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Incentivizing social assistance recipients

Quasi-experimental evidence from trials with earnings exemptions in the Netherlands

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The content of this thesis is the sole responsibility of the author and does not reflect the view of the supervisor, second assessor, Erasmus School of Economics or Erasmus University. Results are based on calculations using non-public microdata from Statistics Netherlands.

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 March 2020 to April 2022. The results indicate that the increased earnings exemptions had a positive effect on the employment rate and the number of hours worked of social assistance recipients. Employment in the extensive margin increased by about 0.27 to 0.35 percentage points (5-6.5%), and by about 0.11 to 0.16 hours per month (4-6%) in the intensive margin. The analysis does not find evidence of an effect on labor income or exits from social assistance. The estimated effects are rather similar between Amsterdam and Rotterdam, except that the earnings exemption in Amsterdam had a significant positive effect on exits from social assistance of 0.13 to 0.16 percentage points (12-15%), while it had no effect on exits in Rotterdam. These findings suggest that “making-work-pay” policies can be an effective way to stimulate labor force participation among social assistance recipients.

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1 Introduction

Last-resort social security schemes have to balance providing income security for those at the lowest end of the income distribution and creating financial incentives to participate in the labor market. Earnings exemptions for income earned while receiving benefits are a common way of promoting labor market participation (see OECD (2022) for an overview of earnings exemptions in the OECD). Such earnings disregards are meant to “make work pay”, as they ensure that earned income is not (fully) deducted from social security benefits. However, the effectiveness of earnings exemptions is not always evident (e.g. Matsudaira and Blank (2014)).

The Netherlands has known different earnings exemptions for individuals on social assistance (SA).¹ The current design, which has been in place since 2004, allows SA recipients to keep 25 percent of their earnings for a maximum period of six consecutive months. However, Dutch municipalities still struggle with activating SA recipients. Each year, only about ten percent of SA recipients enters the labor market (Muffels, 2020). Therefore, the design of the earnings exemption is still up for discussion and several municipalities have decided or are considering to implement increased or longer-term earnings exemptions.

In 2021, two of the largest municipalities in the Netherlands, Amsterdam and Rotterdam, started a trial with an increased earnings exemption for SA recipients, aiming to stimulate searching for and accepting work. This paper studies the effectiveness of these increased earnings exemptions, by estimating the causal effects of the earnings exemptions 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.

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.

This study makes use of monthly administrative microdata from Statistics Netherlands from March 2020 to April 2022 to study the effects of the increased earnings exemptions during the first year after their implementation.

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

Previous studies have found either no effects (Matsudaira & Blank, 2014; Palviainen, 2023) or positive effects of earnings exemptions (Blank et al., 1999; Card & Robins, 1996; Knoef & Van Ours, 2016; McCall, 1996; Van Kempen et al., 2021). Some found conflicting results (Verlaat & Zulkarnain, 2022). However, most studies on earnings exemptions for SA recipients had no identification strategies that allowed for the identification of causal effects (Matsudaira & Blank, 2014; McCall, 1996; Van Kempen et al., 2021; Verlaat & Zulkarnain, 2022). Others focused only on a subgroup of the SA population (Knoef & Van Ours, 2016; Matsudaira & Blank, 2014). This paper adds to this literature by providing the first causal evaluation of an earnings exemption in the Netherlands for the full population of SA recipients.

I find that the increased earnings exemptions had a positive effect on employment and the number of hours worked of SA recipients. Employment in the extensive margin increased by about 0.27 to 0.35 percentage points (5-6.5%), and by about 0.11 to 0.16 hours per month (4-6%) in the intensive margin. The analysis does not find evidence of an effect on labor income or exits from social assistance. The estimated effects are rather similar between Amsterdam and Rotterdam, with one notable exception. Namely, the earnings exemption in Amsterdam had a significant positive effect on exits from SA of 0.13 to 0.16 percentage points (12-15%), while it had no effect on exits in Rotterdam.

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 baseline results and Section 8 some robustness checks on these baseline estimates. Finally, Section 9 gives the conclusions.

2 Related literature

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

2.1 Earnings exemptions for recipients of 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.

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 just part of several treatments that were studied and combined, the effects of the earnings exemptions cannot be isolated in Deventer. In the other two municipalities, the results were mixed. Verlaat and Zulkarnain (2022) find a positive effect on employment and exits from SA in Utrecht during the experiment, and this effect fades out after the experiment ended. In Wageningen, however, Verlaat and Zulkarnain (2022) find a negative effect of the increased earnings exemption on

²The 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).

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, for example, an experiment with an increased earnings exemption took place between 2018 and 2021. The Research & Statistics department from the municipality of Amsterdam 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 heavily from selection bias, because the analysis compared 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 differed from non-participants in terms of unobservable characteristics, most importantly 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. Hence, their results likely greatly overestimate the actual effect of the increased earnings exemption.

The municipality of Rotterdam also evaluated the same trial with increased earnings exemptions that is studied in the current paper (Van Toorn, 2022). Using descriptive evidence, Van Toorn (2022) finds that the implementation of the increased earnings exemption was followed by an increase in the employment rate of SA recipients of about 0.4 percentage points. However, this analysis cannot separate possible time effects from the estimation, as this study does not use a control group.

Finally, McCall (1996) studies the effect of changes in the earnings exemptions for unemployment insurance recipients in the US. Using a continuous-time job search model, McCall (1996) suggests that an increase in the earnings exemption generally corresponds to higher part-time and overall re-employment rates. Furthermore, estimates from a competing risks model confirm that a higher earnings exemption is related with higher part-time employment probabilities. However, these findings are not causal.

To conclude, previous studies have found either no effects (Matsudaira & Blank, 2014; Palviainen, 2023) or positive effects of earnings exemptions (Blank et al., 1999; Knoef & Van Ours, 2016; McCall, 1996; Van Kempen et al., 2021; Van Toorn, 2022). Some found conflicting results (Verlaet & Zulkarnain, 2022). However, most studies on earnings exemptions for SA recipients had no identification strategy that allowed for the estimation of causal effects (Matsudaira & Blank, 2014; McCall, 1996; Van Kempen et al., 2021; Van Toorn, 2022; Verlaet & Zulkarnain, 2022). Others

focused only on 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 this literature by providing the first causal evaluation of an earnings exemption for all SA recipients in two municipalities in the Netherlands. 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 a causal effect, 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.

2.2 In-work benefits provided through the tax system

Earnings disregards for recipients of social security benefits are only a type of financial incentives aimed to promote labor supply incentives among low-income households. Other popular financial incentives 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 policies 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 provided increased earnings exemptions 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-tested social security scheme that guarantees a monthly minimum income for households with earnings lower than 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 for the benefit 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 are allowed to grant a temporary earnings exemption of 25 percent up to about 222 euros⁵ per month during a maximum of six months. Most municipalities indeed offer this temporary earnings exemption to their SA recipients.

In 2021, each month about 360 thousand SA benefits were paid out to people of working-age

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

⁴Generally, 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.

⁵The 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.

in the Netherlands (Statistics Netherlands, 2023b), providing a guaranteed minimum income to about 420 thousand individuals (Statistics Netherlands, 2023a). This means that about 4 percent of adults in the Netherlands were dependent on SA in 2021. In Amsterdam, about 35 thousand SA benefits were paid out to people of working-age 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.

In the whole of the Netherlands, about 8 percent of SA recipients works part-time next to SA (Divosa, 2015). In Amsterdam, this share is slightly lower at about 7 percent (Van Kempen et al., 2021). 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	-	Instead of temporary exemption	On top of 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 an increased earnings exemption for SA recipients, aiming to stimulate searching for and accepting work.⁶ This

⁶The municipality of Tilburg also offered increased earnings exemptions for a while, but this municipality is not considered in this study.

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 were eligible for an increased earnings exemption for income earned in addition to the SA benefits. Whereas before, earnings were, in principle, 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 became 222 euros per month in 2022. The exempted percentage stayed the same.

The exemption was not paid out monthly, but every six months, implying that the monthly benefit remained the same as before the increased exemption, but those with labor income received an extra amount every half year.

When SA recipients earn 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.

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 deducted 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 this, they leave SA and lose the 220 euros, causing the drop from point C to D in the budget line. Budget line AFGDE gives the budget line in the case of 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 a bit lower than line BC.

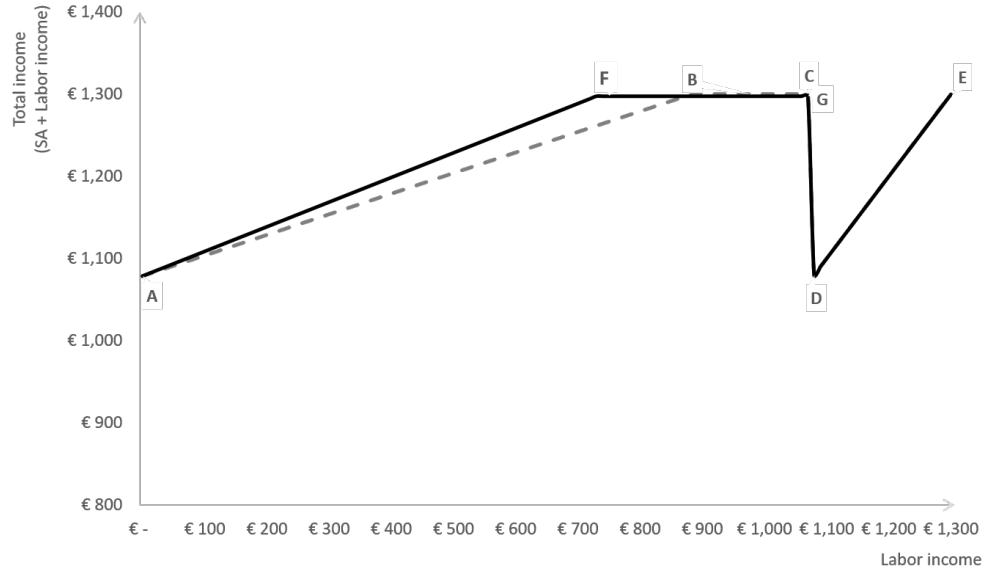
Figure 1b shows how the budget line of a single person on SA benefits was affected by the increased earnings exemption for someone who was no longer eligible for the general temporary exemption. Budget line ABC is the situation without the increased earnings exemption and without the right to the temporary 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, earnings are deducted at 30 percent 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 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

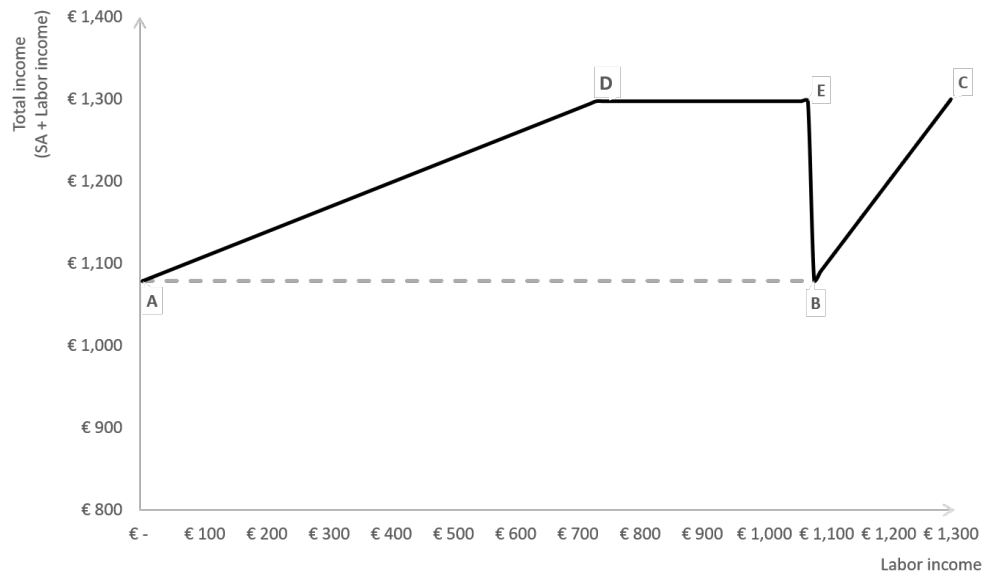
Budget lines with and without increased earnings exemption in Amsterdam.

(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.

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.

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 that municipalities may offer to their social assistance recipients. 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 $\frac{219}{0.125} = 1,752$ euros. Note that at such an earnings level, someone would already have exited SA. Hence, this maximum earnings exemption was never 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.

