Effects of welfare policies based on autonomy and unconditionality: A social experiment with social assistance recipients

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Synthesis

1.1 Introduction

1.1.1 Some history on welfare and social assistance

What should society do with people who have no income? And how effective is what is organized for them? And effective, to what end or for whom exactly? These questions provide a good starting point for this thesis, which is at its core a study on the effects of a social experiment with social assistance recipients, held in the Dutch city of Nijmegen, with the proposition that a regime which is more based on trust and unconditionality will be more beneficial for these recipients.

The first of these questions, 'what to do with people with no income?', is age-old. In Western Europe, social policy coordinated by the government originated in the 14th century. Early examples of legislation are England's *Statutes of Labourers* from 1351 Jean II Le Bon's ordinance from 1351 about beggars in Paris (Lis & Soly, 1980). The *statutes* dictated that everyone without the means to provide for their livelihood could be obliged to work for a fixed wage, while Jean II's ordinance dictated that all healthy beggars were to leave Paris or find a job, and that social support could only be given to elderly, sick and handicapped people (Lis & Soly, 1980). Over the following centuries, governments wrestled with how they should act towards the poor and unemployed. In general, from the 14th and 15th century onward, charity and the church often played a large role in supporting these groups. This role diminished over the centuries, while at the same time the scope of social policy increased from the local level to the national level (De Swaan, 1989). An important motivation for early social policies was fear of criminal behavior by (groups of) poor people unable to sustain themselves. Laws on poverty aimed at preventing the poor from roaming around (De Swaan, 1989). The themes prevalent in these first 14th century social policies recurred in the following centuries up to the present day. These themes encompass the following: the moral question around whether 'being poor' could be seen as consequence of an individuals` own moral failure (e.g. being lazy); the extent to which poor people could be forced to perform labor; and the question of how the 'deserving poor' could be separated from those whose 'own fault' it supposedly was. The views on who deserved help and who did not often led to a combination of repression and care, a combination that was thus in place from the beginning of Western social policy (De Swaan, 1989). The idea that poverty is a consequence of personal moral failure resulted in the notion that the poor needed to be morally educated, or that governments were justified to create degrading and humiliating circumstances for the unemployed to force them into labor. This has been put into practice ever since, well into the 20th century (Lis & Soly, 1980; Paz-Fuchs, 2020).

In Dutch social policy, developments can be seen that are largely similar to those described above. Over the course of the 20th century, the Netherlands had legislation on poverty ('Poor laws') which stemmed from 1854 (with small adjustments made in 1870 and 1912) and lasted until 1965 (Holthoon, 1985). These laws provided subsidies to charitable institutions and provided relief to many categories of people (widowers, elderly, single mothers, unemployed, disabled) – each with different eligibility criteria, which could also differ from place to place. Sometimes people were forced into employment as a requirement for eligibility (Trappenburg, 2020).[1]

A major change in legislation occured when the notion that social welfare was a basic human right instead of charity was put into law in 1965 (Sebrechts et al., 2020). Minister of Social Affairs Marga Klompé explicitly aimed for the Dutch social assistance allowance to be high enough not only to survive, but to afford some proverbial flowers on the table too. When discussed in parliament, there was broad support for the law (De Swaan, 1989). However, in the 1980s, the dominant ideas about welfare shifted once more. Approaches like 'workfare', 'welfare to work' and 'work first' came into being (hereafter I will refer to these similar concepts as 'workfare'), which originated in the liberal welfare regimes of Anglo-Saxon part of the world. Several influential Anglo-Saxon political scientists, like Charles Murray (1984) and Lawrence Mead (1986), questioned the merit of social assistance: its existence would give the population the message that they did not need to work hard, because the government would take care of them if they did not. These political theorists were proponents of more conditionality, based on behavioral requirements, whereby the main role of the government should be to sanction wrong behavior[2] (Sebrechts et al., 2020; Delsen, 2010).[3] The rise of workfare heralded a change from 'providing people with sufficient income' to 'stimulating labor participation', signifying a change from a more hybrid social-democratic / corporatist welfare state regime in the Netherlands toward a more liberal one, or as Delsen phrases it: *"There has been a shift … from a model based on equality and collective solidarity to a model based on freedom of choice and individual responsibility."* This change resulted in welfare programs that are more selective and conditional (Delsen, 2016).

Further reductions of the amount of the social assistance allowance in more recent times should be seen in the same light: they were aimed at stimulating people to leave social assistance by making it less attractive compared to working for pay (Delsen, 2021). The most recent change (at the time of writing) in social assistance policy in the Netherlands was the introduction of the Participation Act, which is in line with and a further reinforcement of this shift

1 In Nijmegen this was done during the lay out of the main city park, the Goffertpark, in the 30s of the previous century – the football stadium built there in that time still has the nickname 'the blood pit' due to the harsh circumstances under which the work was done.

2 A comeback of the historical notion about morality and poverty.

3 The stance of one of these influential thinkers, Charles Murray, was summarized as: "Social policy says to poor people who work hard for a low wage: you are suckers or losers" (Trappenburg, 2020).

towards a more neo-liberal workfare approach. After being in preparation for years, it went into effect on January 1st, 2015. The Participation Act has based social assistance even more on individual responsibility, and less on collective solidarity, and thus represents a further shift towards the (neoliberal, rational choice based) workfare paradigm (Sebrechts et al., 2020; WRR 2017; Groot et al. 2019).[4] It is this Participation Act that is contemporarily the applicable law on social assistance, and with that is also the subject of this study.

The scientific study of the second question posed in this introduction, ‘how effective are these policies’, is of more recent date than the 15th century. To put it into perspective, the first experiments on the effectivity of social policy are considered to be the randomized controlled trials (rct’s) with negative income tax, held in the United States and Canada between 1968 and 1980 – these experiments are actually considered the first social experiments at all (Neuwinger, 2021, Conner et al., 1999, Greenberg & Shroder, 2004). In later years, contrary to these large experiments which aimed to test fundamental changes in social policy, experiments tended to be smaller and they had a more modest scope, testing modifications of existing policy (Greenberg & Shroder, 2004). In recent years, a new wave of experiments[5] with social policy has emerged, often inspired by the (discussion on) basic income. This includes experiments in Canada, Finland, Scotland, Spain, and The United States (e.g. Delsen, 2019; Danson, 2019; Neubringer, 2021; McFarland, 2017; Merril, 2022). Also in the Netherlands several experiments with social assistance have been conducted between 2017 and 2020, taking place in different municipalities: Deventer, Groningen, Tilburg, Utrecht, Wageningen, and Nijmegen. The experiment in Nijmegen is the subject of this thesis.[6] The central question of

4 Margo Trappenburg, described the changes in the welfare system of the Netherlands in the past century in a framework which consists of two axes. The first axis is strict versus mild, the second axis is standardization versus customization (‘maatwerk’). On these axes, welfare policy clearly shifted over time: where the laws on poverty from 1912 could be considered both ‘strict and customized’, the 1965 social assistance law was somewhere in the middle of both axes, an adjustment of this law in 1972 went as far as being both ‘mild and standardized’. The critique on social assistance in the 1980s led to a return to ‘strict and customized’, which the Participation Act has also been ranked by Trappenburg (Trappenburg, 2020). Whether the Participation Act really allows customization is disputed though (c.f. Hilhorst & Van der Lans, 2015).

5 Some of these have been held and ended, while others are still in preparation and some have been cancelled.

6 It should be noted that, while there are six experiments held in different municipalities, which are comparable because they follow the same rules as laid down by the Ministry of Social Affairs, there is a number of other experiments that are often seen as part of this Dutch wave as well. These are more or less similar in aim and intent as ‘the six’, but did not have explicit permission from the Ministry of Social Affairs to deviate from the law, and as a result had to stay within the limits of the law in their experiment. These additional municipalities are: Amsterdam, Apeldoorn, Epe and Oss. In this study I will usually refer to the six experiments given permission for by the Ministry, since these have most in common and there has been close cooperation between researchers and municipalities.

these experiments was whether the population of people receiving social assistance (hereafter: recipients) would benefit more from a social assistance regime based on trust, autonomy and unconditionality than from the current restrictive, selective, control-based Dutch regime – not just in terms of re-integration into the labor market, but for a broader range of other outcomes as well, including health, self-confidence, trust and other social and economic activities.

The effects of specific types of social assistance or labor market re-integration policies have been the object of many studies – usually not conducted as a social experiment and often focusing only on re-integration effects. Internationally, there are so many studies about labor market outcomes that meta-evaluations summarizing outcomes have been conducted (e.g. Kluve, 2010). Koning (2012) gave an overview of some studies about the Netherlands, though his conclusion that 'the stick is better than the carrot' - the title of the article - is at odds with very recent insights from a study about the benefits from sanctions placed on people who have an unemployment allowance (Verlaat et al., 2021). There have been a few other experiments with social assistance in the Netherlands in recent years. In 2012-2013, two field experiments were held in the city of Amsterdam. In an experiment with a stricter approach, people were required to actively seek employment for a month before they were allowed to apply for social assistance. A positive effect was found on income and social assistance reduction, without additional negative side-effects on other outcomes like criminal behavior (Bolhaar et al., 2019 - however, c.f. Stam (2022) did find a negative side effect on criminal behavior). In the other experiment, five different policy regimes were evaluated, including three different 'welfare to work' treatments and one being 'no labor market activation program at all' (Bolhaar et al., 2020). The latter has some resemblance to the 'exempted' treatment from this Nijmegen experiment (more below). Outcomes studied were work and income - where the 'nothing' treatment performed on par compared to the other treatments.[7] Crucial for this thesis is that these few Dutch experimental studies illustrate that the focus tends to be on labor-economic outcomes (work, income, leaving social assistance).

In this thesis, the effect of social assistance treatment on finding a job is not studied; the focus is on outcomes that generally receive less attention, either in academic studies or in the public debate. The aim of this thesis is to contribute to the knowledge about how the way welfare is organized influences recipients' trust, health and willingness to engage in social and economic activities which are not work. This knowledge is provided by analyzing the data of the social assistance experiment held in Nijmegen, which was organized by the

7 Depending on what outcome, and how long in the treatment one looked at. Very roughly though: in general one policy performed worse, one equal and two performed better.

municipality[8] in collaboration with researchers from Radboud Social Cultural Research (RSCR).

1.1.2 The Nijmegen social assistance experiment

The introduction of the aforementioned Participation Act was the direct rationale behind the social experiment conducted in Nijmegen. The introduction of the act led to resistance among politicians and other pundits. They were concerned about the consequences of on the one hand an increasingly bureaucratic and complicated system based on conditionality and distrust, with a strict regime of control and unconditional fines, and on the other hand the vulnerable group who had to deal with this: the people on social assistance (e.g. Tinnemans, 2014; Westerveld, 2015; Vliegenthart, 2016). Expressed concern was explicitly broader than re-integration to work, and included subjects like the health and wellbeing of social assistance recipients. Also among researchers there was increasing doubt about whether the neoliberal workfare paradigm and the idea of welfare recipients adhering to a strict concept of rational choice are well-founded (e.g. WRR, 2017; Groot et al., 2019; Kampen et. al, 2020; Edzes et. al 2021). A crucial element of the Participation Act in this regard is its Article 83, named 'Innovation'. It allows experiments with deviations from a number of other articles in the law, as long as these deviations are studied with the intent to improve the Participation Act. It thus allowed critics to call for and prepare experiments with such deviations.

Simultaneously with the preparation of the Participation Act, there was a revival of interest for the topic of the Universal Basic Income in the Netherlands, among other things due to the publications of the Dutch journalist Rutger Bregman, later published internationally under the title *Utopia for realists* (2017). The combination of the criticism on the Participation Act, the existence of the Innovation article, and the public discourse on the basic income led to calls for local experiments in which the regime of the Participation Act would be replaced with a basic income (Betkó, 2018). It was a major effort to bring these factors together in a concrete experiment. One hurdle was the term 'basic income', which polarized the debate in the Netherlands (e.g. Bommeljé, 2017). Municipalities that wanted to experiment with the Participation Act aimed to have more far-reaching experiments than the national government was willing to allow. If all the requests the Nijmegen council laid down in the resolution calling for experimentation would have been allowed (Westerveld, 2015), the experiment would have resembled a basic income more closely. However, the power to decide was with the central government, which had the right to write the regulations under which the experiments would take place. So, after almost

8 With the writer of this thesis being the municipal project leader.

one and a half years of negotiations between a number of municipalities[9] and the Ministry of Social Affairs (SZW), permission was granted for an experiment that was much more limited in scope than originally planned by the participating municipalities (see e.g. Betkó, 2018; Bommeljé, 2017). This was due to the political preferences of the national government on the one hand, as the government was unwilling to go as far as these municipalities, and due to judicial impossibilities on the other hand. A process like this also occurred for similar experiments elsewhere (Neuwinger, 2021).

Eventually, municipal governments were given permission to experiment in the following ways. In the first place, municipalities were allowed to experiment with the amount of money participants were allowed to keep if they had any income in addition to the social assistance allowance. In the normal regime, this was allowed only for a period of six months, during which people were allowed to keep 25% of their additional income without it being deducted from their allowance, up to a maximum of €200,- per month.[10] In the experiments, municipalities were allowed to increase the duration of this period up to the entire duration of the experiment. They were also allowed to raise the percentage that people could keep up to 50%, while the maximum amount stayed the same. In second place, municipalities had permission to experiment with allowing an exemption from all the rules regarding re-integration (and as a consequence also an exemption from the fines for non-compliance with these rules). Experimenting with this exemption was made conditional by the national government, with the requirement of including another treatment group; a group for which the contact moments and obligations were doubled compared to the regular regime. Furthermore, to keep the scope of the experiments limited, the national government put a maximum on both the number of participating municipalities and on the numbers of participants - in both cases those maxima were not reached in the end. Finally, the experiments were to be held between October 1st, 2017 and October 1st, 2019, later to be extended to January 1st, 2020. See Section 1.4.2 for a reflection on the setup, including the potential impact of this limited scope on the results.

In Nijmegen, the municipality chose[11] combination treatments, combining different ways of dealing with the re-integration obligation and the opportunity to earn extra income (up to the maximum that was possible). This led to two

9 Groningen, Tilburg, Utrecht and Wageningen.

10 The complete regulations covering this topic are actually a bit more complicated than this, with additional rules for single parents and people with a medical declaration stating that they can only work for a limited number of hours. Furthermore, the amount of 200 euros is an indication, since this is amount is corrected for inflation each year, rather than being a fixed amount. However, using this approximate number suffices for the purpose of this study.

11 In consultation with involved researchers from the Radboud University, Niels Spierings and Maurice Gesthuizen.

treatments: 1) the "exempted" treatment which combined the allowance of extra earnings with being exempted from all re-integration obligations, and 2) the "coached" treatment, which combined the allowance of extra earnings with extra contact moments and coaching. The effects of these treatments were to be compared with a control group for which nothing changed, i.e., they remained in the 'regular' regime. While the intent of the national government seemed to be that the extra contacts should result in a stricter regime, this was against the explicit wishes and intention of the local government. The solution chosen by the local government was a coaching-oriented treatment, in which all regular re-integration obligations were replaced by (among others) monthly group coaching towards a self-chosen goal. The only obligation was to participate in the coaching sessions, which technically resulted in a doubling of (obligatory) contact moments, while also keeping the idea of what the municipal council requested intact (a regime more based on trust and autonomy).[12] The design and methodology of the experiment, and description of the treatments, are laid down in Section 1.1.5.

The six municipalities that held the experiments each worked in close concert with an (often local) research institution. In Nijmegen, the municipality joined forces with the department of sociology, part of Radboud Social Cultural Research (RSCR). The researchers of the different experiments worked closely together in an informal partnership, Landelijk Overleg Experimenten Participatiewet (LOEP)[13], and agreed on performing similar analyses when reporting to their municipalities to allow maximum comparability. The main results of that trajectory were average treatment effects on several outcomes, including labor market effects, trust, self-confidence, health and social contacts. This procedure allowed a rather quick publication of results for the municipalities and the national government (Betkó et al., 2020 for Nijmegen, for the other municipalities see Edzes et al., 2020; Gramberg & De Swart, 2020; Muffels et al., 2020a; Muffels et al., 2020b; Verlaat et al., 2020; c.f. De Boer et al., 2020 for the evaluation of all the six experiments by the Centraal Plan Bureau (CPB)). Researchers from RSCR and the municipality of Nijmegen decided to take time to expand on these findings, the results of which are described in this thesis. The main results as described in the report for the municipality will be summarized here first. These are the foundation upon which chapter 3 to 5 are built, and they indicate where the knowledge gaps are - these are further described in Section 1.3. Overall, few significant average treatment effects were found in Nijmegen, as well as in the other municipalities (Betkó et al., 2020; De Boer et al., 2020).

12 The other municipalities which had similar experiments chose likewise treatments to navigate the various demands.

13 National Consultation Experiments Participation Act.

The Nijmegen report concluded[14] that average treatment effects were likely and significant for trust in finding a job (a negative effect for the exempted group, after two years), job search intensity (positive effect for both groups, after one year), trust in the municipality (positive effect for the coached group, after one year), feeling calm and relaxed (negative effect for the exempted group, after two years) and subjective well-being (negative effect for the exempted, after one year).

