Incentivizing social assistance recipients: Quasi-experimental evidence from trials with earnings exemptions in the Netherlands*

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Abstract

This paper studies the causal labor market effects of increased earnings exemptions for social assistance recipients in two large Dutch municipalities: Amsterdam and Rotterdam. Exploiting regional variation in the availability of such increased earnings exemptions, I use difference-in-differences with administrative microdata from January 2020 to April 2023. The results indicate that the increased earnings exemptions had a positive effect on the employment rate, the number of hours worked and the labor income of social assistance recipients. Employment in the extensive margin increased by about 0.9 percentage points (16%), and by about 0.5 hours per month (17%) in the intensive margin. Monthly labor earnings increased by about 6.3 euros (19%). I find no evidence of an effect on exits from SA. The estimated effects on employment and labor earnings are, on average, larger for young SA recipients, women, parents, and SA recipients without health problems, while the effects on exits do not differ between subgroups. These findings suggest that "making-work-pay" policies can be an effective way to stimulate labor force participation among social assistance recipients.

Contents

1 Introduction

Financial incentives are widely implemented to encourage benefit recipients to take up employment. One way to provide such incentives is through earnings exemptions, which allow benefit recipients to keep part of their labor income when they start working. Such earnings exemptions, also known as earnings disregards, are meant to "make work pay", as they ensure that earned income is not fully deducted from social security benefits. However, despite their widespread use, little is actually known about the effectiveness of these disregards in increasing labor market participation among social assistance recipients.

This paper studies the effects of earnings exemptions on labor participation and earnings among social assistance (SA) recipients in two Dutch municipalities. The Netherlands have a long history of using earnings exemptions for SA recipients,[1] and under the current rules, in place since 2004, recipients may keep 25 percent of their earnings for up to six consecutive months. Still, Dutch municipalities struggle with activating SA recipients. Each year, only about ten percent of SA recipients enters the labor market (Muffels, 2020). This prompted two of the largest municipalities in the Netherlands, Amsterdam and Rotterdam, to launch trials with more generous earnings exemptions for SA recipients. I evaluate the effectiveness of these increased exemptions by estimating their causal effects on labor market participation and earnings of SA recipients.

To determine the effects of the increased earnings exemptions in Amsterdam and Rotterdam, I use difference-in-differences regressions, where the identification relies on regional variation in the availability of an increased earnings exemption. Namely, treatment groups consist of SA recipients from Amsterdam and Rotterdam and the control groups consist of SA recipients from municipalities that did not have an increased earnings exemption. This allows for the identification of the causal effects of the increased exemptions. I make use of monthly administrative microdata from Statistics Netherlands from January 2020 to April 2023 to study the effects during the first two years after their implementation.

Few studies have analyzed the effectiveness of earnings exemptions for SA recipients. The existing evidence on the effects on labor participation and earnings is mixed. Some studies have found no effects (Matsudaira & Blank, 2014; Palviainen, 2023), while others report positive effects of earnings exemptions on labor participation and earnings (Blank et al., 1999; Card & Robins, 1996; Knoef & Van Ours, 2016). Exits from social assistance appear to be unaffected by the

[1] See Hoff and Jehoel-Gijsbers (2003) for an overview of the different earnings exemptions for SA recipients in the Netherlands between 1992 and 2002.

earnings exemptions (Card & Robins, 1996; Knoef & Van Ours, 2016). Furthermore, studies tend to focus solely on incentives for taking up full-time employment (Card & Robins, 1996) or on specific subgroups of the SA population (Knoef & Van Ours, 2016; Matsudaira & Blank, 2014). This paper adds to the literature by providing a causal evaluation of earnings exemptions in the Netherlands for the full population of SA recipients, with specific attention to the take-up of part-time employment. In contrast to most earlier studies, these earnings exemptions were available to all types of SA recipients, allowing for the comparison of effects across a broad range of subgroups.

I find that the increased earnings exemptions had a positive effect on the employment rate, the number of hours worked and the labor income of social assistance recipients. Employment in the extensive margin increased by about 0.9 percentage points (16%), and by about 0.5 hours per month (17%) in the intensive margin. Monthly labor earnings increased by about 6.3 euros (19%). I find no evidence of an effect on exits from SA. The effects on employment and labor earnings are, on average, larger for young SA recipients, women, parents, and SA recipients without health problems, while the effects on exits remain indistinguishable from zero for all subgroups.

This paper is structured as follows. Section 2 gives an overview of the related literature. Section 3 presents the institutional context in the Netherlands. The expected effects of the increased earnings exemptions are discussed in Section 4. Section 5 presents the data used, and Section 6 the methods employed. Furthermore, Section 7 presents the results from the baseline and heterogeneity analyses. Section 8 tests the robustness of the baseline findings. Finally, Section 9 gives the conclusions.

2 Related literature

Few studies have focused specifically on the effects of providing earnings exemptions for recipients of last-resort social security schemes such as SA. Studies more commonly evaluate work incentives that are provided through the tax system and are accessible for all lower-income households. This section discusses the literature by first going through the empirical evidence of the effects on financial incentives for recipients of last-resort social security benefits, specifically. This is followed by a brief overview of the more generally evaluated tax-based incentives.

2.1 Earnings exemptions for recipients of last-resort social security benefits

Most recently, Palviainen (2023) estimated the effects of an earnings disregard for SA recipients in Finland. Using coarsened exact matching and difference-in-differences, the author finds no effect on employment rates. Only women seemed to be positively affected by the earnings disregard in terms of employment.

Knoef and Van Ours (2016) study the effect of increased earnings exemptions for single mothers on SA in the Netherlands. Using a difference-in-difference-in-differences approach, based on regional variation in the availability of the increased exemptions and an eligibility criterion based on the age of the children, they find that the earnings exemption increased employment among immigrant mothers by about 19 percentage points and earnings among all mothers by 60 to 100 euros per month. Exits from SA were not affected by the policy.

Moreover, Blank et al. (1999) provide an overview of experimental evidence on the effects of financial incentives for welfare recipients in the United States (US). These authors find that increased earnings exemptions in the US seem to increase both employment and income. These findings coincide with those of Card and Robins (1996), who perform a randomized control trial with an earnings supplement in Canada for long-time social assistance recipients who start working full-time. Moreover, Card and Robins (1996) show that the supplement had no effect on exits from social assistance.

In contrast, Matsudaira and Blank (2014) use differences between US' states in the level of earnings disregards for women on welfare to estimate the effect of the level of the disregard on the labor supply and earnings of single mothers. They find that higher earnings disregards had no effect on the labor supply and earnings of single mothers.

Furthermore, between 2018 and 2020, six Dutch municipalities (Nijmegen, Deventer, Groningen, Utrecht, Wageningen, and Tilburg) ran small-scale randomized controlled trials with SA recipients, where one or more groups received an increased earnings exemption. Verlaat and Zulkarnain (2022) evaluate these six experiments.[2] However, due to the setup of the experiments, the effects in Groningen, Nijmegen, and Tilburg likely suffered from selection bias and are, thus, not further studied by Verlaat and Zulkarnain (2022). Furthermore, as the increased earnings exemption was part of several treatments that were studied and combined, Verlaat and Zulkarnain (2022) are not

[2]The separate reports for each of these experiments are given in Betkó et al. (2020) (Nijmegen), Edzes et al. (2020) (Groningen), Gramberg and De Swart (2020) (Deventer), Muffels, Blom-Stam, and Van Wanrooij (2020) (Wageningen), Muffels, Blom-Stam, and van Wanrooij (2020), and Verlaat et al. (2020) (Utrecht).

able to isolate the effects of the earnings exemptions in Deventer. In the other two municipalities, the results were mixed. In Utrecht, the authors find a positive effect on employment and exits from SA during the experiment that faded out after the experiment ended. In Wageningen, however, the increased earnings exemption had a negative effect on exits from SA, but only after the experiment ended.

Other studies were also not able to identify the causal effects of such earnings exemptions. In Amsterdam, an experiment with an increased earnings exemption took place between 2018 and 2021. The municipality concluded that the increased earnings exemption in this experiment had a large and positive effect on the probability of being employed (Van Kempen et al., 2021). However, it is likely that these results suffer from selection bias, because the analysis compares participants in the experiment to non-participants. Participation was voluntary, and individuals knew beforehand that participating in the experiment meant that they could get the increased earnings exemption. Therefore, individuals who took part in the experiment differ from non-participants, most importantly in terms of motivation. Van Kempen et al. (2021) try to control for this by matching the control units to the treated units on a set of observable characteristics. However, this does not capture the difference in motivation, as this is not observable. Hence, their results likely overestimate the actual effect of the increased earnings exemption.

An analysis by the municipality of Rotterdam of the same trial with increased earnings exemptions that is studied in the current paper did find positive employment effects of the earnings exemption (Van Toorn, 2022). However, the analysis cannot separate possible time effects from the effect of the earnings exemption, as it does not make use of a control group.

To conclude, previous studies have found either no effects (Matsudaira & Blank, 2014; Palviainen, 2023) or positive effects of earnings exemptions on labor participation and earnings (Blank et al., 1999; Card & Robins, 1996; Knoef & Van Ours, 2016; Van Kempen et al., 2021; Van Toorn, 2022). Some found conflicting results (Verlaat & Zulkarnain, 2022). Exits from SA appear to be unaffected by the earnings exemptions (Card & Robins, 1996; Knoef & Van Ours, 2016). However, most of these studies on earnings exemptions for SA recipients had no identification strategy that allowed for the estimation of causal effects (Van Kempen et al., 2021; Van Toorn, 2022; Verlaat & Zulkarnain, 2022) or focused solely on incentives for taking up full-time employment (Card & Robins, 1996). Others studied only single mothers on SA (Knoef & Van Ours, 2016; Matsudaira & Blank, 2014), and it is unlikely that the effects for single mothers would be the same as for the full population of SA recipients. Namely, single parents are generally found to have a more elastic

labor supply in the Netherlands (De Boer et al., 2020).

This paper adds to the literature by providing the first causal evaluation of an earnings exemption in the Netherlands that is available to all types of SA recipients, with specific attention to the take-up of part-time employment. The unique setting with regional differences in the implementation of increased earnings exemptions, combined with the availability of detailed administrative microdata, allows for the estimation of causal effects, which was not possible in most previous studies. Furthermore, this study considers the full population of SA recipients, instead of only a specific subgroup. This increases the external validity, and, thus, the generalizability of the results in this study. Moreover, it allows for the comparison of effects across a broad range of subgroups that were not included in earlier studies.

2.2 Tax-based financial incentives

Earnings disregards for recipients of last-resort social security benefits are only one type of instrument to increase financial incentives for taking up employment among low-income households. Other popular financial incentives that affect the net benefit reduction rate work through the tax system instead of through the social security benefit itself. Examples of such tax exemptions are the Earned Income Tax Credit (EITC) in the US and the Working Families Tax Credit (WFTC) in the United Kingdom (UK).

The EITC is a tax credit in the US, aimed at low-income families with children. The EITC works both as an income transfer and as a work incentive. The EITC is generally found to have a positive effect on labor supply in the extensive margin, but less so in the intensive margin (see Meyer (2010) or Nichols and Rothstein (2015) for an overview).

The WFTC was a tax credit for families in the UK. Recently, it has been replaced by the Working Tax Credit (WTC). The WFTC was only available conditional on working 16 hours per week. Multiple studies estimated that the WFTC had positive effects on the labor supply of single mothers (Brewer et al., 2006; Francesconi & Van der Klaauw, 2007) or single parents (Blundell et al., 2005).

The increased earnings exemptions evaluated in the current study differ from the aforementioned financial incentives through the tax system, as they are solely provided to social assistance recipients. The evaluated policy, thus, has a slightly different target group and, therefore, intended effect. Whereas the EITC and WFTC also aim to provide income transfers, those eligible for the earnings exemptions studied in this paper already receive means-tested social security benefits.

Hence, the earnings exemption does not need to provide additional income transfers and its main goal is activation. This also implies that it does not increase the income of the so-called "working poor"[3], which could be the case for the EITC and WFTC.

Furthermore, the EITC and WFTC are mostly focused on providing financial incentives for families with children. As the earnings exemptions studied in this paper are available to all SA recipients, this increases the generalizability of the results.

3 Institutional context

This section discusses the institutional context of the increased earnings exemptions in Amsterdam and Rotterdam. It starts by explaining the SA scheme from which the earnings are exempted. Then, the general temporary earnings exemption for SA recipients in all Dutch municipalities is discussed, which is followed by the details on the increased earnings exemptions introduced in Amsterdam and Rotterdam.

3.1 Social assistance in the Netherlands

Social assistance (SA) is called *bijstand* in the Netherlands. It is a last-resort benefit for people who have exhausted all other benefits (such as unemployment insurance benefits). SA is a means- and assets-tested social security scheme that guarantees a monthly minimum income for households with earnings below the benefit level.

The benefit levels are set by the national government and are adjusted each year in January and July. Municipalities are responsible for determining benefit eligibility and paying out the benefit. Eligibility is determined at the household level.[4] The net benefit level was set at 1,075.44 euros per month for single persons and at 1,536.34 euros for couples in April 2021.

In principle, any household income is fully deducted from the social assistance benefit. However, Dutch municipalities may grant a temporary earnings exemption of 25 percent up to about 222 euros per month during a maximum of six months in order to incentivize transitions into employment.[5] In practice, most municipalities indeed offer this temporary earnings exemption to their SA recipients (Divosa, 2019) but only about 10 percent of SA recipients with additional earnings actually receives

[3] Individuals who are working, possibly full-time, but who still do not earn enough to provide for themselves.

[4] Generally, every adult legally residing in the Netherlands, with income lower than the benefit level and wealth less than 6,295 euros for single persons and 12,590 euros for couples is eligible for SA.

[5] The maximum exempted amount changes every six months. In January 2021, it was 220 euros; in July 2021, it was 221 euros; in January 2022, it was 224 euros; and in July 2022, it was 226 euros.

the temporary exemption (Divosa, 2019). This is most likely due to the temporary nature of the exemption

In 2021, each month about 360 thousand SA benefits were paid out in the Netherlands (Statistics Netherlands, 2023b), providing a guaranteed minimum income to about 420 thousand individuals of working-age (Statistics Netherlands, 2023a). This means that about 4 percent of adults in the Netherlands were receiving SA in 2021. In Amsterdam, about 35 thousand SA benefits were paid out each month in 2021 (Statistics Netherlands, 2023b), covering about 39 thousand adults (Statistics Netherlands, 2023c) or about 6 percent of the working-age population. In Rotterdam, 32 thousand SA benefits were paid out each month in 2021 (Statistics Netherlands, 2023b), covering about 37 thousand adults (Statistics Netherlands, 2023c) or 8 percent of the working-age population.

Labor market activation of SA recipients in the Netherlands has been a persistent challenge for municipalities and policymakers. A substantial share of recipients faces multiple barriers to employment, including low educational attainment, poor health, and family care responsibilities (Van Echtelt et al., 2023), leading to low exit rates from SA and limited effectiveness of activation policies. Each year, only about ten percent of SA recipients enters the labor market (Muffels, 2020) and 8 percent of SA recipients works part-time next to SA (Divosa, 2019). In Amsterdam, the share of part-time employment is slightly lower than the national average at about 7 percent (Van Kempen et al., 2021), while in Rotterdam, only about 5 percent of SA recipients works next to SA (Gemeente Rotterdam, n.d.).

Table 1

Specifics of temporary earnings exemption and increased earnings exemptions.

	Temporary exemption	Increased exemption (Amsterdam)	Increased exemption (Rotterdam)
% of income exempted	25	30	12.5
Maximum exemption per month (€)	220	219	219
Maximum duration	6 months	None	None
Granted to	Individuals	Households	Individuals
Relation with temporary exemption	-	Temporary exemption gets deducted	In addition to temporary exemption
Payment frequency	Monthly	Semiannually	Semiannually

Note. The table gives the specifications of the different earnings exemptions in April 2021. The maximum exemptions changed every six months for the temporary exemption and every twelve months for the increased exemptions. The maximum exemption for the temporary exemption was always about one to two euros higher than for the increased exemptions.

3.2 Increased earnings exemption

In 2021, the municipalities of Amsterdam and Rotterdam started trials with increased earnings exemptions for SA recipients, aiming to stimulate searching for and accepting work. This section discusses the setup of these trials. The differences between the general temporary earnings exemption and the increased exemptions offered in these trials are summarized in Table 1.

3.2.1 Amsterdam

Since March 2021, all SA recipients in Amsterdam are eligible for an increased earnings exemption for income earned in addition to the SA benefits. Whereas before, earnings were fully deducted from the SA benefit unless the recipient was still eligible for the temporary general earnings exemption, the increased earnings exemption allowed SA recipients in Amsterdam to keep a (larger) part of their earned income even if they had already used up the six months from the temporary earnings exemption. The increased earnings exemption replaced the temporary earnings exemption. Unlike the temporary exemption, there was no maximum duration on the increased earnings exemption.

The increased earnings exemption amounted to 30 percent of monthly income up to a maximum of 219 euros per month upon its implementation. Thus, the increased earnings exemption effectively reduced the marginal tax rate as long as monthly income was less than $\frac{219}{0.3} = 730$ euros per month. For SA recipients that were still eligible for the temporary exemption the marginal tax rate reduced from 75 to 70 percent. For SA recipients that were no longer eligible for the temporary exemption, the increased exemption reduced the marginal tax rate from 100 to 70 percent. The maximum amount of exempted earnings changed slightly each year. It started as 219 euros per month in 2021 and equaled 222 euros per month in 2022. The exempted percentage stayed the same.

In contrast to the temporary exemption, the increased exemption in Amsterdam was not paid out monthly, but every six months, implying that the monthly benefit remained the same as before the implementation of the increased exemption, but those with labor income received an extra amount every half year.

The increased earnings exemption in Amsterdam was paid out to households and not individuals. Hence, a couple on SA could only get an earnings exemption of 219 euros per month for their income taken together. Hence, for couples with already one person working part-time, there was no additional financial incentive for the other person in the couple to also start working.