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.

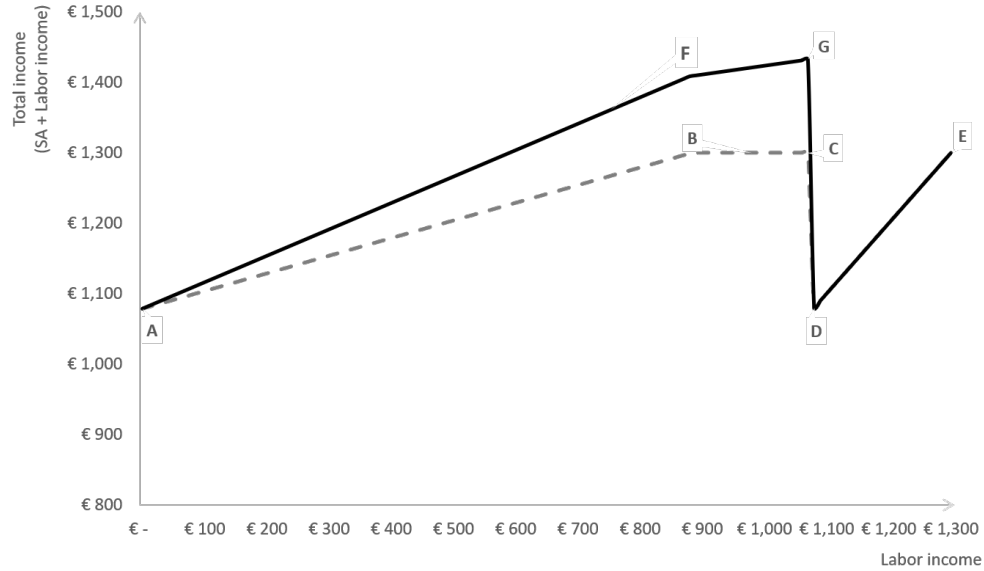
Figure 2a shows how the budget line of a single person on SA benefits was affected by the increased earnings exemption when this person was still eligible for the general temporary exemption. Budget line ABCDE is the situation without the increased earnings exemption but with the temporary exemption. 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 yet. 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 this, they leave SA and lose the exemption, causing the drop from point D to B in the budget line.

Figure 2

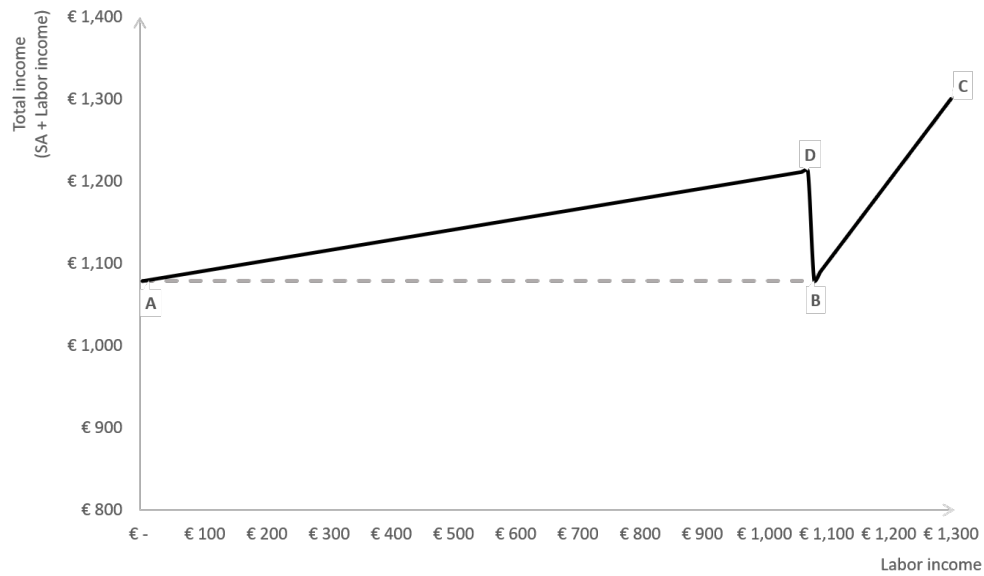
Budget lines with and without increased earnings exemption in Rotterdam.

(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.

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.

4 Hypotheses

This section briefly discusses the effects that may be expected 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. Then, I discuss some of the expected differences between the effects of the earnings exemption for Amsterdam and Rotterdam.

The increased earnings exemptions create financial incentives to work. Hence, I expect the increased earnings exemptions to increase employment among SA recipients in the extensive margin. As the earnings exemptions increase the value of an hour of work, it incentivizes 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 exemption 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 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)⁷, 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. Thus, I expect positive effects of the increased earnings exemptions for SA recipients in Amsterdam and Rotterdam.

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 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.

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 less attractive. Hence, I expect the earnings exemption to decrease the number of exits from SA.

Finally, I expect there to be smaller treatment effects in Amsterdam than in Rotterdam, since a large part of the SA recipients in Amsterdam who would be incentivized by the increased earnings exemption were already able to receive an increased exemption due to a previous experiment with increased earnings exemptions where everyone who applied got an earnings exemption.⁸ Hence, the group which would likely show the biggest effects of the increased earnings exemption already received it during the pre-treatment periods of this study. Furthermore, since the exempted share of earnings was also higher during the experiment in Amsterdam than after the experiment (but with a lower maximum amount), the change from the experiment to the trial in 2021, for some people, might have reduced their labor supply incentives.

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

⁷Lietzmann 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.

⁸This is the experiment studied in Van Kempen et al. (2021), as discussed in Section 2. The experiment lasted from 2018 to 2021. About 5,250 SA recipients from Amsterdam applied for the experiment and, thus, could receive earnings exemption.

construct a monthly panel data set, containing all households who receive social assistance payments at some point between March 2020 and April 2022.⁹

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, and the number of children in the household. 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, and the number of months of work experience between 2014 and 2018.

Table 2 gives descriptive statistics on the treatment and control group, from Amsterdam and Rotterdam, both before and after the implementation of the increased earnings exemption.¹⁰ The treatment group consists of the SA recipients from Amsterdam and Rotterdam, and the control group consists of SA recipients from nine other municipalities that did not have an increased earnings exemption.¹¹

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. I start by discussing the differences in the dependent variables, before the differences in the independent variables.

First, the employment rate among SA recipients is only about 7.5 percent in the control municipalities, but it is even less in Amsterdam and Rotterdam, both before and after the implementation of the increased earnings exemption. Before the treatment, the employment rate among SA recipients is about 2 percentage points lower in the treatment municipalities. After the treatment this difference is slightly smaller at 1.7 percentage points. Second, SA recipients in Amsterdam and Rotterdam work 1.2 hours less per month, on average, than SA recipients in the control municipalities before the implementation of the earnings exemption. After the implementation, this difference

⁹From 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 has likely only small effects on the estimated coefficients as only about 13 percent of the SA households in the sample is a couple. The others are all single-person households. Thus, the choice between a household- or individual-level panel affects only about 13 percent of the observations.

¹⁰Additional descriptive statistics for Amsterdam and Rotterdam separately are given in Appendix B. For more information on the monthly dynamics of the outcome variables, raw trend plots are available in Appendix A. More information on the selection of the control municipalities is given in Section 6.1.

¹¹See Section 6 for more information on the how the treatment and control groups were determined.

Table 2*Descriptive statistics*

Variables	Pre			Post		
	Treatment	Control	T-C	Treatment	Control	T-C
<i>Dependent variables</i>						
Employment rate	0.055	0.075	-0.020***	0.059	0.076	-0.017***
Monthly hours worked	2.814	4.006	-1.192***	3.093	4.120	-1.026***
Monthly labor income	35.214	50.226	-15.012***	38.939	52.584	-13.646***
Exits	0.011	0.012	-0.001***	0.014	0.014	0.000
<i>Independent variables</i>						
Age of first adult	48.171	45.679	2.492***	49.254	46.762	2.492***
Age of second adult	50.682	49.303	1.380***	51.199	49.993	1.206***
Number of females	0.578	0.571	0.008***	0.578	0.568	0.010***
Number of children	0.684	0.653	0.031***	0.658	0.630	0.027***
Recent work history (months)	8.165	8.932	-0.766***	8.167	8.899	-0.732***
Highest education = Low	0.577	0.553	0.024***	0.577	0.553	0.024***
Highest education = Middle	0.310	0.320	-0.010***	0.310	0.320	-0.010***
Highest education = High	0.113	0.127	-0.014***	0.113	0.127	-0.014***
Couple SA receiver	0.133	0.142	-0.009***	0.129	0.138	-0.009***
N (x1,000)	1,314	1,374		1,314	1,374	

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 Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven. 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. Dependent variables employment rate, monthly hours worked, and monthly labor income concern employment, hours worked and income while also receiving SA.

has slightly decreased to 1.0 hour per month. Third, labor income in addition to SA is also lower in Amsterdam and Rotterdam than in the control municipalities. This difference was about 15.0 euros per month before the treatment and about 13.6 euros per month after the treatment.

Furthermore, SA recipients in Amsterdam and Rotterdam are, on average, 1 to 3 years older than those in the control municipalities. Households receiving SA in Amsterdam and Rotterdam also contain more females, on average, and have slightly more 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, on average, lower educated than in the control municipalities. Finally, Amsterdam and Rotterdam have less couples on SA and more single person households on SA, 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.

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 most other large municipalities did not.¹² 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 (see also Section 5).

6.1 Difference-in-differences (DD)

I use a static 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 individual i at time t on a constant α , a treatment dummy D_i , which equals one if an individual lives in Rotterdam or Amsterdam, time-fixed effects $POST_t$, the DD interaction term $D_i \cdot POST_t$, which equals one if an individual lives in Rotterdam or Amsterdam and time t is after the implementation of the increased earnings exemption, and a set of control variables X_{it} :

$$y_{it} = \alpha + \beta D_i + \gamma POST_t + \delta^{DD} D_i \cdot POST_t + \zeta X_{it} + \epsilon_{it}, \quad (1)$$

where δ^{DD} is the DD coefficient of interest.

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

¹²The municipality of Tilburg also offered increased earnings exemptions for a while, but this municipality is not considered in this study.

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 in Appendix C. Moreover, in all analyses, I use only control municipalities that are similar to the treated municipalities in terms of number of social assistance recipients.

Appendix C shows that pre-treatment trends are approximately parallel for employment without control variables, number of hours worked without control variables, and exits (both with and without control variables). For labor income, employment with control variables, and hours worked with control variables, the pre-trends are not parallel. The difference-in-differences estimator may give biased estimates in these regressions. Therefore, as a robustness check, I perform the same analysis with an alternative estimator to the DD estimator that is less reliant on the validity of the parallel trends assumption. More details on this alternative estimator is given in Sections 6.2 and 8.

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. These municipalities are Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven.¹³

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. In Amsterdam, the earnings exemption is still in place indefinitely, which increases the risk of

¹³Tilburg is not included in the control municipalities, even though it also has more than 5,000 SA recipients per month, because this municipality also had an increased earnings exemption in place for a short time during the observed time period.

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.¹⁴ 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.

In the main analysis, I estimate the effects for SA recipients in Amsterdam and Rotterdam combined. Additionally, I then estimate the effects for Amsterdam and Rotterdam separately to study possible differences between the two municipalities.

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 March 2020 to February 2021 and the treatment period of all months from April 2021 to April 2022.¹⁵ Furthermore, as the labor market incentives from the increased earnings exemption in Rotterdam stopped after April 2022, I restrict the studied time frame to April 2022 for both Amsterdam and Rotterdam. This allows for studying the effects of the earnings exemptions during the first year after they were implemented.

When studying the differences between Amsterdam and Rotterdam, I do include March 2021. Namely, I use March 2020 until February 2021 as the pre-treatment period, and March 2021 until April 2022 as the treatment period for Amsterdam. For Rotterdam, I use March 2020 until March 2021 as the pre-treatment period, and April 2021 until April 2022 as the treatment period.

I use wild subcluster bootstrap to calculate the p-values corresponding to all regression coefficients. Such 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). Furthermore, a more common wild cluster bootstrap correction for the p-values tends to under-reject in a difference-in-differences setting with few treated clusters, which is the case in my analysis. Hence, a wild subcluster bootstrap with individual-level clusters is most

¹⁴This reduces the sample size only slightly by about 2 percent and has little effect on the estimated coefficients (see Appendix E).

