share of social assistance recipients in the treated municipalities who live in poverty (EU60 threshold).

Table 3. Share (%) of SA recipients (age 18–29 years) living in poverty, by household type.

	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	Avg.
Single person household	57	59	63	65	66	67	68	68	70	70	65
Dual person household	40	44	47	48	55	55	54	55	53	52	51
Households with children	38	41	46	47	50	50	54	56	58	55	49
Recipients living w. parents	32	34	34	35	34	36	37	38	38	38	36
Other households	53	54	57	59	64	63	64	62	62	61	61
Total	45	47	51	52	54	55	57	58	58	58	54

Note: The numbers are based on individuals included in our sample. Other households mostly comprise multiple family households consisting of several single persons or couples.

2.2.2 Activation policies in Norway and the 2017 reform

Hassel and Palier (2023) characterize the contemporary welfare reform strategy in Nordic countries as *social investment* oriented. The active labor market policies within these welfare states have previously been conceptualized as investment-based, emphasizing employability through substantial investments in human capital (Bonoli 2011; Iacono 2018). In contrast, compulsory activation measures and potential benefit reductions align more closely with an incentive-based approach aimed at addressing youth unemployment and social assistance dependency—an approach that may be more accurately described as *liberal* (Bonoli 2011).

The 2017 national reform marked an expansion of compulsory activation within social assistance, a policy initially introduced in the 1990s as a voluntary measure for municipalities. According to Dahl and Hernæs (2023), at least 122 municipalities adopted the policy at their own initiative before the 2017 reform. This information is based on a survey conducted by Dahl and Lima (2018), which had a response rate of 85%. Some municipalities responded but only provided information on the year of implementation. Consequently, the actual number of municipalities that introduced compulsory activation before the 2017 reform is likely higher than the 122 officially documented. The 2017 reform thus represents a “second wave” of activation in social assistance. It mandates that all recipients under the age of 30 participate in labor market activities, training programs, or activation centers administered by the Norwegian Labour and Welfare Administration (NAV). Additionally, the reform introduced the possibility of sanctioning individuals who fail to comply with these activation requirements. However, multiple studies suggest that such sanctions have been applied to a limited extent (Torsvik et al., 2022; Vilhena 2021; Smedsvik and Iacono 2025).

The survey conducted by Dahl and Lima (2018), which we utilize in our study, provides general insights into the activation policies adopted by welfare offices following the reform. Fewer than 10% of municipalities reported extensively using existing activation measures. Overall, 25% indicated that they had implemented new activation measures, with 20% introducing their own initiatives and an additional 5% relying on state, private, or voluntary providers. Notably, 69% of municipalities reported neither expanding their use of activation measures nor introducing new ones after the reform. The most frequently reported activation measure was job training, followed by courses and job search assistance.

There are a variety of activation measures that can be imposed on a social assistance recipient. Municipalities vary in terms of the types and number of activities they offer. The most common ones are mandatory job search, courses, and work placements. Courses can be job search courses, work-oriented courses, or everyday courses. Typically, activation measures can last from three weeks to a full year. The required number of hours attendance can vary from 1 to 32 hours or more per week. However, there is usually a requirement that both the duration of the measure and the hours of attendance are adjusted to each individual participant (Dahl and Lima 2017).

Compulsory activation is often associated with a workfare approach, as it strengthens economic incentives by linking compliance with activation requirements to benefit retention (Smedsvik and Iacono 2025). The reform also reflects concerns that youth unemployment may be a key driver of long-term marginalization. In 2019, the Norwegian Public Employment Committee (*Syssettingsutvalget*) recommended increased investments in active labor market policies and the introduction of activation requirements for young recipients of disability benefits (NOU 2019). The 2017 activation reform aligns with a broader

policy trend in Norway, where multiple economic transfer programs—such as work assessment allowances, qualification programs, and introduction programs for refugees—have been restructured to specifically target young recipients.

3. Identification strategy

We utilize individual-level data from Norwegian administrative registers, covering all social assistance recipients in municipalities affected by the 2017 compulsory activation reform, between 2010 and 2019. We exclude 2020 to mitigate potential distortions from the COVID-19 pandemic and a major municipal reform, which substantially altered the units of analysis by merging municipalities with different treatment timelines.

To identify municipalities that implemented compulsory activation as a result of the reform, we rely on a survey of NAV (Norwegian Labour and Welfare Administration) offices conducted by [Dahl and Lima \(2018\)](#) among leaders of local welfare administrations in municipalities and selected districts of larger cities. The key survey question used to determine implementation was: *When was compulsory activation implemented at your office?* The survey achieved a response rate of 85%, which is notably high compared to similar surveys used in [Bratsberg et al. \(2019\)](#); [Hernæs et al. \(2017\)](#), where local practice data were obtained from fewer than 50% of offices. Municipalities lacking information on treatment were excluded from the sample, resulting in a final sample of 163 municipalities out of the 428 that existed in Norway during the study period. As shown in [Table 4](#), the staggered adoption of the reform occurred over three years, from 2016 to 2018. In total, 10 municipalities remained untreated by the end of the period.