When SA recipients earned more than the monthly benefit amount, they were no longer eligible

for SA and, hence, also not for the increased earnings exemption. However, total income from SA plus labor income (including the exempted earnings) right before this threshold was higher than total income from labor income right after exiting SA. This was the case, because the earnings exemption increased total income for SA recipients, but it did not change the earnings threshold that determines whether someone receives SA.

The effect of the increased earnings exemption in Amsterdam on the budget line of a household on SA benefits in April 2021 is shown in Figure 1. The budget lines without the increased earnings exemption differ between people who are still eligible for the temporary earnings exemption and those who already used up the six months of the temporary exemption. This difference is depicted in Figures 1a and 1b, respectively.

Figure 1a shows how the budget line of a single person on SA benefits was affected by the increased earnings exemption when this person was still eligible for the general temporary exemption. Budget line ABCDE is the situation without the increased earnings exemption but with the temporary exemption: Between point A and B, earnings are exempted at 25 percent up to a maximum of 220 euros per month. Between point B and C, the SA recipient receives the maximum temporary earnings exemption and any earnings above this maximum are fully deducted. At point C, earned income is exactly equal to the monthly benefit amount, but total income from SA and earned income equals earned income plus 220 euros. However, as soon as the household earns more than than this, they loose their entitlement to SA and to the 220 euros from the exemption, causing the drop from point C to D in the budget line. Budget line AFGDE gives the budget line under the increased earnings exemption. The line looks very similar, but is slightly steeper, as 30 percent of income is exempted under the increased exemption. Furthermore, the maximum exemption is slightly lower at 219 euros per month, so line FC is slightly lower than line BC.

Figure 1b shows how the budget line of a single person on SA benefits was affected by the increased earnings exemption for someone who was no longer eligible for the general temporary exemption. Budget line ABC is the situation without the increased earnings exemption and without the right to the temporary exemption: as there is no exemption, all income is fully deducted and total income from SA and labor remains constant between point A and B. For labor income beyond point B, the household is no longer eligible for SA. Hence, total income now consists only of labor income, and, thus, increases as labor income increases further. Budget line ADEBC gives the budget line in the case of the increased earnings exemption. Between point A and D, 30 percent of earnings are exempted up to a maximum of 219 euros per month. Between point D and E, the SA

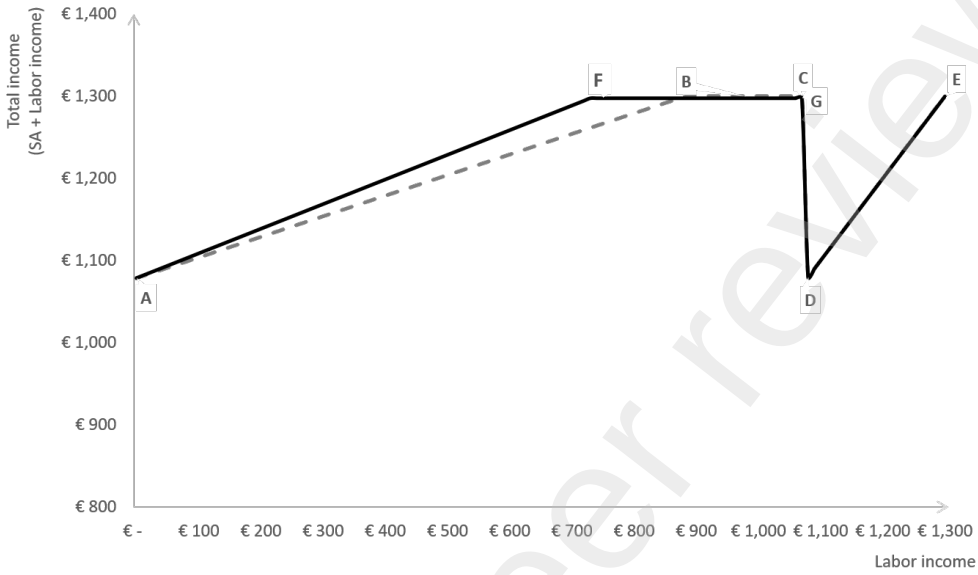
recipient receives the maximum temporary earnings exemption of 219 euros per month. At point E, earned income is exactly equal to the monthly benefit amount, but total income from SA and earned income equals earned income plus 219 euros. However, as soon as the household earns more than than this, they leave SA and lose the 219 euros, causing the drop from point E to B in the budget line.

In principle, all SA recipients in Amsterdam were eligible for the increased earnings exemption. However, for SA recipients younger than 27 years old, the exempted earnings were not paid out every half year, but were instead saved up for them. The saved up money was then paid out once the individuals turned 27 or exited SA.

Figure 1

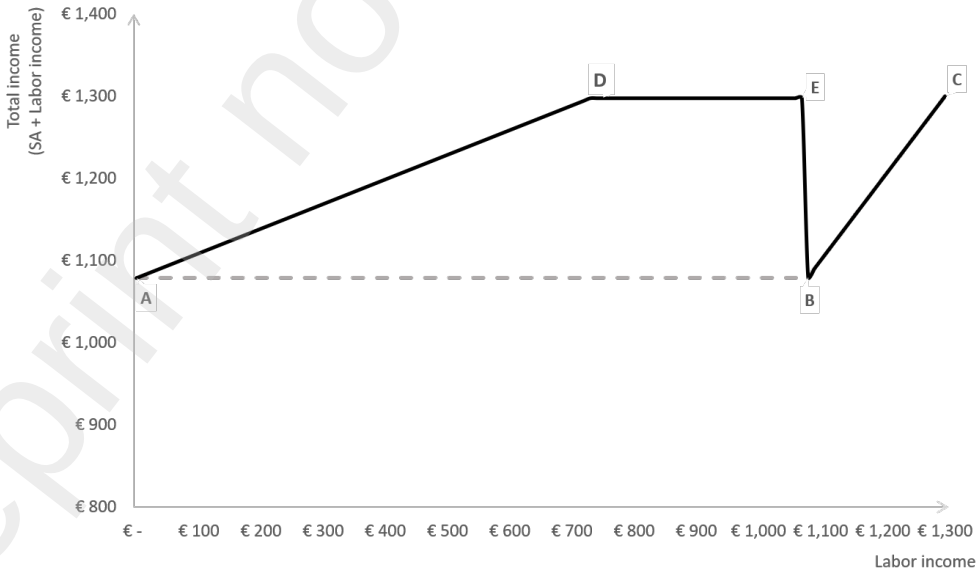
Budget lines with and without increased earnings exemption in Amsterdam.

(a) *With right to temporary exemption*



Note. The figure shows how the increased earnings exemption changes the budget line for SA recipients who are still eligible for the temporary earnings exemption in Amsterdam. ABCDE gives the budget line without the increased earnings exemption. AFGDE gives the budget line with the increased earnings exemption.

(b) *Without right to temporary exemption*



Note. The figure shows how the increased earnings exemption changes the budget line for SA recipients who are not eligible for the temporary earnings exemption in Amsterdam. ABC gives the budget line without the increased earnings exemption. ADEBC gives the budget line with the increased earnings exemption.

Finally, already since 2018, there has been an increased earnings exemption for part of the SA recipients in Amsterdam as part of an experiment. All 5,250 SA recipients who voluntarily applied to take part in this experiment, were eligible between 2018 and March 2021 for an increased earnings exemption of 50 percent up to a maximum of 200 euros per month. In March 2021, the increased earnings exemption studied in this paper was implemented and the SA recipients who took part in the previous experiment from then on received the same earnings exemption as all SA recipients in Amsterdam.

3.2.2 Rotterdam

In Rotterdam, a similar earnings exemption was implemented in April 2021. From the start, it was announced that this earnings exemption would be in place for two years, but that only individuals who were employed during the first year of the trial would be eligible for the exemption during the second year. The complete trial lasted until the end of March 2022.

The share of exempted earnings in Rotterdam was lower than in Amsterdam. It amounted to 12.5 percent of monthly earnings in Rotterdam instead of the 30 percent in Amsterdam. However, unlike Amsterdam, the increased earnings exemption in Rotterdam was offered in addition to the regular temporary earnings exemption. Thus, for SA recipients who had not yet used up their temporary earnings exemption, the total amount of exempted earnings then amounted to 37.5 percent of earned income. This implies that for SA recipients that were still eligible for the temporary exemption the increased earnings exemption effectively reduced the marginal tax rate from 75 to 62.5 percent. For SA recipients that were no longer eligible for the temporary exemption, the increased exemption reduced the marginal tax rate from 100 to 87.5 percent. In either case, the increased exemption could be maximally 219 euros per month. This point was only reached with monthly earnings of $\frac{219}{0.125} = 1,752$ euros. Note that at such an earnings level, someone would already have exited SA. Hence, this maximum earnings exemption could not be reached.

Similar to Amsterdam, the earnings exemption in Rotterdam was paid out every six months. However, in Rotterdam, individuals only got their first payment after they had at least six months of earnings after April 2021. These months did not have to be consecutive.

In Rotterdam, the earnings exemption was determined at the individual level. Hence, if someone received SA as a couple, both individuals could receive the increased earnings exemption if they met the eligibility criteria. This implies that the increased earnings exemption in Rotterdam still created financial incentives for SA recipients in a couple where the partner was already working.

As in Amsterdam, the earnings exemption in Rotterdam did not affect the threshold income that determined eligibility for SA. Hence, there is a drop in total income when individuals earn just more than the monthly benefit amount.

The effect of the increased earnings exemption in Rotterdam on the budget line of SA recipients in April 2021 is shown in Figure 2. The budget lines without the increased earnings exemption differ between people who are still eligible for the temporary earnings exemption and those who already used up the six months of the temporary exemption.

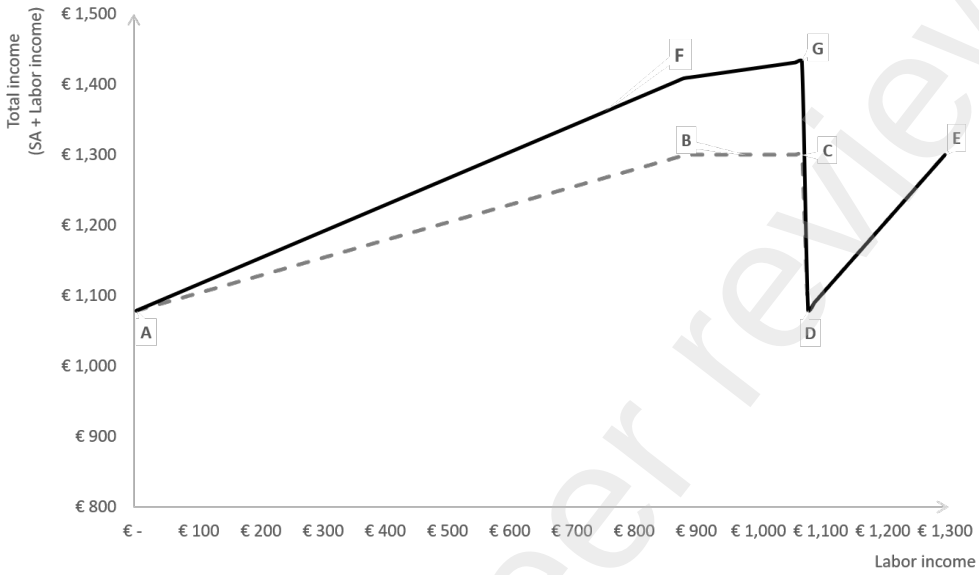
Figure 2a shows how the budget line of a single person on SA benefits was affected by the increased earnings exemption when this person was still eligible for the general temporary exemption. Budget line ABCDE is the situation without the increased earnings exemption but with the temporary exemption. This is the same in Rotterdam as in Amsterdam. Budget line AFGDE gives the budget line in the case of the increased earnings exemption. The line looks very similar to the one in Amsterdam, but is steeper between point A and F, as 37.5 percent of income is exempted under the combination of the increased and temporary exemptions. At point F, the temporary exemption is at its maximum, but the increased exemption is not. Thus, between points F and G, still 12.5 percent of additional income is exempted. At point G, labor income is equal to the monthly SA benefit amount. Just beyond point G, the individual exits SA and loses the right to both the temporary and the increased exemptions. This causes the drop in income from point G to point D.

Figure 2b shows how the budget line of a single person on SA benefits was affected by the increased earnings exemption for someone who was no longer eligible for the general temporary exemption. Budget line ABC is the situation without the increased earnings exemption and without the right to the temporary exemption. This is also the same in Rotterdam as in Amsterdam. Budget line ADBC gives the budget line in the case of the increased earnings exemption. Between point A and D, earnings are deducted at 12.5 percent up to a maximum of 219 euros per month. As this maximum is never reached while on SA, there is no flat portion of the budget line in Rotterdam. Total income just keeps increasing until at point D, earned income is exactly equal to the monthly benefit amount. At point D, total income from SA and earned income equals $1,075.44 * 1.125 = 1,209.87$. As soon as the household earns more than than this, they leave SA and lose the exemption, causing the drop from point D to B in the budget line.

Figure 2

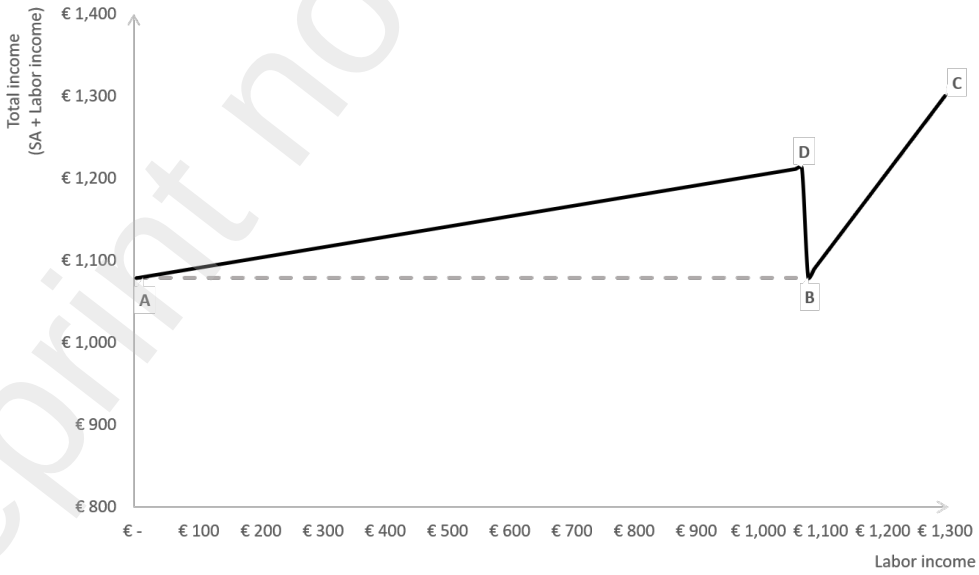
Budget lines with and without increased earnings exemption in Rotterdam.

(a) *With right to temporary exemption*



Note. The figure shows how the increased earnings exemption changes the budget line for SA recipients who are still eligible for the temporary earnings exemption in Rotterdam. ABCDE gives the budget line without the increased earnings exemption. AFGDE gives the budget line with the increased earnings exemption.

(b) *Without right to temporary exemption*



Note. The figure shows how the increased earnings exemption changes the budget line for SA recipients who are not eligible for the temporary earnings exemption in Rotterdam. ABC gives the budget line without the increased earnings exemption. ADEBC gives the budget line with the increased earnings exemption.

In Rotterdam, all individuals over 27 years old were eligible for the increased earnings exemption. Individuals that were between 26 years and 7 months and 27 years old, could already start saving up the exempted earnings, but it would not be paid out before they turned 27. Hence, effectively, the incentives from the increased earnings exemption were affecting all SA recipients aged at least 26 years and 7 months.

4 Hypotheses

This section briefly discusses the effects expected effects of the increased earnings exemptions, based on economic theory. I start by discussing the hypothesized effects on employment while on SA in the extensive and intensive margin, followed by the effects on labor income while on SA and exits from SA.

The increased earnings exemptions create financial incentives to work. As the earnings exemptions increase the value of an hour of work, they incentivize SA recipients without work to start working (Verlaat, 2022). This effect can only be zero or positive. Theoretically, it cannot be negative, as SA recipients can only benefit from the earnings exemptions when they work.

The increased earnings exemptions also create incentives to work more in the intensive margin. As long as individuals earn less than the income that gives them the maximum possible exemption, there are incentives to increase the number of working hours, as this will increase total earnings. However, the effect size depends on the relative sizes of the income and substitution effects (Verlaat, 2022). Theoretically, the effect on the number of hours worked could even be negative. However, empirically, labor supply elasticities are usually found to be positive, especially among lower-income households (Bargain et al., 2014; Boeri & Van Ours, 2014). Above the maximum exempted earnings, there are no more incentives to increase the number of hours of work (Verlaat, 2022). However, if part-time work leads to full-time work (as suggested by findings in Boschman et al. (2021), Elshout and Bos (2023), and Lietzmann et al. (2017)[6], but contrasted by Benghalem et al. (2021) and Eppel and Mahringer (2019)), there may still be a positive effect on employment in the intensive margin for individuals earning above the maximum exempted amount.

Moreover, the increased earnings exemptions increase the incentive to have a higher labor income. An SA recipient can increase their labor income in two ways. First, the SA recipient can increase their working hours, which increases their labor income as long as their hourly wage

[6]Lietzmann et al. (2017) only find positive effects of marginal employment on exits for recipients of unemployment benefits who take up marginal employment several months after the start of their benefit spell.

remains the same; second, they can look for a job that pays them a higher hourly wage and keep working the same number of hours. Through either mechanism, the increased earnings exemptions would have a positive effect on the labor income of SA recipients.