¹⁵The pre-treatment period is relatively short to avoid using both data from before and during the COVID-19 crisis, which started in March 2020. This increases the plausibility of the parallel trends assumption, as municipalities were differently affected by the crisis and the difference in trends went to a structurally different level after the start of the crisis.

appropriate (MacKinnon & Webb, 2018).

The TWFE estimator has received a lot of critique in recent years. In settings with multiple time periods, the static TWFE estimator generally gives 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). However, in the special case when treatment is binary, changes only once from non-treated to treated, and there is no differential treatment timing, the static TWFE estimator still gives unbiased estimates of the treatment effect (De Chaisemartin & d’Haultfoeuille, 2022). Since this is the case in this study, the TWFE estimator is an appropriate estimator for the treatment effects of the increased earnings exemptions.

6.2 Robustness checks

In addition to the main difference-in-differences analyses, I perform several robustness checks.

First, I use an alternative estimator for the causal effect of the increased earnings exemption, namely the synthetic difference-in-differences (SDD) estimator of Arkhangelsky et al. (2021). This estimator is less dependent on the parallel trends assumption than the DD estimator. As shown in Section 6.1, the parallel trends assumption may not hold for some of the outcome variables, especially when including control variables. This may be due to unobserved differences between the treated and control municipalities over time, such as changes in the guidance given to SA recipients.¹⁶ The SDD estimator allows for the estimation of a causal effect even if the parallel trends assumption of DD is not perfectly satisfied

Second, I run placebo regressions with alternative treatment municipalities to ensure that the estimated effects are actually effects of the increased earnings exemption. I use The Hague, Utrecht, and Groningen as (placebo) treated municipalities instead of Amsterdam and Rotterdam, and then exclude Amsterdam and Rotterdam from the sample. In these municipalities, there was no increased earnings exemption at this time. Hence, there should be no difference in trends between The Hague, Utrecht, and Groningen and the other control municipalities.

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

¹⁶For example, between 2019 and 2022, Amsterdam already implemented two new guidance methodologies. See this link for more information.

on SA, and on the exit rate from SA. First, I discuss the pooled results, before going into the separate estimation results from Amsterdam and Rotterdam.

7.1 Baseline

Table 3 gives 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 in the odd columns do not include any control variables and the regressions in the even columns control for the highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

The table shows positive coefficients for the effect of the increased earnings exemption on employment among SA recipients of about 0.003 in columns 1 and 2. These coefficients are both statistically significant at at least a five percent confidence level. This implies that the increased earnings exemptions in Amsterdam and Rotterdam had a positive effect on employment among SA recipients of about 0.27 to 0.35 percentage points. Compared to an employment rate of about 5.5 percent in the sample before treatment, this implies an increase in the employment rate of SA recipients of about 5 to 6.5 percent.

Similarly, Table 3 also shows positive effects of the increased earnings exemption on the number of hours worked by SA recipients. Without controls, this estimate is 0.1657 and significant at a one percent significant level. When including controls, the estimate becomes slightly lower at 0.1150 and is only marginally significant at a ten percent confidence level. Thus, the increased earnings exemption seems to have increased the monthly number of hours worked by SA recipients with about 4 to 6 percent.

Furthermore, the table shows positive coefficients for the effect of the increased earnings exemption on labor income among SA recipients in Amsterdam and Rotterdam. Without controls, this coefficient is 1.3659 and significant at five percent. With controls, the coefficient is smaller with 0.6658 and no longer significantly different from zero. Therefore, it is unclear whether the increased earnings exemptions in Amsterdam and Rotterdam had an effect on the labor earnings of SA recipients, as the positive coefficient without controls might, thus, be due to changes in the composition of the treatment and control groups.

Finally, Table 3 shows positive coefficients of the increased earnings exemption on the exits from SA. In both regression specifications, the coefficient is 0.0010. In the specification without control

Table 3*Baseline effects of increased earnings exemptions in Amsterdam and Rotterdam combined.*

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	0.0035*** (0.001)	0.0027** (0.001)	0.1657*** (0.039)	0.1150* (0.046)	1.3659** (0.453)	0.6658 (0.471)	0.0010* (0.000)	0.0010 (0.001)
Treatment	-0.0202** (0.006)	-0.0147** (0.004)	-1.1920** (0.321)	-0.8951*** (0.181)	-15.0116*** (4.045)	-11.4084*** (2.352)	-0.0010 (0.001)	0.0003 (0.001)
Post	0.0008 (0.001)	-0.0003 (0.001)	0.1134** (0.036)	0.0744 (0.046)	2.3586*** (0.433)	1.7963*** (0.463)	0.0016*** (0.000)	0.0002 (0.000)
Constant	0.0754*** (0.006)	0.0753*** (0.006)	4.0063*** (0.260)	4.8080*** (0.264)	50.2258*** (3.307)	47.5565*** (3.476)	0.0119*** (0.001)	0.0346*** (0.001)
N (x1,000)	4,112	3,417	4,112	3,417	4,112	3,417	5,377	4,046
Controls	No	Yes	No	Yes	No	Yes	No	Yes

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. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients. Full regression results are available in Appendix D.

variables, this coefficient is marginally significantly different from zero at ten percent confidence. In the specification with controls, it is not significantly different from zero. Hence, there is no evidence of an effect of the increased earnings exemptions on exits from SA.

In conclusion, the baseline results indicate that the increased earnings exemptions have had a positive effect on employment and the number of hours worked of SA recipients. Employment in the extensive margin increased by about 0.27 to 0.35 percentage points (5-6.5%), and by about 0.11 to 0.16 hours per month (4-6%) in the intensive margin. This is in line with the hypotheses from Section 4. The analysis does not find evidence of an effect of the increased earnings exemptions on labor income or exits. This is surprising, as it is labor income that determines the actual amount of exempted earnings and, in theory, if the number of hours worked increases, labor income should also increase if the hourly wage is unaffected. However, labor income may also be more volatile than hours worked, making it more difficult for any estimator to identify an effect. Another possible explanation for the absence of a significant effect on labor income may lie in the violation of the parallel trends assumption for labor income. This implies that, due to unobservable differences

between the treatment and control municipalities, the regression may not be able to detect an effect of the earnings exemptions on labor income. Furthermore, the absence of effects on exits contradicts the theoretical prediction that SA recipients would want to stay on SA, as they would no longer be eligible for the increased earnings exemption when they exited SA. This could be partially explained by findings in Boschman et al. (2021), Elshout and Bos (2023), and Lietzmann et al. (2017), who show that working part-time while on SA works as a stepping stone to exit from SA. This effect might cancel out any possible negative effects on exits from SA.

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 in this study are smaller. Moreover, I do not find any evidence for an effect on labor earnings of SA recipients. This can be explained by the fact that women and single parents are generally found to have a relatively elastic labor supply (e.g. 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. Furthermore, the positive effects on employment can be explained by findings from Elshout and Bos (2023), who find that SA recipients in Amsterdam felt motivated by the increased earnings exemption, because the earnings exemption felt like an appreciation of their work.

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 Differences between Amsterdam and Rotterdam

Next, I present 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 Table 4. Panel A presents the results for Amsterdam and panel B for Rotterdam.

The analysis finds positive coefficients for the effect of the increased earnings exemption on the employment rate among SA recipients in both Amsterdam and Rotterdam. Without controls, this coefficient is 0.0030 and significant at five percent in Amsterdam. With controls, the coefficient is

slightly smaller at 0.0023 and no longer significantly different from zero in Amsterdam. Therefore, it is unclear whether the increased earnings exemption in Amsterdam had an effect on the employment rate of SA recipients, as the positive coefficient without controls might, thus, be due to changes in the composition of the treatment and control groups. For Rotterdam, the coefficient without controls is slightly larger at 0.0035, which is also significant at five percent confidence. With controls, the coefficient is 0.0025 in Rotterdam, which is significant at a ten percent confidence level. Thus, the earnings exemption in Rotterdam seems to have had a positive effect on the employment rate of SA recipients. These results for Amsterdam and Rotterdam differ slightly from the pooled results. The coefficients are of very similar magnitude, but they are less significant than in the pooled analysis. This may be due to the larger sample size in the pooled analysis improving the ability to detect effects of the increased earnings exemption.

The table also shows positive coefficients for the effect on hours worked among SA recipients in Amsterdam and Rotterdam. The estimated coefficient without controls in Amsterdam is 0.1381, which is significant at a five percent level. With controls, the coefficient becomes 0.0968, which is no longer significantly different from zero. The estimated coefficients in Rotterdam are somewhat larger, but of similar significance. Without controls, the estimated coefficient is 0.1751, which is significant at five percent. With controls, the coefficient is 0.1119, which is not significant. For both Amsterdam and Rotterdam, this implies that it is unclear whether the increased earnings exemption had an effect on the number of hours worked by SA recipients. These results differ from the pooled results, which found a significantly positive effect of the increased earnings exemptions in Amsterdam and Rotterdam together. As noted above, this may be due to the larger sample in the pooled analysis.

The analysis finds no evidence of an effect on labor income. Table 4 shows positive but insignificant coefficients in columns 5 and 6 for Amsterdam. With controls, this coefficient is 1.1482 and without controls it is 0.5831. However, they are not significantly different from zero. Hence, I find no evidence that the increased earnings exemption affected labor earnings among SA recipients in Amsterdam. In Rotterdam, the estimated coefficient for the effect on labor income is 1.2811 and marginally significant at ten percent without controls. With controls, the coefficient is 0.3905 and not significantly different from zero. Hence, there is also no conclusive evidence of an effect of the increased earnings exemption in Rotterdam on labor income. These results are in line with the pooled results from Section 7.1, which also found no conclusive evidence of an effect on labor income.

Table 4*Baseline effects of increased earnings exemptions in Amsterdam and Rotterdam separately.*

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Amsterdam</i>								
Treat X Post	0.0030** (0.001)	0.0023 (0.001)	0.1381** (0.034)	0.0968 (0.044)	1.1482 (0.407)	0.5831 (0.439)	0.0013** (0.000)	0.0016* (0.000)
Treatment	-0.0172* (0.006)	-0.0154* (0.004)	-0.9567** (0.261)	-0.8517** (0.177)	-12.0881** (3.324)	-10.9738** (2.345)	-0.0012 (0.001)	-0.0003 (0.000)
Post	0.0003 (0.001)	-0.0009 (0.001)	0.0856** (0.034)	0.0364 (0.044)	1.7463*** (0.407)	1.0304** (0.429)	0.0016*** (0.000)	0.0003 (0.000)
Constant	0.0754*** (0.006)	0.0711*** (0.006)	4.0063*** (0.261)	4.7961*** (0.334)	50.2258*** (3.324)	46.3423*** (4.031)	0.0119*** (0.001)	0.0351*** (0.001)
N (x1,000)	3,306	2,715	3,306	2,715	3,306	2,715	4,316	3,220
Controls	No	Yes	No	Yes	No	Yes	No	Yes
<i>Panel B: Rotterdam</i>								
Treat X Post	0.0035** (0.001)	0.0025* (0.001)	0.1751** (0.036)	0.1119 (0.045)	1.2811* (0.431)	0.3905 (0.480)	0.0004 (0.000)	0.0003 (0.000)
Treatment	-0.0234** (0.006)	-0.0137* (0.004)	-1.4510** (0.262)	-0.9167** (0.173)	-18.0965*** (3.317)	-11.4612** (2.245)	-0.0007 (0.001)	0.0009 (0.000)
Post	0.0011 (0.001)	0.0004 (0.001)	0.1316*** (0.036)	0.1106** (0.045)	2.7699*** (0.431)	2.4447*** (0.462)	0.0014*** (0.000)	0.0002 (0.000)
Constant	0.0750*** (0.006)	0.0745*** (0.007)	3.9881*** (0.262)	4.8782*** (0.296)	49.8145*** (3.317)	48.3105*** (3.906)	0.0121*** (0.001)	0.0344*** (0.001)
N (x1,000)	3,159	2,711	3,159	2,711	3,159	2,711	4,149	3,215
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Estimation results from Equation 1 for Amsterdam and Rotterdam separately. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients. Full regression results are available in Appendix D.