3.1 Treatment assignment

Our identification strategy relies on the fact that the reforms implementation was mandated by the national government rather than self-selected by municipalities, implying that treatment assignment was at least partly exogenous. However, the municipalities affected by the reform had previously chosen not to implement compulsory activation at an earlier stage. This concern was also addressed by [Smedsvik and Iacono \(2025\)](#), who found no systematic differences in social assistance receipt between municipalities that had introduced compulsory activation before the reform and those affected by the reform. However, [Dahl and Hernæs \(2023\)](#) find that especially large municipalities have been introduced in earlier waves of implementation.

Table 4. Overview of treatment years.

Treatment	2016	2017	2018	Never treated	Total
N (individuals in sample)	33,619	35,213	4,171	927	70,283
N (individuals at treatment year)	7,056	7,600	870	184	1,551
N (municipalities/city districts)	50	95	11	7	163

Note: This table shows that the staggered adoption of the reform encompasses three years, from 2016 to 2018. Overall, 10 municipalities were not yet treated.

Table 5. Treatment assignment.

	2017	2018	Not yet treated
Number of SA recipients (log)	−.2 (1.29)	−.65 (2.76)	6.74 (4.64)
Age (mean)	.42 (.28)	.18 (.53)	.01 (.98)
Generosity (mean)	−.01 (.04)	.10 (.07)	−.08 (.12)
Activation (log)	.33 (1.21)	.57 (2.39)	−3.98 (2.23)
Time-trend	YES	YES	YES
N	163	163	163

We assess the assignment of treatment at the time of implementation, recognizing that the staggered adoption was influenced, at least in part, by self-selection in the year of adoption. [Table 5](#) examines whether the timing of treatment is systematically associated with a set of covariates, particularly the prevalence of social assistance reciprocity. The underlying intuition is that municipalities with a higher share of passive recipients may have stronger incentives to accelerate the introduction of compulsory activation, potentially leading to endogeneity in treatment assignment—a central concern for our identification strategy. Moreover, treatment assignment may also be linked to anticipation effects, as discussed in [Roth et al. \(2023\)](#).

We use a multinomial regression model to conduct this test, using data pooled over the five years preceding the initial implementation of the reform. Municipalities that adopted the reform in 2016 serve as the reference group. The model accounts for clustered robust standard errors at the municipal level. [Table 5](#) presents the results.

The results indicate that none of the regressors are statistically significant, suggesting rejecting concerns about potential endogeneity in treatment assignment. We therefore hypothesize that variation in treatment timing was driven by administrative processes and other factors unrelated to the social assistance scheme.

3.2 Sample selection

Our sample consists of individuals from 163 municipalities and city districts, representing approximately one-third of all municipalities in Norway during the study period (428 in total). The municipalities and their implementation years were identified through the survey conducted by [Dahl and Lima \(2018\)](#). Of the 309 municipalities that responded to the question regarding when compulsory activation was implemented, 146 reported having adopted these measures at an unknown time before 2016 and were therefore excluded from the sample. This results in a final sample of 163 municipalities and city districts, with 50 implementing the reform in 2016, 95 in 2017, 11 in 2018, and 7 remaining untreated as of June/July 2018.

The panel consists of all social assistance recipients under the age of 30 years in these 163 municipalities, observed annually from 2010 to 2019. The panel is unbalanced, with