The effect on exits from SA is a combination of two opposing mechanisms. First, because SA recipients lose their earnings exemption once they leave SA, there are reduced incentives to exit SA. The loss of the additional earnings from the earnings exemption, causes a sudden drop in the income of SA recipients who do exit (see also Figures 1 and 2). Thus, earning just enough to exit SA becomes relatively less attractive than working part-time while additionally receiving SA benefits. Second, working part-time while on SA can work as a stepping stone to exit from SA (Boschman et al., 2021; Elshout & Bos, 2023; Lietzmann et al., 2017). If the earnings exemptions increase part-time employment among SA recipients, exits may also increase as a result of this stepping-stone effect.

5 Data

This study uses administrative microdata from Statistics Netherlands to estimate the effects of the increased earnings exemption for social assistance recipients in Amsterdam and Rotterdam. I construct a monthly panel data set, containing all social assistance recipients in January 2020 that are aged between 18 and 65 years old and follows them until April 2023.[7]

For each household, the data set contains monthly information on whether they received social assistance payments, the reason why a benefit spell might have ended, whether at least one person in the household has labor income, total labor income in the household, the number of hours of paid labor in the household, whether the social assistance benefit is paid out to a single person or a couple, the number of children in the household, and their age. Non-time-varying information is added on the gender of the adults in the household, the highest education level of the main social assistance recipient, the age of the adults in the household, the number of months of work experience between 2014 and 2018, and whether any of the adults in the household took pain or mental health medication in 2020.

[7] From a policy perspective, effects at the household level are of most interest, as SA is a household-level benefit in the Netherlands. Hence, if one of the adults in the household earns more than the monthly benefit amount, the whole household loses its right to SA. Furthermore, in Amsterdam, the incentives of the increased earnings exemption are working at the household level and not at the individual level. Finally, the choice for a household-level panel likely has limited effects on the estimated coefficients as only 18 percent of the SA households receives SA as a couple. The others are single-person SA receivers. Thus, the choice between a household- or individual-level panel affects only about 18 percent of the observations.

Table 2 gives descriptive statistics on the treatment and control group both before and after the implementation of the increased earnings exemption. The treatment group consists of the SA recipients from Amsterdam and Rotterdam, and the control group consists of SA recipients from four other municipalities that did not have an increased earnings exemption.[8]

The table shows that households in the two treated municipalities differ from households in the control municipalities in several respects, both before and after the implementation of the increased earnings exemption. SA recipients in Amsterdam and Rotterdam are, on average, 1 to 2 years older than those in the control municipalities. Households receiving SA in Amsterdam and Rotterdam also contain slightly more females, on average, and have slightly fewer children than SA households in the control municipalities. Moreover, SA recipients in Amsterdam and Rotterdam have less recent work experience than SA recipients in the control municipalities. Additionally, SA recipients in Amsterdam and Rotterdam are more often middle-educated and less often lower- and high-educated than in the control municipalities. Finally, in Amsterdam and Rotterdam, fewer SA recipients are single parents, compared to the control municipalities.

To account for possible confounding effects of these differences in independent variables, I include them as control variables in the regressions.

Figure 3 shows the raw trends in the outcome variables for the treatment and control group. The employment rate, hours worked, and labor income among SA recipients in the treatment municipalities are lower than in the control municipalities, but the trends move parallel to each other. The monthly exit rates, on the other hand, are about as large in the treatment and control municipalities. All outcomes depict a sharp decline at the beginning of 2020. This is a result of the COVID-19 pandemic. Visual inspection of the figure provides the first suggestive evidence of a positive effect of the increased earnings exemptions on employment, hours worked, and labor income, as the difference in trends decreases slightly after the introduction of the earnings exemptions.

[8] See Section 6 for more information on the how the treatment and control groups are determined.

Table 2

Descriptive statistics

	Pre			Post		
Variables	Treatment	Control	T-C	Treatment	Control	T-C
Age of first adult	47.399	45.641	1.759***	49.066	47.308	1.758***
Age of second adult	44.777	43.777	1.000***	46.889	45.994	0.895***
No females of females = 0	0.400	0.404	-0.003***	0.411	0.417	-0.006***
Number of females = 1	0.599	0.596	0.004***	0.589	0.582	0.007***
Number of children	0.763	0.777	-0.014***	0.738	0.754	-0.016***
Recent work history (months)	5.586	6.565	-0.979***	5.509	6.454	-0.945***
Highest education = Low	0.594	0.599	-0.005***	0.594	0.599	-0.005***
Highest education = Middle	0.307	0.298	0.009***	0.307	0.298	0.009***
Highest education = High	0.100	0.104	-0.004***	0.100	0.104	-0.004***
Single SA receiver	0.701	0.689	0.013***	0.710	0.693	0.017***
Couple SA receiver	0.182	0.178	0.004**	0.171	0.170	0.001***
Single parent SA receiver	0.116	0.133	-0.017***	0.120	0.137	-0.018***
N (x1,000)	936	585		1,604	1,002	

Note. T-C is the difference between the treatment (T) and control (C) group. The treatment group consists of households on SA from Amsterdam and Rotterdam. The control group consists of households on SA from Almere, Nijmegen, Utrecht, and The Hague. N is the number of observations. P-values of a t-test of equal means are denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All variables are measured at the household level, except age and highest education. Highest education concerns only the highest education of the main SA recipient.

Figure 3

Trend plots of the outcome variables in treatment and control municipalities.

Note. The treatment group consists of households on SA from Amsterdam and Rotterdam. The control group consists of households on SA from Almere, Nijmegen, Utrecht, and The Hague. The vertical line symbols the start of the treatment in April 2021. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The analysis on labor income excludes SA households with monthly labor income above 1,800 euros (0.2% of the sample). These data points are most likely the result of administrative errors, as such an income is sufficiently above the SA means test that it is no longer plausible that these households were actually eligible for SA during those months.

6 Methodology

To determine the effect of the increased earnings exemptions in Rotterdam and Amsterdam, I exploit regional variation in the implementation of the earnings exemption in a difference-in-difference design (DD). Only Amsterdam and Rotterdam offered increased earnings exemptions, while other large municipalities did not. Accordingly, I use households from non-treated municipalities as control units for those in the treated municipalities Amsterdam and Rotterdam.

This study focuses on the effect of the increased earnings exemptions on a set of labor market outcomes. I study the effect of the increased earnings exemptions on the share of working SA recipients, the number of hours of work next to SA, labor income in addition to SA, and on the exit rate from SA. These outcomes are defined at the household level, and labor income is log-transformed (see also Section 5).

6.1 Difference-in-differences (DD)

I use a dynamic two-way fixed effects (TWFE) estimator to estimate the effects of the increased earnings exemptions in Amsterdam and Rotterdam. I regress the dependent variables of interest y_{it} for household i at time t on a constant α, a treatment dummy D_i, which equals one if a household lives in Rotterdam or Amsterdam, time-fixed effects γ_t, the time-varying DD interaction term $D_i \cdot T_t$, which estimates the time-varying differences between households living in the treatment municipalities, compared to living in the control municipalities, and a set of control variables X_{it}:

$$y_{it} = \alpha + \beta D_i + \gamma_t + \sum_{t \in T} \delta_t^{DD}(D_i \cdot T_t) + \zeta X_{it} + \epsilon_{it}, \tag{1}$$

where δ_t^{DD} are the dynamic DD coefficients of interest.

This TWFE estimator yields unbiased estimates of the treatment effects in this setting because treatment is binary, changes only once from non-treated to treated, and there is no differential treatment timing (De Chaisemartin & d'Haultfoeuille, 2022). This is a special case, because in other settings with multiple time periods, the TWFE estimator can result in biased estimates due to so-called "forbidden comparisons" (Borusyak et al., 2022; De Chaisemartin & d'Haultfoeuille, 2020; De Chaisemartin & d'Haultfoeuille, 2022; Goodman-Bacon, 2021). Such issues do not arise here, making the TWFE estimator an appropriate estimator for the treatment effects of the increased earnings exemptions.

This DD specification gives the average treatment effect of the increased earnings exemption for social assistance recipients in Amsterdam and Rotterdam if the non-treated municipalities are valid counterfactuals for Amsterdam and Rotterdam. This is the case when two assumptions are satisfied: the parallel trends assumption and the stable unit treatment value assumption (SUTVA).

The parallel trends assumption states that if there had been no earnings exemption, the treated and control units should have developed similarly over time. However, since there is an increased earnings exemption, we do not observe what would have happened if there had been no earnings exemption. Hence, it is not possible to test the validity of the parallel trends assumption directly. Instead, I make the parallel trends assumption plausible by testing for parallel trends before the implementation of the increased earnings exemptions. Moreover, in all analyses, I use only control municipalities that are similar to the treated municipalities in terms of number of social assistance recipients. The findings in Section 7 show that the trends before the implementation of the earnings exemptions developed parallel to each other, providing evidence for the validity of the parallel trends assumption.

To ensure that the control municipalities are as similar as possible to the treated municipalities, I include only municipalities with a substantive SA population. Amsterdam and Rotterdam have the largest SA population of the Netherlands with, respectively, around 40,000 and around 37,000 social assistance recipients per year (Statistics Netherlands, 2023b). Therefore, I use only use municipalities with at least 5,000 social assistance recipients per month as control municipalities. Furthermore, I restrict the control municipalities to those that have not implemented or announced the implementation of similar financial incentives for social assistance recipients. The resulting control municipalities are Almere, Nijmegen, Utrecht, and The Hague. As part of the robustness checks in Section 8, I show that the baseline findings are robust to the choice of control municipalities.

The SUTVA requires that there are no spillovers between the treatment and control units. In this setting, that implies that social assistance recipients from control municipalities should not move to a treated municipality because they expect to gain from the increased earnings exemption. In Rotterdam, this is very unlikely, since someone was only eligible for the increased earnings exemption when they were working during the first year of the trial, and this was announced from the start of the treatment. Therefore, there was little incentive for social assistance recipients to move to Rotterdam, because the time period during which one could become eligible was short. At the time of writing, the earnings exemption in Amsterdam is still in place indefinitely, which increases

the risk of individuals moving to Amsterdam because of the increased earnings exemption. To account for individuals who possibly actively select into treated municipalities, I remove individuals who change municipalities within the observed time period.[9] This ensures that individuals were already living in a treatment or control municipality in 2020, before the implementation of the increased earnings exemptions were announced.

I estimate the effects for SA recipients in Amsterdam and Rotterdam combined. Since the increased earnings exemption started one month later in Rotterdam than in Amsterdam, I remove March 2021 from the data in the main analysis as, in this month, Amsterdam was already treated but Rotterdam was not. The pre-treatment period then consists of all months from January 2020 to February 2021 and the treatment period of all months from April 2021 to April 2023. This allows for studying the effects of the earnings exemptions during the first two years after they were implemented.

P-values corresponding to the regression coefficients are calculated using wild subcluster bootstrap. Clustered p-values are necessary, because treatment is determined at the municipality level, but all variables are observed at the individual level, which leads to correlation between individuals within municipalities and over time. However, due to the limited number of clusters, a regular sandwich estimator for the standard errors would over-reject the null hypothesis of zero coefficients (Cameron et al., 2008). A wild subcluster bootstrap with individual-level clusters is the most appropriate method for estimating the standard errors (MacKinnon & Webb, 2018). However, such an approach is very computationally intensive. Therefore, I opt for a more conservative wild cluster bootstrap correction for the p-values, which has lower computational complexity. The regular wild cluster bootstrap tends to under-reject in a difference-in-differences setting with few treated clusters, but therefore prevents overstatement of the policy impact.[10]

7 Results

In this section, I present the estimated baseline effects of the increased earnings exemptions in Amsterdam and Rotterdam on employment, the number of hours worked, and labor income while on SA, and on the exit rate from SA. First, I discuss the pooled results, before going into the

[9] This reduces the sample size only slightly by about 2 percent and has little effect on the estimated coefficients (see Appendix B).

[10] An often-used alternative choice for the standard errors would be to cluster at a lower level than at the municipality level, such as the individual level. In Appendix B.3, I show that the baseline findings would not differ with another choice of standard error clustering.

separate estimation results from Amsterdam and Rotterdam.

7.1 Baseline

Figure 4 shows the estimated baseline effects of the increased earnings exemptions on employment, the number of hours worked, and labor income while on SA, and on the exit rate from SA, for the full sample. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. The dotted vertical line represents the time of treatment implementation.

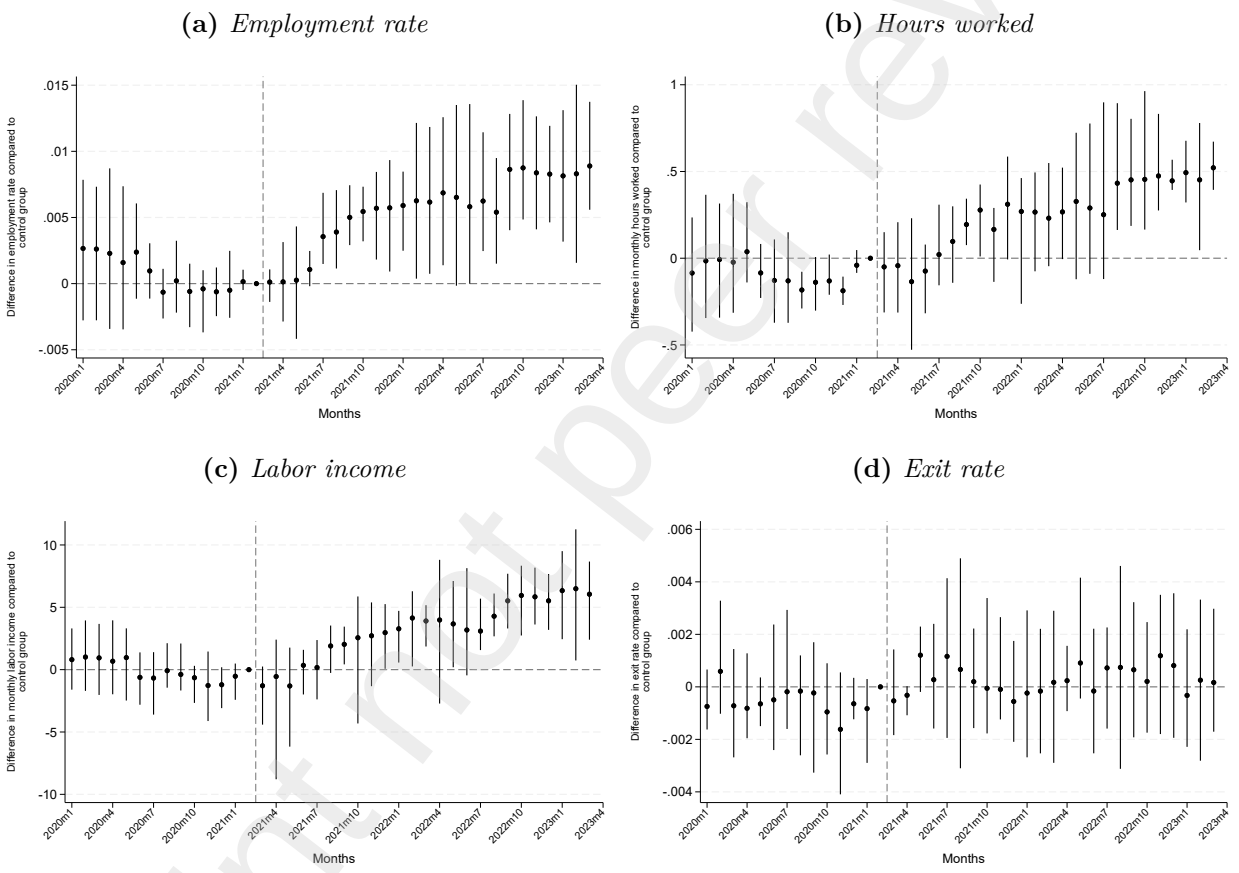
Figure 4a shows positive and significant coefficients for the effect of the increased earnings exemptions on employment among SA recipients after the exemptions were implemented. The effect is close to zero during the first four months after the implementation and then increases to about 0.9 percentage points two years after the implementation. Almost all monthly treatment effects are statistically significant at a five percent confidence level. Moreover, the estimated coefficients before the implementation of the treatment are all not significantly different from zero, providing evidence for the plausibility of the parallel trends assumption. This implies that the increased earnings exemptions in Amsterdam and Rotterdam had a positive effect on employment among SA recipients of about 0.9 percentage points, two years after they were implemented. Compared to an employment rate of about 5.8 percent in the treatment group before treatment, this implies an increase in the employment rate of SA recipients of about 16 percent.

Similarly, Figure 4b also shows a clear upward trend in the estimated effects of the increased earnings exemptions on the number of hours worked by SA recipients. During the first six months after the implementation, the estimated effects are not significantly different from zero. Between seven and seventeen months after the implementation, the coefficients are larger and on the border of significance at 95 percent confidence, switching between just significant and just non-significant between months. From month eighteen onward, all coefficients become significantly different from zero, and equal to about 0.5 hours worked per month. The estimated coefficients from before the implementation of the earnings exemptions are also mostly non-distinguishable from zero, except for the last four months of 2020. During these months, the estimated coefficients are slightly negative and significantly different from zero, implying a slight violation of the parallel pre-trends. However, this violation is mostly due to smaller standard errors during these months, while the size of the coefficients is more or less equal to that in the two months prior. To ensure that this violation is

not a sign of structural differences between the treatment and control groups, I perform several robustness checks on the choice of standard errors and control group in Section 8. Compared to the pre-treatment number of hours worked in the treatment group, the increased earnings exemption has led to an increase in the monthly number of hours worked by SA recipients of about 17 percent, two years after the treatment was implemented.

Figure 4

Baseline difference-in-differences results for Amsterdam and Rotterdam combined.



Note. Estimation results from Equation 1 for the full sample. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

Most of the increase in hours worked is due to increased employment in the extensive margin.