Lastly, in Amsterdam, the increased earnings exemption seems to have increased exits from SA. The table shows a positive coefficient of 0.0013, which is significant at five percent, when not including any controls, and a positive coefficient of 0.0016, which is significant at ten percent, when including controls. This suggests that the earnings exemption in Amsterdam increased exits by 0.13 to 0.16 percentage points. Compared to an exit rate of about 1.1 percent in Amsterdam before

treatment, this implies an increase in the exit rate from SA of about 12 to 15 percent. In Rotterdam, the table shows no effect of the earnings exemption on exits from SA. Both when including and excluding the control variables, the estimated effects are not significantly different from zero. These findings for Amsterdam differ from those for Amsterdam and Rotterdam together, which found no conclusive evidence of an effect on exits from SA. The findings for Rotterdam are in line with the pooled results.

In conclusion, the results for Amsterdam and Rotterdam separately are mostly in line with the pooled results, with several notable exceptions. First, the effects are less often significant, especially in the regressions with control variables, which is likely related to the smaller sample sizes in the separate estimations. The fact that the effect sizes remain mostly similar compared to the pooled estimation and between Amsterdam and Rotterdam, does suggest that there is an effect of the earnings exemption in both municipalities and that the increased power in the pooled estimation is necessary to detect this effect.

Second, Amsterdam and Rotterdam differ in terms of the effect on exits from SA. In Amsterdam, the separate analysis finds positive effects of the increased earnings exemption on the exit rate from SA, which is not the case in Rotterdam. In the pooled estimation, this difference between the two municipalities is averaged out, leading to the inconclusive evidence of an effect on exits in the pooled estimation. This suggests that the earnings exemption in Amsterdam may have been more effective at fully activating SA recipients than the exemption in Rotterdam, although complete exit from SA is and should not be the only goal for SA recipients because a large part of SA recipients is not able to work (more) in the short-term. As shown by Elshout and Bos (2023), many SA recipients in Amsterdam struggle with mental or physical health problems that prevent them from working (more hours). Also care for children or other family members is often an obstacle for SA recipients to work or increase their working hours, even if they feel incentivized by the earnings exemption. Thus, exits may not be feasible for many SA recipients in the short term, implying that policy makers should not measure the effectiveness of activation policies for SA recipients solely by their effects on exits.

Moreover, the estimated coefficients in Amsterdam are smaller than Rotterdam in most cases. This is in line with the expected effects, as part of the SA recipients in Amsterdam already received an earnings exemption in the pre-treatment periods. These recipients would be expected to show the largest effects of the earnings exemption, implying that the estimated effects in Amsterdam may be underestimations of the actual effects of an increased earnings exemption. Alternatively, the share

of exempted earnings also reduced in March 2021 for this group of SA recipients. This may have had a negative effect on their labor supply and could, thus, explain the smaller effects in Amsterdam. However, the differences between Amsterdam and Rotterdam could also be related to differences in the setup of the earnings exemptions. For example, in Amsterdam there are no additional incentives from the increased exemption to find work for couples on SA, where one person is already working. In Rotterdam, this is the case. Furthermore, in Rotterdam, the maximum earnings exemption is set at such a level that it is never reached, implying that the financial incentives to work more hours do not stop at a certain income level. This is the case in Amsterdam, where the maximum earnings exemption is reached at an income of 740 euros per month. However, despite these differences in setup of the earnings exemptions between Amsterdam and Rotterdam, the differences in estimated effects are actually fairly small.

8 Robustness checks

This section discusses the results from the robustness checks. First, I show that an alternative estimator for the causal effect of the increased earnings exemption, namely the synthetic difference-in-differences (SDD) estimator, finds similar effect sizes as the baseline estimator. Second, I present the results from the placebo regressions with alternative treatment municipalities, finding no effects. In Appendix E.2, I discuss another alternative estimator, namely the difference-in-difference-in-differences estimator. These results are also in line with the baseline regression results, but are estimated on a selective part of the SA population.

8.1 Synthetic difference-in-differences (SDD)

The SDD estimator, developed by Arkhangelsky et al. (2021), is an extension of the DD estimator discussed in Section 6.1 and the synthetic control estimator of Abadie et al. (2010). Through weighting of the control units and the pre-treatment time periods, the SDD estimator is less reliant on the parallel trends assumption. The parallel trends assumption in the DD estimation may not hold if there are unobserved differences between the treated and control municipalities over time, such as changes in the guidance given to SA recipients. This may introduce bias in the estimates of the treatment effects.

For the SDD estimation, I aggregate the data at the municipality level, because the SDD estimator requires a balanced panel data set, where all treated units receive treatment at the

same time.¹⁷ However, this reduces the number of observations and the power of the estimation procedure. Therefore, the estimates from the SDD regressions are only intended as a robustness check on the size of the effects from the DD estimation, as the SDD regressions are much less powered and will, therefore, have more difficulty identifying any significant effects.

The unit and time weights in the SDD estimator are used in a two-way fixed-effects regression to estimate the treatment effect δ^{SDD} as

$$(\hat{\delta}^{SDD}, \hat{\alpha}, \hat{\beta}, \hat{\gamma}, \hat{\zeta}) = \arg \min_{\delta, \alpha, \beta, \gamma, \zeta} \left\{ \sum_{m=1}^M \sum_{t=1}^T (y_{it} - \alpha - \beta D_m - \gamma POST_t - \delta^{SDD} - \zeta X_{mt})^2 \hat{\omega}_m^{SDD} \hat{\lambda}_t^{SDD} \right\}, \quad (2)$$

where $\hat{\omega}_m^{SDD}$ and $\hat{\lambda}_t^{SDD}$ are the estimated unit and time weights for municipality m at time t , respectively.

The results from the SDD estimation are given in Table 5. These estimated effects are fairly similar in magnitude to those in the baseline results in Table 3. The SDD estimates for the effect on hours worked are slightly bigger than the DD estimates, and the SDD estimates for the effect on labor income are slightly smaller. The other coefficients are almost identical to the baseline estimates. However, as expected, all SDD estimates are less significant. The SDD estimation only finds one significant coefficient, namely for the effect of the increased earnings exemption on the employment rate among SA recipients, when including control variables. Although this effect is only significantly different from zero at a ten percent confidence level.

These findings confirm those from the baseline DD regression. As the magnitude of the estimated SDD coefficients is almost exactly the same for the effect on employment in the extensive margin and even slightly bigger for employment in the intensive margin as in the DD estimation, this suggests that the estimated effects in the baseline DD regressions are not biased by violations of the parallel trends assumption. For the other outcomes, the DD estimation could also not detect any effects of the increased earnings exemptions. The absence of significance in the SDD estimation is likely due to the low sample size in the estimation that results from the aggregation of the data at the municipality level and does not devalue the baseline DD results, because the goal of the SDD estimation was to test the robustness of the effect size and not of the standard errors.

¹⁷If the data set is not aggregated first, the data set is not balanced and units receive treatment at differing times. Because different households enter and exit social assistance, the household-level panel is not balanced. Furthermore, households who enter social assistance after the start of the treatment, are still eligible for the increased earnings exemption. Hence, their treatment starts later than for households who were already receiving social assistance upon the implementation of the increased earnings exemption, leading to differential treatment timing in the individual-level data set.

Table 5

SDD results of increased earnings exemptions in Amsterdam and Rotterdam combined.

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATE	0.0039 (0.002)	0.0039* (0.002)	0.1763 (0.123)	0.1780 (0.115)	0.8280 (1.969)	0.8640 (1.928)	0.0011 (0.001)	0.0011 (0.001)
N	264	264	264	264	264	264	264	264
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Estimation results from Equation 2 for the full sample. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Placebo-based standard errors at municipality level in parentheses. P-values denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

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. I use The Hague, Utrecht, and Groningen as (placebo) treated municipalities instead of Amsterdam and Rotterdam, and exclude Amsterdam and Rotterdam from the sample.

The results from the placebo regressions are given in Table 6. Panel A presents the estimates obtained when using the municipality of Groningen as the placebo treated municipality; in panel B, The Hague is used as the placebo treated municipality; and in panel C, Utrecht is used as the placebo treated municipality. The table shows no significant treatment effects for all outcomes in all three panels, except for the effect on the employment rate when using Groningen as the placebo municipality and when including control variables. However, this effect is only marginally significant at a ten percent confidence level. Hence, overall, one can conclude from Table 6 that the

Table 6*Placebo effects of increased earnings exemptions in Groningen, The Hague, and Utrecht.*

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Groningen</i>								
Treat X Post	0.0007 (0.001)	0.0046* (0.001)	-0.0363 (0.041)	0.1595 (0.046)	-1.0868 (0.471)	1.3738 (0.486)	-0.0008 (0.000)	-0.0011 (0.000)
Treatment	0.0087 (0.006)	0.0087 (0.005)	0.2602 (0.289)	0.2164 (0.205)	1.4964 (3.747)	1.2403 (2.708)	0.0012 (0.001)	-0.0005 (0.001)
Post	0.0007 (0.001)	-0.0008 (0.001)	0.1182** (0.041)	0.0645 (0.047)	2.4888*** (0.471)	1.7176** (0.487)	0.0017*** (0.000)	0.0004 (0.000)
Constant	0.0743*** (0.006)	0.0690*** (0.008)	3.9754*** (0.289)	4.9712*** (0.408)	50.0478*** (3.747)	48.1367*** (5.290)	0.0118*** (0.001)	0.0351*** (0.001)
<i>Panel B: The Hague</i>								
Treat X Post	0.0018 (0.001)	-0.0004 (0.001)	0.0334 (0.052)	-0.0844 (0.058)	0.4731 (0.608)	-0.9083 (0.584)	0.0007 (0.000)	0.0012 (0.000)
Treatment	-0.0229*** (0.002)	-0.0174*** (0.002)	-1.0774*** (0.084)	-0.7034*** (0.086)	-13.5934*** (1.267)	-9.1328*** (1.233)	-0.0020** (0.000)	-0.0015** (0.000)
Post	0.0003 (0.001)	-0.0001 (0.001)	0.1052* (0.052)	0.1097 (0.059)	2.2397** (0.608)	2.1637*** (0.587)	0.0014*** (0.000)	-0.0001 (0.000)
Constant	0.0824*** (0.002)	0.0742*** (0.010)	4.3360*** (0.084)	5.1558*** (0.398)	54.3863*** (1.267)	50.2449*** (5.145)	0.0125*** (0.000)	0.0353*** (0.001)
<i>Panel C: Utrecht</i>								
Treat X Post	-0.0009 (0.001)	-0.0007 (0.001)	-0.1049 (0.038)	-0.0686 (0.051)	-0.8752 (0.480)	-0.4882 (0.527)	-0.0006 (0.000)	-0.0014 (0.000)
Treatment	0.0024 (0.006)	-0.0006 (0.005)	0.3496 (0.285)	0.1277 (0.187)	4.2743 (3.630)	1.8448 (2.472)	0.0011 (0.001)	0.0015 (0.000)
Post	0.0009 (0.001)	-0.0001 (0.001)	0.1259** (0.038)	0.0924 (0.053)	2.4624*** (0.480)	1.9467** (0.533)	0.0017*** (0.000)	0.0004 (0.000)
Constant	0.0751*** (0.006)	0.0708*** (0.009)	3.9642*** (0.285)	4.9945*** (0.431)	49.7106*** (3.630)	48.1017*** (5.327)	0.0118*** (0.001)	0.0348*** (0.001)
N (x1,000)	2,089	1,787	2,089	1,787	2,089	1,787	2,749	2,129
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Estimation results from Equation 1, with Groningen, The Hague, and Utrecht as placebo treatment municipalities instead of Amsterdam and Rotterdam. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients. Full regression results are available in Appendix D.

DD estimator does not find any significant treatment effects in the placebo treatment municipalities.

In conclusion, the results from the DD regressions with placebo municipalities 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 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 a trial with an increased earnings exemption 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 employment and the number of hours worked by SA recipients. Employment in the extensive margin increased by about 0.27 to 0.35 percentage points (5-6.5%), and by about 0.11 to 0.16 hours per month (4-6%) in the intensive margin. The baseline analysis does not find evidence of an effect of the increased earnings exemption on labor income or exits.

The estimated effects are rather similar between Amsterdam and Rotterdam, with one notable exception. The earnings exemption in Amsterdam had a significant positive effect on exits from SA of 0.13 to 0.16 percentage points (12-15%), while it had no effect on exits in Rotterdam.