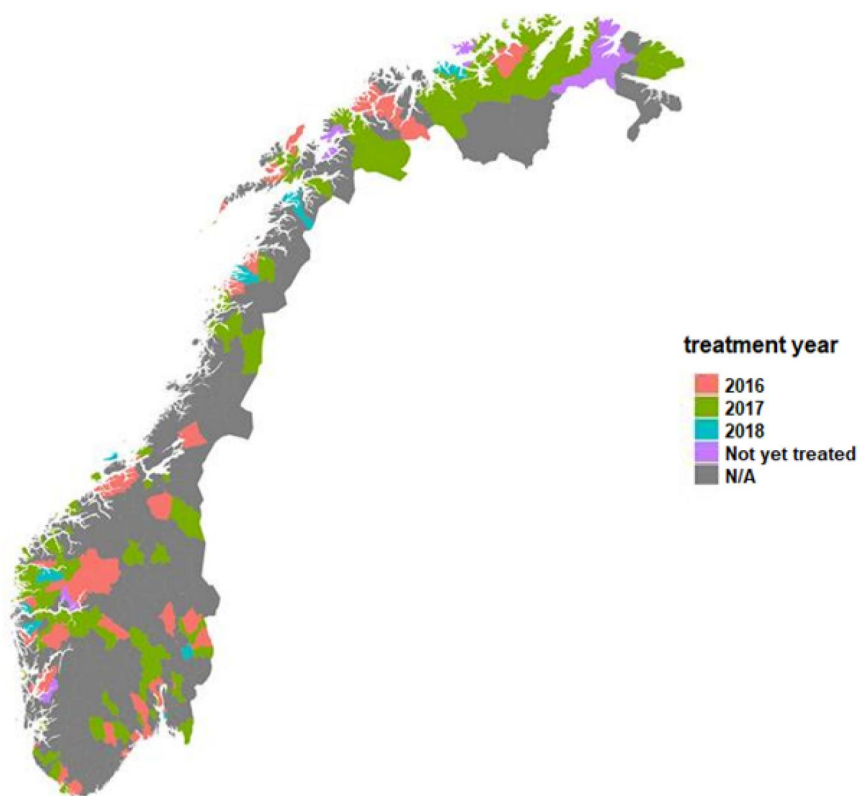


Figure 1. Map of the treated areas.

sample sizes ranging from 13,892 to 15,775 individuals per wave. This sample represents approximately one-third of the total population of social assistance recipients aged 18–29 years in Norway, which is estimated at around 43,000 individuals ([Smedsvik and Iacono 2025](#)). The total sample size in the main model is 70,342.

As illustrated in [Fig. 1](#), treated municipalities and their respective treatment years appear to be geographically well-distributed across the country. However, as noted in [Dahl and Hernaes \(2023\)](#), the earliest adopters of the reform were predominantly larger municipalities, as shown in [Table 4](#). It is important to note that the most densely populated areas are concentrated in the southern, eastern, and western regions of Norway, while northern municipalities tend to cover significantly larger geographic areas despite lower population densities.

3.2.1 Sample selection correction

Selection into the sample is based on social assistance take-up, which may introduce selection bias. Several studies address this issue using auxiliary regressions to correct for non-

random take-up (e.g. [Hernæs et al., 2017](#); Dahl and Hernae 2023). We address this bias using a two-step Heckman selection model ([Heckman 1979](#)), following the approach in [Smedsvik et al. \(2022\)](#).

In the first step, we estimate a probit model for all individuals i aged 18–30 years in year $t \in [2010, 2019]$, using individual-level covariates such as the household type, number of children, age, education and unemployment benefit status. We include also employment status in the selection equation, assuming it strongly influences take-up but does not directly affect the amount of assistance received, which is determined by income and household composition. We then compute the inverse Mills ratio from the estimated selection probabilities and include it as an additional covariate in the event-study specification that will be presented below in Section 4.1.

4. Methodology and results

This study aims to assess the causal effects of introducing compulsory activation policies on the benefit adequacy of social assistance. We compare municipalities that adopted the reform (treated) to those that had not yet implemented it (not yet treated). Given the staggered implementation of the reform, we use a difference-in-differences model with staggered treatment adoption, complemented by an event study design to assess the validity of the parallel trends assumption. For this approach to yield consistent estimates, several key assumptions must hold, as discussed in [Callaway and SantAnna \(2021\)](#) and [Sun and Abraham \(2021\)](#).

The first and most critical assumption is the parallel trends assumption, which requires that baseline outcomes follow a similar trajectory across treated and untreated municipalities before the intervention. This is tested by examining pre-treatment differences (pre-trends), measured by the coefficients on leads. Additionally, this test helps address the assumption of limited treatment anticipation, which requires that treatment has no causal effect before implementation ([Roth et al., 2023](#)).

Another important assumption is that treatment is an absorbing state, meaning that once an individual is treated, they remain treated in subsequent periods. This assumption holds if individuals do not migrate between municipalities to avoid or seek compulsory activation. Since already treated municipalities are excluded from the analysis and the number of untreated municipalities in 2017 and 2018 is relatively small, any violation of this assumption is expected to be minor. However, it should be acknowledged that this assumption may not be fully satisfied.

4.1 Model specification

[Equation \(1\)](#) presents the main specification of the two-way fixed effects model with the leads and lags described earlier. Suffix i refers to individuals residing in municipality m at time t :

$$SA_{i,m,t} = \alpha_i + \lambda_t + \delta_m + \sum_{\ell} \mu_{\ell} 1\{t - E_i = \ell\} + \gamma X_{i,t} + u_{i,m,t} \quad (1)$$

All estimates include α_i and λ_t , which represent individual and time fixed effects, controlling for the individual and time-invariant unobserved heterogeneity (e.g. gender and immigration status), respectively. Municipality fixed effects δ_m are added, including an

additional municipality-specific linear time trend. The municipality-specific time trend controls for the trends that are spuriously related to the effect of compulsory activation, as in [Dhakar et al., \(2013\)](#). μ_ℓ are the coefficients for the lead and lag variables at relative times ℓ . Leads and lags serve as indicators for unit i being ℓ periods away from the initial treatment at time t . Leads and lags thus represent a time until or since the treatment variable and show the years until (leads) or after (lags) an individual received treatment. The coefficients from the leads and lags are interpreted as measuring the effect of the treatment at different lengths of exposure [Callaway and Sant'Anna \(2021\)](#). E_i is the time of treatment for unit i , inferred from residence in the treated municipality m . $X_{i,t}$ contains individual-level covariates such as the household type, number of children, age, education, unemployment benefit status, household income (excluding social assistance), and the inverse Mills ratio controlling for potential selection effect. $u_{i,m,t}$ is the error term. The standard errors are clustered on municipalities, as the social assistance scheme is administered by municipalities.