As fewer SA recipients work zero hours, the average hours worked among all SA recipients increases. This explains over 90 percent of the increase in average hours worked. However, also the average hours worked among SA recipients with nonzero hours went up after the implementation of the increased earnings exemptions. Before the increased exemptions, the average hours worked of employed SA recipients in Amsterdam and Rotterdam was about 50.2 hours per month ($\frac{2.91}{0.058}$). Two years after the implementation, this had increased to about 50.9 hours per month ($\frac{2.91+0.5}{0.058+0.009}$). This explains the remaining 10 percent of the increase in average hours worked.

Furthermore, Figure 4c shows positive effects of the increased earnings exemption on the monthly labor income among SA recipients in Amsterdam and Rotterdam. As was the case for the employment rate and the number of hours worked, the estimated effect grows over time. During the first five months after the treatment was implemented the treatment does not seem to take effect yet. Afterwards, the coefficients increase to about 6.3 euros, two years after the treatment implementation. Most monthly treatment coefficients are significantly different from zero at 95 percent confidence. Moreover, before the implementation of the treatment, the regression does not pick up any differences in trends between the treatment and control groups, providing evidence for the validity of the parallel trends assumptions. Taken together, this implies that the increased earnings exemptions in Amsterdam and Rotterdam have led to an increase in the monthly labor income of SA recipients of about 6.3 euros per month. Compared to the average labor earnings in the treatment group before treatment of about 32.8 euros per month, this implies a relative increase in labor earnings of SA recipients of about 19 percent.

As for hours worked, most of the increase in labor income is due to increased employment in the extensive margin. About 83 percent of the increase in average labor income is due to a higher share of employed SA recipients. The remaining 17 percent is about equally divided between higher labor income due to more hours worked per person (8%) and due to higher wages (9%).

Finally, Figure 4d shows coefficients for the effect on exits that are not significantly different from zero, both before and after the implementation of the treatment. Hence, there is evidence for the validity of the parallel trends assumption, but no evidence that the increased earnings exemptions affected exits from SA.

In conclusion, the baseline results indicate that the increased earnings exemptions have had a positive and growing effect on the employment rate, the number of hours worked, and monthly labor earnings of SA recipients. Two years after the exemptions were implemented, employment in the extensive margin had increased by about 0.9 percentage points (16%), and by approximately

0.5 hours per month (17%) in the intensive margin. Monthly labor earnings increased by about 6.3 euros (19%). The analysis finds no evidence of an effect of the increased earnings exemptions on exits from SA. These results are in line with the hypotheses from Section 4. The absence of a significant effect on exits can be explained by the counteracting mechanisms of, on the one hand, lower financial incentives for exits from SA, and, on the other hand, the stepping-stone effect of part-time employment. These effects might cancel each other out.

These findings generally corroborate the findings in previous empirical literature that finds either no or positive effects of earnings exemptions on exits, employment, and earnings (e.g. Knoef and Van Ours (2016), Palviainen (2023), Verlaat and Zulkarnain (2022)). Compared to Knoef and Van Ours (2016), who studied the effects of increased earnings disregards for single mothers in the Netherlands, the estimated effects on employment in this study are smaller. This can be explained by the fact that women and single parents are generally found to have a relatively elastic labor supply (e.g. Meghir and Phillips (2010), or De Boer et al. (2020) for estimates of labor supply elasticities in the Netherlands). Hence, the full population of SA recipients should respond less to earnings exemptions than single mothers on SA. However, similar to Knoef and Van Ours (2016), I find no significant effect on exits from SA. The positive effects on employment corroborate earlier qualitative findings from Elshout and Bos (2023), who conclude that SA recipients in Amsterdam felt motivated by the increased earnings exemption, because the earnings exemption felt like a recognition of their work. The growing effects over time can be explained as a result of the time it takes for information on the policy to reach SA recipients, of initial distrust of policy changes, or of adjustment costs that individuals may face (Zaresani, 2020).

Taken together, these findings suggest that an increased earnings exemption for SA recipients can stimulate labor market participation and that this does not reduce exits from SA. Hence, "making-work-pay" policies can be an effective way to stimulate labor force participation of SA recipients.

7.2 Heterogeneity analysis

Figures 5, 6, 7, and 8 show the heterogeneity of the baseline effects across different subgroups of SA recipients. I check for heterogeneity along several dimensions: age, gender, presence of children in the household, and suffering from mental and/or physical health issues. Additional heterogeneous results are available in Appendix A.

Figure 5 compares the effects of the increased earnings exemption between three age groups of

single[11] SA recipients: young (18-35 years old), middle-aged (35-50 years old), and older (50+ years old) SA recipients. The figure shows clear heterogeneous effects of the increased exemptions. Young SA recipients respond strongest to the policy. The higher earnings exemptions led to an increase in their employment rate of approximately 2 percentage points (40%), which is about 2.5 times as large as the average effect. Moreover, hours worked per month among young SA recipients increased by about 0.9 hours (38%), which is also more than twice the average. The same holds for labor income, which increased by about 10 euros per month (40%) among young SA recipients. However, older SA recipients seem to be hardly affected by the increased exemptions. Their employment rate, hours worked, and labor income do not increase significantly because of the increased exemptions. The effects for middle-aged SA recipients fall between those of young and older SA recipients and are still larger than the average effects for the full sample of SA recipients. Regarding the effects on the exit rate from SA, there are no differences between the age groups. In all three age groups, the increased exemptions have no significant effect on the exit rate from SA. These findings are in line with general notions that younger SA recipients are generally easier to activate and tend to have shorter SA spells. Older SA recipients are, on average, much more difficult to activate, and my findings show that increasing financial incentives for this group also barely affects their labor market participation.

Figure 6 shows that the effects of the earnings exemptions on employment, hours worked, and labor income are about twice as large for women as for men, two years after the implementation.[12] As employment, hours worked, and labor income were actually slightly lower among women before the implementation of the earnings exemptions, the relative effect on women is even larger. These results corroborate previous findings in the literature, that show that women tend to be more responsive to financial incentives than men, especially single mothers (Meghir & Phillips, 2010; Palviainen, 2023). About 34 percent of the women in the treatment group is a single mother.

The effects of the earnings exemptions on employment, hours worked and labor income are somewhat larger for household with children. This is depicted in Figure 7. The effect on employment is about 1.2 percentage points among households with children and about 0.6 percentage points among households without children. However, compared to their pre-treatment levels, the relative increase is actually slightly larger for households without children. The number of hours worked

[11]This analysis includes only single SA recipients, to distinguish age groups clearly. Within couples, the ages of the recipients can fall within different categories, such that categorization into age groups is not straightforward.

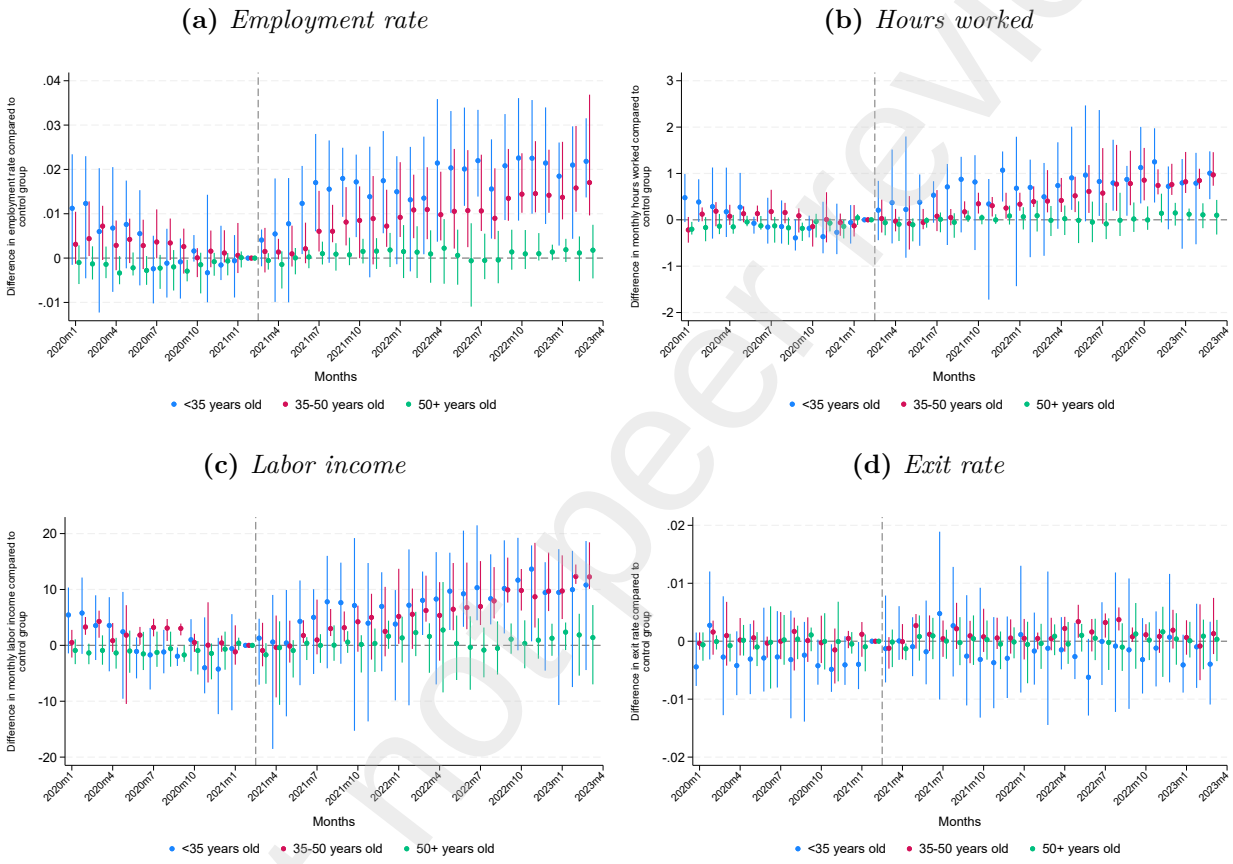
[12]This heterogeneity analysis is limited to single-person SA recipients to distinguish clearly between male and female SA recipients. This means that the analysis compares single men and single women, and does not allow for conclusions about the within-couple division of labor.

also increases more among households with children (+0.7 versus +0.3 hours per month). When compared to the pre-treatment levels, the relative increase is slightly larger among households with children. The effect on labor income is also slightly larger among households with children than among households without children (+9.3 euros versus +4.1 euros). Compared to the pre-treatment levels, the relative increase is again slightly larger among households with children. The additional heterogeneity analyses in Appendix A show that the larger effects for households with children are mostly driven by households with one or two kids aged below twelve. These findings are in line with the literature that shows that parents of younger children have a relatively elastic labor supply (De Boer et al., 2020; Meghir & Phillips, 2010). As was the case for the other heterogeneity analyses, there are no structural differences in the effects on exits between the subgroups. Although households with children are the only subgroup for which I find some suggestion of a positive effect on exits, albeit small and not consistently present in each month.

Finally, two years after the implementation of the increased exemptions, the effects on employment, hours worked and labor income are about twice as large for social assistance recipients without mental or physical health issues, compared to those with mental or physical health problems, as shown in Figure 8. The positive employment effects also come about earlier for those without health issues. Namely three months after the implementation of the increased earnings exemptions, employment, hours worked and labor income of SA recipients without mental or physical health problems already start to increase because of the earnings exemptions, while it remains unaffected for those with mental or physical health issues. After about nine months, the labor market outcomes for the group with health problems also seem to be improving, although these coefficients are mostly not significantly different from zero. There are no differences in the effects on exits from SA between the two groups. These findings are not surprising, as they show that healthier SA recipients respond more strongly to the financial incentives created by the earnings exemptions. These SA recipients are generally easier to activate and tend to have shorter SA spells.

Figure 5

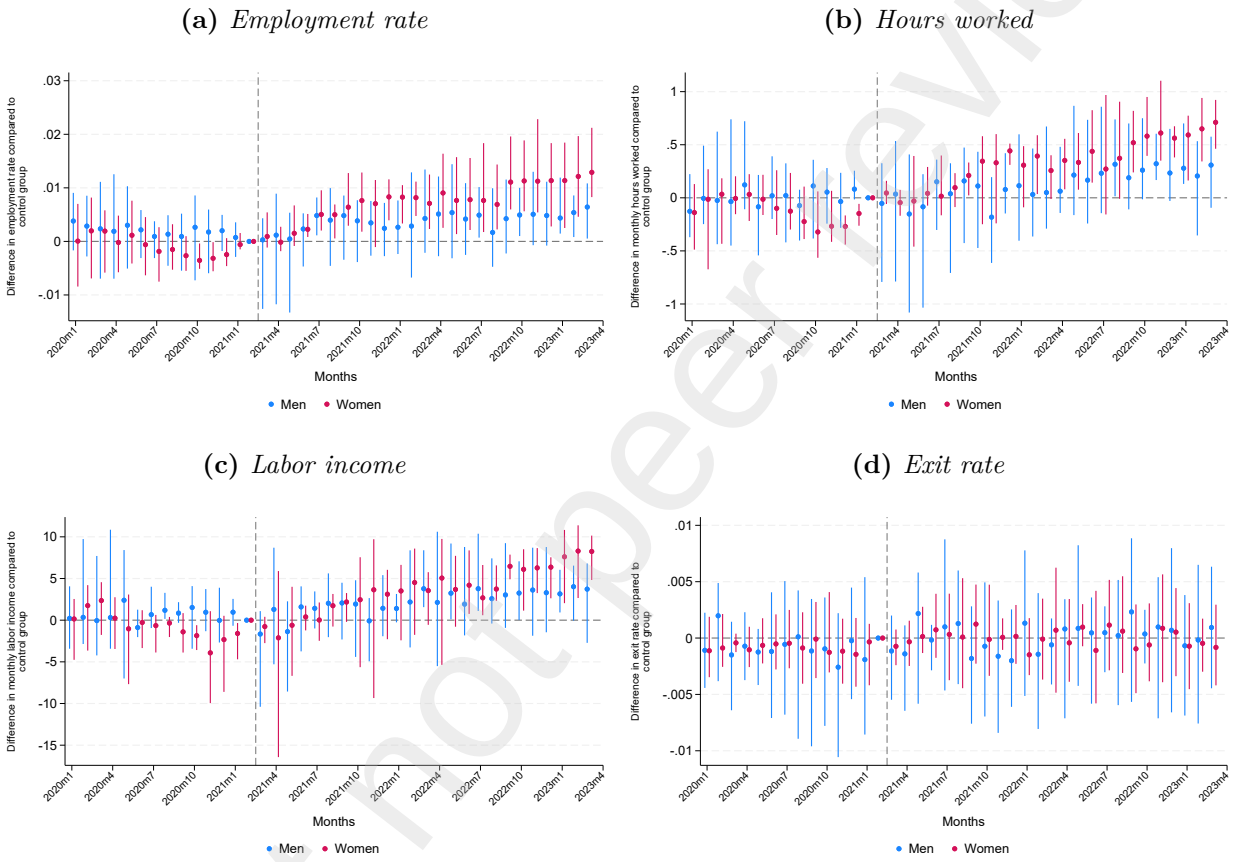
Baseline difference-in-differences results for Amsterdam and Rotterdam combined, by age groups.



Note. Estimation results from Equation 1 by age groups. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. Sample includes only single SA benefit recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

Figure 6

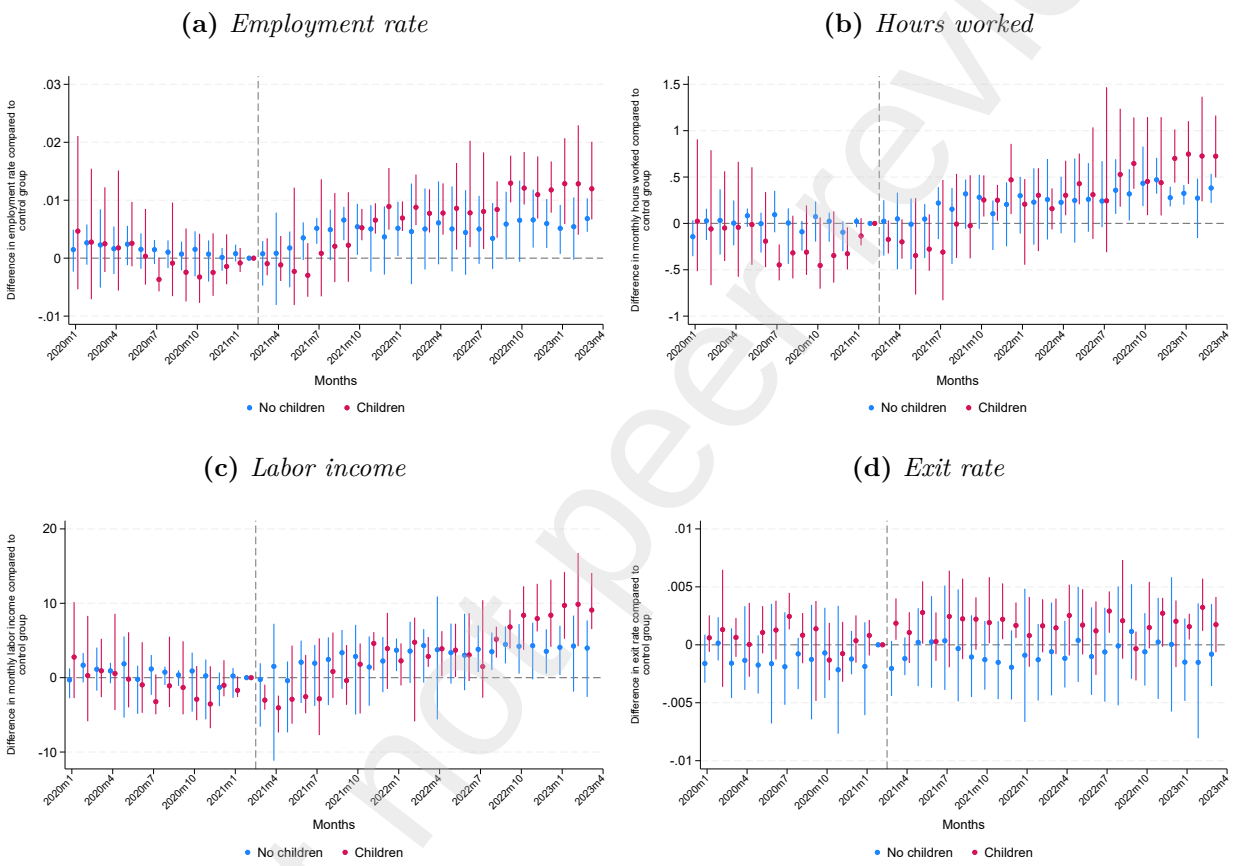
Baseline difference-in-differences results for Amsterdam and Rotterdam combined, by gender.