Taken together, these findings suggest that an increased earnings exemption for SA recipients can stimulate labor market participation, and that this has no negative and possibly even a positive effect on 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. Most importantly, for some of the DD regressions,

the parallel trends assumption did not seem to hold, as there were already non-parallel trends before the implementation of the increased earnings exemptions. This suggests that there are unobserved differences between the treatment and control municipalities that may bias the estimated effects, as also discussed in Section 6.1. Although Section 8 showed that the synthetic difference-in-differences estimator, which is less reliant on the validity of the parallel trends assumption, found similar treatment effects as the DD estimator, the risk of biased coefficients cannot be completely ruled out.

Furthermore, the analysis did not find an effect of the increased earnings exemptions on labor income, even though the incentives on employment work through the earnings from work. This is a surprising and somewhat contradictory finding, although a possible explanation may lie in the inability of the estimator to identify an effect due to the higher volatility of labor earnings or the violation of the parallel trends assumption for labor earnings.

Additionally, 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.

Moreover, 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.

Finally, this paper only studies short-term effects of earnings exemptions. However, it may take time for the effect of an increased earnings exemption to materialize. It may take time before information on the availability of the earnings exemption to reach SA recipients. Moreover, SA recipients may feel distrustful of new policies and, thus, not respond right away. Even if individuals feel incentivized by the increased earnings exemption, finding work might also take time. Hence, when studied over a longer period than one year, the positive effects of the earnings exemption may be bigger.

Future research could, therefore, also study longer-term effects of increased earnings exemptions for SA recipients. Amsterdam would be an interesting case to study as the trial with the increased earnings exemption is still continuing there at least until the end of 2023. Relatedly, such a study could also analyse the dynamic effects of earnings exemptions. Additionally, future research could consider the effects of earnings exemptions on other outcomes, such as poverty, mental health and debts. As increased earnings exemptions allow SA recipients to keep a (higher) share of their earnings, this should positively affect their financial position and, thus, 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 debt has not yet been established. Furthermore, 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 weigh up against the costs of the earnings exemption. Finally, this study found that the earnings exemption in Amsterdam significantly increased exits from SA, while it did not in Rotterdam. However, the question remains what causes this difference between the two municipalities and why this difference appears only in exits and not in the other outcome variables. This relates to a larger question still to be answered: what causes a certain financial incentive to be effective in one situation, while it is not in another situation? And what is the role of the design of the incentive and of the institutional context in the effectiveness of such a policy? These would be fruitful topics for further research.

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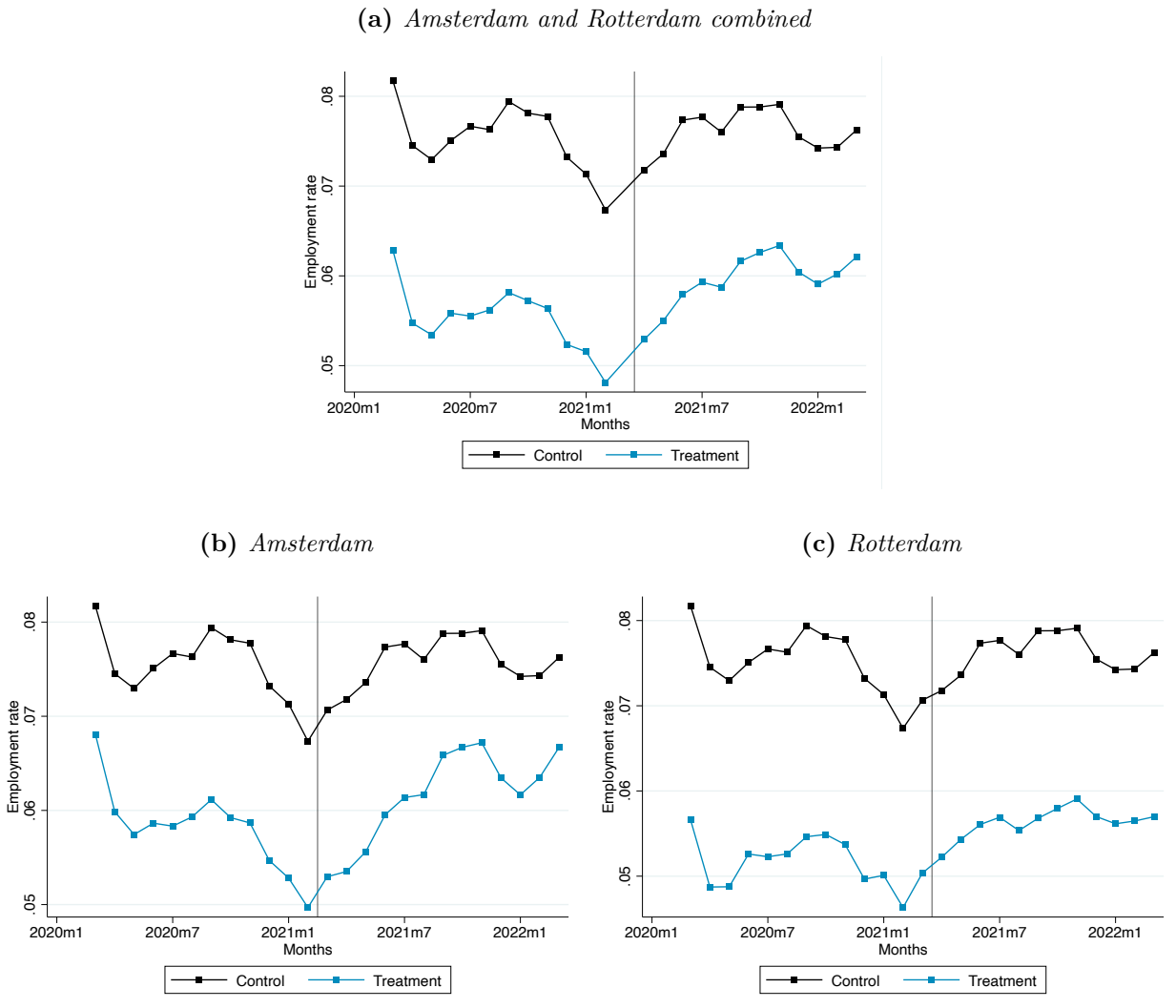
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Appendix A Trend plots

Figure A1

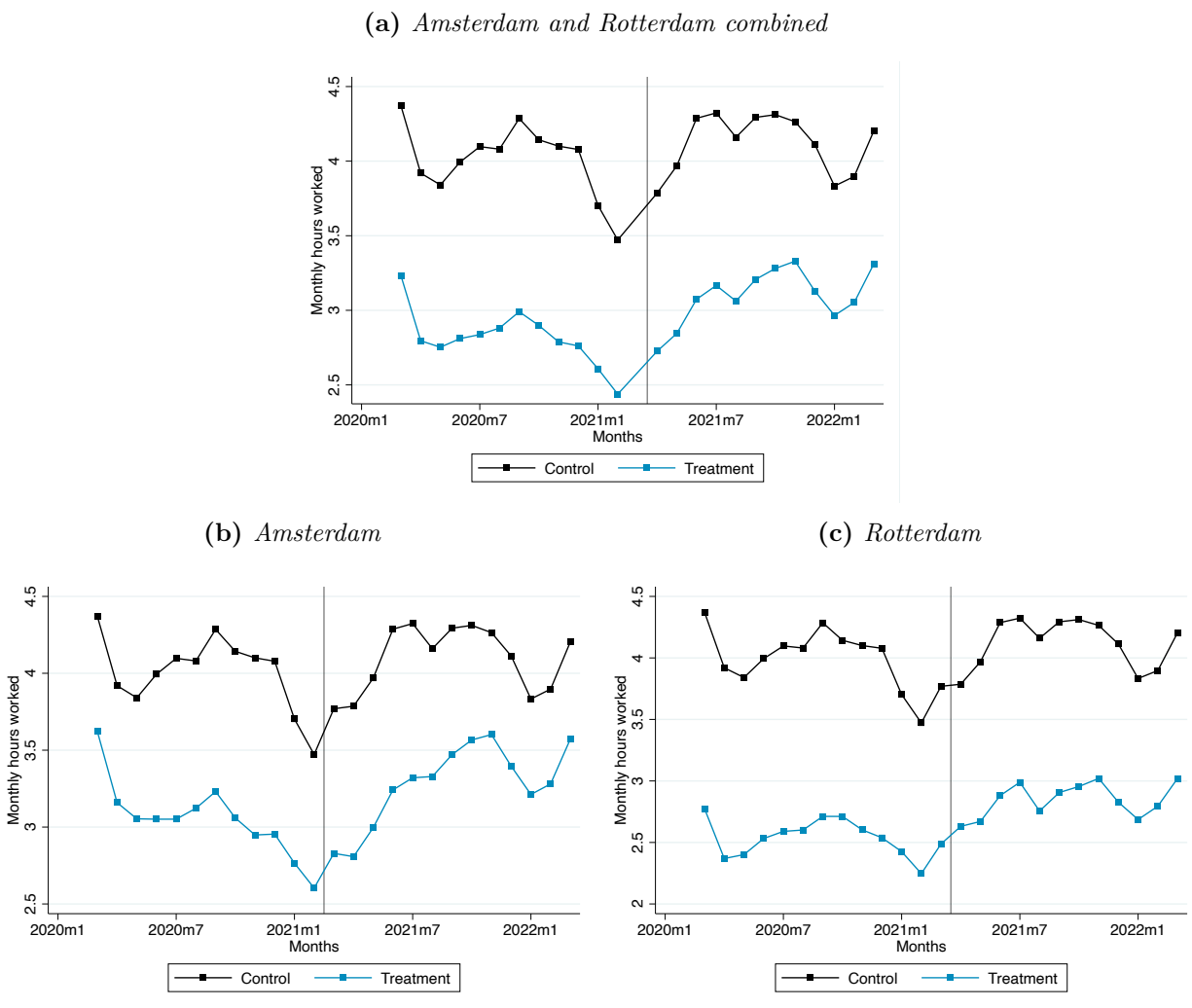
Trend plots of employment rates in treatment and control municipalities.



Note. The treatment group in (a) consists of households on SA from Amsterdam and Rotterdam; in (b) of households on SA from Amsterdam; and in (c) of households on SA from Rotterdam. In all three graphs, the control group consists of households on SA from Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven. The vertical line symbols the start of the treatment in April 2021 in (a), in March 2021 in (b), and in April 2021 in (c).

Figure A2

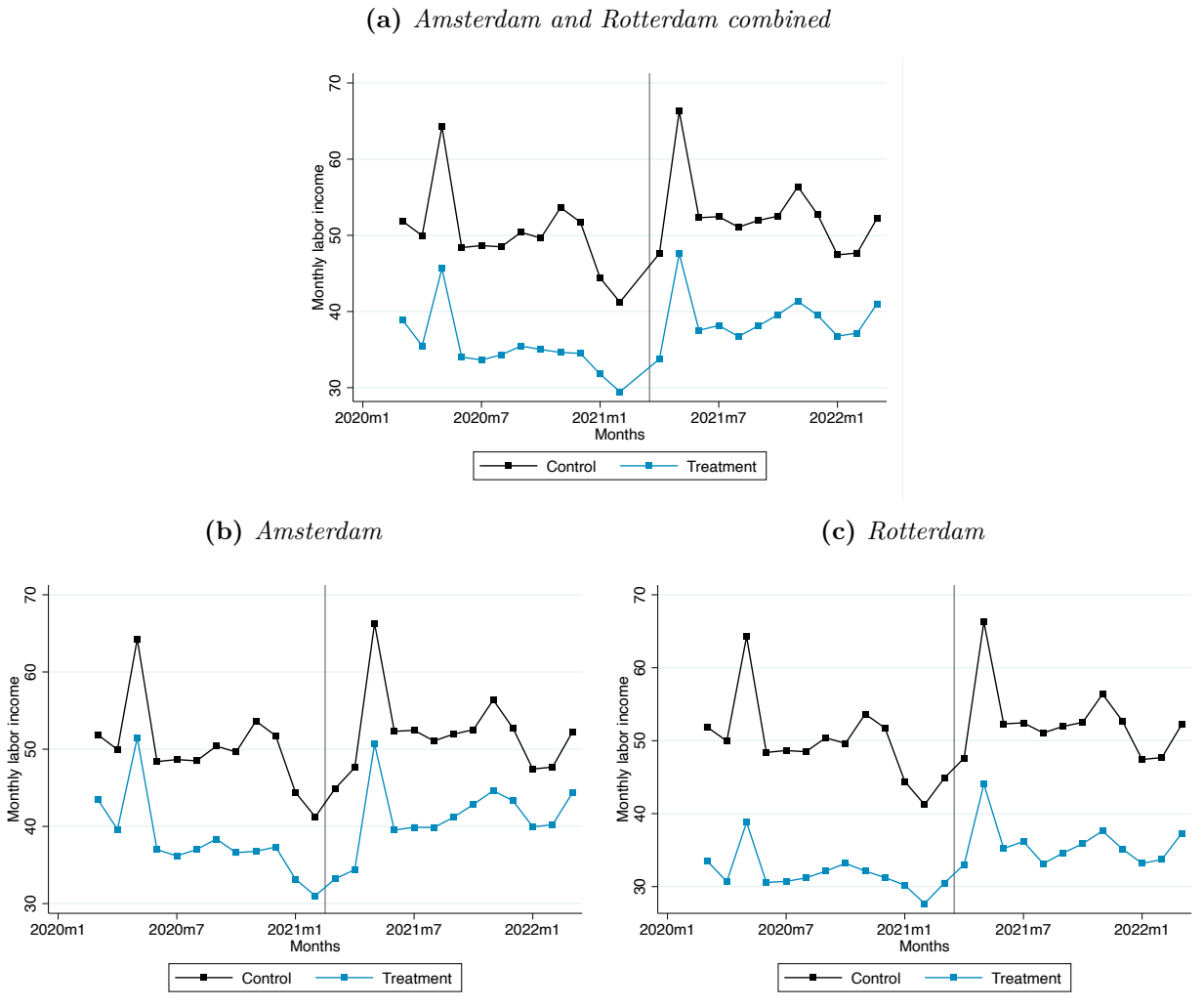
Trend plots of hours worked in treatment and control municipalities.