The overall conclusion, for Nijmegen as well as the other municipalities, was that while the experimental treatments may not have shown to be much of an improvement as compared to the regular regime, they were not worse either (Sanders et al., 2020; Edzes et al., 2021). This is an important finding, since it indicates that the current system, based on a stick and carrot workfare regime, is not the most effective approach, making this a more ideological rather than an evidence-based choice. Journalist and historian Rutger Bregman, who, as previously mentioned, had an important role at the start of the discussion about these experiments due to the influence that his ideas about basic income had on local policy makers, reflected on this in a podcast: *"for years, social security became stricter, more based on distrust … based on the fear that if you leave people alone, they will continue to sit on the couch, they won't do anything, based on a cynical view on human beings, … the interesting thing is, you can do a lot of different things, and it does not really matter, so you can be more sympathetic towards people … it won't give great results, people will not suddenly get great jobs … [but] if you can choose between being nice and being an asshole, why wouldn't you be nice?"* (Bregman & Frederik, 2020). While not phrased in an academic vocabulary, this does hit the core of these initial findings.

1.1.3 Knowledge gaps, innovations and research questions

The reports mentioned in the previous section provide a basic answer to the question addressing the effects of a social assistance regime based on trust and unconditionality for recipients, compared to the regular regime - particularly beyond the outcome of employment. What this thesis focuses on is 1) *whether there are subgroups which benefit more from the experimental treatments as compared to the control group*[15], and 2) *establishing a better and scientifically grounded view on the possible working of the mechanisms involved*. These have remained largely unknown, and hence highlight gaps in the current knowledge. More elaborate statistical analyses (mediation and moderation) than used so far in reports for the government, as well as the use of qualitative data, will help to shed light on these topics.

14 These are the highlights, further interpretation and information about results that seem conflicting as well as methodological reflections can be found in the report.

15 With this, I conveniently also answer the third question with which I started this introduction: "…effective, for what or whom exactly?"

Moreover, in addition to addressing the knowledge gap on the working of mechanisms and the heterogeneity of effects, adding new empirical evidence from an experiment with active labor policy is an innovation in the Netherlands in itself. Previous researchers have noted that there is precious little high-quality policy evaluation research in the social domain in the Netherlands, leading to policy being dictated by 'fashion' instead of evidence (Van Geuns, 2013; Koning, 2012). The fact that only scarce research is conducted in the Netherlands in the domain of 'work and income' (Van Geuns, 2013) is even more remarkable when we consider that for many years, the Netherlands was the country in Europe that spent most on active labor market programs[16] (Kluve, 2010).

However, at least as important as improving our knowledge about the Nijmegen experiment and making that knowledge available for national and international policy makers and academics, is the fact that this experiment created a unique dataset which can be used for other, broader subjects of academic interest. For this experiment, a sample of recipients who live on a minimum income, is followed over the course of two years, and asked about a multiplex of issues in yearly surveys. With this information we can address broader theoretical issues. For instance, an important topic in the research on the relation between welfare and trust is the question of causality: does more unconditional welfare create (social and/or political) trust (e.g. Bauer, 2015; Kumlin et al., 2018; Leenheer, Gesthuizen & Savelkoul, 2021; Nannestad 2008)? This issue has, to the best of our knowledge, never before been tested in a social experiment that allows for causal inference. Since our experiment compares different types of welfare and inquires about both trust and many other topics that have often contributed to an increase or decrease in trust, this study makes a significant contribution to this debate.

Using the unique dataset to enhance our knowledge on both the Nijmegen experiment as well as to address broader topics, this study is innovative in several ways. The primary focus is on applying methodologies innovative to this field and providing new empirical findings, while also contributing to generating new theory. Methodologically, this study offers at least three contributions. The first of these is the fact that a social experiment is conducted at all. Randomized social experiments are on the one hand considered the preferred method for policy evaluation (Campbell & Stanley, 1963; Shadish, Cook & Campbell, 2002), if not the 'golden standard' (Smith, 2000; Kluve, 2010; Neuwinger, 2021), but they are also complicated, raise ethical concerns and are expensive (c.f. Greenberg & Shroder, 2004; Heckman & Smith, 1995) - and consequently they are very rare. Second, the social experiment has been designed so that outcomes beyond the usual suspect (labor market re-integration) could be studied, because in addition

16 Policies aimed on employment, for instance on keeping workers employed, increasing their productivity or bringing those without a job into employment.

to registration data there have been a number of surveys. Due to these surveys, a outcomes for which there are often no registration data can be studied as well. Third, a diverse and rich collection of qualitative data have been gathered during the experiment, and these are used in this study to generate theory and to better understand the obtained results.

While theory building was not the primary focus, the data obtained using the mixed method data collection enabled a contribution to generating new theory and hypotheses. This has led to a new hypothesis regarding selection bias in this type of social experiments, and new hypotheses regarding which groups can benefit more from a more unconditional social assistance policy (for various outcomes). Furthermore, this study offers a multidisciplinary theoretical perspective, utilizing literature from, among others, sociology, economy, political science, psychology and health sciences.

Empirically, the study provides insight into Dutch active labor market policy, on which relatively little academic research has been conducted (Van Geuns, 2013; Koning, 2012). This is especially the case for outcomes other than labor market participation: these are rarely studied in social experiments, or not at all. This is mainly the case because former social experiments were mainly conducted by economists, with an economic (labor market re-integration) focus (Greenberg & Shroder, 2004, Delsen 2019) – something that has been addressed in medical literature as a failure of social experiments (Conner et al., 1999). Furthermore, this study contributes to the findings of the current wave of global experiments with basic income-inspired welfare (Delsen, 2019; Danson, 2019; Neubringer, 2021; McFarland, 2017). An important empirical contribution is that the outcomes on health and social and economic activities other than work are refined by showing the effects per subgroup. The reports made for the local and national government which have been published so far have not made this distinction (Betkó et al., 2020 for Nijmegen, for the other municipalities see Edzes et al., 2020; Gramberg & De Swart, 2020; Muffels et al., 2020a; Muffels et al., 2020b; Verlaat et al., 2020; De Boer et al., 2020).

These contributions manifest themselves in the following (sets of) research questions which are answered in Chapter 2-5:

- *Which factors contribute to people participating, or not participating, in the experiment with social assistance, as it is held in Nijmegen?* (Chapter 2)
- *To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes in the political and social trust among different groups of recipients?*

  To what extent are expected increases in political and social trust between recipients in different social assistance regimes explained by (a) policy evaluation, (b) social integration, and (c) psychological well-being? (Chapter 3)

- *To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes in the health of recipients?*
  Do the treatments have different effects on health for different subgroups of participants, depending on their resources and if so, for which groups are the effects stronger? (Chapter 4)
- *To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes on time spent by recipients on activities beneficial for society and/or further employment?*
  Do the treatments have different effects on time spent on social and economic activities for different subgroups of participants, and if so, for which groups are the effects stronger? (Chapter 5)

The answers to these research questions are given in a summarized fashion in Section 1.2.1 through Section 1.2.4 of this synthesis, as well as in more detail in the following chapters.

1.1.4 Theoretical framework behind the experiment and general propositions

Before we move to these results, the theoretical framework behind the experiment deserves attention: the reasons as to why social assistance based more on trust, autonomy and unconditionally would benefit recipients for a number of outcomes, in the first place. As already addressed in Section 1.1.2, the current paradigm on welfare, including social assistance, is one based on workfare, which relies heavily on ideas of welfare recipients being 'rational actors' who need to be activated by external 'stick and carrot' incentives (e.g. Delsen, 2016; Arts, 2020; WRR, 2017; Kampen et. al, 2020). Important original proponents of this line of thought were e.g. Charles Murray (1984) and Lawrence Mead (1986). On the other hand, for decades there is critique on the idea of the *Homo Economicus* which presumes humans to be purely driven by a cost-benefit analysis with the goal to get as much income as possible while putting in as little effort as possible (e.g. Sunstein & Thaler, 2008; Tversky & Kahneman, 1981; Sen, 1977). The idea behind the Nijmegen social assistance experiment, as well as the similar experiments in other Dutch cities, is that social assistance can be improved by relying less on these rational choice-based models, and to include more insights from the behavioral sciences, as laid down in Groot, Muffels & Verlaat (2019) and Edzes et. al (2021).

The specific theoretical background and the concepts and mechanisms are described in depth in the chapters - with especially Chapter 2 giving an extensive overview in Section 2.3.2. The most important ideas behind the experiment are the following. The theory on the concept of reciprocity states that people who feel they are treated fairly by another party, will make a better effort to behave in a way which is in line with what the other party wants (Fehr and Gächter, 2000). It was expected that the experimental treatments, which involve fewer obligations and sanctions and which are more unconditional and give more autonomy to the recipients, will lead to participants feeling treated more fairly, and them behaving more in line with government aims (for instance job searching, or participation) which leads to more positive behavior. The experimental treatments appeal more to recipients' intrinsic motivation, allowing them to choose to do what they think will help them, instead of doing things to avoid the financial punishment that comes with non-compliance (in line with theory on self-determination (e.g. Deci and Ryan, 1985). Furthermore, the idea is that the obligations and administrative burdens in the regular regime lead to stress and reduced mental bandwidth among recipients, weakening their executive functions (Mullainathan & Shafir, 2013; Haushofer & Fehr, 2014; Moynihan, Herd & Harvey, 2014). Having fewer obligations and rules should thus increase mental bandwidth and intrinsic motivation, and subsequently have positive effects on a number of outcomes in the treatment groups. Additionally, participants were allowed to earn additional income through temporary or part-time work. On the one hand, this could be interpreted as a more rational choice based financial incentive. On the other hand, it is also a sign of trust which the government invests in the participants[17].

Based on the above, the expectation was that participants in both treatment groups would experience positive impact across a number of outcomes, including labor market activation, health and wellbeing, trust and other social and economic activities.

1.1.5 Research design and methodology

Design

To test these expectations, an experiment was designed. From the start of the preparation, it was clear that the experiment would have the form of a randomized controlled trial (rct) / (randomized) social experiment.[18]

The experiment was held among the approximately 8,000-8,500 social assistance recipients living in Nijmegen at the time of the experiment. Of these,

17 Behind the idea of reducing the allowance with the amount of money earned is the distrust that if people are allowed to keep partial income in addition to an allowance, the 'calculating citizen' might decide that an allowance plus partial income for part-time work is to prefer above full-time work.

18 From the beginning, all six similar experiments in the Netherlands were referred to as 'rct' in most texts. It was occasionally discussed, in LOEP and with the academic guidance committee of ZonMW, whether these settings (including the Nijmegen one) were really 'controlled' enough to fulfill that definition - at least as that term is used in the medical field, which is the design's origin. Although I predominantly use the term 'social experiment' in this thesis, the terms 'rct' and 'social experiment' are often used interchangeably in the context of these Dutch experiments..

approximately 6,000 were eligible for the experiment (see Section 2.4.1 for the criteria, and Chapter 3, 4 and 5 for study flow diagrams). Participation was voluntary and participants were allowed to quit the experiment at any time they wanted. Participants were randomly allocated to a group (one of the two treatment groups or the control group) after signing up for the experiment, and it was not possible to switch groups. As briefly mentioned in Section 1.2, we were allowed to experiment with three types of interventions: 1) giving people the option to keep extra money when they earned additional income besides the social assistance allowance; 2) exempting people from the regulations and fines regarding re-integration; 3) replacing the regular re-integration regime with one which is more intensive and has more contact moments. Based on the wishes of the municipal council as laid down in a motion (Westerveld, 2015), this led to two combination treatments. In the first group, participants were allowed to keep additional income and were exempted from all re-integration regulation. Hereafter I will refer to this group as the 'exempted group' or 'exempted'. In the second group, participants were allowed to keep additional income and had the regular re-integration obligations replaced with an alternative scheme. This scheme consisted of participants gaining full autonomy regarding their aim (volunteering, part-time work, full time work, self-employed or not, etc.) and obligatory group-coaching sessions that focussed on personal development and later the self-chosen goals of the recipient. Hereafter, I will refer to this group as the 'coached group' or 'coached'. Together, this leads to three groups in the experiment: the exempted, the coached, and the control group; the latter whose members were treated as usual. The exact treatment designs (for instance how the coaching in the coached treatment was given form), and the regular Participation Act regime are further detailed in Appendix 1. Randomization was conducted by giving each participant given a random percentage, with percentages linked to one of the groups. 1-34% was used for the exempted group, 35%-67% was used for the coached group and 68-100% for the control group[19]. The recruitment was designed in a way to reach the full range of people on social assistance, including for instance those recipients who have reading difficulties. This is described in detail in Section 2.4.1.

Methods & data

As already addressed in Section 1.3 'innovation', both quantitative and qualitative data were collected and concurrent analytical approaches were used – mainly forms of regression analyses for the quantitative part, and deep reading and a combination of open and closed iterative coding (see e.g. Vennix, 2011) for the qualitative part. The quantitative part is essential to systematically measure

19 This randomization was done by Marieke Selten, MSc from the research and statistics department of the municipality of Nijmegen.

the impact of the treatments on the outcomes, in such a way that the results are more generalizable to the larger population of recipients. The qualitative part is crucial in gaining understanding as to why the results are what they are, to explain the way the mechanisms work, to illustrate what the quantitative outcomes mean for people, and to shed light on processes which were not expected by just relying on theory and quantitative outcomes.

Most data were collected through surveys. Participants were surveyed three times: firstly in a baseline interview just prior to the experiment; subsequently after approximately one year; and finally after two years (i.e. a few months before the end of the experiment). Hereafter, these points in time will be referred to as 't=0', 't=1', and 't=2' respectively. The contents of these surveys were largely the same in all six municipalities, as the scientists involved in the LOEP agreed to conducting similar surveys, to allow for comparisons to be made later on. Most of the items in the survey were taken from previously validated questionnaires, developed and used in (inter)national studies. Questions were adapted to a more accessible language level (B1) when possible, given the fact that many social assistance recipients have a relatively low level of education or language comprehension. The surveys consisted of questions on a broad array of outcomes, among others work and job-seeking behavior, health, self-confidence, trust, contact with others, and attitudes towards social assistance. It also included two open, qualitative questions at the end. The first question was whether the participants had anything they wanted to add to their answers to the closed questions or the questionnaire at large. The second question was for the interviewer: whether they had observed anything during the interview which could be relevant for the study. Surveys were held as Computer Assisted Personal Interviews (CAPI), usually in the participant's house (unless they chose otherwise). The interviews were conducted by a professional survey company, Labyrinth (Utrecht), specialized in hard-to-reach groups.

Registration data about people on social assistance were obtained from the municipality of Nijmegen. These were mainly used in the first study (Chapter 2) to compare participants with non-participants, and to gain insight in demographic characteristics of participants who were not included in the survey. The qualitative sources used were interviews, both group and individual, with participants and officials involved. A list can be found in Appendix 2.

1.2 Contributions and results per chapter

The main body of this dissertation consists of the four chapters about the Nijmegen social assistance experiment, each being a study which can be read as a separate article[20]. All of them use the data gathered during the experiment. The second chapter has a more methodological focus, which helps to put the effects into perspective. The chapter lays out whether there is selection bias among the people who chose to participate, and by which mechanisms it is driven. In the following three chapters, the effects on different outcomes are studied, which are respectively: trust, health, and social and economic activities other than work. The study on trust shows, in addition to average treatment effects, which mechanisms drive a change in trust. The last two chapters study for which subgroups the different treatments work best. Together, these studies provide new and relevant insights on this specific social experiment with social assistance, and on the broader discussions about experimental methodology, welfare and the studied outcomes.

1.2.1 The who and the why? (Chapter 2)

The first paper has a methodological focus, and explores who chose to participate in the Nijmegen social assistance experiment. Of course, given the voluntary nature of the experiment, this is relevant for the external validity of the study. If the people who chose to participate differ much from those who did not, this limits the generalizability. Knowing which groups are overrepresented can indicate whether the quantitative findings will be an over- or underestimation, if the experimental treatments would be used for the entire population. Additionally, this study sheds light on the more general issue of participation in social experiments, relevant to future studies.

As a first step in a mixed methods design, a quantitative analysis is conducted on which groups are over- or underrepresented. Next, drawing from qualitative data gathered from the civil servants who were responsible for the recruitment of participants, light was shed on the underlying mechanisms that explain why people chose to participate or not. Finally, the qualitative data are used to discover whether there are other subgroups of recipients who might be over- or underrepresented, which cannot be determined in the quantitative analysis due to lack of data on the relevant variable. The research question of this chapter is: *"Which factors contribute to people participating, or not participating, in the experiment with social assistance, as it is held in Nijmegen?"*

Using available registration data regarding social assistance recipients of the municipality (i.e. demographic characteristics like age and education),

20 Meaning there is some degree of overlap between them, since they share for instance the context and methodology.

participants were compared with recipients who would have been allowed to participate but chose not to. Nine characteristics were tested, broadly encompassing three categories (individual, household and allowance characteristics). This testing revealed that age, gender, cost sharing[21], and the time a recipient was on social assistance did not differ statistically between participants and non-participants. People born in the Netherlands, higher educated people, singles and people who had additional income next to the social assistance allowance were overrepresented among participants, while recipients who were already exempted from re-integration were underrepresented. The qualitative analyses furthermore indicated that an additional mechanism occurred – which could not be assessed quantitatively – which influenced whether people participated or not: stress. People who experienced a lot of stress often chose not to participate. What this means for the interpretation of the other papers is reflected upon in the overall conclusion (Section 3.1).