Note. Estimation results from Equation 1 by gender. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. Sample includes only single SA benefit recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

Figure 7

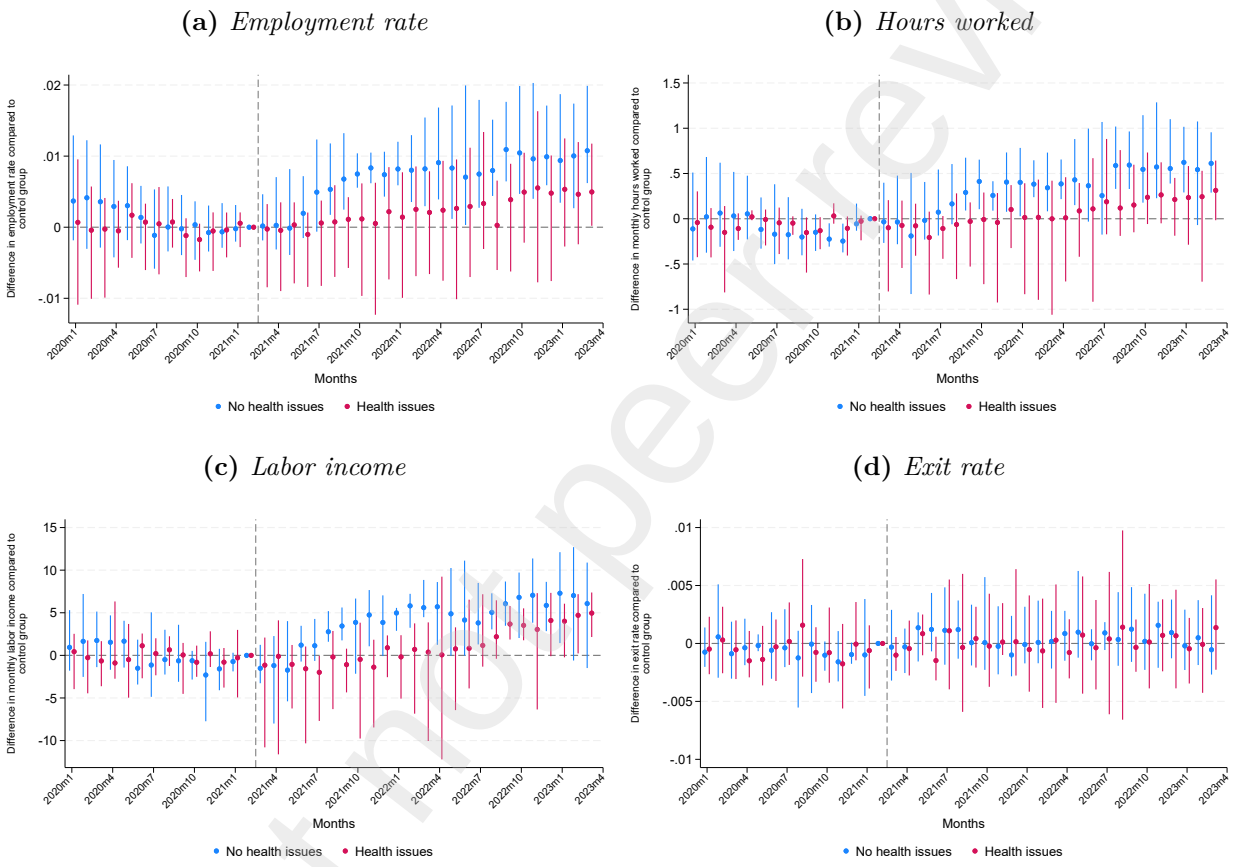
Baseline difference-in-differences results for Amsterdam and Rotterdam combined, by presence of children.



Note. Estimation results from Equation 1 by presence of children in the household. Appendix A shows additional heterogeneous effects by age of the youngest child. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

Figure 8

Baseline difference-in-differences results for Amsterdam and Rotterdam combined, by usage of mental or physical health medication at baseline.



Note. Estimation results from Equation 1 by usage of mental or physical health medication in the household at baseline. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

8 Robustness and placebo checks

This section discusses the results from the robustness checks. First, I show that other selections of control municipalities lead to similar effects as the baseline estimation. Second, I present the results from multiple placebo regressions with alternative treatment municipalities, finding no effects.

8.1 Robustness to choice of control municipalities

I test the robustness of the baseline findings to the choice of control municipalities. The control municipalities in the baseline analysis are chosen such that they are as similar as possible to the treated municipalities, without having implemented or announced the implementation of any new financial incentives for SA recipients during the observed time period. This improves the validity of the parallel trends assumption. To ensure that the baseline effects are not driven by this choice of control municipalities, I run two robustness analyses. In the first analysis, all municipalities except Amsterdam and Rotterdam are used as control municipalities. In the second analysis, only Utrecht and The Hague are used as control municipalities.

The results in Appendix B.1 and B.2 show that the choice of control municipalities barely affects the findings. They show that using all municipalities as control municipalities leads to effects similar to the baseline findings, both in size and significance. However, as expected, the pre-trends become less clean, with some pre-treatment coefficients being significantly different from zero. When using only Utrecht and The Hague as control municipalities, the estimated effect sizes do not differ from the baseline findings, but the standard errors are larger. This is due to the fact that the sample size is about 10 percent smaller when not including Almere and Nijmegen in the analysis.

8.2 Placebo tests with alternative treatment municipalities

To ensure that the results are actually the effect of the increased earnings exemption, I run several placebo regressions with alternative treatment municipalities. These municipalities did not have an increased earnings exemption in place at the time. Hence, the DD estimator should find no significant changes in the trends in the outcome between the placebo treatment municipalities and the other control municipalities. First, I use the control municipalities from the baseline estimations as (placebo) treated municipalities instead of Amsterdam and Rotterdam, and exclude Amsterdam and Rotterdam from the sample. Next, to ensure that the results are not driven by size differences between the treatment and control municipalities, I run a placebo regression, where

the control municipalities from the baseline estimation are used as treatment municipalities and several smaller municipalities are used as the control group. Last, to exclude confoundedness with local labor market conditions, I rerun the baseline estimations, but with the two largest neighboring municipalities of Amsterdam and Rotterdam as placebo treatment municipalities.

The results from the placebo regressions are given in Appendix B. Appendix B.5 shows the results from the first placebo tests. When using Almere, Nijmegen, The Hague, and Utrecht as placebo treatment municipalities, the regressions do not pick up any significant differences in the labor market outcomes of SA recipients in the placebo treatment municipality, compared to the control municipalities. When comparing the control municipalities to several smaller municipalities, the placebo regressions also do not pick up any significant effects, as shown in Appendix B.6. This implies that the estimated baseline effects are not driven by differences in municipality size. Finally, in the last placebo test, presented in Appendix B.6, the estimated coefficients are also not significantly different from zero. This means that the baseline effects do not seem to be driven by local labor market differences.

In conclusion, the results from the placebo tests provide additional evidence that the estimated baseline effects are indeed effects of the increased earnings exemptions in Amsterdam and Rotterdam. As the DD estimator cannot detect any significant changes in the trends between the different placebo treated and control municipalities, but it can for the actual treatment municipalities, these results support the baseline findings.

9 Discussion and conclusion

In 2021, two of the largest municipalities in the Netherlands, Amsterdam and Rotterdam, started trials with increased earnings exemptions for social assistance (SA) recipients, aiming to stimulate searching for and accepting work as the earnings exemption would "make work pay". This paper studied the effectiveness of these increased earnings exemptions, by estimating the causal effects of the earnings exemption on the share of SA recipients with labor income, the number of hours worked while on SA, the amount of labor income in addition to SA, and exits from SA.

I used difference-in-differences (DD) to determine the effect of the increased earnings exemptions in Rotterdam and Amsterdam, where the treatment and control groups are determined based on regional variation in the implementation of the earnings exemption. This allowed for the identification of the causal effects of the increased earnings exemption.

In line with previous literature, I find that the increased earnings exemptions had a positive effect on the employment rate, the number of hours worked and the labor income of social assistance recipients. Employment in the extensive margin increased by about 0.9 percentage points (16%), and by about 0.5 hours per month (17%) in the intensive margin. Monthly labor earnings increased by about 6.3 euros (19%). I find no evidence of an effect on exits from SA. The estimated effects on employment and labor earnings are, on average, larger for young SA recipients, women, parents, and SA recipients without health problems, while the effects on exits do not differ between subgroups.

Taken together, these findings suggest that an increased earnings exemption for SA recipients can stimulate labor market participation, without reducing exits from SA. Hence, "making-work-pay" policies can be an effective way to stimulate labor force participation of SA recipients.

The results are subject to several limitations. Firstly, the effectiveness of any financial incentive policy depends on its design, implementation and the knowledge of its existence among its target population, but it was not possible to estimate how these factors influenced the effectiveness. Although both Amsterdam and Rotterdam went to great lengths to inform SA recipients of the availability of the increased exemptions, there were still signals that significant shares of SA recipients were not aware of the policies. This likely lowered the effectiveness of the policy, although it is unclear how large this effect was. Moreover, even though Amsterdam and Rotterdam differed in their designs of their earnings exemptions, it was not possible to disentangle how these differences influenced the effectiveness of the exemptions.

Second, it is likely that the effects of an increased earnings exemption rely on the institutional setting. In the Netherlands, part-time work is common. Hence, finding part-time work to complement SA benefits is relatively easy in the Netherlands. In countries with fewer options to work part-time, this might limit the effects of an increased earnings exemption, if the step from no work to full-time work is too big for some SA recipients.

Third, due to data restrictions, it was not possible to study whether the increased earnings exemptions also affected entries into SA. Previous research has shown that financial incentives for SA recipients can affect both the decision to exit SA and to move into SA, as they can make it more financially attractive to receive SA (Card & Robins, 2005). It is likely that SA entry decisions in Amsterdam and Rotterdam were also affected by the increased earnings exemptions. However, because the sample consists only of those already receiving SA before the COVID-19 pandemic in order to disentangle the effects of the earnings exemption form that of the COVID-19 pandemic, I do not observe entries into SA. Therefore, it is not possible to identify any possible entry effects.

Fourth, since this paper only concerns SA recipients from Amsterdam and Rotterdam, the external validity of the results depends on the comparability of SA recipients from these municipalities to those in other municipalities within the Netherlands and in other countries. As Amsterdam and Rotterdam are highly urbanized areas, it might be easier for individuals who feel incentivized by the earnings exemption to actually find work. If that is the case, effects of earnings exemptions in less urbanized locations might be lower. Furthermore, as Section 5 already showed, SA recipients from Amsterdam and Rotterdam differ from individuals in the control municipalities. For example, SA recipients in Amsterdam and Rotterdam had lower employment rates, were older and had less recent work experience. These differences affect the external validity, as employment rates, age and recent work experience likely affect the effectiveness of earnings exemptions. However, the sign of this effect is not clear. A low employment rate without earnings exemption might leave room for a larger effect of an exemption on employment, but it could also be a sign that it is difficult to find part-time work for SA recipients, diminishing the possible positive effects on employment. Younger SA recipients are generally easier to reintegrate on the labor market than older recipients, implying that an earnings exemption with a young SA population might have larger effects than estimated in this study. Lastly, recent work experience increases one's chances of quickly reintegrating on the labor market. Hence, municipalities or countries where SA recipients have more recent work experience are likely to show larger effects of an earnings exemption than estimated in this study.

Finally, this paper is only concerned with the short-term effects of the earnings exemptions, which may understate the longer-run effects. It takes time for the effect of an increased earnings exemption to materialize, as exemplified by the growing employment effect sizes over time. This is due to several reasons. First, information on the availability of the earnings exemption needs to reach SA recipients before it can affect their employment decision. This takes time. Moreover, SA recipients may feel distrustful of new policies and, thus, not respond right away. And, even if individuals feel incentivized by the increased earnings exemption, finding work does not happen instantly, as individuals face adjustment costs (Zaresani, 2020). Hence, when studied over a longer period, the positive employment effects of the earnings exemption may turn out to be bigger. Specifically, positive long-term effects on exits cannot be ruled out, as the stepping-stone effect of part-time employment takes longer to materialize due to its sequential nature. For the earnings exemptions to have a stepping-stone effect, it must first induce SA recipients to take up part-time work, before the part-time work can act as a stepping stone to full-time employment and exit from SA. The study may lack a long enough time horizon to observe this effect.

Future research could, therefore, study the long-term effects of increased earnings exemption. This can give further insight into the longer-term dynamics of the employment effects of earnings exemption, job retention, and the relevance of the stepping-stone effect compared to the increased financial incentive to remain in SA. Moreover, this study was not able to study the relation between the characteristics of the earnings exemptions and its effectiveness. Therefore, the question remains what causes a certain financial incentive to be more or less effective? And how does this differ between subgroups? These would be fruitful topics for further research. Along similar lines, future research may wish to analyse the relation between the institutional setting and the effectiveness of financial incentives in order to deepen understanding of what determines the effectiveness of financial incentives. Additionally, future research could consider the effects of earnings exemptions on a broader set of outcomes, such as poverty, mental health and debts. As increased earnings exemptions allow SA recipients to keep a (higher) share of their earnings, this positively affects their financial position and, thus, may decrease poverty among SA recipients. Furthermore, Elshout and Bos (2023) show that SA recipients in Amsterdam experience reduced stress and spend the extra earnings from the earnings exemption mostly on preventing and paying off debts, but a causal relationship between increased earnings exemptions and mental health or indebtedness has not yet been established. Finally, this study did not address the cost-effectiveness of the policy. If the costs of the earnings exemptions are accessible, future research could study whether the lower spending on SA benefits outweigh the costs of the earnings exemption.

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Data statement

This paper uses non-public microdata provided by Statistics Netherlands, made available for scientific research via remote access.

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Appendix A Additional heterogeneity analyses

A.1 Differences between Amsterdam and Rotterdam

This appendix presents the estimated effects of the earnings exemption on employment in the extensive and intensive margin, and labor income while on SA, and on exits from SA, for Amsterdam and Rotterdam separately. This is given in Figure A1.

The analysis finds positive and significant effects on the employment rate of SA recipients in both Amsterdam and Rotterdam. The estimated coefficients are slightly larger in Amsterdam than in Rotterdam, although the confidence intervals of the estimates overlap a great deal. Therefore, I cannot say with certainty that the earnings exemption in Amsterdam had a bigger effect than the exemption in Rotterdam.

The figure also shows that the effects of the earnings exemptions on hours worked are almost equal in Amsterdam and Rotterdam. Since the effect on hours worked also includes zeros and the estimated extensive margin employment effect was somewhat larger in Amsterdam, this suggests that the per person increase in hours worked was larger in Rotterdam than in Amsterdam.

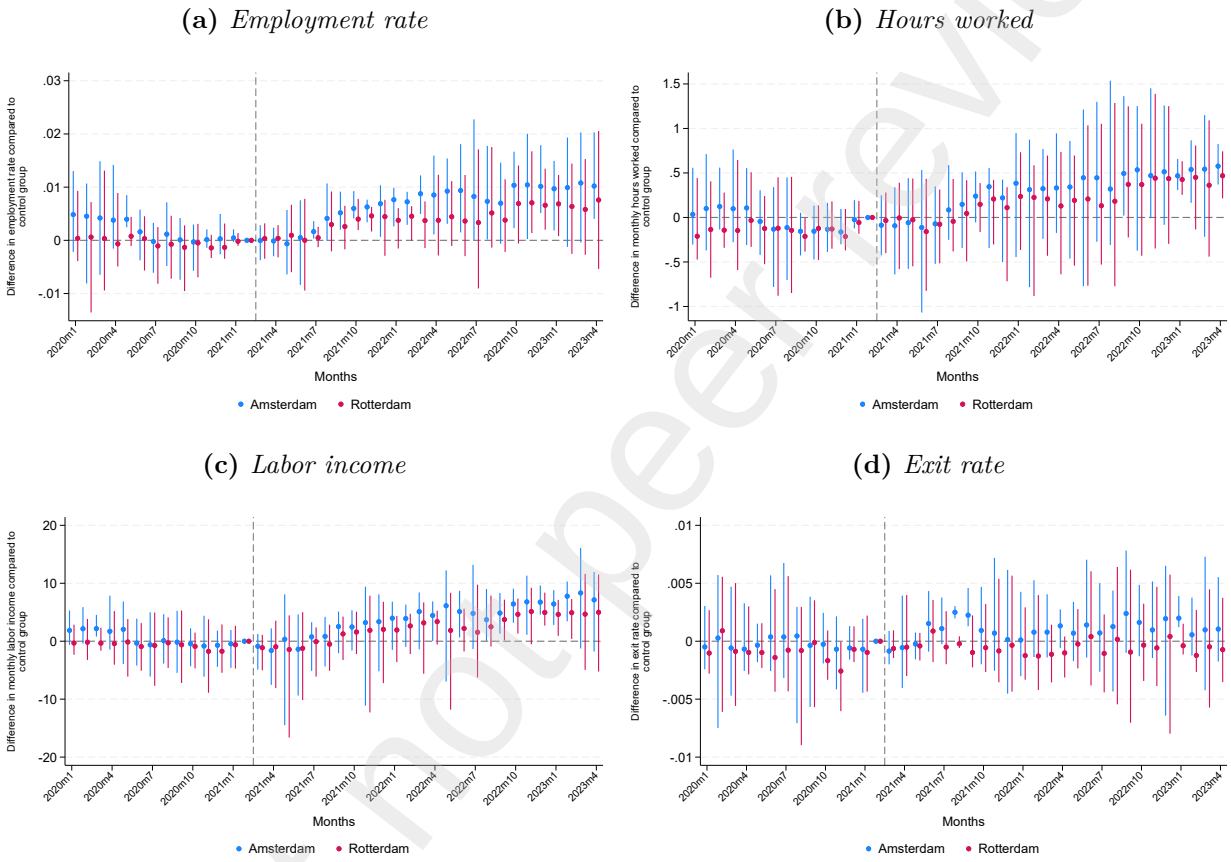
With regard to labor income, the estimated coefficients are also slightly larger in Amsterdam than in Rotterdam. However, there also seems to be some violation of the parallel pre-trends in Amsterdam. Therefore, it is unclear whether this difference is actually due to the increased earnings exemption.