Note. The treatment group in (a) consists of households on SA from Amsterdam and Rotterdam; in (b) of households on SA from Amsterdam; and in (c) of households on SA from Rotterdam. In all three graphs, the control group consists of households on SA from Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven. The vertical line symbols the start of the treatment in April 2021 in (a), in March 2021 in (b), and in April 2021 in (c).

Figure A3

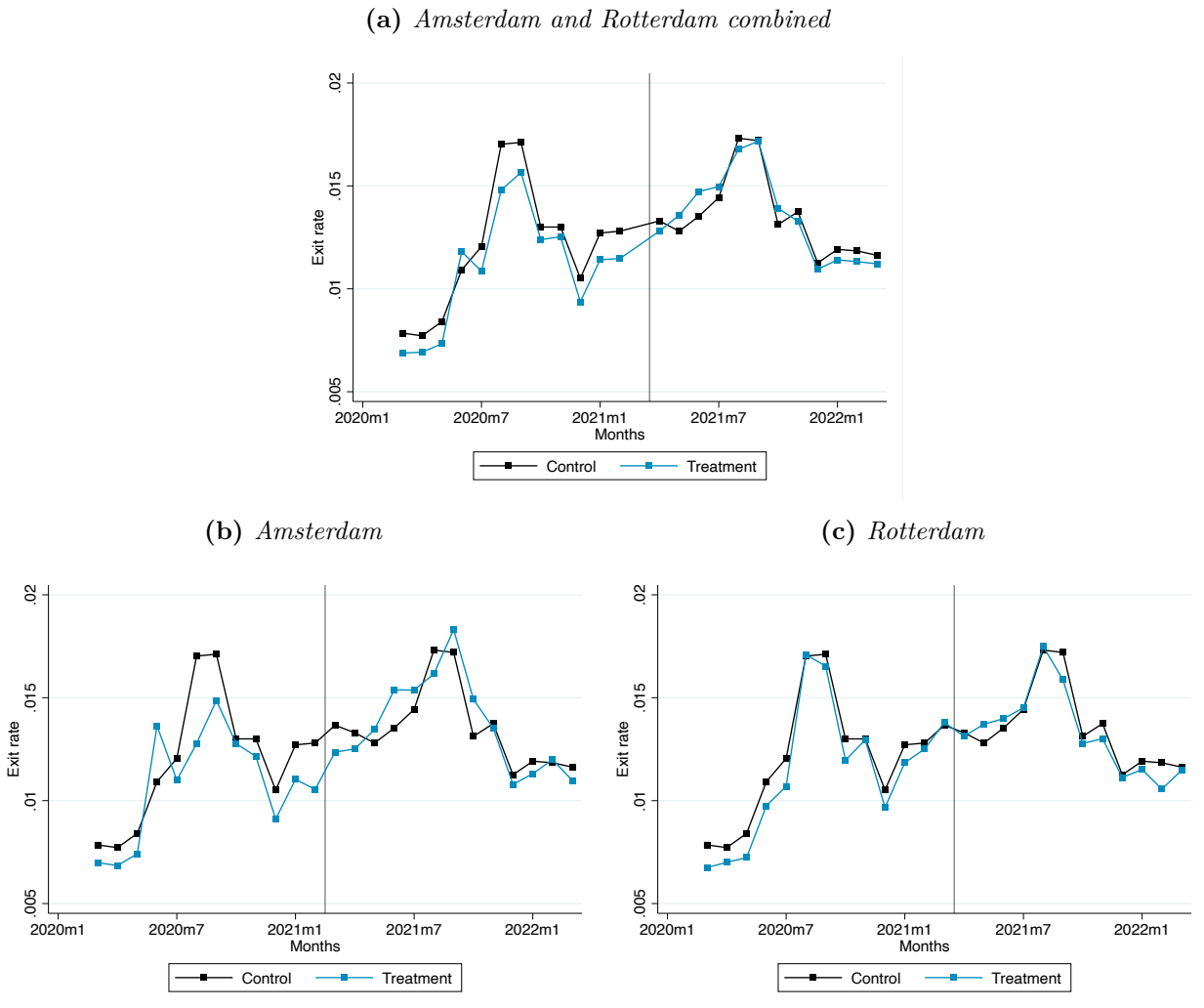
Trend plots of labor income in treatment and control municipalities.



Note. The treatment group in (a) consists of households on SA from Amsterdam and Rotterdam; in (b) of households on SA from Amsterdam; and in (c) of households on SA from Rotterdam. In all three graphs, the control group consists of households on SA from Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven. The vertical line symbols the start of the treatment in April 2021 in (a), in March 2021 in (b), and in April 2021 in (c).

Figure A4

Trend plots of exit rates in treatment and control municipalities.



Note. The treatment group in (a) consists of households on SA from Amsterdam and Rotterdam; in (b) of households on SA from Amsterdam; and in (c) of households on SA from Rotterdam. In all three graphs, the control group consists of households on SA from Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven. The vertical line symbols the start of the treatment in April 2021 in (a), in March 2021 in (b), and in April 2021 in (c).

Appendix B Additional descriptive statistics

Table B1

Descriptive statistics for Amsterdam only

Variables	Pre			Post		
	Treatment	Control	T-C	Treatment	Control	T-C
<i>Dependent variables</i>						
Employment rate	0.058	0.075	-0.017***	0.061	0.076	-0.014***
Monthly hours worked	3.050	4.006	-0.957***	3.273	4.092	-0.819***
Monthly labor income	38.138	50.226	-12.088***	41.032	51.972	-10.940***
Exits	0.011	0.012	-0.001***	0.014	0.014	0.000
<i>Independent variables</i>						
Age of first adult	48.780	45.679	3.101***	49.821	46.721	3.101***
Age of second adult	50.529	49.303	1.226***	51.042	49.955	1.087***
Number of females	0.556	0.571	-0.014***	0.556	0.568	-0.012***
Number of children	0.642	0.653	-0.011***	0.619	0.631	-0.012***
Recent work history (months)	8.550	8.932	-0.382***	8.557	8.902	-0.345***
Highest education = Low	0.554	0.553	0.001	0.554	0.553	0.001
Highest education = Middle	0.303	0.320	-0.017***	0.303	0.320	-0.017***
Highest education = High	0.143	0.127	0.016***	0.143	0.127	0.016***
Couple SA receiver	0.130	0.142	-0.012***	0.127	0.138	-0.011***
N (x1,000)	697	1,374		755	1,489	

Note. T-C is the difference between the treatment (T) and control (C) group. The treatment group consists of household on SA from Amsterdam. The treated group consists of households on SA from Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven. 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. Dependent variables employment rate, monthly hours worked, and monthly labor income concern employment, hours worked and income while also receiving SA.

Table B2*Descriptive statistics for Rotterdam only*

Variables	Pre			Post		
	Treatment	Control	T-C	Treatment	Control	T-C
<i>Dependent variables</i>						
Employment rate	0.052	0.075	-0.023***	0.056	0.076	-0.020***
Monthly hours worked	2.537	3.988	-1.451***	2.844	4.120	-1.276***
Monthly labor income	31.718	49.814	-18.097***	35.769	52.584	-16.815***
Exits	0.011	0.012	-0.001***	0.013	0.014	0.000
<i>Independent variables</i>						
Age of first adult	47.524	45.721	1.803***	48.566	46.762	1.804***
Age of second adult	50.857	49.318	1.539***	51.332	49.993	1.339***
Number of females	0.603	0.571	0.033***	0.602	0.568	0.034***
Number of children	0.730	0.652	0.078***	0.702	0.630	0.072***
Recent work history (months)	7.732	8.932	-1.199***	7.728	8.899	-1.171***
Highest education = Low	0.600	0.553	0.047***	0.600	0.553	0.047***
Highest education = Middle	0.317	0.320	-0.003***	0.317	0.320	-0.003***
Highest education = High	0.082	0.127	-0.045***	0.082	0.127	-0.045***
Couple SA receiver	0.136	0.142	-0.006***	0.132	0.138	-0.006***
N (x1,000)	668	1,489		617	1,374	

Note. T-C is the difference between the treatment (T) and control (C) group. The treatment group consists of household on SA from Rotterdam. The treated group consists of households on SA from Groningen, Almere, Leeuwarden, Enschede, Arnhem, Nijmegen, Utrecht, The Hague, and Eindhoven. 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. Dependent variables employment rate, monthly hours worked, and monthly labor income concern employment, hours worked and income while also receiving SA.

Appendix C Parallel trends assessment

To test for parallel pre-trends, I estimate the following regression:

$$y_{it} = \alpha + \beta D_i + \gamma month_t + \sum_{k=-11}^{13} \delta_k^{DD} D_i \cdot month_k + \zeta X_{it} + \epsilon_{it}, \quad (3)$$

where δ_k^{DD} is the dynamic difference-in-differences coefficient. For all values of k smaller than zero (leads), these coefficients should not differ significantly from zero for the pre-trends to be parallel.

Table C3 gives the estimated δ_k^{DD} coefficients for all values of k smaller than zero (all leads). The table shows that the pre-trends in columns one, three, seven, and eight are (approximately) parallel. These are the regressions with outcomes employment, number of hours worked, and exits. For employment and hours worked, the pre-trends are only parallel without control variables. With control variables, there are significant differences between the treatment and control municipalities in some months, as compared to $k = -12$. For labor income, the pre-trends are not parallel both with and without control variables.

The fact that the parallel pre-trends do not hold for labor income, employment with controls, and hours worked with controls, implies that the difference-in-differences estimator may give biased estimates in these regressions. Hence, we should treat those outcomes with caution and consider alternative estimator that are less reliant on the validity of the parallel trends assumption.