This answers the 'who' question, although it is important to note that while the relations we found in the quantitative part were significant, the effect sizes are modest and the correlations are considered very weak under conventional guidelines, or weak in the case of 'recipients earing additional income'. As for the 'why', an important part of the reason to participate was whether people benefited directly from participation or not. This reason could be monetary, but it could also relate to how much freedom and autonomy people expected to gain. Subgroups that expected to benefit above average are overrepresented; for instance people who already had a part-time job in advance and who saw an opportunity to keep more money from these earnings in the experimental treatments. On the other hand, there were subgroups that had little to gain, for instance those people already being partly exempted in the regular regime due to being deemed unfit to work. Already exempted and having little chance on additional income, this subgroup is under represented. In addition to 'who benefits', the opposite also occurred and people who could be expected to benefit more participated less. This is the case with people under a lot of stress, for whom the alternative treatments were specifically supposed to be helpful by alleviating stress due to fewer regulations and possible additional income. They participated less – exactly because of experiencing stress, in such a high degree that they could not commit themselves to a two year long experiment with something as essential as their income.

1.2.2 How welfare policies can change trust (Chapter 3)

The third chapter deals with the effect of social assistance on trust, and the underlying mechanisms of this effect. The relation between welfare and trust is

21 For a detailed explanation see chapter 2; in short, people who are not a couple but do live together and can thus share living costs get a lower allowance than people who cannot share costs.

a widely debated topic in academia, as is the direction of causality: does welfare influence trust, does trust influence welfare, or are there reciprocal relations (e.g. Bauer, 2015; Kumlin et al., 2018; Leenheer, Gesthuizen & Savelkoul, 2021; Nannestad 2008)? Given our experimental method, and the availability of data on factors commonly seen as important intermediary drivers of trust, both the causal relation between experimental conditions of welfare and trust, as well as the role of these mechanisms in explaining the potential impact of our treatments could be tested. The outcomes 'social trust', 'political trust in the national government', and 'political trust in the local government' were studied. The mechanisms, which are expected to be strengthened in the experimental treatments and thus increase trust, concern policy evaluation (both policy effectiveness and policy satisfaction), social integration (both social contacts as well as whether people feel part of society) and psychological well-being (both mental health and subjective well-being).[22]

The research questions are: *"To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes in the political and social trust among different groups of recipients?*
To what extent are expected increases in political and social trust between recipients in different social assistance regimes explained by (a) policy evaluation, (b) social integration, and (c) psychological well-being?"

Empirically, the study shows a significant average treatment effect for the change in political trust at the local level over time in both the exempted and the coached group, while no effect was found for either social trust or political trust at the national level for either group. In the subsequent analysis that assesses if any of the studied mediating factors helps explain this main effect, only one mechanism was found, and only for the exempted group: whether the participants were satisfied with the policy. This explains about one third of the rise in local political trust for this group. For the coached group, none of the studied mechanisms explain the rise in local political trust. This could indicate a stronger reciprocal reaction (Fehr and Gächter, 2000) without an underlying mechanism driving it – after all, the local government visibly invested more time and effort in this group. This leads to the conclusion that it is possible to increase political trust at least at the local level through welfare policy (in a causal relationship), and this confirms theoretical insights on procedural justice which states that when people deal with the government they are at least as interested in being heard and treated fairly, as wanting an advantageous outcome (Lind & Taylor, 1988; Christensen & Lægreid, 2005). The fact that it was this local level where

22 See Chapter 3 for the full logic on why the treatments are expected to have a positive effect on policy evaluation, social integration and psychological well-being and why these in turn are expected to play a part in increasing trust.

we found the effects is not very surprising, since it was the local government that was most (and most visibly) involved in the experiment.[23]

1.2.3 Influencing health through social assistance (Chapter 4)

The fourth chapter studies the effects of the treatments on health. Health is measured as subjective well-being, mental health and self-rated health, the last encompassing aspects of both physical and mental health. In addition to studying the average treatment effect[24], the effects for different subgroup of participants are studied. The literature was ambiguous regarding what to expect. On the one hand, based on studies on coping and resources (Thoits, 1995; McKee-Ryan et al., 2005), one could expect that the resources a person possesses might allow him/her to benefit more from the additional options that the treatments offer, compared to the usual regime. On the other hand, studies in the medical domain have argued that particularly the underprivileged, with fewer resources, might benefit more (Gibson et al., 2020). In this study, groups are studied based on their personal resource (self-confidence), social resources (living together and social contacts), economic resources (education, being debt-free and having part-time work), and finally not having a migrant background (which taps into multiple resources). The research questions[25] are *"To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes in the health of recipients?"* and *"Do the treatments have different effects on health for different subgroups of participants, depending on their resources and if so, for which groups are the effects stronger?"* The outcomes are assessed after both one and two years of treatment.

Contrary to expectations at the start of the experiment, there was a negative average treatment effect after 1 year for the exempted group for subjective well-being, although this was temporary and no longer observed after 2 years. No significant average effects were found for the other outcomes, for neither the exempted nor the coached treatment. However, positive effects for several subgroups were found, mostly related to self-rated health. In the exempted group, self-rated health improved after one year compared to the control group for people without debts, who are part of a couple, with a migration background or with low self-confidence. In the coached group, self-rated health improved after one year for those without debts and low self-confidence. All these effects

23 The local level was therefore the logical level to find something, if only one effect was to be found. If an effect would have been found for instance only on the national level, the outcome would have been harder to interpret.

24 This is part of the municipal report, and repeated here as a step toward the subgroup analysis.

25 These are not mentioned explicitly in the chapter itself, since it is written for a public health audience, and in that publishing tradition research questions (and hypotheses) are often not explicitly written down.

were detected after one year for both groups, while some were no longer significant after the second year – but given that the effect sizes remain largely the same, this is most likely due to loss of statistical power, as attrition led to a smaller sample size. Regarding subjective well-being, only the higher-educated in the exempted group benefited less from the treatment after two years. There were no subgroup effects for mental health.

Overall, it was concluded that there are subgroups of recipients who, from a health perspective, can benefit a lot from different social assistance policies. At first sight and considering the contrasting expectations, there is however no clear pattern in the outcomes. It turned out that three (relative weaker) subgroups with fewer resources benefited more from the experimental treatments, and two (relative stronger) subgroups with more resources benefited more from the experimental treatments. An explanation for the lack of a clear pattern is that the specific social-assistance context plays a part here. In hindsight, we know that people with debts have little benefit from any additional income, so for them the treatments have less to offer. Taking this into account, it does seem that predominantly weaker groups benefit more from the treatments.

1.2.4 Fewer obligations for welfare recipients, more social and economic activities? (Chapter 5)

The last chapter has a setup similar to the previous one: average effects and effects on specific subgroups are studied. However, in this chapter the outcomes studied are social and economic activities, which are not work. Finding or doing paid work were excluded in this chapter because the outcome is already known from the municipal report (Betkó et al., 2020). The activities among welfare recipients that we have taken into account were: training/education, volunteering, informal care and setting up a business as self-employed. The outcomes were studied after the entire experimental duration, and robustness checks after 1 year were conducted as well. As an additional feature of this study, given the lack of specific literature that brings together these outcomes and social assistance, a more bottom up approach was taken. It was hypothesized which subgroups would perform better, based on analysis of the qualitative data.

The research questions are 1) *"To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes on time spent by recipients on activities beneficial for society and/or further employment?"* and *"Do the treatments have different effects on time spent on social and economic activities for different subgroups of participants, and if so, for which groups are the effects stronger?"*

The qualitative part of the study suggested that particularly the following groups could be expected to perform more social and economic activities as a result of the treatments: older people, higher educated people, people with a migration background, those with relatively weaker health, and parents with (young) children living at home. The main reasons why these groups were expected to turn towards these activities are twofold, following from the interviews and focus group material. There were groups that had a hard time succeeding on the labor market, such as people with a migration background or with health issues, who were more likely to turn to other activities when given the chance - a chance they got in the treatments, while not being permissible in the regular regime. And there were groups that seemed to have a stronger than average intrinsic drive towards other activities, like higher educated and elderly people.

In the quantitative analysis, no average treatment effects (i.e. for the entire treatment groups) were found to be significant compared to the control group, for either of the treatments. However, there were two subgroups for which the expected, significant positive treatment effects were found. Participants with a migrant background showed a stronger increase in social and economic activities in both treatment groups; and people with relatively weaker mental health showed a stronger increase in social and economic activities in the exempted group. It was concluded that these two disadvantaged groups benefited more from alternative forms of social assistance. There are indications that a mechanisms at work could have been, among others, labor market discrimination and unrecognized certificates, which made it more difficult for people with a migrant background to find a regular job. For people with mental health problems, the exempted treatment was likely to take away most stress from obligations, therefor creating mental bandwidth which could be used to their advantage.

1.3 Process evaluation

Before moving from the findings of the separate chapters to the general conclusion, some attention should be given to the subject of 'process evaluations', and to what extent such an evaluation has been conducted for this experiment. In their Digest of Social Experiments (2004), Greenberg and Shroder differentiate three types of analyses that are commonly conducted in a social experiment. The first and most obvious is assessing the outcome or effect of the experimental treatments, which is the main focus of this thesis. Another common analysis focusses on cost-benefit. This analysis is not conducted for this experiment, as it is impossible to do this in a valid way given the large number of outcomes where potential effects were expected - each with its own costs and benefits and their own overhead costs. The third type of analysis that is commonly conducted as part of a social experiment is the process evaluation, the goal of which is to *"examine the processes through which an intervention generates outcomes, that is,* *how they work"* (Public Health England, 2018). This is a broad goal which can be specified in a number of sub-questions, among others about whether the implementation went as intended, whether the treatments were administered as planned, whether the theoretical framework underpinning the experiment corresponded with what happened in a real life setting, why people did or did not participate in the experiment, and whether there are subgroups that experience greater or lesser impact from the treatments (Greenberg & Shroder, 2004; Public Health England, 2018).[26]

The last two of these sub-questions have already been addressed in this thesis. The entire second chapter is a detailed study addressing the question why people participated, and chapter four and five specify for which subgroups the treatment-impact is stronger and more likely or weaker and less likely. The questions on whether implementation and administration of the treatments went according to plan is important, because if this is not the case, the outcomes cannot be seen as 'effect of the treatment'. Here it should be mentioned that an external process evaluation was conducted to answer these questions on the process, for our experiment as well as the five similar experiments in other cities in the Netherlands (ZonMw, 2020). An academic advisory committee of the Dutch scientific advisory council ZonMw was responsible for this, commissioned by the Ministry of Social Affairs. The goal of this process evaluation was testing whether the experiments were in line with a strict evaluation framework, which encompassed goals, interventions, the target group, design, analysis and execution. Among others, bias-prevention and potential differences between treatments as planned and treatments as given were studied. The main conclusion was that *"Based on the current available information, the committee concludes that all experiments sufficiently meet the academic criteria which were set in advance."* (ZonMw, 2020, p47).

The number of contact moments that participants had with the re-integration services in the entire year 2018 were reported to the advisory committee, for their evaluation. If the treatments were administered as planned, the exempted group should have had the least contacts, the coached group the most, and the control group should be somewhere in between. Data from the re-integration office, which encompasses all contacts (personal, digital and by phone) show the average number of re-integration contacts in 2018 per person was:

Exempted: 3.2
Control: 7.6
Coached: 13.9

For non-participants, this number is roughly somewhere between 6 and 7, which approaches the number of the control group rather closely. This is a clear indication that the administration of the treatments indeed went according to

26 Though one might argue whether 'impact on specific subgroups is 'process' or 'effect'.

plan.

This does not mean the advisory committee did not provide additional feedback on the process. They did, focusing mainly on elements that might undermine the interpretation of the research results: the combinations of interventions; the difference between the treatments and 'care as usual'; the setup in which the employees in contact with the participants were not randomly chosen; the question of selective participation; and the lack of data to determine the long term effects of the experiment on re-integration in the labor market (ZonMw, 2020).

Issues of selective participation have of course been discussed in the second chapter and in the (overall) conclusion. As for the lack of data for long term effects on re-integration, this is of no consequence since re-integration is not object of study in this thesis. Below we will reflect on the impact of the experiments' duration more generally. The drawbacks of the combination of treatments, and the difference between the treatments and care as usual are discussed in more detail below under 'limitations'.

Turning to the random allocation of civil servants who administer the treatment, such random allocation can indeed prevent excessive influence of the personal qualities of these employees on the outcome. However, this was impossible in Nijmegen given the nature of the treatments. The coached treatment, for example, required employees to follow a specialized group coaching training. The goal of the experiment is not to test whether any given coach can foster better outcomes with a certain protocol; one could say the training of the coaches is part and parcel of the treatment. Furthermore, when one coach has to deal with several treatments (including control) in the same period of time, there is another risk: that the lines between the treatments get blurred; something which is recognized as well in the ZonMw process evaluation (p44).

Regarding the question whether the mechanisms worked in practice as they were theorized, some answers are provided in this study, especially in the chapter about trust (where mediation is used to test what mechanism drove the increase in local political trust) and the chapters using qualitative data. This is addressed in Section 1.4 of this chapter (Conclusion, limitations and discussion). Additionally insights on the working of the mechanisms are provided in a report produced by an intern to the municipality and this project (Scholten, 2020).[27] As a part of this internship, 9 in-depth interviews were held with participants. As an example, the report showed that when asked about trust, participants mentioned that the fact that the municipality conducts an experiment like this improved their trust in the municipality. At the same time, participants explained that the general experience of being on social assistance is one of being distrusted

27 Supervised by János Betkó as part of this this study.

by the government, even during the experiment. They illustrated this by expressing that in the experiment they can still only keep 50% of their additional earnings, instead of 100%. Moreover, they gave examples of how they were subjected to fraud investigations, also during the experiment. This is a good indication that, on the one hand, the core mechanism works as intended, while on the other hand, it provides a possible explanation for why the effects found are quite modest. A further exploration of the qualitative data might give insights into other mechanisms, and which outcome measures are most likely to be influenced by this.

1.4 Conclusion, limitations and discussion

1.4.1 Overall conclusion

The general proposition of the experiment was that alternative forms of social assistance based on autonomy, trust and unconditionality would yield positive effects for recipients, on a broad range of outcomes (as argued extensively in e.g. Groot et al., 2019; Edzes et. al 2021). The aim of this thesis is to learn which subgroups benefit from the experimental treatments, and what mechanisms are at work. These outcomes are discussed below. Before addressing this, the outcomes of the methodological study on selection bias in Chapter 2 are discussed briefly, because these underscore the generalizability of our findings to the population of welfare recipients in Nijmegen.

Though there was some selection bias when comparing the sample with the rest of the population, the correlations are very modest by conventional standards. This indicates that in general it is likely that effects found can be translated to the population of social assistance recipients in Nijmegen. Furthermore it is important to note that for some subgroups that are underrepresented in the experiment, in some cases a stronger treatment effect was found for one or more outcomes (see chapters 4 and 5). This implies that if selection bias is taken into account, it is more likely that treatment effects will stronger for the general population as compared to the results found in our studies (also see below under ‘Effects for subgroups’).

Limited general treatment effects and underlying mechanisms

The municipal report (Betkó et al., 2020) showed few average treatment effects, meaning that there are not so many differences in outcomes between the experimental treatments and the regime as usual, i.e., the control group – at least not to the extent that they are statistically significant under normal conventions. This study confirms the findings as provided in the municipal report. The general treatment effects that are found are indeed limited in scope and number. For local political trust, a stronger increase was found in both the exempted and the coached group after one year, and for subjective well-being a weaker increase was found in the exempted group after one year – after which the effect disappears again. For the other outcomes that are part of this study – political trust on the national level, social trust, self-rated health, mental health, and social and economic activities other than work – no general, or average, treatment effects were found.

It should be noted however that it is a close call in some cases. It is therefore crucial to stress the adage "*the absence of evidence is not the evidence of absence*". In the words of Amrhein et al. (2019): *"For several generations, researchers have been warned that a statistically non-significant result does not 'prove' the null hypothesis".* This is especially the case for (clinical) trials which lack statistical power: *"Randomised controlled clinical trials that do not show a significant difference between the treatments being compared are often called "negative." This term wrongly implies that the study has shown that there is no difference, whereas usually all that has been shown is an absence of evidence of a difference. … To interpret all these "negative" trials as providing evidence of the ineffectiveness of new treatments is clearly wrong and foolhardy."* (Altman & Bland, 1995).

In the specific context of our experiment, few outcomes were just outside the boundaries of what is defined as 'marginal significance' ($p<0.10$). In Chapter 4, a positive effect was found for the coached group regarding mental health, with $p<0.16$, and in Chapter 5 on social and economic outcomes a positive effect was found for the coached group after two years, with $p<0.11$. Given the circumstances (e.g. limited duration, relatively few participants, experimental treatments deviating only mildly from the regime as usual, see Section 1.4.3), these results at the very least warrant further study, preferably with larger groups of participants, so that it can be determined whether these p-values are due to a lack of a real effects or a lack of statistical power.