[Table 6](#) displays the different outcome variables (SA adequacy, income adequacy, participation in ALMPs, transfers, and SA reciprocity), and individual-level characteristics used as covariates in the model specification 1.

[Table 7](#) displays the distribution of individuals for different leads and lags, with lead 1 being omitted (the year before treatment) as a reference point. As shown, most of the individuals in the analysis are treated in 2016 and 2017, with a smaller group being treated in 2018.

[Table 8](#) presents the results from the difference-in-differences model specification in [Equation \(1\)](#), gradually adding fixed effects and linear time trends. All models yield consistent findings, indicating a significant reduction in social assistance adequacy starting in the third year of treatment. The estimated effect ranges from a decline of 2.41 to 3.25 percentage points, corresponding to approximately 9% relative decrease in adequacy due to the reform. Moreover, there are no significant differences among individuals before treatment (from lead -6 to lead -2), supporting the validity of the parallel trends assumption across all three models. The post-treatment trend suggests a continued decline in benefit generosity following the reform.

To further illustrate these findings, [Fig. 2](#) visualizes the results of the main model [3] in an event-study framework, providing a graphical representation of the parallel trends hypothesis.

To place these results in a broader context, [Table 9](#) examines the reforms effects on related outcomes, including other transfers (both with and without social assistance), participation in activation programs (measured in months), income adequacy, duration of social assistance receipt (in months), and absolute social assistance levels (log of monthly social assistance).

We observe no changes in the duration of social assistance reciprocity, measured by months. The results indicate no significant impact on income adequacy, suggesting that the reform did not influence overall poverty levels.

[Figure 2](#) shows the leads and lags for SA adequacy from [Table 8](#), column 3. The year before treatment (-1) is omitted as it is standard in the literature. Notably, we find no significant effect on total welfare transfers from public authorities. However, when excluding social assistance, the results suggest a positive effect on other transfers. This indicates that the reduction in social assistance is offset by increased reliance on alternative welfare programs, ultimately leading to a slight increase in total transfers (including social assistance).

Table 6. Outcome variables and individual-level characteristics by implementation year, 2016–2018.

Implementation year	2016	2017	2018	Not yet implemented
Outcome variables				
Social Assistance adequacy (%)	38	37	37	35
Income adequacy (hh. income as % of median)	61	62	64	66
Participant on ALMP (number of months 0–12)	5.4	5.4	5.5	4.6
Transfers (sum of cash benefits from the public)	122,799	121,147	129,750	111,387
Transfers (excluding social assistance)	88,381	87,901	92,884	83,531
Social assistance reciprocity period (months)	4.68	4.75	5.2	4.1
Individual level characteristics (hh. type - % of sample)				
Single person household	32	30	29	26
Dual person household	8	8	6	9
Households with children	32	33	36	34
Recipients living w. parents	15	16	18	17
Other households	13	13	11	15
Other individual level characteristics				
Number of children	0.51	0.52	0.58	0.56
Education (finished upper secondary edu. or more)	20 %	20 %	19 %	16 %
Average age	23.69	23.72	23.7	23.75
Unemployment benefit				
Regular unemployment benefit	4 %	4 %	4 %	5 %
Work assessment allowance	15 %	15 %	15 %	12 %
Employment scheme benefit	6 %	6 %	6 %	6 %
No unemployment benefit	30 %	30 %	30 %	27 %
Not registered as unemployed	45 %	46 %	45 %	51 %
Other characteristics (not included in models)				
Labor income	47,378	49,974	47,468	63,005
Weighted household income	209,850	213,584	220,219	229,116
Total observations	66,463	71,740	8,229	1,832
Number of individuals	33,619	35,213	4,171	927

Note: This table displays the set of outcome variables [1—social assistance adequacy, operationalized as the monthly benefit as a % of the EU-poverty threshold (<60% of median monthly household income); 2—income adequacy, operationalized as the yearly weighted household income (sum of all income and transfers after tax) for the recipients as a share of the national median; 3—participation in ALMPs, operationalized as participation in all types of activation programs (number of months); 4—transfers, meaning the (gross) sum of economic transfers from public programs in NOK (both taxable and non-taxable benefits); 5—transfers, as in (4) however excluding social assistance; and 6—social assistance reciprocity, number of months]; and individual-level characteristics used as covariates (household type; number of children; education attainment; average age; unemployment benefit status; other characteristics; labor income; and weighted household income) from 2016 to 2018. All monetary amounts are in real values (2015 NOK).