Lastly, the figure shows that the effects on exits from SA are not significantly different from zero in both municipalities. However, the estimated coefficients do increase after the implementation in Amsterdam. In Rotterdam, the coefficients do not change after the implementation.

In conclusion, the analyses for Amsterdam and Rotterdam separately found some differences in the estimated effects between the two municipalities. There are several possible causes for these differences, such as differences in the implementation or design of the earnings exemptions. For example, in Amsterdam the implementation of the population-wide increased exemption was preceded by a smaller-scale experiment with increased exemptions. Combined with the extensive information campaigns in Amsterdam, this may have led to a higher awareness of the policy in Amsterdam than in Rotterdam. Furthermore, in Rotterdam, the exemption percentage is set at a lower level than in Amsterdam, possibly incentivizing fewer SA recipients to take up work. However, despite these differences in the setup of the earnings exemptions between Amsterdam and Rotterdam, the differences in estimated effects are actually fairly small.

Figure A1

Baseline difference-in-differences results for Amsterdam and Rotterdam separately.



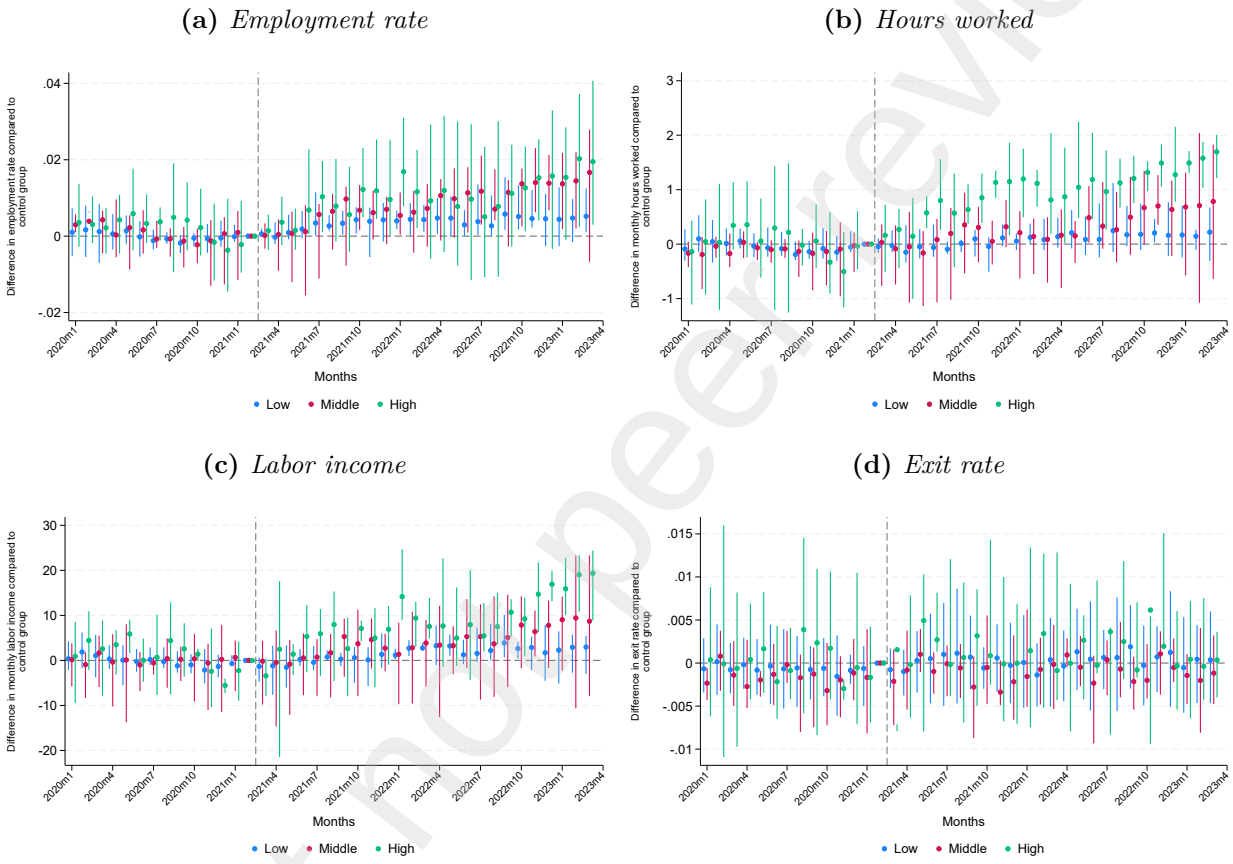
Note. Estimation results from Equation 1 for the full sample, by treatment municipality. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

A.2 Heterogeneity by education level

This appendix compares the effects of the increased earnings exemptions by the education level of the SA recipients. As shown in Figure A2, the estimated effects of the earnings exemptions on the employment rate of labor income of SA recipients are slightly smaller for lower-educated SA recipients than for middle- and high-educated SA recipients. However, these differences are not significant. With regard to hours worked, the effect for high-educated SA recipients is about 0.8 hours per month larger than for middle- and lower-educated SA recipients. This difference is significant at a five percent level. There are again no differences in the effect on exits from SA. They are indistinguishable from zero in all three groups.

Figure A2

Baseline difference-in-differences results, by education level.



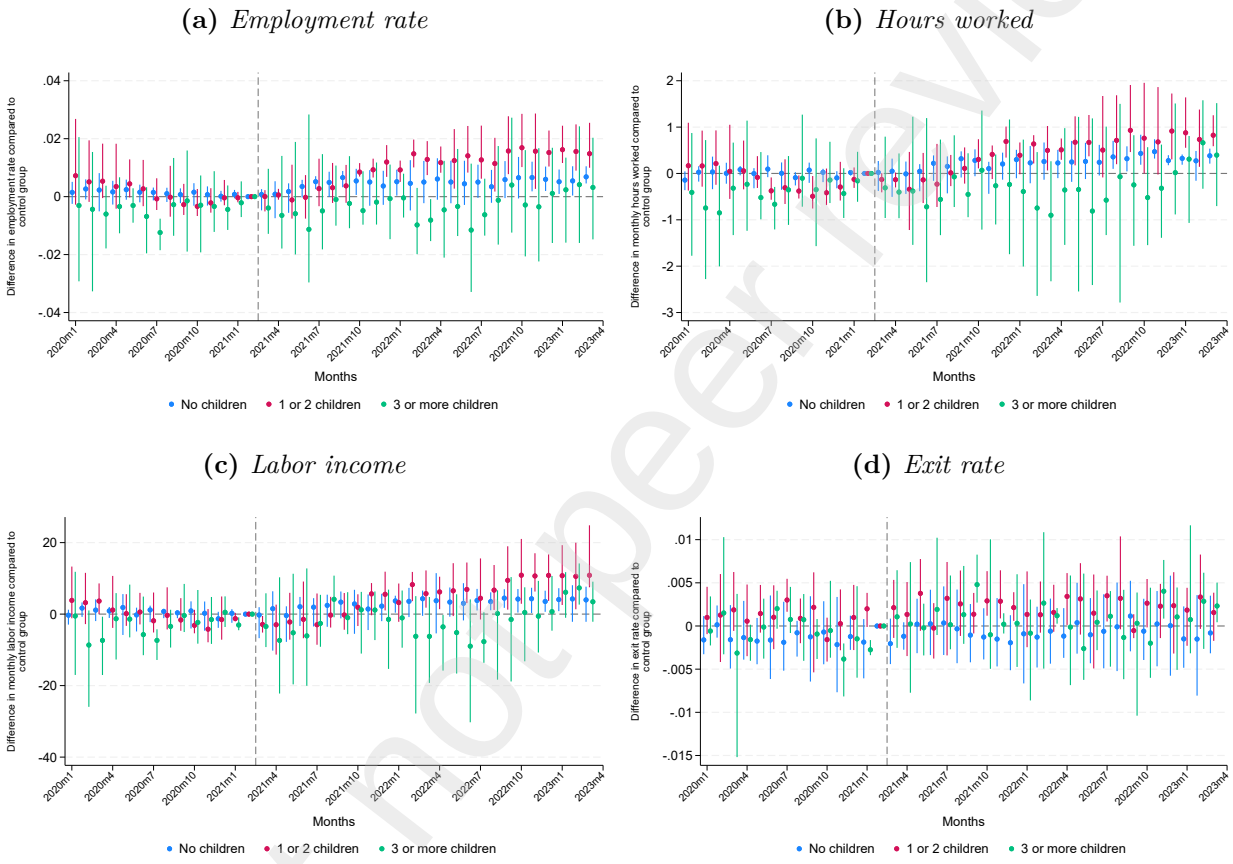
Note. Estimation results from Equation 1 for the full sample, by education level. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. Sample includes only single SA benefit recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

A.3 Heterogeneity by number of children

This appendix shows that the effects of the increased earnings exemptions on the employment rate, number of hours worked, and labor income of SA recipients is largest for households with one or two children. The effects are also positive and significant for households without children, but they are smaller in comparison. There appears to be no effect on the employment outcomes of SA households with three of more children. With regard to the effects on the exit rate from SA, there are no differences between household with fewer or more children.

Figure A3

Baseline difference-in-differences results, by number of children.



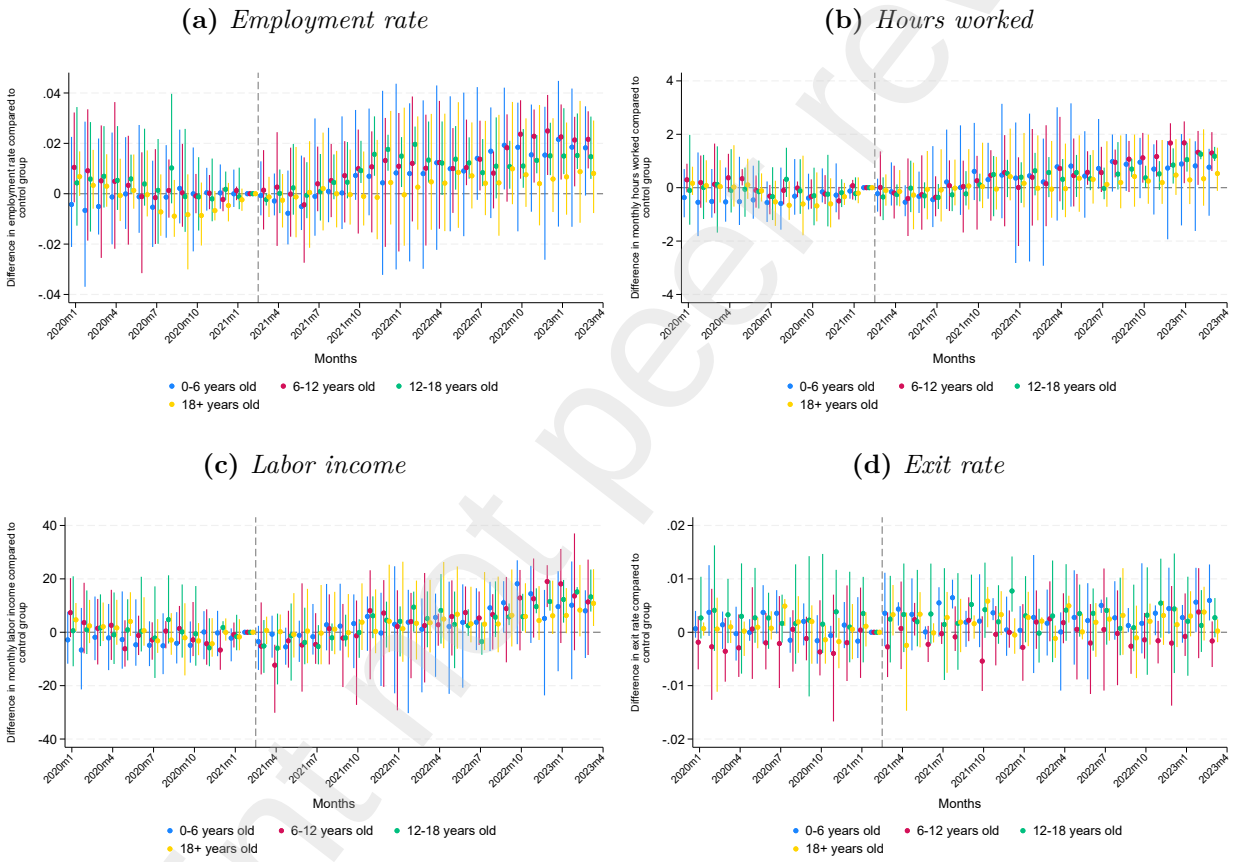
Note. Estimation results from Equation 1 for the full sample, by the number of children in the household. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

A.4 Heterogeneity by age of youngest child

This appendix shows that the effects of the earnings exemptions on employment, hours worked, and labor income are slightly larger for social assistance recipients with children aged below twelve, although the confidence intervals are mostly overlapping. There are no differences in the effect on exits.

Figure A4

Baseline difference-in-differences results, by age of the youngest child.



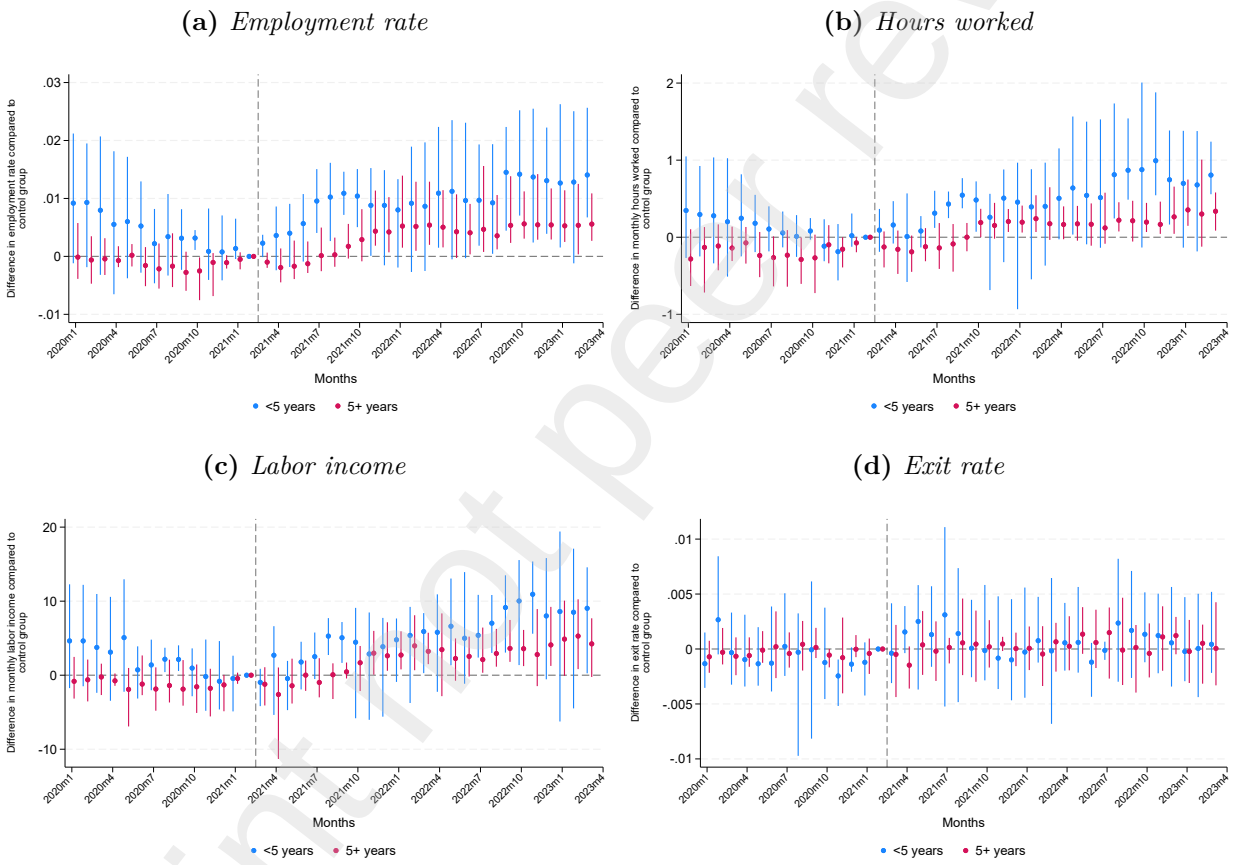
Note. Estimation results from Equation 1 for the full sample, by age of the youngest child. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

A.5 Heterogeneity by duration on social assistance

This appendix shows that the effects of the earnings exemptions on employment, hours worked, and labor income are slightly larger for social assistance recipients who have been receiving SA for less than five years. The effects are smaller for those receiving SA for more than five years, but still positive and significantly different from zero. There are no differences in the effect on exits.

Figure A5

Baseline difference-in-differences results, by duration on social assistance.



Note. Estimation results from Equation 1 for the full sample, by duration of receiving social assistance. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

Appendix B Robustness and placebo tests

B.1 All municipalities as controls

The control municipalities in the baseline analysis are chosen such that they are as similar as possible to the treated municipalities, without having implemented or announced the implementation of any new financial incentives for SA recipients during the observed time period. This appendix shows that the baseline effects are not driven by this choice of control municipalities. In this robustness analysis, all municipalities except Amsterdam and Rotterdam are used as control municipalities.