We can partly explain these general findings by going back to the expected theoretical mechanisms and the empirical assessment thereof in the chapters. Regarding the findings on trust, the possible mechanisms behind these were formally tested through mediation analysis – these were policy evaluation, social integration and psychological well-being. The findings showed that the way the participants in the exempted group evaluate the policy is an important driver of the increase in political trust in the local municipality. Specifically, the only relevant component in this was the satisfaction of the participants with the policy, it being perceived as fair and not too burdensome. Whether the policy was deemed effective or not did not play any role. For the coached group, no driving mechanism was found in the mediation analysis, which leads to the assumption that the reciprocity mechanism itself is at work, without some underlying mediator driving it. This makes sense, because the coached group received obvious attention and investment by the authorities in the experiment: the coaching. As for the mechanism behind the relatively lower increase of subjective well-being in the exempted group compared to control group: this was not formally tested. A possible explanation for this effect is that participants in the exempted group needed some time to get used to their gained autonomy. With the disappearance of their obligations, their contact with the municipality and re-integration office disappeared as well.[28]

As for the general effects which are mentioned above that were borderline insignificant (e.g. for the outcomes mental health as well as social and economic activities), it can be noted that the results do resonate with the core ideas from the general theory on which the experiments were founded. Recipients experiencing less stress, having more bandwidth, having intrinsic motivation, and having reciprocal feelings towards the government; all of which are expected to have a general positive effect on several outcomes. What mechanism played the largest role is likely to depend on the specific outcome, or the partial outcome in the case of social and economic activities. For instance, 'reciprocity' may translate into more effort by the participants, which could result in setting up a self-employed business, but it seems less likely to stimulate informal care. Informal care on the other hand is something that might be made possibly by experiencing less stress.

Effects for subgroups within treatments and underlying mechanisms

Despite the fact that there were few average treatment effects, there were beneficial effects from the treatments for certain subgroups. Significant treatment effects were found for subgroups of participants, in the studies on health and social and economic activities.

In the study on health, the experimental treatments were shown to work better in a number of cases. Higher-educated recipients benefited less from the exempted treatment after two years when it came to subjective well-being. Regarding self-rated health, participants in the exempted group who were part of a couple, without debts, with a migration background, or low self-confidence, were better off after one year. In the coached group, people without debts and with low self-confidence were better off after one year. In the second year, some of these effects disappeared, but additional analyses showed that this was more likely due to lower statistical power because of attrition, than due to a decrease of the effect. In the study on social and economic activities other than work, there were two subgroups for which the treatments led to an increase in hours spent on social and economic activities after two years: people with a migration background (in both treatments) and people with relatively poor mental health (only in the exempted treatment).

28 The qualitative data gathered showed that a number of participants in the exempted group made a phone call to the municipality, asking "what do I have to do now?", and feeling confused about the answer "Well, whatever you want, you are exempted from regulations". Participants felt a bit lost.

It is interesting here that particularly some of the relatively disadvantaged groups performed better, compared to the control group. People with a migration background benefited more both in terms of health and social and economic activities other than work. Also both subgroups (people with a migration background and people with a relatively poor mental health) that perform better regarding social and economic activities other than work are relatively disadvantaged.

Now, the question is: with how much certainty can it be stated that the disadvantaged are worse off in the default system of the Participation Act, as experienced by the control group? Starting with the results on health, it seems not very clear cut: the treatment(s) worked better for three subgroups that were more disadvantaged, including people with a migration background, and for two that were more advantaged. But one of these more advantaged groups – people without debts – should probably not be taken into account in this regard. After all, participants *with* debts cannot benefit completely from the treatments due to the way the broader Dutch social system works, so the finding that they benefit less, while interesting in itself, should not be seen as a causal effect of the treatment alone[29]. Discounting debts, that leaves three more disadvantaged groups which were found to benefit more from the experimental treatments, versus one more advantaged group. Adding the results from social and economic activities, this becomes five to one. In a horse race, those would be good odds and it approaches the point where there is at least preliminary evidence that disadvantaged groups are, on average, better off with the experimental treatments based on trust, autonomy and unconditionality, than with the regular Participation Act regime. This tendency to a relatively better performance by disadvantaged groups is in line with a very recent study on re-integration of people with a migration background in Nijmegen. Researchers find that this group very much values a regime with considerable personal attention, supporting the findings in this thesis for people with a migration background (Spierings et al., 2021).[30] This also fits with insights from health literature, which states that disadvantaged groups are often better served with more unconditional, basic income-like treatments (Gibson et al., 2020).[31]

These findings on disadvantaged subgroups are all the more interesting

29 See section 1.2.4 of this synthesis, and chapter 4 for a more detailed explanation.

30 This study also indicated a rise in trust in the local government when receiving supportive coaching, in line with the findings in this study regarding trust.

31 One of the studies on labor market activation policies where the difference in effectiveness for vulnerable people was tested is Bolhaar et al. (2020). The study found indications that vulnerable groups might benefit above average from one (out of four tested) type of labor activation policy compared to not having any labor market activation program – although these results are hard to interpret compared with this study, among others because vulnerability was measured with an index consisting of many categories including age, education, reason for welfare entry and financial situation.

when this pattern is combined with the finding about selection effects in Chapter 2. It showed that people with a migration background were underrepresented in the study. Similarly, there was a selection effect which made people who experience a lot of stress and similar mental health problems less eager to participate, and caused people who did choose to participate to drop out because they could not cope with the stress of participating. This is an indication that the effects for these subgroups are likely to be underestimated when these findings are translated to the social assistance population as a whole. Participants with a migration background are found to benefit more from the treatments regarding health, and both participants with a migration background as well as participants with a relatively poor mental health experienced stronger treatment effects in the study on social and economic activities other than work. If the number of people with these characteristics are relatively low in the sample – compared to the general population – due to selection and / or drop-out bias, the effect in the total population can be expected to be more substantive. Furthermore, considering the relatively low number of observations in the experiments, the relative short duration of the treatments, and the circumstances that made it likely that this experiment led to an underestimation of the treatment effects (see 'limitations'), the fact that significant results did show up in the subgroup analyses is testimony for strong and consistent effects among those groups.

In the studies on subgroup effects, we did not include statistical tests to ascertain what mechanism were at work driving such differences in treatment effects. However, for the study on the effects of social and economic activities, the qualitative inquiry among participants shed some light on the mechanisms which cause more social and economic activities to be employed. That inquiry indicates that the effects found for participants with a migration background are, in addition to other possible mechanisms, due to labor market discrimination and the non-recognition of foreign certificates, which drove migrant background participants towards activities like self-employment, volunteering, and schooling. The mechanism behind the effectiveness of the exempted treatment for people with a relatively poor mental health seem to be the desire to break social isolation, as well as looking for alternatives for work, since working is simply not possible.

Final thoughts on the overall conclusion

The main questions this study started with were 1) *whether there are subgroups which benefit more from the experimental treatments as compared to the control group*, and 2) *a better and scientifically grounded view on the possible working of the mechanisms involved*. Above, both of these questions have been answered to a substantial degree. However, there are still unanswered questions– around whether general effects are really lacking or whether they cannot be detected due to a lack of statistical power; also, some supposed ways the mechanisms work are not empirically tested or substantiated with qualitative evidence, as they are instead the result of well-grounded reasoning. Below, there will be some reflection on the limitations of this study and implications of the results for further research as well as social policy in the Netherlands.

1.4.2 Reflection

After having discussed the overall conclusion in the previous section, there are a number of topics that deserve some consideration and reflection, among others, the design and limitations of this study.

Combination treatments

As mentioned in the previous section, the treatments combined two parts: the possibility to earn extra income and alternative rules regarding re-integration. This resulted from the request of the municipal council, that wanted to compare the regular Participation Act regime with an alternative system. (c.f. Neuwinger (2021) on the intersection between policy and science in these kind of experiments). From an academic perspective, clearer results could be obtained by testing each component of the treatments (additional income, exemption, coaching) separately (ZonMw, 2020).[32] At the same time, the case can be made that in reality these different components interact with each other, and it is their combination that partly drives the outcome. A good illustration of this is that participants described (see 1.3 Process Evaluation) that the treatments only limitedly increase their trust, because they still felt distrusted due not being allowed to keep all money they earned. From that perspective, testing a lot of different components in isolation is *not* a better way to conduct an experiment like this, because rather than a new treatment, participants will experience the same treatment with minor details changed, while the sum of these minor details might be greater than the parts due to the interaction between components.

The 'control treatment' being actually many treatments

The difference between the treatments and the control group is another limitation which deserves some attention. The 'treatment as usual' - the control group - is in reality not one treatment, but a multitude of treatments. There is a large variety of people receiving social assistance, ranging from those fit to

32 In the original design there was a fourth treatment, consisting of the option to earn additional income, while the rest stayed the same. This group was cancelled due to the limited number of applicants. If it would have been part of the experiment, I could have deducted its effects from the found effect of the combination treatments, which would give an indication of the effect of other components (c.f. ZonMw, 2020, p32). What part of the effect would be due to the other part of the treatment and what would be due to the interaction of both parts would still be unknown.

work to people with a very large distance from the labor market.[33] The regional re-integration office only has money available for about one third of the social assistance recipients, which are often those deemed the most work-fit. So a substantial part of the recipients has no contact about re-integration in the regular regime, and is effectively[34] 'exempted', since they are not asked to fulfill re-integration obligations.

When these people are randomized in the control group, part of the control treatment overlaps with the exempted treatment: this is inevitable as well as legitimate, since being (partly) exempted is part of the system we compare the experimental treatments with. When these people are randomized in the exempted group they do not experience a big difference in this regard– the main difference is the guarantee that their situation will not change over the next two years. This arguably leads to an underestimation of the treatment effect of the exempted group, because not all participants in the exempted group have actually experienced a treatment that really differed from what they experienced before.[35] This also means that having considerable numbers of participants in the treatments is all the more important for experiments like these.

Number of participants and duration

The most severe limitation of the study is the number of participants. The experiment started with over 100 participants per group. Due to attrition and non-response[36], some analyses ended up with less than 60 participants with valid scores after two years in a group. The attrition does not seem to be selective (see the robustness checks in chapter 4 and 5), which reduces concern about drop-out bias influencing the outcomes. However, the low number of participants makes that large and consistent effects are needed to reach statistical significance. This increases the risk that effects are discarded as non-existing, where in fact there is an issue with statistical power.

Of course, the debate about statistical significance and the meaning of non-significant results has been going on for decades among statisticians and other scientists. It bears mentioning that the academic guidance committee

33 While recipients who were diagnosed with a job handicap that is so severe that regular work is not possible were excluded from the experiment, that still leaves people in the target population with minor job handicaps, or who are out of the labor process for decades. See Section 2.4.1 for who was eligible to participate.

34 And partly even officially.

35 On the other hand, the fact that people have a guarantee that they are officially exempted for two years might be an important part of the treatment in itself, because people who have no contact about re-integration in the regular system can feel anxiety about potentially having to oblige to re-integration measures in the near future.

36 For some items non-responding was larger than for others.

expressed in several meetings with the LOEP that given the circumstances,[37] researchers should treat non-significant p-values carefully, and should not conclude too easily that the treatments have no effect.[38] Many scientists warn against the use of statistical significance as a threshold between 'effect' and 'no effect' – this dichotomy leading to incorrect conclusions in academic papers and bad policy recommendations (e.g. Amrhein et al., 2019; McShane et al., 2019). Statistical significance is used as trichotomy instead of a dichotomy in this study, by including 'marginally significant' – which according to some critics is better, but still not enough (McShane et al., 2019). For this thesis however, it was chosen to limit 'found effects' up to this threshold of (marginal) significance, while effects just above that threshold are recommended for further study.[39]

Another limitation of the study is the duration, something which is acknowledged in the process evaluation. This states that the experiment was rather short and this carried the risk that it was not possible to scientifically / statistically show results (ZonMw, 2020, p. 23). This is related to the type of outcomes: for some it is inevitable that it will take quite some time before the treatment has a chance to have an effect. For instance, for an outcome depending on the mechanism of reduced stress due to the treatment, the stress level of the participant needs to have had the time to drop. More general, it is safe to assume that certain behavioral effects need time. Anecdotal but still illustrative was a participant in the exempted group who mentioned at a meeting that it took him exactly over a year before he fully realized he was *really* exempted, and at that time he had enough trust in this that he started a study program, no longer hindered by re-integration obligations.

Effects: measuring after 1 or 2 years?

Related to the point above, it is important to account for the choices made regarding over what duration effects were measured in the different chapters. For trust, the effect was tested after one year, for social and economic activities after two years[40], and for health at both time points. These choices were made for good reasons. For some outcomes, it can be expected that the approaching end of the experiment can have an effect on the score. As an example, Chapter 3 on trust studied a dependent variable which is, according to theory (Dinesen & Bekkers, 2017; Newton, 2009; Newton, Stolle & Zmerli, 2018; Paxton, 2007;

37 Which are also mentioned in this section, like the relatively short duration, the limited differences between treatment and control, and the complicated real world setting.

38 "Don't be a p-value knight" was the literal comment by one of the members.

39 Full disclosure: my perspective is colored by my situation as a civil servant. A lot of policy is made with the best intentions and with the idea that it will probably work – but without much certainty that it will. I think most policy makers would be highly enthusiastic to know beforehand if a certain policy would have a 80 percent certainty to be more effective than the preceding policy – while in academia, an effect with a p-value > 0.10 and < 0.20 often effectively does not exist.

40 Though I ran robustness checks on the difference between t=1 and t=0 as well.

Rothstein & Uslaner, 2005; Whiteley, 1999), quite susceptible to the mechanism of reciprocity: people trust the government more, because they in turn get more trust from the government. When brought into the setting of a social experiment, one should ask: what does it do with the participants when they know that the experiment will end soon and the trust they received was about to be taken away? At the same time, in the study on social and economic effects, reciprocity played a lesser role, and it is hard to believe that somebody would stop giving informal care (one of the components of the dependent variable) just because the experiment was about to end in the near future. The researchers and municipal and national officials involved in the Dutch experiments were aware of this, and this was reason to postpone the end of the experiments for a few months. That created a longer timespan between the final survey and the end of the experiment (ZonMw, 2020). The question is whether this was enough. Participants were asked in the final survey if they were worried about the upcoming end of the experiment, 250 respondents answered that question. In the exempted group, 50% of the participants were worried regularly or sometimes about the coming end of the experiment; in the coached group, this was 37,4%, and in the control group 21.8%[41].

Of course, looking at effects after one and two years both has advantages. After one year, the number of participants is larger, because fewer people have dropped out the experiment, making it easier to detect statistical effects. At the same time, is it possible that effects are temporary, and that when measuring only after one year one mistakenly identifies a temporary effect as a non-temporary one. In this study, this could theoretically be the case for the results on trust, but given the reasoning above there is a stronger case that the end of the experiment influenced the results on trust, than that the rise in (local political) trust would be temporary affair had the treatment continued.

Reflection on social experiments / randomized controlled trials in a social setting

Many researchers consider the rct / social experiment design the preferred method for policy evaluation, even 'the golden standard' (see Section 1.1.5). However, the process evaluation by the ZonMw advisory committee mentions the difficulty of having to "*execute a relative rigid scientific design in a dynamic practice*" and says "*it is the question whether an rct-design in applied research is always the best choice. An rct is mainly suited for singular interventions, while many interventions are complex and take place in a quickly altering reality, where*

41 For the control group it is not clear why people are worried about the end of the experiment, because they continue to receive exactly the same treatment after the experiment ends. Regardless, the difference between people who are worrying about the end of the experiment between control and coached and (especially) control and exempted is large, and statistically significant when tested in a Chi-2 test.

researchers have little or no influence. It is recommended for the future to consider alternative research designs when studying the effect of policy experiments" (ZonMw, 2020). This a similar sentiment can be found in relation to international basic income experiments as argued by Michael Danson, who mentions that controlled trials originated from medical and agricultural settings which only have a limited set of relevant variables. This is very different from experiments with basic income-like systems, where a multitude of individual, household and other variables are involved (Danson, 2019, p99-100). This difference makes sense when we consider that for instance in medical trials, it is possible to work with methods that leave participants unaware about whether they are in the control or the treatment group ('double blind') – this is inconceivable for an experiment like the one in this study. In the end, each design has strengths and weaknesses. For this study, the social experiment design is useful, because it clearly shows the effects of several outcomes, for different treatments. At the same time, measures should be taken to compensate for weaknesses as much as possible. In addition to other measures taken and described elsewhere (e.g. checking for biases), an important part of this study is the inclusion of qualitative methods. Used in addition to the statistical analyses, this provides methodological triangulation and helps to shed light on the way the relevant mechanisms work in the complex social context.

The intersection between politics and science

It is interesting to notice how several of the limitations mentioned here are a consequence of the political negotiations on how far municipalities were allowed to go. If the experiments could have been conducted in a way in line with the original intent of the municipal council (Westerveld, 2015) and researchers, there would have been fewer treatments (increasing the statistical power), the difference between treatment and control groups would have been bigger, and there would not have been interference from regular fraud investigations (Scholten, 2020), diminishing the effects (as described under section 1.3). Some of these issues have been explicitly addressed in the national process evaluation (ZonMw, 2020). All this implies that the watering down of the original design of the experiment[42] reduced the effectiveness of the treatments, and thus the experiment. The implications of this, as well as other results and reflections, are discussed below in the final sections of this synthesizing chapter.

42 Caused by the necessity to find a political compromise between the local municipalities that wanted to experiment and the national government which was much less inclined to do so in the first place and which wanted, if experiments had to be held, to stick close to the regular Participation Act even in the experiment.