Table 7. Leads and lags.

Leads and lags	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	Total
-6	6,592	6,870	759								14,230
-5		6,306	6,782	811							13,903
-4			6,186	7,064	855						14,101
-3				6,479	7,200	842					14,526
-2					6,883	7,497	881				15,265
0							7,056	7,600	870		15,515
1								7,321	7,624	804	15,745
2									7,011	7,328	14,330
3										6,737	6,737

Note: Leads and lags are indicators for individual i being e periods away from the initial treatment at time t . Leads and lags thus represent a time until/since the treatment variable and show the years until (leads) or after (lags) an individual received treatment.

Furthermore, no significant effect is found on participation in active labor market programs. This raises the question of whether the reform effectively enabled local offices to expand activation services for claimants. Finally, the negative and significant effect on monthly social assistance levels in the third post-reform year (lag 3) reinforces the findings in Table 8, demonstrating that the observed reduction in adequacy is not merely driven by changes in the poverty line (denominator of the adequacy ratio).

4.2 Robustness

Recent econometric literature (notably, Callaway and Sant’Anna 2021; Sun and Abraham 2021; Roth et al., 2023; Borusyak, Jaravel, and Spiess 2024) highlights that the canonical difference-in-differences model with staggered treatment adoption may be biased in the presence of heterogeneous treatment effects if key assumptions are not strictly met. While alternative estimators have been developed to address this issue (see Baker et al. (2025) for a recent survey), the software used in this study (microdata.no) does not support the implementation of external packages developed to run these estimators. We discuss how we attempt to mitigate this limitation in the discussion section.

In this section, we will focus on three important robustness checks: (a) Local median income as poverty threshold; (b) Restricting the sample to long-term recipients; (c) placebo treatment.

4.2.1 Local median income as poverty threshold

As an alternative measure for assessing the adequacy of social assistance, in this subsection, we use the municipality-specific weighted median household income after taxes and transfers as the poverty threshold when estimating individual social assistance adequacy. The rationale for this approach is that social assistance is a locally administered benefit, and recipients’ needs are largely shaped by local living costs, particularly housing expenses. Since housing and other essential costs vary significantly across municipalities, defining

Table 8. Difference-in-differences (DiD) results.

Social assistance adequacy	[1]	[2]	[3]
−6	.24 (0.9)	.28 (0.91)	.22 (.96)
−5	.02 (1.2)	.07 (1.25)	.02 (1.26)
−4	−0.93 (1.4)	−0.9 (1.42)	−0.96 (1.4)
−3	−.9 (1.25)	−.88 (1.26)	−.93 (1.2)
−2	−.8 (.83)	−.8 (.84)	−.81 (.8)
0	−.68 (.79)	−.79 (0.77)	−.69 (0.77)
1	−.76 (.88)	−.98 (.86)	−.87 (.81)
2	−.83 (1.13)	−1.22 (1.12)	−1.02 (.94)
3	−2.53* (1.57)	−3.13** (1.58)	−3.23** (1.40)
Controls	Yes	Yes	Yes
Individual and year	Yes	Yes	Yes
Municipal FE	No	Yes	Yes
Linear time trend	No	No	Yes
Municipal-specific linear time trend	No	No	Yes
N (obs)	147,691	147,691	147,691
N (individuals)	70,092	70,092	70,092

Note: All models include clustered robust standard errors (in parentheses) at the municipal level. −6 to 3 is the relative time to/since treatment.
* and ** denote significance levels at 10% and 5%, respectively.

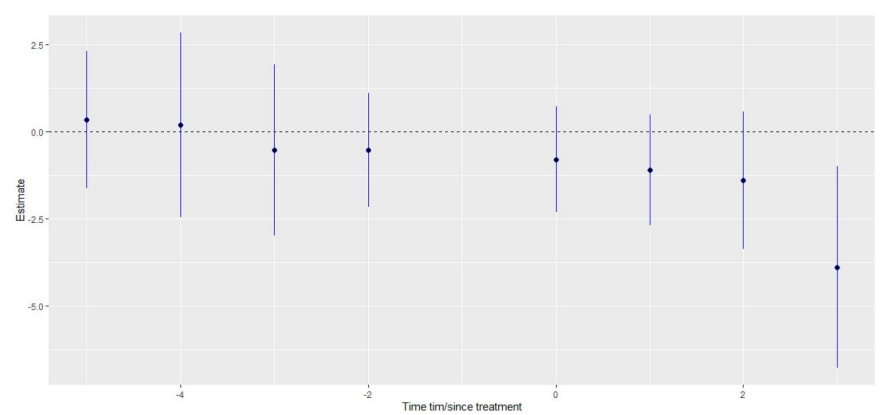


Figure 2. Event study—SA adequacy.