Figure B1

Difference-in-differences results for Amsterdam and Rotterdam combined, using all municipalities as controls.

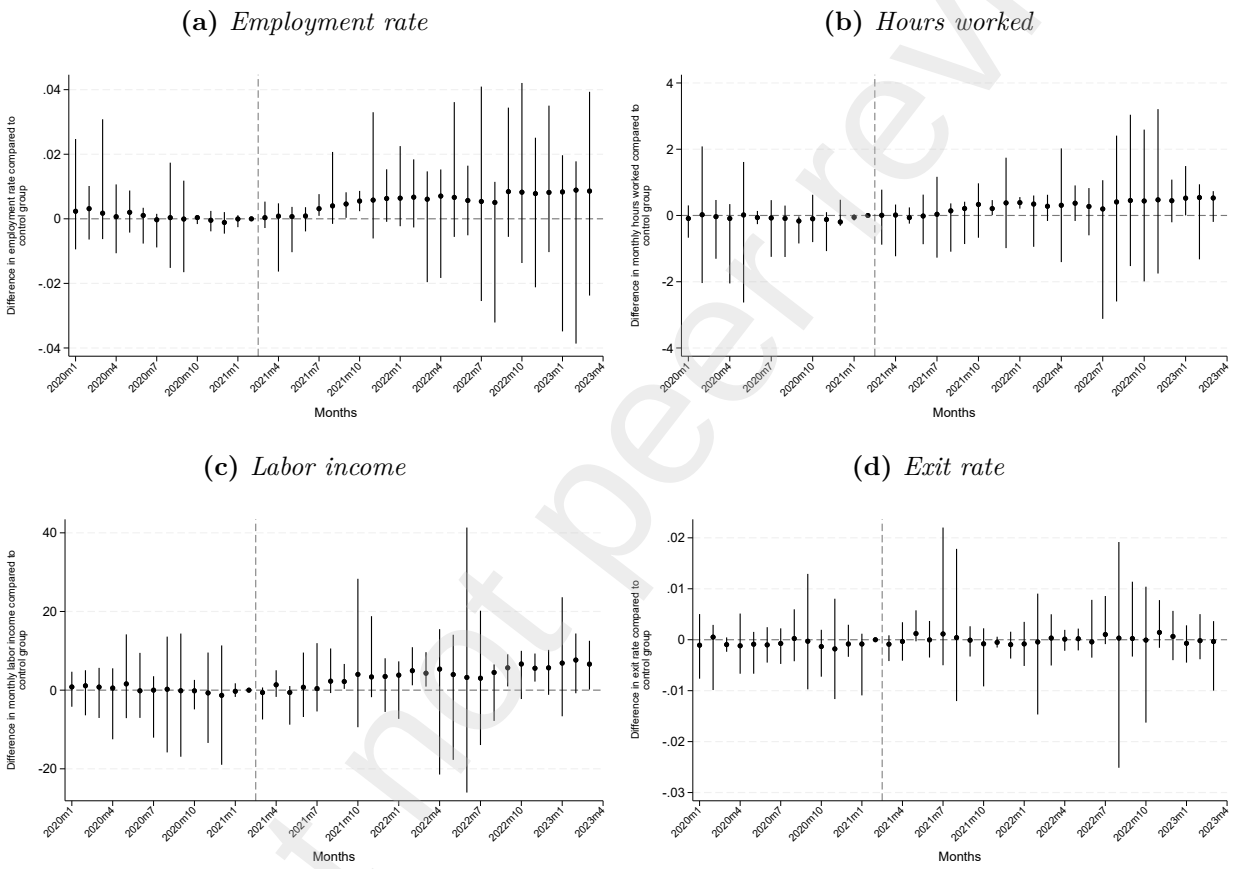
Note. Estimation results from Equation 1 for the full sample, using all municipalities except Amsterdam and Rotterdam as control municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. Standard errors are clustered at the municipality level.

B.2 Only Utrecht and The Hague as controls

This appendix shows the results of a robustness analysis, where only Utrecht and The Hague are used as control municipalities, as these are most similar to Amsterdam and Rotterdam in terms of their population characteristics. Figure **??** shows that the baseline effects are hardly affected by the choice to also include Almere and Nijmegen as control municipalities. The effect sizes are similar, but standard errors are larger when only using Utrechta and the Hague as control municipalities.

Figure B2

Difference-in-differences results for Amsterdam and Rotterdam combined, using only Utrecht and The Hague as controls.



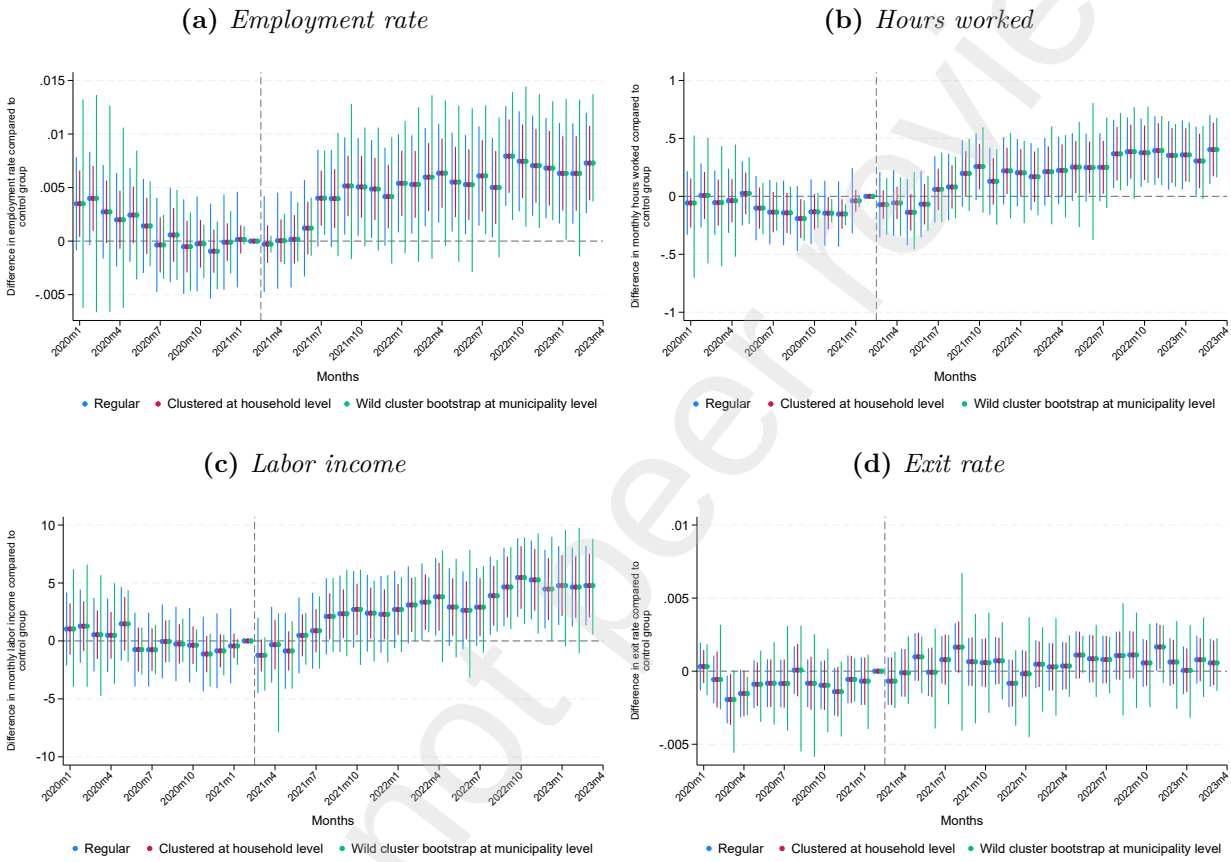
Note. Estimation results from Equation 1 for the full sample, using only Utrecht and The Hague as control municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

B.3 Other standard errors

This appendix shows that the baseline findings do not depend on the choice of standard errors. Figure B3 presents the baseline results with three alternative clustering levels: no clustering, clustered at household level with regular sandwich estimator, and clustered at municipality level with regular sandwich estimator. The results clearly show that the chosen wild clustered bootstrap standard errors are the most conservative as they lead to larger standard errors than all other three options.

Figure B3

Baseline difference-in-differences results with alternative standard errors.



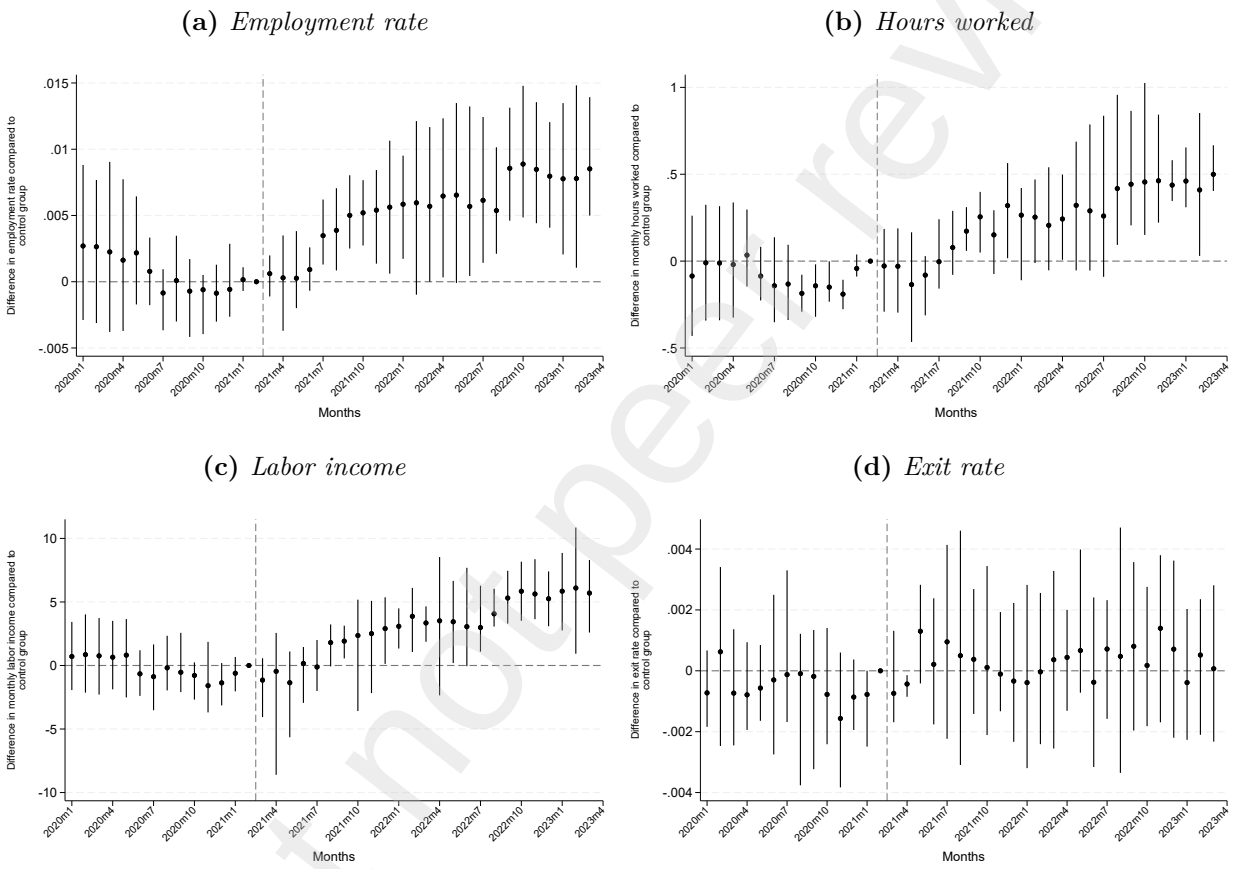
Note. Estimation results from Equation 1 for the full sample. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using with four types of standard errors: no clustering, clustered at household level with regular sandwich estimator, clustered at municipality level with regular sandwich estimator, and clustered at municipality level with wild cluster bootstrap.

B.4 Include movers

The baseline difference-in-difference analysis excludes SA recipients who moved to a different municipality during the observed time period. This appendix shows that this had little effect on the estimated coefficients.

Figure B4

Baseline difference-in-differences results for Amsterdam and Rotterdam combined, including movers.



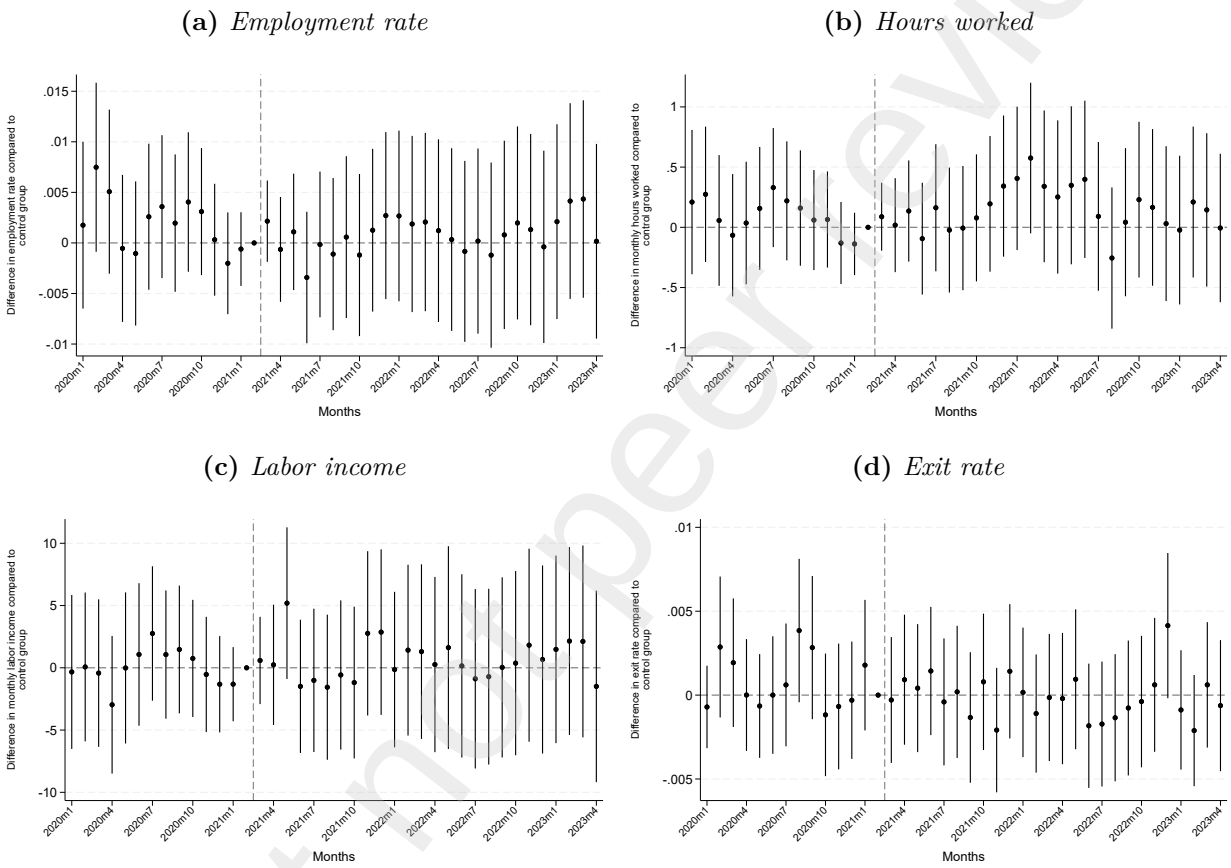
Note. Estimation results from Equation 1 for the full sample, including households who moved during the observed period. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

B.5 Control municipalities as placebo treatment municipalities

To ensure that the results are actually the effect of the increased earnings exemption, I run several placebo regressions with alternative treatment municipalities. I use the control municipalities from the baseline estimations as (placebo) treated municipalities instead of Amsterdam and Rotterdam, and exclude Amsterdam and Rotterdam from the sample. The results in Figures B5, B6, B7, and B8 show that the baseline effects are not driven by this choice of control municipalities, as the regressions do not pick up any significant differences in the labor market outcomes of SA recipients in Almere, Nijmegen, The Hague, and Utrecht, compared to the other control municipalities.