1.4.3 Further research

Building on what we have learned so far and the known limitations, below follows a discussion on avenues of future research. Distinctions are made between similar experiments in the future, further research following the outcomes of this study, and further research on the data gathered in this experiment. To start with the latter, the municipality has many data which have not been explored so far. A post-experiment survey has been conducted, approximately one year after the end of the experiment, which fell outside the scope of this thesis. Additionally, there are evaluations from participants as well as reports from exit-conversations with participants, that have not yet been studied by researchers.

In addition to using these new data, the existing data can be used to get a better view of how the mechanisms derived from theories work in practice, and if they really do work as expected. The internal report (Scholten, 2020) already provides interesting suggestions about, for instance, how the treatments influence the mechanism 'stress'[43] and why mechanisms do or do not influence the outcome 'trust'. A systematic analysis of the mechanism would be a valuable addition to the information gathered so far.

The overall conclusion provides (at least) two interesting leads for further research. In the first place, a more systematic study could be conducted, focused on whether the disadvantaged groups are indeed worse off in the current social assistance regime, and similar workfare based policy, than in more welfare orientated policies. And if is this is the case, it is essential to identify the mechanism(s) at work. In the second place, an important methodological question is raised around whether the approaching end of the experiment may influence the outcomes of the last measurement. If so, it would perhaps be better to evaluate experiments based on the second to last measurement rather than the very last one that was collected just before the end of an experiment.

As for future experiments: based on this study, a second round of similar experiments can be highly recommended, in which lessons learned from this wave would be incorporated. This would include: designing new experiments to last longer (which needs adjustment of Article 83 of the Participation Act); having various municipalities conduct the same experiment, with the same treatment, the same participant recruitment and the same way of collecting the data, so that data can be pooled, hence increasing the statistical power considerably; having a time schedule that allows for checking if randomization was successful on a number of key characteristics; and having the experimental treatments differ more from the default regime, which functions as control group. Given the fact that there were over 50 municipalities interested in the

43 Which seems to be in a real life setting more complicated than just 'fewer obligations and more money equals less stress'.

round of experiments before the start (Bommeljé, 2017) and eventually only 6 actually experimenting under Art 83 of the Participation Act, a larger study should certainly be possible to instigate, as long as the national government is willing to allow (and facilitate) this.

1.4.4 Policy implications

First of all, the meaning of these findings for the current social assistance regime of the Participation act should be stressed. Even when ignoring that some general effects are only borderline insignificant, the experimental treatments studied in this thesis are overall no worse than the regime as usual, i.e. the control group, on the outcomes analyzed in this study. Thus, the findings resemble the overall conclusion of the Dutch social assistance experiments, which are mentioned in Section 1.1.2 "The Nijmegen social assistance experiment", and which have been communicated by the joint local researchers in LOEP (Sanders et al., 2020; Edzes et al. 2021). The lack of negative results of the experimental treatments is an important finding in itself and a challenge to the workfare paradigm and the idea that welfare should be repressive in order to activate people. Of course, the workfare paradigm and the regime that follows from this view can be an (ideology driven) political choice[44], but the experimental evidence from this study and similar ones shows that the justification for the current system should nor can be that it is "the most effective".

To refer back to the national process evaluation, local researchers have been given the recommendation to clarify to the partaking municipalities how results should be interpreted, and how this type of effect studies can be improved in the future, thereby also giving attention to the role of the research design (ZonMw, 2020). This recommendation is followed here, among others because of the potential for future experiments in the line of what is described in this study. Some issues are addressed already under 'future research', given the position of experiments like this on the crossroads of policy and science (Neuwinger, 2021). Neuwinger even argues that due to this position, it is impossible to have experiments with a real (paradigmatic) basic income, because there will just be too many (e.g. political and judicial) constraints. Of course, this experiment was not a basic income experiment, but it is worth stressing that, if society and politicians really are interested in the question if something works, it should be possible to study this, and such a study should be facilitated, regardless if people are in favor or against the policy.

44 One that, in addition to the question of its effectiveness, seems to have other negative side effects. For instance, a recent study shows that withholding money to people under the Participation Act are depending on this money leads to increased crime, c.f. M. Stam (2022). While the study of Stam focused on a specific age group and not on re-integration sanctions, these often have the same mechanism – people are withheld the income they need.

Of course, even without future experimentation, many useful policy lessons can be taken from this study. This is the case for the Netherlands, at both the local and national level, but also internationally, for countries which have a similar workfare-based system, or consider to have one. Notions about who is deserving of help through social assistance and who is not, and if the government should oblige people to work and how far they can go in that respect, are still as much a part of the political debate on social policy nowadays as they were in the past, up to centuries ago, as described at the start of this chapter. A contribution of this study – and similar studies in other municipalities – is that it shows that at a fundamental level the Dutch social assistance regime does not seem to work better than more humane alternatives based on giving recipients trust and autonomy. The stick (the current regime) does not work better than the carrot (our experimental treatments). If anything, there are indications that these alternatives work better; and for some subgroups the current system appears to work rather badly, compared to alternative treatments. This seems to be predominantly the case for disadvantaged groups, though more research is needed on that subject. It should also be a policy priority to understand why the current social assistance system does not seem to work as well for specific groups, e.g. for people with a migration background, regarding both health and social and economic activities other than work, or for people with mental health issues regarding social and economic activities.

Something which was not the explicit aim of this study, but which turned up and is relevant enough to warrant mentioning: when people on social assistance earn additional income, this has consequences for the tax allowance they are entitled to, which can result in a lower allowance sometimes only taking effect more than a year later (see Betkó et al., 2019, for how these regulations interact with the experiments). This can lead to financial hardship or debts. In the experiment, the municipality provided information about this to participants a number of times, in different ways. Nevertheless, it is known from the interviews that some participants still forgot to report their income to the tax office, which brought them into trouble. Given the theoretical framework of this experiment, this makes sense: people who experience financial scarcity (and most social assistance recipients do) have less mental bandwidth available and are therefore less equipped to handle these kind of tasks, as they experience a lot of stress (Mullainathan & Shafir, 2013[45]; Haushofer & Fehr, 2014). This suggests that it

45 Even though recent studies argue that, especially on the idea that poverty reduces mental bandwidth and subsequently increases time discounting and risk aversion, the empirical evidence is not conclusive and requires further study and theoretical improvement (de Bruijn & Antonides, 2021). However, the critique focusses specifically on economic scarcity (poverty) - part of the reasoning in this study is that in addition to economic scarcity, social assistance recipients will suffer from reduced mental bandwidth due to the cognitive ballast of all obligations in the regular system - theory in line with e.g. ideas on administrative burden.

might be a good idea if local municipalities take a welfare responsibility to contact every person on social assistance as soon as they start gathering extra income and assist them with reporting the income to the tax office. In this way, financial hardship due to the tax allowance system can be avoided and this avoidance is not being made the responsibility of the recipients themselves, for whom we know a part is not up to that task.[46]

Throughout this synthesis, a plea is made for more large scale, longer and better experiments of this type. To conclude this synthesis, one final consideration: the Netherlands spends between 6 and 7 billion per year on social assistance allowance – not including costs for regular unemployment and other allowances, nor including the cost of the bureaucracy needed for distribution. This is a lot of money[47]. If there is even a small chance that this will lead to relevant improvement, it is in my opinion, financially and morally, a no-brainer to incidentally invest a fraction of that amount in scientific studies on how to improve the system. If only a small percentage of the hundreds of thousands of people receiving social assistance improves their health or reduces their allowance dependency, the invested money will be returned with a mighty dividend, in addition to improving the life and well-being of the people involved.

46 Which in turn might have a positive effect on trust in the local government, if it makes a visible effort to avoid recipients getting into this kind of trouble.

47 Comparable with over half of the current (2022) budget for Defense in the Netherlands.

2

The who and the why?

This chapter has been published in a slightly different version as "The who and the why? Selection bias in an UBI-inspired social assistance experiment" in Delsen (2019) Empirical Research on an Unconditional Basic Income in Europe. See the section Contributions of authors for information on co-authors.

2.1 Introduction

In recent years, at several places in the world, the concept of the 'universal basic income' (UBI) has (re)gained a foothold in political, academic and public debates. It has become increasingly popular for a number of reasons, among them: dissatisfaction with the current social security system and the stress it causes the people depending on it, (anxiety about) the disappearance of low skilled jobs due to automatisation and robotisation, the success of 'direct cash transfers' in development aid, and a wish to reduce poverty.[48] This renewed interest has led to (the preparation of) a number of social experiments across the globe (*e.g.* Finland, Stockton (USA), Ontario (Canada)[49], and in Scotland (UK) (Danson, 2019), with either the basic income, or basic income-inspired social security policies. In the Netherlands a number of local experiments have been initiated, including one by the municipality of Nijmegen (Muffels & Gielens, 2019).

This experiment in Nijmegen has the form of a randomized controlled trial (RCT). RCT's are sometimes considered the 'gold standard' of social experiments (see for example: Smith, 2000: 1; Kluve, 2010), or at least are the preferred method (Campbell & Stanley, 1963; Shadish, Cook & Campbell, 2002). That does not mean that this approach is without disadvantages. On the contrary: numerous publications point to the dangers and deficiencies of the RCT design. Mentioned, for example, are the ethical and political controversy regarding the random distribution of people over treatment and control groups (Smith, 2000; Heckman & Smith, 1995), as well as the difficulty to generalize to a wider population (Shadish, Cook & Campbell, 2002). The methodological literature also rightfully stresses the danger of biases in (social) experimental designs. Among them are selection bias, reporting bias, attrition bias, performance bias (Higgins & Green, 2011), substitution bias, dropout bias (Heckman *et al.*, 2000; Heckman & Smith, 1995), and randomization bias (Heckman & Smith, 1995).

The Nijmegen experiment sheds light on how and to what degree some of these issues play a role empirically indeed. We focus on selection effects, which mainly consist of selection bias, but also involve to a lesser extend other biases (this is further elaborated on in Section 2.3.1). Like the comparable experiments that are currently conducted in the Netherlands as well as some in other countries across the globe, it is based on voluntary participation.[50] This is legitimate from an ethical point of view, i.e. not to experiment on people against their wishes. Unfortunately though, this voluntariness causes the issue of

48 An overview of these argument can be found for example in Bregman (2017), the same line of argumentation can be found in the proposals by local politicians, *e.g.* Westerveld (2015) for Nijmegen.

49 Ontario's basic income pilot ended on 31 March 2019 (https://globalnews.ca/news/4422214/ontario-basic-income-pilot-end/). The Finnish basic income experiment has ended on 31 December 2018 (see De Wispelaere, Halmetoja & Pulkka, 2018).

50 For example, the Ontario experiment was based on voluntary participation as well.

selection bias to recur. It is known that voluntary participation influences the generalizability of the outcomes (Greenberg & Shroder, 2004). Based on general insights from the social sciences, we know that the outcomes central to a social experiment can differ between social groups (see e.g. Inglehart, 1997; Klandermans, 1984). This is likely to influence groups willingness to participate, as people realize they have more or less to gain from an experiment.

Starting from the notion that it is methodologically plausible that biases can occur, in this study we formulate general expectations along which lines (like social groups and people's characteristics) they may occur. In the current methodological literature on social experiments, there is to the best of our knowledge little reference made to specific empirical studies or results regarding the characteristics of people along which biases are found. Our study explicitly theorizes and tests which selection biases are found in the Nijmegen experiment, and how strong those biases are. We are able to contribute to the research in this field, due to the large amount and variety of the data we have collected. Not only do we have extended registry data on the participants, but also on the entire target group.[51] In addition to that, we use qualitative data dealing with reasons for participation and dropping out. This enables us not only to (statistically) test if selection effects occur, like on gender, age, country of birth, household composition and characteristics related to the social allowance. But the mixed method approach also enables us to further explain why some of these effects occur. Thus, the Nijmegen experiment functions as a 'case study' for how these biases turn out in practice: in the specific setting of a social experiment on social assistance based on elements of the UBI, in which participation is voluntary and which is partly aimed at labor market activation of participants. While relevant for social experiments in general, our analysis bares specific relevance for the local social assistance experiments in the Netherlands and UBI-related experiments around the world. The local experiments we conduct in the Netherlands are mentioned among 'the experiments with the basic income' in both media (e.g. Boffey, 2015; Hamilton, 2016) and academia (for which the book this chapter was originally published in is a testimony). As the results of these experiments will get drawn into that international debate, it is important they are interpreted correctly. Taking biases into account should be part of that.

The research question that will be answered in this chapter is: *which factors contribute to people participating, or not participating, in the experiment with social assistance, as it is held in Nijmegen?* The specific factors we will zoom in on are standard socio-economic and demographic characteristics, and characteristics related to the social assistance allowance.

Below in Section 2.2, we will first give some context that is relevant to the

51 Mainly the data in the registry of the WerkBedrijf (the local reintegration organisation) and the data of the department Zorg & Inkomen (Care & Income) of the municipality.

experiment. In Section 2.3, we will discuss the theory on which the experimental design is based. It will show that some of these theoretical grounds and treatments are expected to make the experimental policy have a stronger than average effect on only a subset of the social welfare population. Combining this logic with more general social science theories, we will formulate a number of hypotheses on the socio-economic and demographic characteristics for which we expect an over- and under-representation in this UBI-based social experiment. In Section 2.4 and Section 2.5 we subsequently discuss the methodological set up of this study and present the results. Finally, in the closing Section 2.6, we will get back to the larger implication of our results, also for other social experiments.

2.2 Context

Some background of the social system in the Netherlands is necessary to understand the experiment's set up and the possible selection effects. With the term 'social assistance' we refer to the final safety net of the Dutch welfare state, the Participation Act (*Participatiewet*).[52] Municipalities are fully financially responsible for the implementation of the Participation Act. If a person loses her job, she will not have to fall back immediately to social assistance. First, there is the more generous salary related unemployment benefit (Unemployment Act, *Werkloosheidswet, WW*). The first two months the unemployment benefit is 75%; after that it is 70% of the last earned wage. Only when this expires - after a number of months (with a maximum of 38 months) based on the age of the dismissed person and on the years that person had a paid job - she will receive a social assistance benefit. Next to unemployment, people receive social assistance, for example, after a divorce, after graduating without being able to find employment, or after being granted a refugee status. Social assistance is aimed to be high enough to keep somebody out of poverty. For a single person, it amounts to € 1,025.55 per month (70% of the legal minimum wage). For couples, it amounts to the minimum wage, which is € 1,465.07.[53] In general, to stay above the poverty line, people on social assistance also need to make use of national and local anti-poverty arrangements (e.g. national tax allowances to compensate for rental or health insurance costs, local compensations for necessities that cannot provided for in another way, or local contributions to children from poor families so they can participate in sports or cultural activities). The existence of a legally guaranteed minimum income does not imply that poverty according to this standard does not exist in the Netherlands. Not everybody who is entitled to assistance or an anti-poverty arrangement receives it. The number of poor

52 All Dutch laws can be found at the website *wetten.overheid.nl.*

53 These amounts are for 2019.

citizens is higher than the number of welfare benefits.[54] The risk of poverty and long-term poverty is highest among social assistance recipients. The poverty rate among social assistance recipients lies between 40-45% (Delsen, 2016). Dutch municipalities operate a policy income limit between 110% and 130% of the guaranteed minimum income.

The participation Act came into effect on 1 January 2015, replacing the former social assistance act (WWB). It represented a (further) shift from 'welfare' to 'workfare' and from collective solidarity to individual responsibility (Delsen, 2016). It drew quite some criticism, from politicians, municipalities and civil society (e.g. Tinnemans, 2014; Vliegenthart, 2016). Among others, people thought the act too bureaucratic, too strict, and too much operating from a paradigm of distrust. In combination with the other regulations concerning taxes and local subsidies, the entire social security system was also considered to be too complex.

In the period before the Participation Act came into effect, the UBI was put on the (political) agenda, among others due to a popular journalistic publication in Dutch on the subject (Bregman, 2017).[55] Consequently, critics of the Participation Act also looked at the Universal Basic Income (UBI) as a potential solution to a number of problems they had with the act. They thought a UBI, or a UBI-inspired social assistance, could reduce bureaucracy and the complexity of the welfare state – a salient issue for many citizens on social assistance (e.g. lower-educated people), and reduce poverty and stress among people depending on social assistance (*e.g.* Boffey, 2015; Ranshuijsen & Westerveld, 2015; Westerveld 2015). Thus the goals of the Nijmegen experiment are manifold. By changing the welfare regime to an approach based on trust and the capabilities of people on social assistance, they are expected to have an easier time finding a job (either full time, part time or self-employed), are expected to be healthier and happier, experience less stress, be less poor and to participate more in society.

The experimentation Article 83 of the Participation Act (e.g. Muffels & Gielens, 2019) allows municipalities to divert from the act on a number of issues for a limited time, to experiment with the goal of improving the effectiveness of the act. Importantly, however, municipalities need permission from the central government to do so. Without going into great detail in the wishes of all municipalities that expressed interest, the objections of the national government to several experimental proposals, the negotiations and the restrictions put on

54 The central poverty line of the Netherlands Institute for Social Research (Sociaal en Cultureel Planbureau, SCP) (for single person € 1,063 in 2014) is based on having an income which is 'not much but sufficient' and includes what is considered necessary to eat, live, buy clothes and take part in social activities.