Table 9. DiD results—additional outcomes.

	Transfers (w. SA)	Transfers	Activation	Inadequacy	SA reception	SA monthly
-6	-.01 (0.02)	-.02 (.03)	-.02 (.11)	-1.7* (.95)	.02 (.15)	.00 (.03)
-5	.01 (.03)	-.01 (.05)	-.03 (.14)	-2.84** (1.4)	.06 (.18)	-.01 (.04)
-4	-.04 (.04)	-.04 (.04)	.01 (.15)	-2.95** (1.38)	.04 (.19)	-.03 (.05)
-3	-.04 (.03)	-.04 (.04)	-.04 (.13)	-2.91** (1.32)	-.01 (.14)	-.03 (.04)
-2	-.03 (.02)	-.03 (.02)	-.12 (.08)	-1.66** (.77)	-.06 (.1)	-.03 (.03)
0	.01 (.02)	.05** (.02)	-.07 (.09)	1.03* (.65)	-.12 (.09)	-.03 (.02)
1	.01 (.03)	.06* (.03)	-.08 (.12)	.81 (.99)	-.07 (.12)	-.03 (.03)
2	.05 (.03)	.15*** (.05)	.01 (.15)	2.01* (1.13)	-.04 (.14)	-.04 (.04)
3	.09 (.05)	.22*** (.07)	-.01 (.22)	2.55* (1.59)	-.05 (.20)	-.12** (.05)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual, year, and municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
Municipal time trend	Yes	Yes	Yes	Yes	Yes	Yes
N (obs)	147,601	128,870	147,691	147,626	147,691	147,691
N (individuals)	70,059	63,967	70,092	70,069	70,092	70,092

Note: This table displays the DiD results for Equation (3) on other relevant outcomes, such as (log) transfers (with and without SA), activation (months of participation), income adequacy, SA reception (number of months), and the log of monthly SA levels. Transfers are the (gross) sum of economic transfers from public programs (both taxable and non-taxable benefits). All models include clustered robust standard errors (in parentheses) at the municipal level. All monetary amounts are in real values (2015 NOK). The estimates for Activation, SA reception and SA monthly, include control for household income (excluding social assistance). Estimates for Transfers (w. SA) and Transfers control for household income (excluding transfers). The estimates for income adequacy do not include control for household income (this is the outcome).

*, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

adequacy based on the local rather than the national median income allows for a more precise reflection of these geographic disparities. This robustness check ensures that our measure of adequacy accounts for regional differences in economic conditions and living standards, providing a more contextually relevant assessment.

Table 10 presents the comparison between national and municipal poverty thresholds, while Table 11 presents the results.

Results from the model in Table 11 appear to be coherent with the results shown in Table 8, indicating a significant reduction in social assistance adequacy (around 3.5%)

Table 10. SA adequacy by national or municipal median wage as poverty threshold.

	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	Avg
SA adequacy (national median)	39	38	37	38	38	38	38	37	37	37	37.4
SA adequacy (municipal median)	38	37	38	38	38	37	36	36	36	37.2	38

Note: This table displays the average SA-adequacy for the total sample included in the study from 2010 to 2020. It shows that when defining adequacy by the local specific median weighted household income after tax and transfers as poverty threshold, adequacy is marginally lower compared to using the national median as threshold.

Table 11. Event study—SA adequacy (municipal median as poverty threshold).

Social assistance adequacy	[1]	[2]	[3]
–6	.4 (0.90)	.4 (0.9)	.36 (.93)
–5	.16 (1.26)	.25 (1.27)	.18 (1.27)
–4	–0.69 (1.42)	–0.63 (1.43)	–0.72 (1.4)
–3	–.63 (1.27)	–.6 (1.28)	–.68 (1.21)
–2	–.63 (.81)	–.61 (.84)	–.64 (.8)
0	–.84 (.77)	–.86 (0.75)	–.75 (0.75)
1	–1.01 (.88)	–1.06 (.85)	–.92 (.81)
2	–1.25 (1.14)	–1.35 (1.09)	–1.13 (.95)
3	–3.23** (1.58)	–3.41** (1.57)	–3.47** (1.41)
Controls	Yes	Yes	Yes
Individual and year	Yes	Yes	Yes
Municipal FE	No	Yes	Yes
Linear time trend	No	No	Yes
Municipal-specific linear time trend	No	No	Yes
N (obs)	147,691	147,691	147,691
N (individuals)	70,092	70,092	70,092

Note: All models include clustered robust standard errors (in parentheses) at the municipal level. –6 to 3 is the relative time to/since treatment.

**denote significance levels at 5%.

Table 12. Event study—long-term recipients.