Figure B5

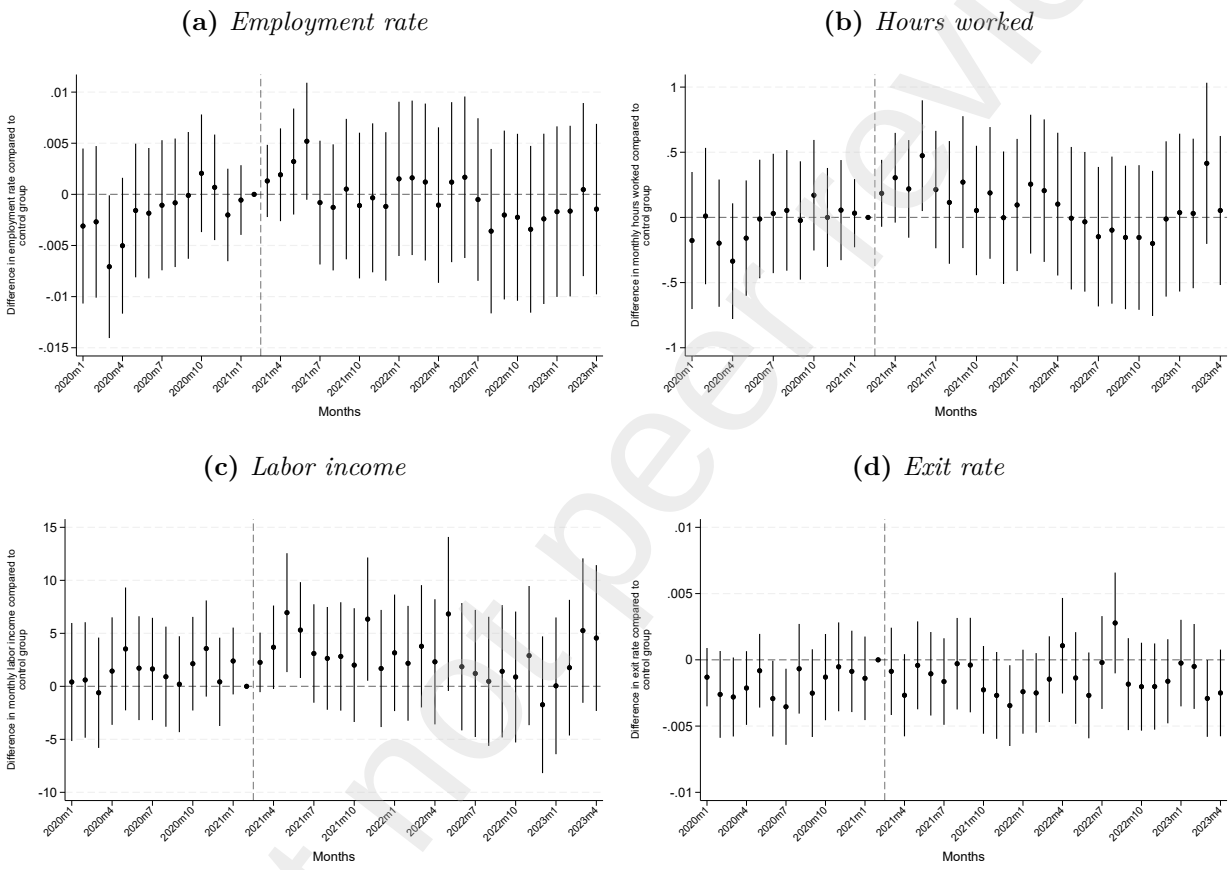
Baseline difference-in-differences results with Almere as placebo treatment municipality.



Note. Estimation results from Equation 1 for the full sample, using Almere as placebo treatment municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using clustered standard errors at the household level.

Figure B6

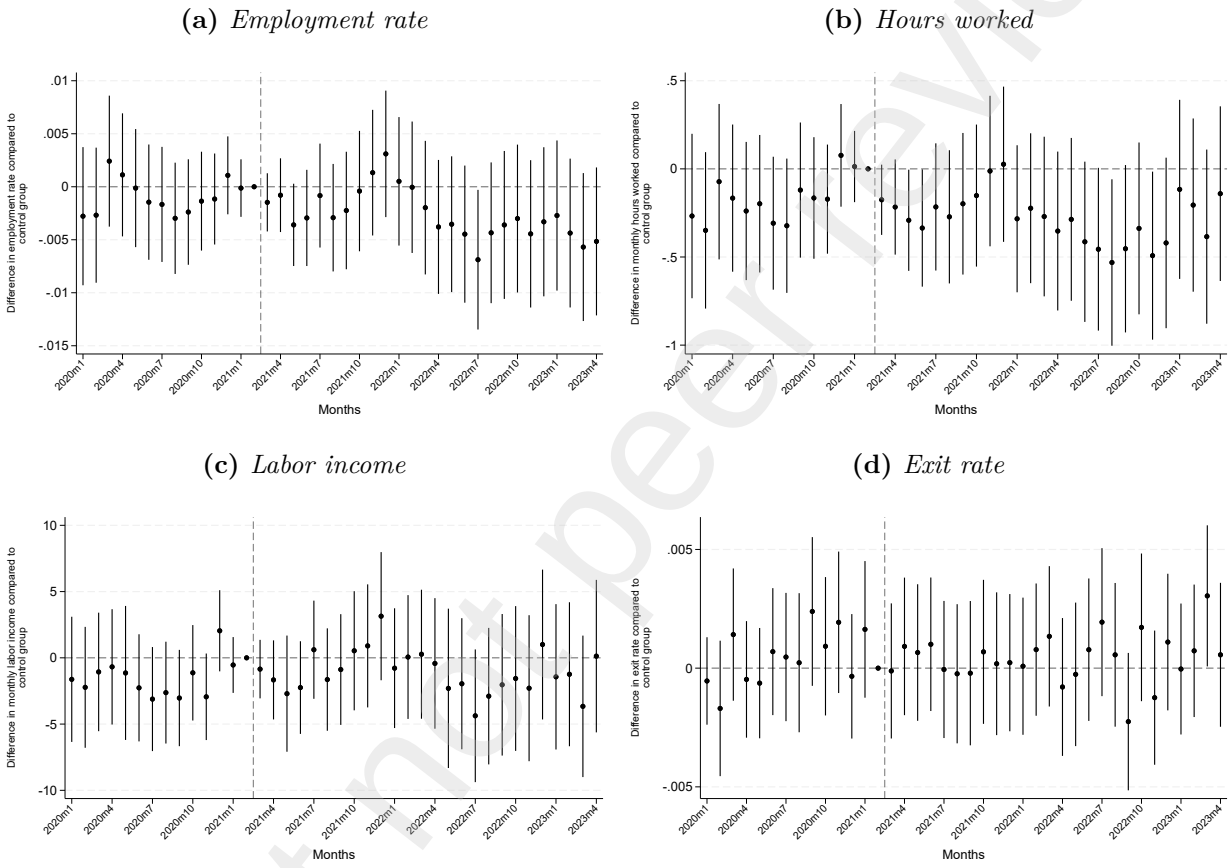
Baseline difference-in-differences results with Nijmegen as placebo treatment municipality.



Note. Estimation results from Equation 1 for the full sample, using Nijmegen as placebo treatment municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using clustered standard errors at the household level.

Figure B7

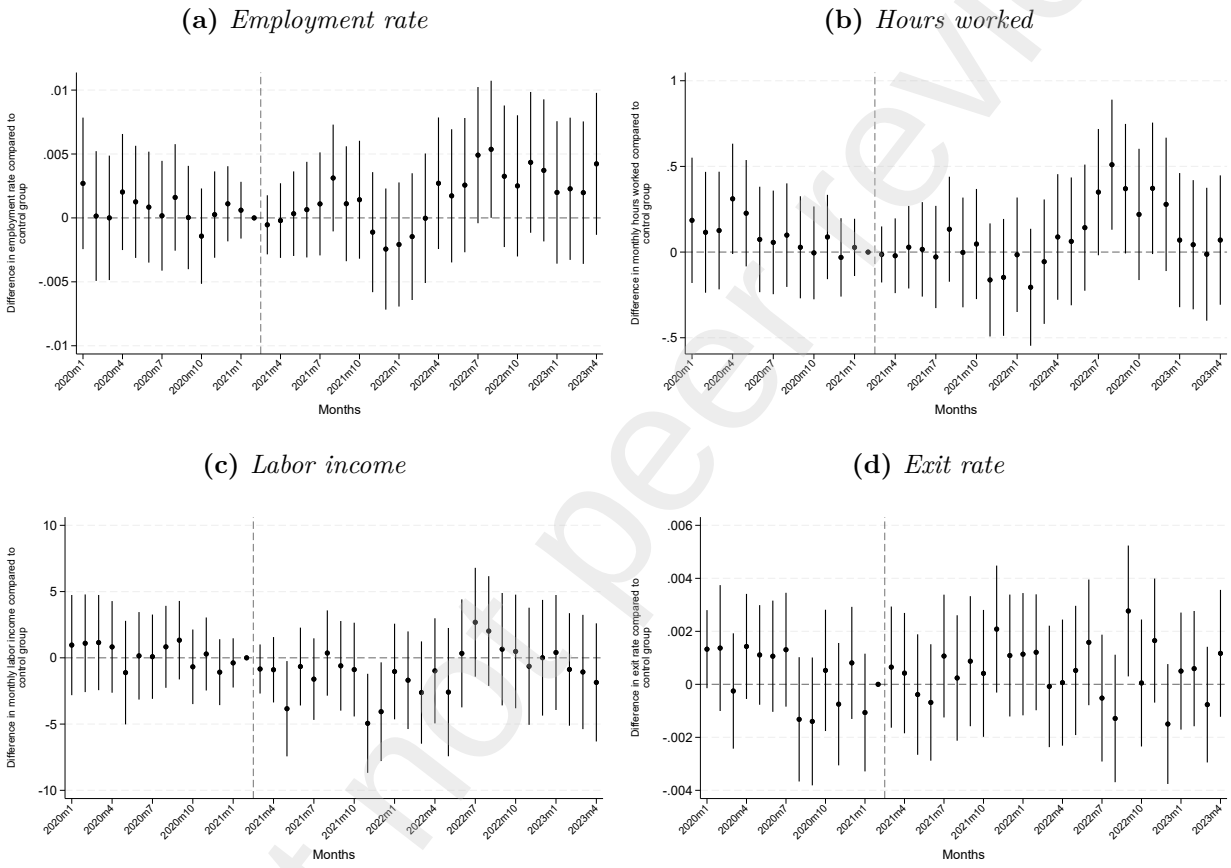
Baseline difference-in-differences results with The Hague as placebo treatment municipality.



Note. Estimation results from Equation 1 for the full sample, using The Hague as placebo treatment municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using clustered standard errors at the household level.

Figure B8

Baseline difference-in-differences results with Utrecht as placebo treatment municipality.



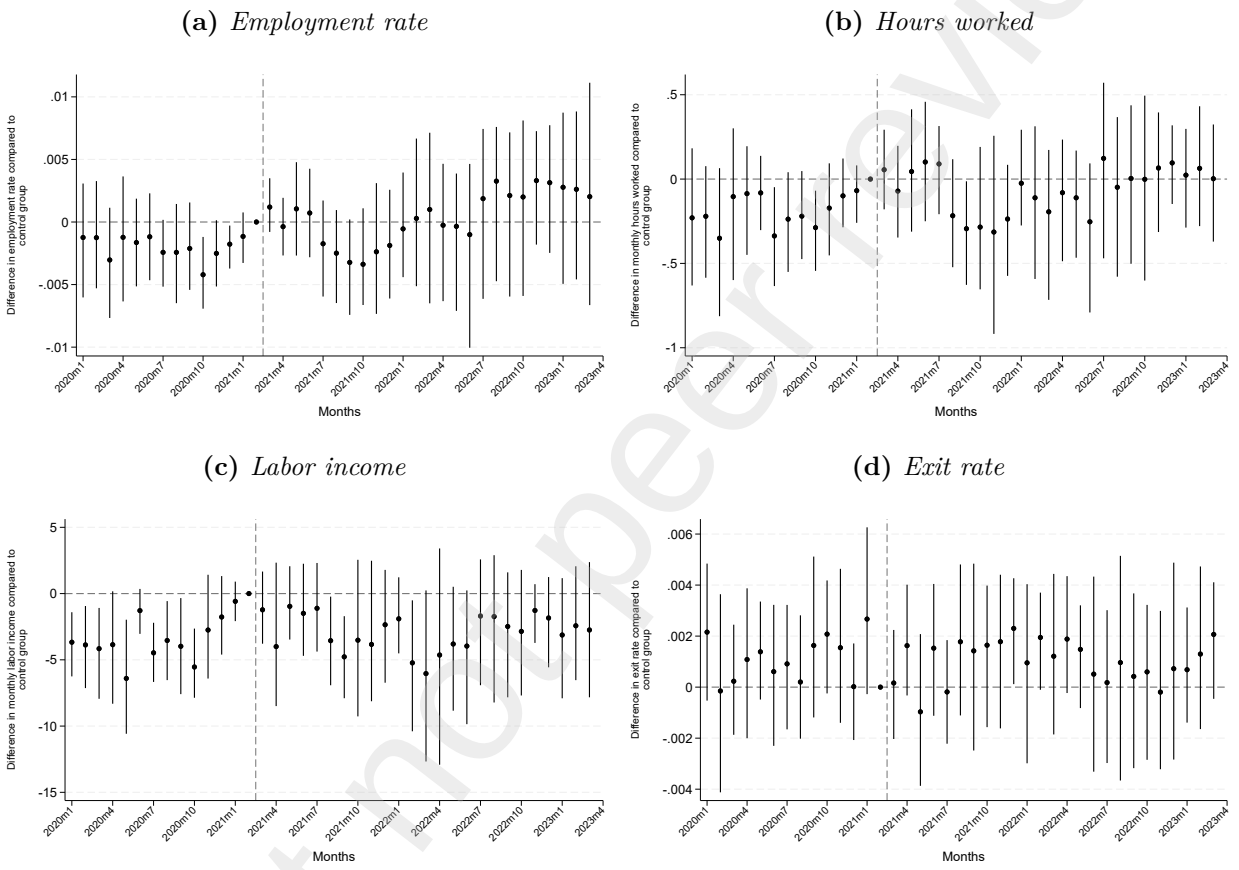
Note. Estimation results from Equation 1 for the full sample, using Utrecht as placebo treatment municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using clustered standard errors at the household level.

B.6 Control municipalities compared to smaller municipalities

Next, to ensure that the results are not driven by size differences between the treatment and control municipalities, I run a placebo regression, where the control municipalities from the baseline estimation are used as treatment municipalities and several smaller municipalities are used as the control group. The smaller municipalities used are Maastricht, Apeldoorn, Deventer, Zwolle, Amersfoort, Haarlem, and Dordrecht. The results in Figure B9 show that these placebo regressions do not pick up any significant differences between the control municipalities and the smaller municipalities. This provides evidence that the baseline findings are not driven by differences in municipality size between the treatment and control groups.

Figure B9

Placebo difference-in-differences results, using smaller municipalities.



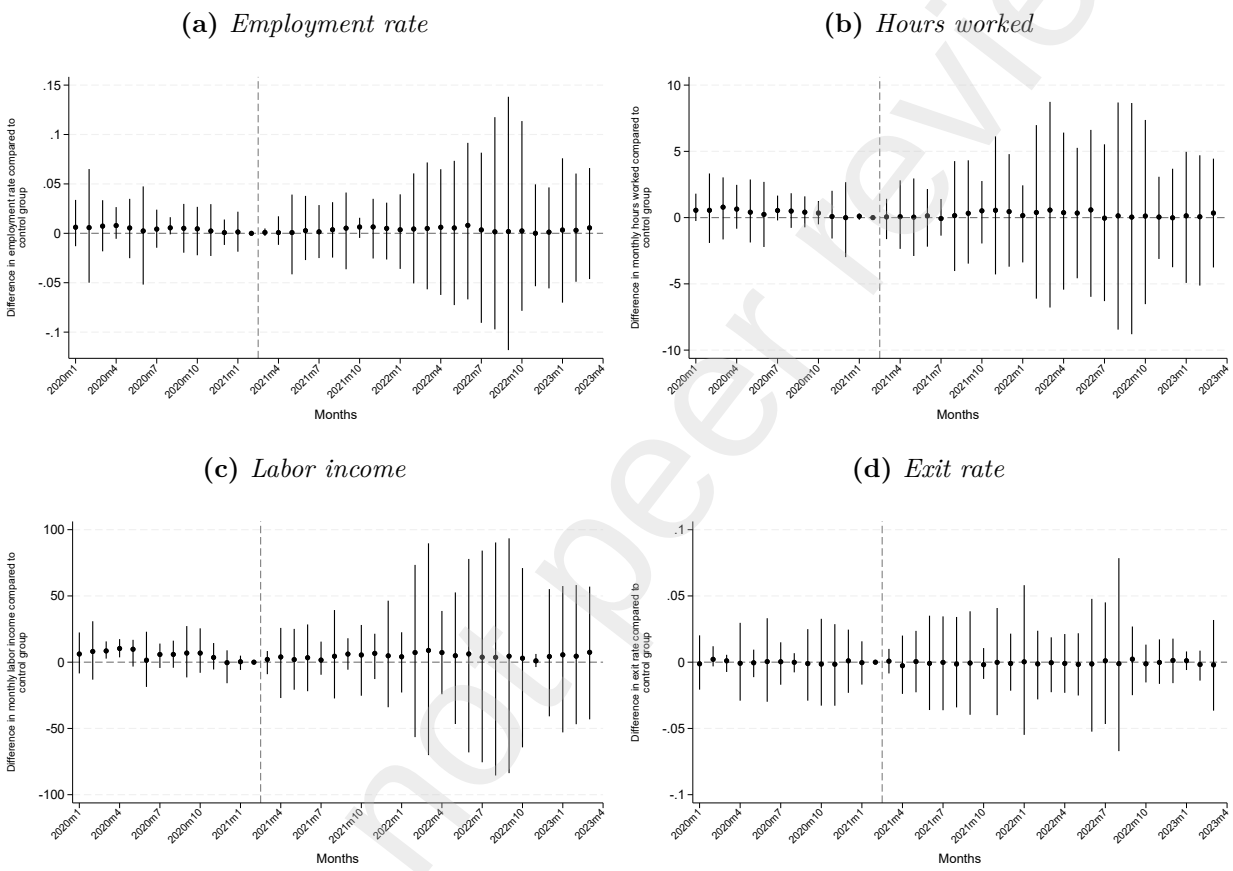
Note. Estimation results from Equation 1 for the full sample, comparing the control municipalities to even smaller municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.

B.7 Neighboring municipalities as placebo treatment municipalities

To exclude confoundedness with local labor market conditions, this placebo analysis shows that when using two large neighboring municipalities of Amsterdam and Rotterdam as placebo treatment municipalities (Haarlem and Dordrecht), the regressions do not detect any effect.

Figure B10

Placebo difference-in-differences results, using neighboring municipalities.



Note. Estimation results from Equation 1 for the full sample, using two neighboring municipalities, Haarlem and Dordrecht, instead of Amsterdam and Rotterdam as placebo treatment municipalities. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. The regressions control for the highest education level, type of SA benefit, age of the main SA recipient, the number of females in the household, the number of children, and the recent work history of (both) SA recipients. 95% confidence intervals are calculated using wild cluster bootstrap.