55 This was originally released in Dutch as 'Gratis geld voor iedereen' (2014).

the experiment by the Ministry of Social Affairs[56], the end result was that only a limited number of municipalities were allowed to depart from the act. Two deviations were allowed: one with the obligation that people on social assistance have to look for a job, and the second with the restrictions for people on social assistance to earn money in addition to receiving social assistance.[57] If municipalities chose to experiment with the (abolishment of the) obligation to look for work, the national government set the condition that they also had to experiment with a more 'strict' regime.

The municipality of Nijmegen chose[58] to experiment with both increasing the opportunity to earn money from work, and with relieving the participants from the obligation to look for work (and all obligations that are related to that). Thus it was obliged to experiment with a stricter regime as well. The required stricter regime did pose some problems in the design of the experiment.[59] As participation was supposed to be voluntary, a very strict regime would not make participation very appealing (and likely contribute to selection bias). Therefore, the municipality of Nijmegen, the local reintegration organization (WerkBedrijf Rijk van Nijmegen, hereafter: WerkBedrijf)[60] and the involved researchers from the Radboud University (us), designed an approach specifically for this experiment. It had a relative high frequency of obligatory contact moments for all participants, while still catering as much as possible to the wishes and capabilities of the participants in that treatment. The chosen method consisted of the combination of group meetings, where people could meet and learn from each other; coaching on life and job skills; and giving participants maximum freedom in choosing their goals (full-time work, part-time work, volunteering, or self-employment). These group meetings were combined with the desired training or support needed to reach these goals. In Section 2.4.1 the design is described in more detail.

56 For those interested, Betkó (2017) gives an overview of this, and the broader context – it is only available in Dutch though.

57 Under normal rules, a person on social allowance is allowed to keep 25% of money earned through work, up to a maximum of € 200 per month, for a maximum of six months. In the experiment, participants are allowed to keep 50%, up to a maximum of € 200 per month, for the entire duration of the experiment which is 22 months.

58 The city council voted for the proposal to experiment with a large majority.

59 In Nijmegen, as well as in the other municipalities that held similar experiments.

60 The Werkbedrijf is engaged in job placement and reintegration. It is an administrative-regional association of municipalities, social partners (employers and trade unions) and the UWV (Uitvoeringsinstituut Werknemersverzekeringen; Employee Insurance Agency).

2.3 Theoretical background and hypotheses

Our theoretical expectations build on two theoretical frameworks. We start off with theory on biases in social experiments, which we will connect to theories on the impact of the experiment's treatments. Taking these together, we will formulate why we expect certain groups of people on social assistance (not) to be interested in applying for the experiment and thus which selection effects might be expected. The premise is here that people who have more to gain (financially or otherwise) are more likely to participate.

2.3.1 Social experiments and selection bias

Important for our study is what is exactly meant by 'selection bias'. A useful definition is given in the Cochrane Handbook for Systematic Reviews of Interventions: "Selection bias refers to systematic differences between baseline characteristics of the groups that are compared." (Higgins and Green, 2011, 8.4.1). The idea of an RCT is that, due to the random allocation of people over the groups, selection bias is adequately taken care of. But in (social) experiments like ours, where participation is voluntarily, there are two groups to begin with: one with people who want to participate, and one with the rest of the population. Within the group of participants, there should be no selection bias between treatment(s) and control. But if one wants to generalize effects to the whole population, which usually will be the case, selection bias rears its head once more, as has been documented in the literature on social experiments (Heckman & Smith, 1995). Heckman and Smith argue further that the question which factors affect the decision to apply for or participate in an experiment (e.g. advertising, local labor markets, income, ethnic background, and gender) is often not answered in RCT's.

In addition to selection bias, there is another bias that needs to be addressed when discussing selection effects: this is drop-out bias. Drop-out bias (*e.g.* Heckman *et al.*, 2000) happens when not all participants (fully) complete the treatment. Particularly those in the control groups have fewer incentives to stay in. Some might even drop out before the actual start of the experiment, which leads to a selection effect.[61]

In this chapter, we compare the full target population with the people who start with the experiment. Thus, we collect empirical evidence on the overall effects of selection bias and early-on drop-out bias (that occurs between the application for the experiment and the start of it). In other words, our study sets out to contribute to filling the gap in the empirical knowledge on these biases.

61 This could also be seen as a specific form of randomization bias, where participants in a specific group (often the control group) are participating less in the data gathering (Smith, 2000). In this case, the randomization process leads to selective drop out; to perceive this process is more important than the exact labelling, be it drop-out or randomization bias.

2.3.2 Why the experiment should work and thus holds appeal

The two treatments in the Nijmegen experiment are expected to have positive effects on the participants, and thus to appeal to them to participate in the experiment. At the same time, the treatment effects might differ according to the people's characteristics, which is also likely to translate to the appeal.

The expectations regarding the treatment are theoretically largely grounded in the behavioral sciences and in behavioral economics. Over the past decades, we have seen an increasing realization, not only in the social sciences but also in the economic discipline, that people do not solely make rational choices, and are no '*homo economicus*' (see e.g. Sunstein & Thaler, 2008; Tversky & Kahneman, 1981; Sen, 1977). Many choices are made under the influence of social and psychological processes. It is the combination of both rational choice and such behavioral processes that we focus on.

We assume the economic, rational-choice mechanism to be relevant as people are being allowed to keep part of their earnings besides their allowance (thus increasing their income), and triggers them to work in (part-time) jobs that might not be worth the effort in the existing means-tested social assistance system. As a consequence, such activities should increase their social capital (e.g. social network and skills) as well as their human capital (e.g. work experience and knowledge), which increases their value on the labor market, helping them to find full-time employment and increasing the likelihood of ending their dependency of social assistance. Indeed, people who have a part-time job next to their allowance find it easier to escape social assistance by means of work (Divosa, 2015). It is important to note that, while we use the term 'part-time' here, this also applies to people with short periods of full-time work. If somebody works full time, usually one is not eligible for an allowance. If somebody works fulltime for a (very) short period, for instance one or two weeks, the principle is the same as for someone who works part-time. When the monthly income is lower than a social assistance allowance, one is eligible and the income from work is deducted from the allowance (when not meeting the conditions as described in footnote 57).

From behavioral theories we use several concepts. For instance, Fehr and Gächter (2000) described the concept of reciprocity. The theory states how, driven by reciprocity, people make an effort, even against their own interest, to reward those who approach them in a positive way and punish those that approach them negatively. We translated this into a more positive, trust-based approach in the experiment, which is expected to yield better results than the usual approach (as described in Section 2.3.2), which is prompted by distrust (see also Muffels & Gielens, 2019). Moreover, in behavioral literature there is a growing strand focusing on the effects of poverty on stress, and stress on poverty. It suggests a downward spiral feedback loop. Poverty leads to increased stress and lesser bandwidth. This in turn can decrease people's willingness to engage in activity they perceive as risky (risk aversion) and increase time discounting (the tendency not to perceive positive effects or rewards in the future). It also can lead to a decrease of self-control and willpower. In turn, this impairs people's decision making and reduces their cognitive bandwidth, thus making it more difficult to escape from poverty (Mullainathan & Shafir, 2013; Van Geuns, 2013; Haushofer & Fehr, 2014; Moynihan, Herd & Harvey, 2014). In our experiment, this is incorporated in different ways. First, the opportunity to increase their income with part-time work can reduce financial scarcity and the accompanying stress. Second, and arguably more importantly, the stress of being subject to a large number of rules and regulations should be reduced. Whether participants get the first treatment, in which they are exempted from the obligation to re-integrate (i.e. apply for jobs), or the second treatment in which they get coached towards a self-selected goal. In short, all these behavioral mechanisms should contribute to participants of the treatment experiencing a higher degree of autonomy and less burden compared to the regular rules.

Summarized, we expect the treatments to have positive effects on employment, income, participation, well-being and health. This is because the treatments enable the people to make more of their own choices (instead of the local government making them for them), empower them in finding a job or other activity besides their social assistance allowance and give them the opportunity to earn a modest amount of money besides their allowance. Both these mechanics could reduce the stress participants experience, which in turn could lead to other positive effects.

By and large, these are evidently also reasons why the experiment is interesting for participants. Even though they are most likely not familiar with a concept like bandwidth, we clearly communicated: fewer rules, more freedom, more opportunities to keep a bit of money. At the core of this reasoning we thus have two incentives reflecting the different theoretical schools discussed above. There is the economic incentive, from rational-choice based theory, that people are able to earn more money. And there is the psychological incentive, based on behavioral theories, that participants are given more autonomy and freedom and the expectation that the extra trust that the local government gives them will be repaid in a reciprocal way.

2.3.3 Expectations on likelihood to participate

From these core mechanisms discussed above we can now derive expectations on which different groups of people are (more) likely to participate in the experiment than others, and thus which biases in participation we can expect. Given data availability and general social science theories, we will focus on three groups of characteristics that will act as independent variables: individual's factors, like gender, education and age; characteristics of the household people live in, like the presence of children; and characteristic directly linked to the social assistance situation and history of the potential participant, like the number of years somebody received social assistance.

Individual factors

First, there is little reason to expect that the rational-economic factors differ between men and women. The behavior aspect does, however, relate to average gender differences. Several studies have shown that women are more chronically stressed and experience more minor stressors, also considering similar life events (Matud, 2004; cf. Deater-Deckard & Scarr, 1996). Such higher stress perceptions might cause more incentives to participate as one of the treatments implies fewer rules. And even thought our second treatment increases the intensity of support, its focus on group sessions and coaching might actually increase women's likelihood to participate as well, since prior research has shown that solutions for stress are more directed towards social support (Ptacek, Smith & Dodge, 1994). On the other hand, one could argue that women more often care for children, and have less opportunity to engage in part-time work. This is controlled for though, since we include household composition (including the presence of children) as a variable. In short, the experiment's predicted impact of stress reduction can be expected to particularly appeal to women:

H1: Women are more likely to participate in the social assistance experiment than men.

Regarding age, we expect that the older the individuals, the less likely it will be that they will apply for the experiment, because of more limited economic incentives. In the current labor market, it is harder to find a job for older people. Especially the group that is 55+ has a difficult time getting a job (CBS, 2017; Lötters et al., 2013). So, for these people, there is less chance to benefit from the possibility to earn an extra bit of money. Older people's economic expectation in this respect, might be substantiated by their experiences with the municipality of Nijmegen. Local government has neither the budget nor the personnel capacity to help all people on social assistance with finding a job. Older people are referred to the WerkBedrijf for additional support less often than younger people (possibly because it is reasoned younger people have more chance to benefit from a re-integration trajectory into the labor market). This also means that part of this group also has less behavioral reasons to participate: in practice, they are already left alone by the municipality and its obligations. To summarize:

H2: The older persons on social assistance are, the less likely they are to participate in the experiment.

According to the human capital theory, education is a form of human capital that add skills and thus increases chances on the labor market (Becker, 1964). In addition, an education degree functions as a signal for potential employers on a person's productivity, ability and trainability (Spence, 1973; Arum & Shavit, 1995). This leads to the assumption that in general the higher people are educated, the easier it is to find a (part-time) job. Higher-educated people are thus more likely to benefit from the possibility to generate extra income in this way. Furthermore, literature shows that the higher people are educated, the more they value their autonomy (Inglehart, 1997). Based on this a positive effect might be expected:

H3: The higher people are educated, the more likely they are to participate in the experiment.

As for country of birth (used to determine whether somebody has a migration background), we expect that it is easier for people who are born in the Netherlands to find a part-time job than for others who are not born in the Netherlands, possibly due to experiences with labor market discrimination (e.g. van Doorn, Scheepers & Dagevos, 2012). So as a group, people born in the Netherlands have a stronger economic incentive compared with people born in other countries (both western as non-western). Moreover, these non-Dutch groups will also be less likely to participate for the practical reason that all the recruitment has been in Dutch, and not all individuals in these groups are proficient in the Dutch language.[62]

H4: People born in the Netherlands are more likely to participate in the experiment.

Household-related factors

Due to the way the tax laws work in the Netherlands, single parents usually have more opportunities to have a small income beside an allowance. Without going into detail of tax exemption regulations, we can say in general that several tax allowances depend on income. The higher the income, the lower the tax allowances.[63] Specifically for single parents the income threshold is higher (they can earn more money before losing tax allowances). Thus the economic incentive to participate is stronger. Then again, in addition to our core theoretical mechanisms, we should realize that single parents spend a lot of time caring for their child(ren). Living on a social minimum income, they do not have opportunities to get relief through (for example) extra childcare or hiring a cleaner. These considerations of practical nature would lead to an expectation that single parents are less likely to participate. So for the group of single parents, there are contradicting expectations. Of course, the possibility exists that both effects occur, and they cancel each other out. Because we expect to find effects on the specific sub category 'single parents', we use it as the reference category in our logistic regression model.

62 See section 2.4.1 for reasons behind this choice in design.

63 This only goes for the national tax allowances. On the local level, the municipality of Nijmegen considers a person on social assistance as eligible for local arrangements.

H5a: Single parents are more likely to participate in the experiment.
H5b: Single parents are less likely to participate in the experiment.

A specific household type is the group of so-called 'costs sharers' (*kostendelers*). The Participation Act states that, when more people who are not family[64] live together at the same address, they will – per person - receive a lower social assistance allowance[65], since they can share necessary costs, like the costs for housing. Because of the lower allowance they get and the way that interacts with taxation law (cost sharers have a lower income, and are thus less or not at all hindered by income thresholds for tax allowances), it is more beneficial for them to complement their allowance with an income from a (part-time) job. This gives this group a relatively bigger economic incentive to participate in the experiment. On the other hand, in the category 'miscellaneous': we have reasons to assume that the people who are costs sharers are not the most promising to find a job. The fact that somebody is unable to sustain a housing unit on his or her own, suggests that something is not going well beyond having to live on social assistance. After all, the social allowance should be high enough to enable living in an apartment or house (whether or not from a social housing corporation). As with single parents, we have contradictory expectations, and similarly, we will formulate both of them. Again, effects might cancel each other out.

H6a: People who are costs sharers are more likely to participate in the experiment.
H6b: People who are costs sharers are less likely to participate in the experiment.

Individuals' allowance situation and history

The first category of allowance characteristics is the number of years somebody received social assistance. For them, the same mechanism is relevant as for people of age. The longer people receive an allowance, the less chance they have in the (current) labor market (decreasing their likelihood of participating according to rational-choice theory), and in practice there is less chance they are sent to the WerkBedrijf for re-integration. Often, with no recent working experience, the chance of obtaining a part-time job is relatively small. And in practice, a part of this group is not bothered by the municipality with re-integration obligations.

H7: The longer people receive social assistance, the less likely they are to participate in the experiment.

The second category of allowance characteristics is whether somebody has an exemption from the obligation to work. In general, people only get such an exemption for a weighty reason, such as (temporarily) not being able to work due

64 And (thus) neither a 'couple'.

65 Single person 70%; two persons each 50%, total 100%; 3 persons 43.33% each, total 130%; 4 persons 40% each, total 160%; 5 persons 38% each, total 190%, etc.

to a medical reason. Therefore, from a rational-choice perspective it is logical that for a large part of the group with an exemption, the economic incentive is not there. In addition, the obligations of these people differ from the people without an exemption. They already have more autonomy due to fewer obligations. So, also from the behavioral perspective they have fewer reasons to participate.

H8: People with an exemption from the obligation to work are less likely to participate in the experiment.

Finally, there are people who earn money in addition to their allowance. Under normal rules, these people have to hand in all of the money they earn (or more correctly: their allowance is reduced with the amount of money they earn). The exception is the first six months somebody earns additional income: in this period, people are allowed to keep 25%, up to € 200. In the experiment though, people are allowed to keep 50% of what they earn, up to € 200. For people who already had an additional income, the economic incentive is thus much stronger than for the average person on social assistance. After all, if these people are randomly designated to one of the treatment groups, without having to change their behavior (they are already working), they have an increase in income.

H9: People who already earned money next to their allowance before the experiment, are more likely to participate in the experiment.

Below in Table 2.1 we summarize our expectations.

Table 2.1 Summary of demographic, household and allowance factors explaining biases in participation in the Nijmegen social assistance experiment.

Individual	Household	Allowance
Gender (f+)	**Household composition (single / couple/single parent / couple with children): single parent (+) or (-)**	***Number of years on social assistance (-)***
Age (-)	**Costs sharing (+) or (-)**	***Exemption from obligation to work (-)***
***Education* (+)**		**Receives an income additional to the allowance (+)**
Country of birth (NL+)		

Note: *italics* if related to behavioral theory, **bold** if related to rational choice theory, ***bold italics*** if both.

2.4 Data and methods

2.4.1 The Nijmegen experiment

In the Nijmegen UBI-inspired experiment people had to apply to participate. After the close of the registration, we randomized participants proportionally over three different groups of which the applicants knew the content before applying: two treatment groups and one control group. Participants in the first treatment group were allowed to keep more money when earning an income besides their social allowance payment *and* were relieved from all obligations requiring them to look for work. Participants in the second treatment group were allowed to keep more money when earning an income besides their social allowance payment *and* got intensive (group) coaching to help them reach the goals they set for themselves. The second treatment is the ‘strict’ group (as described in Section 2.2). However, eventual results for this treatment should be interpreted carefully as our strict regime does not favor the stick-with-nail over the carrot, which ‘strict’ might suggest. The treatment involves a human centered, social and empowering regime, based on personal choice and autonomy, with an obligatory component. This is also relevant for our study. The experiment is designed to be appealing to everybody, so it should not lead to selection bias (which the inclusion of a heavy-handed treatment might). The people in the control group were treated in exactly the same way as before the experiment, but will participate in a survey four times, over a period of three years. For this they receive a modest gift voucher once per year, to prevent drop out. The survey is conducted using computer assisted personal interviewing (CAPI). These interviews were held by a commercial research bureau[66], that was hired for this job.