	SA adequacy
–6	.4 (1.5)
–5	–.27 (1.87)
–4	–1.01 (1.8)
–3	–.33 (1.69)
–2	–.42 (1.11)
0	–1.7 (1.04)
1	–1.47 (1.26)
2	–2.18 (1.58)
3	–4.91** (2.07)
Controls	Yes
Individual, year, and municipal FE	Yes
Linear time trend	Yes
Municipal specific linear time trend	Yes
N (obs)	19,524
N (individuals)	4,408

Note: All models include clustered robust standard errors (in parentheses) at the municipal level. –6 to 3 is the relative time to/since treatment.

** denote significance levels at 5%.

starting in the third year of the treatment. This seems to validate the results of the main model in which we used the national poverty threshold.

4.3 Long-term recipients

Recipients of social assistance may enter or exit the sample at any point within the period covered by the leads and lags. One contributing factor is the age eligibility criterion: individuals exit the treatment upon turning 30, while new recipients become eligible at age 18 year. Additionally, program participation—both take-up and exit—is likely influenced by the reform itself. To address this, we restrict our analysis to individuals aged 18–29 who had been receiving social assistance for at least one year before the initial treatment and remained in the sample throughout all post-treatment years (Table 12).

Interestingly, when the sample is restricted to 4,413 individuals who are long-term recipients, the reduction in social assistance adequacy becomes even stronger, climbing to above 5% in the third year since the treatment. This confirms the trade-off between introduction of compulsory activation and welfare generosity, as shown in the main model in Table 8.

4.3.1 Placebo treatment

We also run a placebo model, as suggested by [Cunningham \(2021\)](#), where we estimate the DiD model for social assistance recipients in the age group of 30 to 40 years who were not directly affected by the reform. This is an additional test for the reliability of our main estimates and for the assumption of parallel trends.

We find no effect on the age group of 30 to 40 years, which suggests that in the absence of treatment, we would also eventually find no program results for the treated group less than 30 years. That is, the placebo results in [Table 13](#) indicate no effect of the reform on untreated recipients of social assistance in the closest age group.

5. Discussion

This article revisits the puzzle on the strength of the empirical trade-off between activation and poverty alleviation highlighted by [Nelson \(2013\)](#) and [Iacono \(2017\)](#); [Noel \(2020\)](#); and attempts to provide causal evidence on whether the implementation of activation policies affects the benefit levels of minimum income schemes in Norway. A key distinction from previous cross-country studies is our focus on the individual-level benefit received, whereas

Table 13. Placebo treatment.

	SA adequacy
-6	-.34 (1.34)
-5	-1.2 (1.73)
-4	-1.85 (1.8)
-3	-1.87 (1.46)
-2	-.7 (1.01)
0	1.55 (1.75)
1	2.0 (1.71)
2	2.27 (1.89)
3	-.82 (2.34)
Controls	Yes
Individual, year, and municipal FE	Yes
Linear time trend	Yes
Municipal specific linear time trend	Yes
N (obs)	113,757
N (individuals)	50,001

cross-country analyses primarily examine institutional benefit levels. Additionally, we use a compulsory activation reform as a causal identifier, while prior studies typically use public spending on active labor market policies (ALMPs) as a proxy for activation.

Our findings support the hypothesis that ALMPs have a negative effect on social assistance adequacy, as shown in [Table 8](#) and [Fig. 2](#). These results partly coincide with evidence from the Netherlands that the effects are mainly on the intensive margin, namely the mandatory activation program reduced the number of welfare recipients but had no effect on the overall number of individuals outside work, education or training ([Cammeraat, Jongen, and Koning 2022](#)). In our case, we see that the benefit levels are reduced on the specific program (social assistance) but have little effect on overall disposable income. Furthermore, as it is also the case for [Card et al. \(2010, 2018\)](#), program effects are first visible after several years.

DiD approaches with staggered adoption generally suffer from heterogeneous treatment effects, which should receive some attention when interpreting the results. Heterogeneous treatment effects imply that the treatment is not the same for all treated groups. In case of heterogeneous treatment effects, even if the treatment effect is positive, β_{post} may be negative due to negative weights. This is particularly an issue when longer-run treatment effects receive negative weights [Roth et al. \(2023\)](#). This issue has led us to opt for dynamic two-way fixed effects (TWFE) specifications, with dummies for the time relative to treatment. The main advantage of this approach is that it provides sensible estimates when there is heterogeneity in the time since treatment [Roth et al. \(2023\)](#). We normalize with respect to the last pre-treatment period, as suggested by [Callaway and Sant'Anna \(2021\)](#) and [Cunningham \(2021\)](#).

We document in [Table 1](#) that the institutional levels decided by politicians fell in the period of the reform, with an adequacy reduction of 1%. The remaining effect of almost 3% in received benefit levels is driven by case workers and local administrations, who are responsible for means-testing, activation requirements and follow-up. This finding provides further support for the arguments by [Lipsky \(2010\)](#) that caseworkers are an essential part of policy implementation and that the effects of reforms largely play out at this level.