Table C3*Tests of parallel pre-trends.*

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
k = -11	-0.001 (0.002)	-0.001 (0.002)	0.015 (0.109)	-0.029 (0.116)	-1.488 (1.432)	-1.902 (1.524)	0.000 (0.001)	0.000 (0.001)
k = -10	-0.001 (0.002)	-0.001 (0.002)	0.051 (0.109)	-0.001 (0.116)	-5.670*** (1.430)	-6.831*** (1.520)	0.000 (0.001)	0.000 (0.001)
k = -9	0.000 (0.002)	-0.001 (0.002)	-0.045 (0.109)	-0.124 (0.116)	-1.416 (1.429)	-2.431 (1.519)	0.002*** (0.001)	0.001 (0.001)
k = -8	-0.002 (0.002)	-0.003* (0.002)	-0.122 (0.109)	-0.206* (0.115)	-2.046 (1.429)	-3.164** (1.518)	0.000 (0.001)	0.000 (0.001)
k = -7	-0.001 (0.002)	-0.002 (0.002)	-0.061 (0.109)	-0.162 (0.116)	-1.221 (1.430)	-2.454 (1.520)	-0.001** (0.001)	-0.002* (0.001)
k = -6	-0.002 (0.002)	-0.004** (0.002)	-0.156 (0.109)	-0.277** (0.116)	-1.963 (1.431)	-3.327** (1.521)	0.000 (0.001)	0.000 (0.001)
k = -5	-0.002 (0.002)	-0.004** (0.002)	-0.106 (0.109)	-0.240** (0.116)	-1.657 (1.430)	-3.166** (1.519)	0.000 (0.001)	0.001 (0.001)
k = -4	-0.002 (0.002)	-0.004** (0.002)	-0.174 (0.109)	-0.284** (0.116)	-6.066*** (1.430)	-7.781*** (1.519)	0.001 (0.001)	0.000 (0.001)
k = -3	-0.002 (0.002)	-0.004** (0.002)	-0.178 (0.109)	-0.338*** (0.115)	-4.270*** (1.428)	-6.558*** (1.516)	0.000 (0.001)	0.000 (0.001)
k = -2	-0.001 (0.002)	-0.003** (0.002)	0.044 (0.109)	-0.109 (0.115)	0.339 (1.427)	-1.702 (1.515)	0.000 (0.001)	-0.001 (0.001)
k = -1	0.000 (0.002)	-0.002** (0.002)	0.105 (0.109)	-0.040 (0.115)	1.198 (1.426)	-0.588 (1.514)	0.000 (0.001)	-0.001 (0.001)
Constant	0.082*** (0.001)	0.065*** (0.001)	4.371*** (0.054)	4.282*** (0.090)	51.883*** (0.709)	37.809*** (1.182)	0.008*** (0.000)	0.030*** (0.001)
N (x1,000)	4,112	3,417	4,112	3,417	4,112	3,417	5,377	4,046
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Estimation results from Equation 3 for Amsterdam and Rotterdam combined. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients. Full regression results are available in Appendix D.

Appendix D Detailed regression results

Table D1

Full DD effects of increased earnings exemptions in Amsterdam and Rotterdam combined.

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	0.0035*** (0.001)	0.0027** (0.001)	0.1657*** (0.039)	0.1150* (0.046)	1.3659** (0.453)	0.6658 (0.471)	0.0010* (0.000)	0.0010 (0.001)
Treatment	-0.0202** (0.006)	-0.0147** (0.004)	-1.1920** (0.321)	-0.8951*** (0.181)	-15.0116*** (4.045)	-11.4084*** (2.352)	-0.0010 (0.001)	0.0003 (0.001)
Post	0.0008 (0.001)	-0.0003 (0.001)	0.1134** (0.036)	0.0744 (0.046)	2.3586*** (0.433)	1.7963*** (0.463)	0.0016*** (0.000)	0.0002 (0.000)
Highest educ. = Middle		-0.0017 (0.002)		0.1001 (0.108)		1.7171 (1.168)		0.0015*** (0.000)
Highest educ. = High		0.0038 (0.002)		0.3999*** (0.116)		5.3239*** (1.293)		0.0060*** (0.000)
Single person		-0.0343*** (0.003)		-2.7072*** (0.221)		-31.9519*** (2.537)		-0.0027*** (0.000)
Age		-0.0006*** (0.000)		-0.0320*** (0.006)		-0.2241*** (0.071)		-0.0004*** (0.000)
Female		0.0135*** (0.002)		0.6886*** (0.112)		9.3348*** (1.465)		-0.0042*** (0.000)
Number of children		0.0051*** (0.001)		0.2485*** (0.048)		4.0256*** (0.558)		-0.0007*** (0.000)
Work history		0.0063*** (0.000)		0.3396*** (0.016)		4.4677*** (0.218)		0.0003*** (0.000)
Constant	0.0754*** (0.006)	0.0753*** (0.006)	4.0063*** (0.260)	4.8080*** (0.264)	50.2258*** (3.307)	47.5565*** (3.476)	0.0119*** (0.001)	0.0346*** (0.001)
N (x1,000)	4,112	3,417	4,112	3,417	4,112	3,417	5,377	4,046
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Full 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. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

Table D2*Full DD effects of increased earnings exemptions in Amsterdam separately.*

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	0.0030**	0.0023	0.1381**	0.0968	1.1482	0.5831	0.0013**	0.0016*
	(0.001)	(0.001)	(0.034)	(0.044)	(0.407)	(0.439)	(0.000)	(0.000)
Treatment	-0.0172*	-0.0154*	-0.9567**	-0.8517**	-12.0881**	-10.9738**	-0.0012	-0.0003
	(0.006)	(0.004)	(0.261)	(0.177)	(3.324)	(2.345)	(0.001)	(0.000)
Post	0.0003	-0.0009	0.0856**	0.0364	1.7463***	1.0304**	0.0016***	0.0003
	(0.001)	(0.001)	(0.034)	(0.044)	(0.407)	(0.429)	(0.000)	(0.000)
Highest educ.		-0.0000		0.1482		2.3991		0.0015***
= Middle		(0.002)		(0.140)		(1.406)		(0.000)
Highest educ.		0.0052*		0.4220**		5.6666**		0.0061***
= High		(0.002)		(0.153)		(1.724)		(0.000)
Single person		-0.0352***		-2.9358***		-34.3380***		-0.0028***
		(0.003)		(0.149)		(2.163)		(0.000)
Age		-0.0005***		-0.0302***		-0.1925*		-0.0004***
		(0.000)		(0.008)		(0.085)		(0.000)
Female		0.0136***		0.6897***		9.4831***		-0.0045***
		(0.003)		(0.137)		(1.802)		(0.000)
Number of children		0.0049***		0.2297**		3.8967***		-0.0008***
		(0.001)		(0.064)		(0.756)		(0.000)
Work history		0.0065***		0.3554***		4.6703***		0.0002***
		(0.000)		(0.009)		(0.144)		(0.000)
Constant	0.0754***	0.0711***	4.0063***	4.7961***	50.2258***	46.3423***	0.0119***	0.0351***
	(0.006)	(0.006)	(0.261)	(0.334)	(3.324)	(4.031)	(0.001)	(0.001)
N (x1,000)	3,306	2,715	3,306	2,715	3,306	2,715	4,316	3,220
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Full estimation results from Equation 1 for Amsterdam separately. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

Table D3*Full DD effects of increased earnings exemptions in Rotterdam separately.*

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	0.0035** (0.001)	0.0025* (0.001)	0.1751** (0.036)	0.1119 (0.045)	1.2811* (0.431)	0.3905 (0.480)	0.0004 (0.000)	0.0003 (0.000)
Treatment	-0.0234** (0.006)	-0.0137* (0.004)	-1.4510** (0.262)	-0.9167** (0.173)	-18.0965*** (3.317)	-11.4612** (2.245)	-0.0007 (0.001)	0.0009 (0.000)
Post	0.0011 (0.001)	0.0004 (0.001)	0.1316*** (0.036)	0.1106** (0.045)	2.7699*** (0.431)	2.4447*** (0.462)	0.0014*** (0.000)	0.0002 (0.000)
Highest educ. = Middle		-0.0015 (0.003)		0.1722 (0.122)		2.2044 (1.415)		0.0014*** (0.000)
Highest educ. = High		0.0044 (0.003)		0.5145*** (0.115)		6.3667*** (1.569)		0.0060*** (0.000)
Single person		-0.0332*** (0.003)		-2.6297*** (0.263)		-31.2723*** (3.172)		-0.0028*** (0.000)
Age		-0.0006*** (0.000)		-0.0380*** (0.003)		-0.2927*** (0.036)		-0.0004*** (0.000)
Female		0.0156*** (0.002)		0.8170*** (0.077)		10.9413*** (0.989)		-0.0041*** (0.000)
Number of children		0.0056*** (0.001)		0.2655*** (0.056)		4.1691*** (0.688)		-0.0007*** (0.000)
Work history		0.0064*** (0.000)		0.3385*** (0.021)		4.4634*** (0.294)		0.0003*** (0.000)
Constant	0.0750*** (0.006)	0.0745*** (0.007)	3.9881*** (0.262)	4.8782*** (0.296)	49.8145*** (3.317)	48.3105*** (3.906)	0.0121*** (0.001)	0.0344*** (0.001)
N (x1,000)	3,159	2,711	3,159	2,711	3,159	2,711	4,149	3,215
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Full estimation results from Equation 1 for Rotterdam separately. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * p < 0.10, ** p < 0.05, *** p < 0.01. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

Table D4

Full placebo DD effects of increased earnings exemptions with Groningen as placebo treatment municipality.

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	0.0007 (0.001)	0.0046* (0.001)	-0.0363 (0.041)	0.1595 (0.046)	-1.0868 (0.471)	1.3738 (0.486)	-0.0008 (0.000)	-0.0011 (0.000)
Treatment	0.0087 (0.006)	0.0087 (0.005)	0.2602 (0.289)	0.2164 (0.205)	1.4964 (3.747)	1.2403 (2.708)	0.0012 (0.001)	-0.0005 (0.001)
Post	0.0007 (0.001)	-0.0008 (0.001)	0.1182** (0.041)	0.0645 (0.047)	2.4888*** (0.471)	1.7176** (0.487)	0.0017*** (0.000)	0.0004 (0.000)
Highest educ. = Middle		0.0003 (0.003)		0.2619 (0.151)		3.4096* (1.705)		0.0015*** (0.000)
Highest educ. = High		0.0052 (0.003)		0.5484*** (0.156)		7.0174** (2.199)		0.0061*** (0.001)
Single person		-0.0342*** (0.004)		-2.9471*** (0.214)		-34.8728*** (3.137)		-0.0030*** (0.000)
Age		-0.0006*** (0.000)		-0.0396*** (0.004)		-0.2958*** (0.047)		-0.0004*** (0.000)
Female		0.0168*** (0.002)		0.8630*** (0.090)		11.8497*** (0.915)		-0.0043*** (0.000)
Number of children		0.0059*** (0.001)		0.2637*** (0.081)		4.2235*** (1.003)		-0.0008*** (0.000)
Work history		0.0068*** (0.000)		0.3643*** (0.012)		4.8296*** (0.169)		0.0002*** (0.000)
Constant	0.0743*** (0.006)	0.0690*** (0.008)	3.9754*** (0.289)	4.9712*** (0.408)	50.0478*** (3.747)	48.1367*** (5.290)	0.0118*** (0.001)	0.0351*** (0.001)
N (x1,000)	2,089	1,787	2,089	1,787	2,089	1,787	2,749	2,129
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Full estimation results from Equation 1, with Groningen as the placebo treatment municipality instead of Amsterdam and Rotterdam. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

Table D5

Full placebo DD effects of increased earnings exemptions with The Hague as placebo treatment municipality.

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	0.0018 (0.001)	-0.0004 (0.001)	0.0334 (0.052)	-0.0844 (0.058)	0.4731 (0.608)	-0.9083 (0.584)	0.0007 (0.000)	0.0012 (0.000)
Treatment	-0.0229*** (0.002)	-0.0174*** (0.002)	-1.0774*** (0.084)	-0.7034*** (0.086)	-13.5934*** (1.267)	-9.1328*** (1.233)	-0.0020** (0.000)	-0.0015** (0.000)
Post	0.0003 (0.001)	-0.0001 (0.001)	0.1052* (0.052)	0.1097 (0.059)	2.2397** (0.608)	2.1637*** (0.587)	0.0014*** (0.000)	-0.0001 (0.000)
Highest educ. = Middle		-0.0003 (0.003)		0.2264 (0.155)		2.8381 (1.716)		0.0014*** (0.000)
Highest educ. = High		0.0046 (0.003)		0.5027** (0.176)		6.2162** (2.432)		0.0059*** (0.001)
Single person		-0.0335*** (0.004)		-2.9179*** (0.225)		-34.4835*** (3.270)		-0.0030*** (0.000)
Age		-0.0006*** (0.000)		-0.0386*** (0.004)		-0.2814*** (0.046)		-0.0004*** (0.000)
Female		0.0161*** (0.002)		0.8400*** (0.100)		11.5913*** (1.051)		-0.0043*** (0.000)
Number of children		0.0061*** (0.002)		0.2738*** (0.085)		4.3774*** (1.047)		-0.0007*** (0.000)
Work history		0.0068*** (0.000)		0.3644*** (0.011)		4.8315*** (0.169)		0.0002*** (0.000)
Constant	0.0824*** (0.002)	0.0742*** (0.010)	4.3360*** (0.084)	5.1558*** (0.398)	54.3863*** (1.267)	50.2449*** (5.145)	0.0125*** (0.000)	0.0353*** (0.001)
N (x1,000)	2,089	1,787	2,089	1,787	2,089	1,787	2,749	2,129
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Full estimation results from Equation 1, with The Hague as the placebo treatment municipality instead of Amsterdam and Rotterdam. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

Table D6

Full placebo DD effects of increased earnings exemptions with Utrecht as placebo treatment municipality.