The choice for combination treatments was not made from a purely academic perspective. As described in the methodological literature on social experiments, it is common that practical, institutional and political factors influence experimental designs (Heckman & Smith, 1995; Shadish, Cook & Campbell, 2002). This also applies to the Nijmegen experiment. The combination of treatments is an example of this. From a pure academic perspective, separate treatments would have been more logical. But combining them resembled the most the alternative treatment the municipal council asked for in their proposals.

It is worth noting that not all people on social allowance were eligible for the experiment. There were a number of national and local criteria that led to exclusion from the experiment, some legal and some practical. They are described in Table 2.2 (see also Muffels & Gielens, 2019). As an example, we excluded people under 27 because they are by law excluded from any possibility to earn income next to the allowance, not even in an experiment. Thus the experiment would

66 Labyrinth Onderzoek & Advies, Utrecht.

Table 2.2 Reasons for exclusion from Nijmegen social assistance experiment.

Age under 27 at the start of the experiment.
Reaching retirement age before the end of the experiment.
Exceptional vulnerable people in a specific register (doelgroepenregister).
History of violence against public servants.
Already participating in an intensive job-trajectory (with commitments to a third party(employer), or one that was bound to lead to result on short notice.

appeal less to them, and including them would introduce a specific selection bias and complicate the analysis.

For people to participate, the experiment not only had to be appealing, but they also had to know about its existence. To reach a broad and large group of potential participants, a wide range of recruitment instruments was used. The most important are given in Table 2.3. Important to note here - particularly given our research question - is that advertisement was only in Dutch. Consequently, we expect a selection bias in terms of ethnicity, as among the people on social assistance there are those who do not speak Dutch. This choice was made given the disproportional costs of translating the surveys used in this project in a valid way, as well as the practical impossibility of multi-language group sessions.

1 December 2017 was the official start date of the experiment. To increase the number of participants, people on social assistance were given a second chance to register, and on 1 April 2018 another group joined.[67] The enlistment procedure was by and large the same as the original one, only more modest. However, this time three subgroups of people on social assistance got a personal letter, stressing that the experiment could be of particular interest to them. These groups were people not yet on social assistance during the former advertisement campaign, people who turned 27 since then, and people who already had a part-time job.

2.4.2 Data and models

This study is based on both quantitative and qualitative data. The main source consist of registration data from the municipality of Nijmegen, on its habitants who are on social assistance. These data include a wealth of information, among others on gender, age, household composition, time on social assistance, education, country of birth, ethnicity, the neighborhood where people live, whether somebody earns money in addition to the social assistance, and special circumstances (like

67 The experiment ended on 1 December 2019.

Table 2.3 Means of recruitment of participants for Nijmegen social assistance experiment.

A three minutes animation video, spread through social media and shown in city offices.
A number of meetings in different parts of town, where a presentation on the experiment was given, people could ask questions and immediately apply. The meetings were announced in various ways, including in letters and through social media.
Posters and flyers, among others at city offices, the reintegration organisation, and civil organisations.
A personal letter, with a flyer, send to everybody.
Advertisement through social media.
Information in (digital) newsletters.
People were told about the experiment in personal contacts with employees of the social assistance department and the regional reintegration organisation.
Advertisement in local media.
Press release sent to local media.
During the moment where people on social assistance are obliged to hand in a monthly required form, there were civil servants on the floor of the city office to tell them about the experiment. People could enrol on the spot.

living in an institution, or being homeless). We combined the data file of all social assistance recipients with the data file containing the participants of the experiment. This allows us to compare participants with non-participants on these background characteristics.

In total, there were at the start of the experiment about 7,500-8,000 social assistance allowances in Nijmegen, of which approximately 1,000 are payments to couples – which means there were 8,000-8,500 social assistance recipients.[68] Adding together all the people who were on social assistance on either 1 December 2017 or 1 April 2019, and after applying the exclusion criteria for the experiment (see Section 2.4.1)[69], approximately 6,010 people remained. These constitute the target population, of whom 258 started the experiment in the

68 This might seem quite a wide range, but it is good to keep in mind that the number of people on social assistance is never stable; people enter and exit the scheme every day. For example, both the entire population of people on social assistance and the target population for this experiment differed at the time of the first and the second wave (December 2017 and March 2018).

69 These cannot be applied completely though. For example, the assessment on whether or not the re-integration trajectory an applicant was already receiving was a barrier for participation, was done on a case by case basis by a professional, and not based on a list of criteria that could be used to reduce our population.

first wave, and 66 in the second, leading to 324 participants in this study and 5,686 non-participants. If a participant exits social assistance, and enters again during the period of the experiment, they re-entered the experiment and got the treatment they were part of before leaving social assistance.

Our outcome is dichotomous: participates or not. That is why we use logistic regression models, in which a positive coefficient will indicate that a group of people is over-represented in our experiment and a negative coefficient indicates under-representation. In addition we use a Chi-square in combination with a Cramer's V to measure the association between participation in the experiment and the relevant independent variables bivariately as a first step.

2.4.3 Operationalization

Gender is measured as either male or female. Age is recoded into four categories: 27-34, 35-44, 45-54 and 55-64. Education is recoded into four standard categories on highest completed education: basic, lower secondary, higher secondary, and tertiary education. Unfortunately, for over 40% of the people education is categorized as "unknown" in the municipal administration. We included this as a separate category. Country of birth is measured in three categories[70]: the Netherlands, another Western country, or a non-Western country, according to the official Dutch government's classification. Household composition is measured in four variabilities: alone (no children), single parent, couple (no children), or couple with children. Costs sharing is measured as a simple yes or no. The time somebody received social assistance is measured in years since last entry, divided in the following variabilities: less than a year, between one and three years, and longer than three years.[71] This only refers to current social assistance spell. Whether somebody has an exemption from the obligation to work is measured in three categories: no exemption, partial exemption and a full exemption. The variable "Has an income next to the allowance" is measured in euro's. For this analysis we recoded it as a simple yes or no, 'yes' meaning any amount of income in 2017 until 1 December, the starting date of the experiment. The descriptive statistics for these variables are shown in Table 2.4.

2.4.4 Qualitative data

In addition to the quantitative data, we use qualitative data that indicate reasons as to why people chose to participate, chose not to, and chose to drop out after applying. We use these to verify and further interpret the results of the quantitative analyses. They can shed light on whether the mechanisms at work

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70 Following the definition of Statistics Netherlands.

71 This is based on the experience the municipality has with social assistance duration. Under one year, people have reasonable chance to find a job. Between one and three years, it gets more difficult, and once somebody is longer than three years on social assistance, the chance of leaving it due to finding a job is very small.

Table 2.4 Descriptive statistics of explanatory variables in the Nijmegen social assistance experiment.

Variable		N	Mean/ percentage	Minimum	Maximum	St. dev.
Gender		6,010				
	Male	2,969	49.4			
	Female	3,041	50.6			
Age (years)		6,010	46.27	27	64	10.24
	27-34	1,043	17.4			
	35-44	1,498	24.9			
	45-54	1,942	32.3			
	55-64	1,527	25.4			
Education		6,010				
	Primary	1,130	18.8			
	Low secondary	1,057	17.6			
	High secondary	835	13.9			
	Tertiary	545	9.1			
	Unknown	2,443	40.6			
Country of birth		6,010				
	Netherlands	3,382	56.3			
	Other western	310	5.2			
	Non-western	2,318	38.6			
Household situation		6,010				
	Single, no children	4,034	67.1			
	Single with children	1,109	18.5			
	Couple, no children	340	5.7			
	Couple with children	527	8.8			
Cost sharer		6,010				
	Yes	519	8.6			
	No	5,491	91.4			
Time on social assistance (years)		6,010	*	0	21*	*
	Less than 1 year	891	14.8			
	Between 1 and 3 years	1,791	29.8			
	More than 3 years	3,328	55.4			
Exemption from the obligation to work						
	No exemption	3,502	58.3			
	Partial exemption	331	5.5			
	Full exemption	2,177	36.2			
Income from work						
	Yes	713	11.9			
	No	5,297	88.1			
	Amount (€)	737	3,586.58	10	14,925.00	3,367.17

* Due to administrative reasons, the maximum duration registered is 21 years, making the mean, maximum and standard deviation scores unreliable.
Source: Register data Nijmegen municipal administration (December 2017, April 2018).

fit the theoretical mechanisms, can help to explain unexpected results and reveal additional processes.

We have two sources of qualitative data that give us valuable insights in the motives of potential participants. First, we interviewed three civil servants of the municipality of Nijmegen who were responsible for the recruitment. They spoke to hundreds of people on social assistance, who came to hand in their monthly form (see under Section 2.4.1) about participating in the experiment. Thus they have a good overview on the reasons why people did or did not want to participate in the experiment. Second, the research bureau that conducted the survey for testing the treatment effects administrated the reasons given for not filling out the survey by applicants; in total 44 applicants refused to cooperate in the survey, for various reasons.

2.5 Results

The results of our analyses are presented in Tables 2.5 and 2.6. The bivariate analyses are shown in Table 2.5 and the multivariate logistic regression models are presented in Table 2.6. Table 2.7 contains the summary of the biases in our sample. Our main conclusions are based on the full logistic model (Table 2.6, Model 3). The other models and bivariate analysis are used to establish what overall differences exist between the sample and the target population and to which factor they can be ascribed. By comparing the different logistic regression models, we can see what proportion is explained by the different characteristics (by comparing the Nagelkerke R^2) and how characteristics that seem to be relevant at first sight are explained (away) by other characteristics, indicating spurious relationships.

As for the qualitative data, we analyzed whether findings were mentioned by multiple sources (either by more recruiters, or by a recruiter and in the information provided by the research bureau), and the importance given to the findings (for example: was something mentioned as ‘an important reason’ by a recruiter, or was something relatively often a drop- out cause in the document from the research bureau). Where we came across unexpected findings we reread the qualitative material from that perspective to see whether it offered clues to explain that particular finding.

2.5.1 Individual characteristics

The first demographic characteristic, a person’s gender, has no statistically significant relationship with the likelihood of participation in any of the models. There is thus no indication that selection bias based on gender takes place and H1 is rejected. Similarly, we find no final significant relationship in the logistic models for age, leading to the rejection of H2. Age is statistically significant in the bivariate analysis, where notably people in the oldest age category show a decreased chance of participation, but this is cancelled when controlled for other variables. One recruiter stressed that younger people participated relatively more to earn extra income, and older people opted relatively more for a less restrictive approach.

Education in itself, our third individual characteristic, shows a statistically significant impact in all analyses. Tables 2.5 and 2.6 show that especially people in the two lowest educated categories have lower likelihoods to participate in the experiment. Those with higher secondary and tertiary education are more likely to participate. It is remarkable that the tertiary educated participate slightly less than the higher secondary educated. This might be related to another important result for education: the people of whom the educational level is missing in the municipal registration are also more likely to participate. Overall, these results lead to support for H3. The recruiters also observed that education plays a role in participation; higher-educated people were more interested. One elaborated that the higher-educated people were generally more motivated by the increased autonomy and decreased regulations, while lower-educated participants were generally more interested in the opportunity to earn extra income (similar to what was observed concerning 'age'). These insights suggest that the selection bias on education is (at least partly) caused by the higher-educated people's wish to have more autonomy, which corresponds with the theory used to derive hypothesis 3 (Inglehart, 1997). This would mean that different mechanisms appeal to differently educated people.

Country of birth (or: migration background) is significant in both analyses as well. People who are born in the Netherlands are more likely to participate in the experiment, while people born in other countries are less likely. For Western countries, this effect is only marginally significant ($p < 0.1$). H4 is accepted. We expected this among others on the language barrier, and this is supported by the qualitative data. One recruiter mentioned language as a deterrent for participation, and the document from the research bureau explicitly mentioned it as a reason for dropping out in four cases. Language seems a crucial part in explaining why both non-Western and Western people born abroad did participate less.

2.5.2 Household characteristics

The household characteristics show mixed results. Model 3 in Table 2.6 shows that the composition of the household is a significant factor in participation. We had contradictory expectations, either single parents would participate more compared to all other categories (H5a) due to increased possibilities to gather additional income, or participate less (H5b) due to not having the time to engage in an experiment. The results show something else though. Single parents have indeed a significant higher likelihood to participate compared to couples (with

Table 2.5 Likelihood to participate and not to participate in the Nijmegen social assistance experiment, Chi square and Cramer's V.

	Does not participate (N = 5,686)		Participates (N = 324)		Chi^2	P	Cr-V
	Number	Percentage	Number	Percentage			
Individual characteristics							
Gender					1.07		0.01
Male	2,818	49.6	151	46.6			
Female	2,868	50.4	173	53.4			
Age					12.48	**	0.05
27-34	972	17.1	71	21.9			
35-44	1,420	25.0	78	24.1			
45-54	1,826	32.1	116	35.8			
55-64	1,468	25.8	59	18.2			
Education					64.32	**	0.10
Primary	1,107	19.5	23	7.1			
Lower secondary	1,026	18.0	31	9.6			
Higher secondary	762	13.4	73	22.5			
Tertiary	509	9.0	36	11.1			
Unknown	2,282	40.1	161	49.7			
Country of birth					39.85	**	0.08
Netherlands	3,145	55.3	237	73.1			
Non-western	2,243	39.4	75	23.1			
Western	298	5.2	12	3.7			
Household characteristics							
Household composition					26.42	**	0.07
Single no kids	3,801	66.8	233	71.9			
Single parent	1,036	18.2	73	22.5			
Couple with kids	510	9.0	17	5.2			
Couple no kids	339	6.0	1	0.3			
Costs sharing					4.98	*	0.03
Costs sharer	502	8.8	17	5.2			
No costs sharer	5,184	91.2	307	94.8			

Table 2.5 Continued.

	Does not participate (N = 5,686)		Participates (N = 324)		Chi^2	P	Cr-V
	Number	Percentage	Number	Percentage			
Allowance characteristics							
Duration allowance					23.75	**	0.06
Less than a year	829	14.6	62	19.1			
Between 1-3 years	1,666	29.3	125	38.6			
More than 3 years	3,191	56.1	137	42.3			
Exemption from obligation to work					33.60	**	0.08
No exemption	3,272	57.5	232	71.0			
Partial exemption	306	5.4		7.7			
Full exemption	2,108	37.1	95	21.3			
Income					255.89	**	0.21
No income	5,102	89.7	195	60.2			
Income	584	10.3	129	39.8			

** $p < 0.01$, * $p < 0.05$, ~ $p < 0.10$
Source: Register data Nijmegen municipal administration (December 2017, April 2018).

and without kids), but hardly differ compared to singles without kids. The dividing line is between singles and couples. Further, it is noticeable that couples without kids are very unlikely to participate, even compared to other couples. All this leads to the rejection of H5b, and the partial acceptance of H5a (though it is the question of our assumptions on why single parents are more likely to participate can hold).

We also tested the role of costs sharing (i.e. people sharing a household without being a couple, and receiving a lower per person social assistance allowance, given their opportunity to share costs). We had contradictory expectations, having good reasons to assume both over- and under-representation. Without controlling for other factors, costs sharers have a significantly lower likelihood to participate as is shown in Table 2.5. In the multivariate model 3 it is still negative, but no longer significant, indicating that the expected selection bias is absent. Both H6a and H6b are rejected.

2.5.3 Allowance characteristics

Our last cluster of factors focuses on the characteristics of people's allowance. A longer duration of the allowance decreases the likelihood to participate according to the bivariate model (Table 2.5). In the logistic models (Table 2.6)

Table 2.6 Likelihood of participation in the Nijmegen experiment, logistic regressions with individual, household and allowance characteristics, N = 6,010.

	Model 1		*Model 2*		*Model 3*	
	B	Sig.	B	Sig.	B	Sig.
Individual characteristics						
Gender						
Male	Ref.		Ref.		Ref.	
Female	0.15		-0.04		0.01	
Age						
27-34	Ref.		Ref.		Ref.	
35-44	-0.12		-0.15		-0.05	
45-54	0.06		0.07		0.19	
55-64	-0.43	*	-0.35	~	-0.09	
Education						
Primary	Ref.		Ref.		Ref.	
Lower secondary	0.12		0.07		0.13	
Higher secondary	1.29	**	1.23	**	1.16	**
Tertiary	1.07	**	1.03	**	0.96	**
Unknown	1.05	**	1.00	**	0.88	**
Country of birth						
Netherlands	Ref.		Ref.		Ref.	
Non-western	-0.75	**	-0.69	**	-0.68	**
Western	-0.63	**	-0.62	*	-0.58	*
Household characteristics						
Household composition						
Single parent			Ref.		Ref.	
Single no kids			-0.14		-0.03	
Couple no kids			-2.88	**	-3.14	**
Couple with kids			-0.46		-0.66	*
Cost sharing						
No cost sharing			Ref.		Ref.	
Cost sharing			-0.54	*	-0.41	

Table 2.6 Continued.