As mentioned briefly in Section 1, [Dahl and Hernæs \(2023\)](#) find no short-run effect on benefit receipt, education or employment for the 2017 compulsory activation reform, suggesting that changes in benefit adequacy do not come from changes in the composition of recipients. [Smedsvik and Iacono \(2025\)](#) also find no significant use of benefit sanctions related to these policies. In continuation of these findings, the results from this study suggest that the reform has led to small overall changes for young social assistance recipients. Thus, an important policy implication is that the findings provide further justification for investigating the details behind the implementation of the reform. We find no effect on participation in activation programs, while [Torsvik et al. \(2022\)](#) and [Vilhena \(2021\)](#) find evidence that caseworkers are reluctant to impose sanctions for violations of compulsory activation. This suggests that other aspects of the activation reform should receive more attention from researchers, with an emphasis on how labor market activation is organized and demanded.

Although we document a strongly increasing descriptive poverty trends for young social assistance recipients within the period of analysis, and significantly negative effects of the compulsory activation reform on social assistance adequacy, our additional results on income adequacy imply that there are no substantial effects of the compulsory activation reform on poverty levels among young social assistance recipients. This finding requires

further discussion, as it highlights a partial limitation of our study: we lack qualitative data on the decision-making process that led case workers to reduce social assistance benefits. As a result, we cannot establish a clear causal link between lower social assistance adequacy and the extent to which individuals compensated for this reduction through earnings or other transfers. It is plausible that the decline in social assistance benefits is, at least in part, a mechanical consequence of individuals receiving additional income from work or other transfers. This is particularly relevant in the Norwegian context, where no earning disregard policy exists meaning that any additional income directly reduces social assistance benefits. Therefore, while the compulsory activation reform led to a reduction in social assistance benefits, this decline may not solely reflect the deterrent effect of lower benefit generosity. Instead, it may also be attributable to the fact that, for some individuals, activation resulted in increased earnings and access to other transfers.

Finally, we raise a concern about whether municipalities can increase the capacity or portfolio of activation programs to meet the requirements of the reform. With respect to the issue of external validity, it must be noted that there are several important differences between the Norwegian welfare model and other European welfare systems, which limits the direct transferability of our results but not our framework and methodological approach. The findings of this study should encourage further revisitation of this hypothesis in other welfare systems to reach reliable conclusions on this relationship.

6. Concluding remarks

This article re-examines the trade-off between emphasizing active labor market policies (ALMPs) and maintaining welfare support. Focusing on Norway, we first document a rising trend in descriptive poverty among young social assistance recipients over the past decade. A key policy concern is whether ALMPs negatively impact social assistance levels. Reducing benefit levels may strengthen incentives for recipients to engage in the labor market, potentially increasing employment but also challenging the poverty-alleviating function of minimum income schemes. Existing research presents conflicting evidence on this relationship, creating a puzzle. For instance, [Nelson \(2013\)](#) finds that an increased emphasis on workfare has led to less generous welfare provisions. In contrast, [Iacono \(2017\)](#) and [Noel \(2020\)](#) do not find a binding trade-off between ALMPs and social assistance adequacy at the aggregate level in European countries.

This article leverages the Norwegian social assistance scheme to reassess this trade-off and address the existing puzzle. Utilizing high-quality administrative data, the analysis shifts from a cross-country European perspective to the municipal and individual levels in Norway. To build on the existing literature, the study examines a recent activation reform targeting young social assistance recipients. By using an event-study design with a difference-in-differences (DiD) approach and exploiting variation in municipal implementation, this article provides one of the first within-country contributions to this debate.

The results of the analysis indicate a significant negative effect of changes in ALMPs on the adequacy of social assistance in Norway in the longer run. This is in line with the findings of [Nelson \(2013\)](#). We find that most of this reduction occurs in the individual assessment of recipients' needs by local administrations and case workers. Importantly, we are concerned that there is no effect on participation in activation programs, which raises questions about the implementation of activation programs at the local level. Finally, the fact

that this second wave of activation in Norwegian social assistance seems to exhibit modest results on employment and income, especially compared to the first wave, could suggest that the effectiveness of activation programs has not improved substantially in recent years.

Furthermore, we find that this reduction is in parallel matched by an increase in other transfers, resulting in no overall change in social transfers for this group. There is also no indication that the reform affected household income adequacy. We also find no indication that the reform affected overall transfers from the public, which could suggest that other transfers contributed to mitigating reductions in social assistance. In other words, on the lines of [Iacono \(2017\)](#) and [Noel \(2020\)](#), social investment does not need to be inimical to the poor.

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Data availability

The set of codes to fully replicate the results of this work (upon granted access to Microdata.no) will be made available to reviewers to ensure a transparent code review process, and upon publication, they will be made publicly available on Open Science Framework here: <https://osf.io/rv6c4/>. The views expressed herein are those of the authors and do not necessarily reflect those of our institutions. All errors are our own.

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