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	-0.0009 (0.001)	-0.0007 (0.001)	-0.1049 (0.038)	-0.0686 (0.051)	-0.8752 (0.480)	-0.4882 (0.527)	-0.0006 (0.000)	-0.0014 (0.000)
Treatment	0.0024 (0.006)	-0.0006 (0.005)	0.3496 (0.285)	0.1277 (0.187)	4.2743 (3.630)	1.8448 (2.472)	0.0011 (0.001)	0.0015 (0.000)
Post	0.0009 (0.001)	-0.0001 (0.001)	0.1259** (0.038)	0.0924 (0.053)	2.4624*** (0.480)	1.9467** (0.533)	0.0017*** (0.000)	0.0004 (0.000)
Highest educ. = Middle		0.0009 (0.003)		0.2818 (0.145)		3.5584* (1.631)		0.0015*** (0.000)
Highest educ. = High		0.0065* (0.003)		0.5806*** (0.141)		7.2144*** (1.969)		0.0060*** (0.001)
Single person		-0.0343*** (0.004)		-2.9466*** (0.216)		-34.8411*** (3.153)		-0.0030*** (0.000)
Age		-0.0006*** (0.000)		-0.0399*** (0.004)		-0.2975*** (0.048)		-0.0004*** (0.000)
Female		0.0165*** (0.002)		0.8587*** (0.087)		11.8389*** (0.901)		-0.0043*** (0.000)
Number of children		0.0058*** (0.002)		0.2590*** (0.081)		4.1838*** (1.001)		-0.0008*** (0.000)
Work history		0.0067*** (0.000)		0.3642*** (0.012)		4.8288*** (0.169)		0.0002*** (0.000)
Constant	0.0751*** (0.006)	0.0708*** (0.009)	3.9642*** (0.285)	4.9945*** (0.431)	49.7106*** (3.630)	48.1017*** (5.327)	0.0118*** (0.001)	0.0348*** (0.001)
N (x1,000)	2,089	1,787	2,089	1,787	2,089	1,787	2,749	2,129
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Full estimation results from Equation 1, with Utrecht as the placebo treatment municipality instead of Amsterdam and Rotterdam. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

Appendix E Additional robustness tests

E.1 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.

Table E1

Baseline effects of increased earnings exemptions in Amsterdam and Rotterdam combined, including movers.

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post	0.0035*** (0.001)	0.0026** (0.001)	0.1664*** (0.035)	0.1085* (0.040)	1.4124** (0.428)	0.6397 (0.401)	0.0011* (0.000)	0.0011 (0.001)
Treatment	-0.0198** (0.006)	-0.0143** (0.004)	-1.1691** (0.314)	-0.8746*** (0.178)	-14.7395** (3.966)	-11.1795*** (2.324)	-0.0004 (0.001)	0.0012 (0.001)
Post	0.0011 (0.001)	0.0001 (0.001)	0.1280*** (0.033)	0.0935* (0.040)	2.4981*** (0.410)	1.9878*** (0.396)	0.0014*** (0.000)	0.0022*** (0.000)
Highest educ. = Middle		-0.0018 (0.002)		0.0899 (0.104)		1.5947 (1.111)		0.0012*** (0.000)
Highest educ. = High		0.0043* (0.002)		0.4303*** (0.125)		5.6927*** (1.372)		0.0069*** (0.001)
Single person		-0.0353*** (0.003)		-2.7764*** (0.224)		-32.8151*** (2.613)		-0.0004 (0.000)
Age		-0.0005*** (0.000)		-0.0301*** (0.006)		-0.2018** (0.069)		-0.0004*** (0.000)
Female		0.0130*** (0.002)		0.6701*** (0.108)		9.0964*** (1.398)		-0.0034*** (0.000)
Number of children		0.0052*** (0.001)		0.2576*** (0.048)		4.1179*** (0.555)		-0.0010*** (0.000)
Work history		0.0062*** (0.000)		0.3354*** (0.016)		4.4123*** (0.213)		0.0003*** (0.000)
Constant	0.0747*** (0.005)	0.0746*** (0.006)	3.9689*** (0.253)	4.7594*** (0.248)	49.7579*** (3.220)	47.0951*** (3.309)	0.0102*** (0.001)	0.0280*** (0.002)
N (x1,000)	4,217	3,505	4,217	3,505	4,217	3,505	4,287	3,560
Controls	No	Yes	No	Yes	No	Yes	No	Yes

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. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.

E.2 Difference-in-difference-in-differences (DDD)

Next, I use the difference-in-difference-in-differences estimator of Olden and Møen (2022) as an additional robustness check on the sign of the effects of the increased earnings exemption. By including an extra layer of differences as compared to the baseline DD estimator, the DDD estimator may reduce possible bias in the DD estimate because of omitted variables or violations of the parallel trends assumption. However, the extra layer of differences in this case requires restricting the sample to include only younger SA recipients, which reduces the sample size and external validity of the estimates. Hence, the effect size found in the DDD estimation need not be of similar size as the effects resulting from the DD estimation.

I use the age threshold that determines eligibility for the increased exemption as the extra layer of differences in the DDD estimation.¹⁸ I use households where all adults are younger than 26 years and 7 months old as additional ineligible units in the DDD estimation, and households with at least one adult older than 27 as additional eligible units. To keep the treated units comparable to the control units, I then restrict the age of the eligible units to only include individuals between ages 27 and 30.

I estimate the DDD estimate of interest δ^{DDD} with the following regression specification:

$$y_{igt} = \alpha + \beta D_i + \gamma POST_t + \delta^{DDD}(D_i \cdot POST_t \cdot AGE_{it}) + \eta_{1,a} AGE_{it} + \eta_{2,a} POST_t \cdot AGE_{it} + \eta_{3,ag} D_i \cdot AGE_{it} + \eta_4 D_i \cdot POST_t + \zeta X_{it} + \epsilon_{it}, \quad (4)$$

where AGE_{it} is a dummy equal to one if at least one individual in the household i is older than 27 years. $\eta_{1,g}$ is the average difference in y_{igt} for those older than 27 years with respect to the ineligible group, $\eta_{2,g}$ gives the possible trends differences between those younger than 26 years and 7 months and those older than 27 years, $\eta_{3,g}$ captures possible differences between the treatment and control group for those that are older than 27 years, and η_4 estimates the average difference between the treated and control municipalities over time.

The identifying assumptions of DDD are similar to that of DD. Namely both, a variant of the parallel trends assumption and SUTVA need to hold. In the DDD specification, the parallel trends assumption implies that the difference in outcomes between the eligible and non-eligible groups in

¹⁸Namely, in Amsterdam, social assistance recipients younger than 27 years old save up the money from the earnings exemption and receive it once they turn 27 or exit social assistance. In Rotterdam, social assistance recipients younger than 26 years and 7 months old are not eligible. Social assistance recipients between 26 years and 7 months old and 27 years old save up the money from the increased earnings exemption if they work, and get paid once they turn 27.

the treated municipalities should develop similarly over time as in the control municipalities (Olden & Møen, 2022). SUTVA requires that there are no spillovers between the treated and control units. Since I allow for individuals to 'switch' from the ineligible to the eligible group when they become old enough during the observed time period, SUTVA could be theoretically violated.¹⁹ However, as argued by Cammeraat et al. (2022), such violation of SUTVA is most likely not a big problem, as long as the share of switchers is relatively small. By taking a relatively large bandwidth of ages around the age threshold, the share of non-switchers is much larger than the share of switchers.²⁰

Table E2 presents the results of the DDD estimation for Amsterdam and Rotterdam pooled. The first row gives the estimated DDD coefficient of interest. For all outcomes, the estimated signs of the effects are similar to the DD estimates. They are, however, larger but less significant. This can be explained by the fact that the DDD regressions only include younger SA recipients, who generally reintegrate faster onto the labor market. Hence, they might also respond the strongest to an earnings exemption. Moreover, the smaller sample in the DDD regression results in a less powered regression, making it more difficult to detect any effect of the earnings exemptions.

Overall, these results are still in line with the results from the baseline DD regressions, as the signs of the coefficients are all still the same. However, as explained above, the coefficients cannot be compared directly to those in the baseline regression, as they concern only a part of the SA population.

¹⁹This could be the case, for example, if individuals know they are going to become old enough for the treatment and hence already act as if they are eligible (anticipation).

²⁰However, the switchers may lead to an underestimation of the effect of the earnings exemption, if SA recipients indeed anticipate that they will become eligible (Cammeraat et al., 2022).

Table E2*Full DDD results of increased earnings exemptions in Amsterdam and Rotterdam combined.*

	Employment		Hours worked		Labor income		Exits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat X Post X Age	0.0048 (0.007)	0.0072 (0.007)	0.4557 (0.441)	0.7129 (0.508)	6.2989 (5.243)	9.3434 (4.989)	0.0021* (0.001)	0.0020 (0.001)
Treatment X Age	-0.0096* (0.004)	-0.0111** (0.004)	-0.5794* (0.239)	-0.7436* (0.285)	-7.4202** (2.488)	-9.5564* (3.673)	-0.0006 (0.001)	-0.0007 (0.001)
Age X Post	-0.0012 (0.007)	-0.0098 (0.006)	-0.2592 (0.417)	-0.7357 (0.388)	-1.9469 (5.237)	-7.6465 (4.612)	-0.0028** (0.001)	0.0001 (0.001)
Treat X Post	0.0052 (0.005)	0.0072 (0.004)	0.0143 (0.233)	0.0084 (0.237)	-0.5247 (2.291)	-0.6720 (2.164)	0.0017 (0.002)	-0.0005 (0.002)
Post	0.0034 (0.004)	0.0099** (0.004)	0.4356* (0.214)	0.8768*** (0.198)	6.2009** (2.286)	11.4291*** (2.008)	0.0040*** (0.001)	-0.0051** (0.002)
Age	0.0150*** (0.004)	0.0185*** (0.004)	1.5555*** (0.234)	0.8718** (0.346)	19.1114*** (2.468)	6.6862 (4.635)	-0.0009 (0.001)	0.0024* (0.001)
Treat	-0.0118 (0.008)	-0.0140* (0.006)	-0.9334 (0.436)	-0.9945** (0.264)	-11.3130 (5.503)	-11.7753** (3.379)	0.0006 (0.001)	0.0035 (0.002)
Highest educ. = Middle		0.0138** (0.005)		0.8478** (0.305)		9.6231** (3.557)		0.0051*** (0.000)
Highest educ. = High		0.0706*** (0.010)		4.5403*** (0.507)		46.1669*** (7.160)		0.0190*** (0.001)
Single person		-0.1011*** (0.017)		-8.9856*** (1.473)		-102.1840*** (15.005)		-0.0070*** (0.001)
Age		-0.0033*** (0.001)		-0.0266 (0.046)		0.6973 (0.523)		-0.0025*** (0.000)
Female		0.0109** (0.004)		0.6704** (0.235)		8.1750** (2.894)		-0.0057*** (0.001)
Number of children		-0.0143*** (0.002)		-0.8456*** (0.087)		-9.5439*** (0.971)		-0.0005*** (0.000)
Work history		0.0030*** (0.000)		0.1607*** (0.013)		2.0464*** (0.162)		0.0003*** (0.000)
Constant	0.1075*** (0.006)	0.2395*** (0.023)	5.1825*** (0.269)	12.2586*** (2.053)	60.6483*** (3.384)	115.2830*** (19.103)	0.0215*** (0.001)	0.0928*** (0.003)
N (x1,000)	450	392	450	392	450	392	886	613
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note. Full estimation results from Equation 4 for the full sample. Dependent variables employment, hours worked, and labor income concern only employment, hours worked and income while also receiving SA. N is the number of observations. Clustered standard errors at municipality level in parentheses. Wild subcluster bootstrap p-values at individual level denoted as * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Even columns control for highest education level, receiving SA for single persons, the age of the main SA recipient, the number of females, the number of children, and the recent work history of (both) SA recipients.