	Model 1		*Model 2*		*Model 3*	
	B	Sig.	B	Sig.	B	Sig.
Allowance characteristics						
Duration allowance						
More than 3 years					Ref.	
Less than a year					0.04	
Between 1-3 years					-0.12	
Exemption from obligation to work						
No exemption					Ref.	
Full exemption					-0.45	**
Partial exemption					0.32	
Income						
No income					Ref.	
Income					1.72	**
Intercept	-3.42	**	-6.01	**	-6.61	**
-2 Log Likelihood	2,407.34		2,377.40		2,196.23	
Nagelkerke R^2	0.06		0.07		0.15	

** $p < 0.01$, * $p < 0.05$, ~ $p < 0.10$
Source: Register data Nijmegen municipal administration (December 2017, April 2018).

though, the significance of this duration effect disappears. Overall, we reject H7. Exemption from the obligation does have an effect. In the full logistic model the effect of a full exemption is highly significant and decreases the likelihood to participate. We therefore accept H8. In line with the theory behind our hypothesis and the importance of economic reasoning, a number of people expressed disinterest in the experiment to recruiters, due to their exemption from the obligation to work. Given the prominence of the possibility to earn money next to the allowance, they felt the experiment was not for them (being unable to work). Last, having an income has a significant effect on the likelihood to participate in all analyses. People with an additional income were far more likely to participate. H9 is supported.

2.5.4 Additional findings in the qualitative data

Regarding the general mechanisms, all three recruiters stressed the possibility to earn extra income was the most mentioned reason for participation. One recruiter expressed that a number of people chose not to participate though, because of uncertainty about the interaction between additional income and tax allowances. Likewise, people in a debt-restructuring programme[72] were less interested, since they are not allowed to keep extra income. Two recruiters mentioned people were willing to participate in the experiment because of the option to do more on own initiative and have less interference from the municipality, when looking for a job. Here we see that the (behavioral) motivation for more autonomy plays a role in peoples decision to participate.

In addition, there is one related mechanism for not participating that is mentioned by all three recruiters. Many people on social assistance indicated they were not able to cope with the pressure of participating. They were scared of the changes it would bring, and thus did not participate. Similarly, the research bureau recorded that five people dropped out, due to too high psychological and emotional pressure. While stress reduction is a sub-mechanism that is expected to help people improve in other areas and was one of the goals of the experiment (Westerveld, 2015), actual stress appeared to be also a barrier to participate and thus a cause for bias. When testing the impact of the treatment in later studies, this means that there is some selection bias on the dependent variable that is likely to lead to underestimating the actual effect (King, Keohane & Verba, 1994).

Finally, the qualitative interviews highlighted other additional factors that influenced participation, and in some cases might have led to a selection bias. First, as for reasons to participate, some people mentioned that, though not a reason for participating in itself, the option to quit the experiment at any time they wanted was conditional for their participation. It suggests that conducting social experiments in which participants are not allowed to quit before its ending could lead to selection bias (likely from people who highly value autonomy and prefer fewer restrictions). Some participants were particularly interested because of the intensive treatment, wanting the coaching, and even the obligatory meetings as an extra motivation for themselves to get active. Also, one recruiter mentioned that it was easier to recruit people with whom there already was a personal connection. And some people were aware of the original aim of the experiment, the local basic income, and wanted to participate because they support that idea.[73] All these arguments indicate that socio-ideological motivations can also foster participation, but the frequency in which they came up is limited, and thus are not expected to have a lot of effect on the outcome of the experiment.

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72 According to the Natural Persons Debt Restructuring Act (Wet schuldsanering natuurlijke personen, Wsnp) Dutch municipalities are obliged to help their residents with problematic debts. The main objective of the Wsnp is to offer (financial) perspective to individuals in a desperate financial situation as a result of debts.

73 Survey data show that advocates of UBI may vote for the introduction of UBI because they favour the concept and want to stimulate the discussion, but are well aware that there are minuses (see Delsen & Schilpzand, Chapter 3, this volume).

Second, additional reasons to refuse participation were mentioned as well: distrust towards the (local) government, bad experiences in the past with reintegration services, and principled objection to randomization. In the Nijmegen case, the ethical objections against randomization in an RCT thus not brought up by policy-makers and politicians, as literature suggest, (*e.g.* Smith, 2000; Heckman & Smith, 1995; Greenberg & Shroder, 2004), but only by some potential participants themselves. Also, some people gave the impression not being interested in making an effort, and preferring to stay out of sight. Altogether these reasons cluster in suggesting that the unmotivated might be under-represented, and (highly) motivated are somewhat over-represented – the latter particularly so in the second treatment group (as participants could decide to drop out to 'escape' from obligatory meetings).

2.5.5 Overall results

Overall, our statistical analyses show that all three clusters of factors matter, but not all variables of each cluster. As summarized in Table 2.7, we found that in comparing participants and non-participants, we should take into account education level, country of birth, household composition, exemption from the obligation to work and additional income. The most substantial effects were found for education, household composition (for 'couple no kids') and additional income. Most effects found were in line with our expectations, but only for about half of the factors tested we found a significant effect.

Table 2.7 Summary of statistical significance ($p < 0.05$) of variables explaining differences between participants and non-participants in the Nijmegen social assistance experiment.

Variable	Chi-square	Multivariate regression (Model 3)	Effect	Matches expectation?
Gender (female)	No	No		No
Age	Yes	No		No
Education	Yes	Yes	+	Yes
Country of birth	Yes	Yes	NL: +	Yes
Household composition	Yes	Yes	Single kids > single no kids > couple kids > couple no kids	Partial
Costs sharing	Yes	No		No
Duration	Yes	No		No
Exemption	Yes	Yes	-	Yes
Additional income	Yes	Yes	+	Yes

2.6 Conclusion and discussion

2.6.1 Conclusion

The question guiding this chapter was “which factors contribute to people participating, or not participating, in the experiment with social assistance, as it is held in Nijmegen?” Answering this question sheds more empirical light on selection effects in social experiments, as well as on its causes, which we were able to do due to the registry data on the entire target population combined with a mixed methods approach. We formulated multiple hypotheses based on rational-choice theory and insights from behavioral science. Our empirical analyses show no significant differences between the participants and the target population regarding gender, age, costs sharing and the time somebody has an allowance.

The other five hypotheses yield significant results, though only four of them are fully confirmed. Higher-educated people (hypothesis 3) and people being born in the Netherland (hypothesis 4) are significantly more likely to participate. Having a full exemption for the obligations regarding reintegration reduces the likelihood to participate. The composition of the household plays a significant role, but not exactly in the way we anticipated. Based on the theoretical mechanisms, we expected effects on single parents. Though we found single parents significantly more likely to participate than couples (with and without kids), the main difference was between singles (also the ones without kids) and couples. This leads to the rejection of H5b and the partial acceptance of H5a. In addition, it is remarkable that couples without kids are very unlikely to participate. The reason for this, and for under-representation of couples in general, might be an interesting topic for further research. It has been noticed before that unemployment tends to come in couples (Ultee, Dessens & Jansen, 1988), maybe the mechanisms behind that can offer some explanation. Finally, a relatively strong significant effect was found for people who already had an income before the experiment (hypothesis 9). This was to be expected: the rational-choice mechanism works most directly and visibly for this group. People who did not have an income yet, even if they would have above average gains from finding a part-time job, still needed to get that job to enjoy this advantage. People who already had income only needed to subscribe (and be lucky enough not to be randomized into the control group) to have a significant increase of income.

From the perspective of having much to gain from participating, we could argue that participation in the experiment is on the low side. With so much to gain and nothing to lose, it is remarkable that over 80% with a registered income does not bother to participate. This might be due to what is known of how reciprocity functions: negative reciprocity is generally stronger than positive (Fehr & Gächter, 2000). People who feel they are unjustly disadvantaged by the government might not want to cooperate (in an experiment conducted by that government) even if it is in their own interest. Indeed, potential participants approached by recruiters mentioned the unwillingness to participate, caused by earlier bad experiences with a branch of government, or a general lack of trust therein.[74] Another concept that might shed light on the relative modest participation of people who already had an income is that of 'administrative burden' (Moynihan, Herd & Harvey, 2014). In their study Moynihan and colleagues show how administrative hurdles that seem small, can have big effects on the individuals effected by them. This can lead to refraining from participating in an programme advantageous to them. Part of this concept is the fact that people tend to overvalue their *status quo*, even against an objectively superior situation. Other authors have also noted that bureaucratic procedures and (lacking) information about eligibility are hurdles when applying to welfare support (Renema & Lubbers, 2018). All this is an important reminder that, though personal, material gain (as put forward by the rational-choice model) is a strong motive, it is far from all explaining.

Based on the overall results, we can draw the conclusion that the effects of selection bias are not that strong. The correlations (Cramer's V, in Table 2.5) are all rather weak, except the one for 'income': that one score is still weak in conventional terms. At the same time, laying bare for which factors bias occurs is important for analyzing and interpreting the results of the experiment, even if the bias is weak. It does not only help to interpret the generalizability of the differences between control and treatment groups, but also informs which control variables should be included in multivariate analyses when researching the differences between the developments among the target population and the treatment groups.

One unexpected but important result, particularly for studies targeting economically deprived groups as done in this experiment, is the effect of stress on participation, which was shown by the qualitative data. As described earlier, stress reduction among this group was one of the reasons politicians started this experiment, and stress reduction plays an important role in the mechanisms. This very same stress is also a reason for people not to participate and to drop out. On second thought, we might have expected this. We approached 'stress' as something negative that could be alleviated by the experiment, and thus as a reason for people to participate. But the literature also shows that this same stress impedes people's ability to make long term decisions (in their own interest). In that sense, this outcome is logical.

74 One recruiter mentioned people had bad experiences with additional income. Having income in addition to an allowance is something that notoriously often goes wrong. This can lead to people losing their allowance, of having to repay tax allowances over a year after receiving them. For people on the subsistence minimum, this has grave consequences. It is not farfetched to assume that people who have experienced this once, will not be very eager to participate in an experiment with additional income.

2.6.2 Limitations

The premises in this chapter is that selection effects are determined by what people have to gain by participating, either financially or otherwise (through gaining more autonomy, or stress relief). One could argue that there are other mechanisms that contribute to participating in an RCT experiment. Maybe this group or another participates in general more often in experiments, regardless the specific contents. There are studies that analyze participation in questionnaires (e.g. Suchman & McCandless, 1940) and laboratory experiments (e.g. Slonim et al., 2013). There is to the best of our knowledge no similar research for RCT's though. Moreover, we cannot simply assume that selection effects in replying to questionnaires (for instance: higher educated people participate more, Suchman and McCandless (1940)) or participating in short laboratory experiments (people who volunteer more often participate more, Slonim et al. (2013)) take place as well in an intensive social experiment that takes almost two years. At the same time, we cannot rule out that there exist similar effects for RCT's. In some ways, this could even be expected. Though we have people participating who are homeless or institutionalized, it is conceivable that these are not represented according the weight of their category. The very reclusive people, who hardly leave their house or do not open their mail, are obviously more difficult to reach.

Another (somewhat related) consideration is whether the selection effects we find are not partly due to the mechanism of comprehension of the experiment. Higher educated people understand the setup (and the opportunities it offers) probably better than lower educated ones, and people born in the Netherlands will on average understand it better than people born outside. This could play a role, no doubt. On the other hand, everything possible has been done in the recruitment to make the experiment as accessible as possible: personal contact, communication in an easy understandable language, animated movies and presentations throughout the city for those who are less literate. The relative small size of the selection effects indicate this was at least partially successful.

2.6.3 Contributions to science and policy

In this study, we have empirically shown a number of selection effects in our experiment. We conclude that the selection effects in our social experiment can be traced back mostly to selection bias, and to a lesser extent to drop-out bias. The statistical data revealed the presence of selection bias and the qualitative data confirmed drop-out bias. Heckman and Smith (1995) mention that little is known about the effects of (among others), gender, ethnic background and advertisement in social experiments. Our experiment has addressed both gender and ethnic background (through country of origin), and sheds light on advertisement due to the attention we have given the recruitment processes. We assume advertisement plays a role, in limiting the selection effects on the one hand and due to the language used in the recruitment of participants on the other. In addition to these results, we have identified a number of other factors that can play a role in participating (or not) in an RCT through our interviews with recruiters; further research might show how relevant these are.

Our experiment also gives insight in the question whether rational-choice or behavioral concepts (most noticeably reciprocity (Fehr & Gächter, 2000)), the effects caused by stress related to poverty (Van Geuns, 2013; Haushofer & Fehr, 2014) and bandwidth (Mullainathan & Shafir, 2013) are decisive in these selection effects. The outcome is that based on our results, we cannot conclude one mechanism is supported more over the other. The effect found for education and exemption can be explained by both mechanisms and the role of country of origin adds practical expectations regarding selection effects to the story (the recruitment procedure was in Dutch only). The impact of income provides considerable support for the rational-choice mechanism, while the qualitative data underscore the importance of stress and behavioral arguments as well. We suppose that the two mechanisms are complementary. The different mechanisms might sometimes partly balance each other out as they motivate groups of people at the different end of the scale. To illustrate, younger and lower-educated people seem to be mostly interested in earning extra income, while older and higher-educated people are more interested in autonomy and a less restrictive regime. As for the rational-choice argument: it is crucial that people understand their advantage. Not all people might understand that, or underestimate it. People have to be aware of the rationality of a choice to be able to be motivated by it.

This study's results are also relevant for policy makers, among others exactly because of this possible lack of understanding people seem to have on what they have to gain. The results for costs sharers and single parents have particular implications for local policy. Based on rational-choice arguments, both groups were expected to participate more, which is not the case (at least: the overrepresentation of singles in general does not seem to imply that specific single *parents* are more likely to participate, and for the reasons we assumed). This suggests a lack of awareness of the possibilities. If a municipal government, such as the Nijmegen one, wants to stimulate their inhabitants on social assistance allowance to earn an income in addition to the allowance – and thus cost the municipality less money - it has to make sure those financial and non-financial advantages are known by and understandable for these people. Even if it is only the normal policy of being allowed to keep 25% of additional earnings.

The negative effect of stress on participation also has implications for our experiment, and similar social experiments (including those related to the UBI) based on voluntary participation in the Netherlands and worldwide. A group that is supposed to have specific advantages from the treatments (that is: people who experience stress due to the social assistance system) is most likely under-represented due to selection bias and drop-out bias. When this is the case, the effects of the treatments could be substantially underestimated. Both researchers and policy makers who want to experiment with social assistance policy should be well aware of this.

3

How welfare policies can change trust

A slightly different version of this chapter is accepted is published in the international peer reviewed journal Basic Income Studies. See the section Contributions of authors for information on co-authors.

3.1 Introduction

Trust, welfare regime and poverty have been studied in different combination, but important knowledge gaps remain, particularly on the micro-level linkage between receiving welfare assistance and trust (Kumlin et al., 2018; Nannestad, 2008; Soss, Hacker & Mettler, 2007).

It is well documented that trust, both social and political, is an important asset for society. Social trust is "associated with health, happiness, prosperity, long life and a sense of belonging … Political and social trust promote active citizenship and the effective implementation of public services, reduce tax evasion, … and help clear the path to political agreement and compromise" (Newton, Stolle & Zmerli, 2018, p.38). This means that implementing policies that increase populations' trust are likely to be beneficial for society as a whole. In terms of different policy domains, empirical studies also found a consistent link between the design of welfare policies and high political and general social trust (e.g. Bergh & Bjørnskov, 2011; Edlund, 1999; Kumlin & Rothstein, 2005; Rothstein & Uslaner, 2005, Christensen & Lægreid, 2005).

Similarly, a positive connection between socio-economic security and trust has been established in industrialized democracies (Newton, Stolle & Zmerli, 2018; Paxton, 2007; Whiteley, 1999; cf. Spierings, 2019). Poverty makes people less trusting (Newton, Stolle & Zmerli, see also Michener, 2018), or in the words of Newton (2009: 357) trust is "a privilege of the rich, successful, and educated". Thus, even in welfare states, socio-economically deprived citizens form a low-trust group due to relatively scarce positive experiences with society (Newton, 2009).

While intuitively logical, these results on trust & welfare policy and trust & poverty coin a puzzle: it are particularly the groups that rely on welfare states' safety nets by receiving welfare benefits that could be expected to have positive policy experiences that may increase political and social trust. This linkage, however, has received hardly any attention, and there is a dire need for more knowledge about the micro-level linkage between receiving welfare policies and trust (Kumlin et al., 2018; Nannestad, 2008, but see Michener , 2018). The few existing studies show mixed results, mainly suggesting that the design of the welfare policies is crucial in either weakening or strengthening trust (e.g. Greiner, Ockenfels & Werner, 2012; Hyggen, 2006; Kevins, 2019; Kumlin & Rothstein, 2005).

To shed more empirical and theoretical light on this issue, we present a social experiment with different social assistance schemes in country characterized as a high-trust welfare state, i.e., the Netherlands (Rothstein & Uslaner, 2005). In 2015, the Dutch national government passed a law allowing municipalities to vary some social assistance regulations in order to test policy effectiveness. The city council of the 10th largest city in the country, Nijmegen, decided to do so and included political and social trust as outcome (Groot, Muffels & Verlaat, 2019; Betkó et al., 2019; Betkó 2018). In a randomized control trial setting, two alternatives for the standard control-and-sanction-focused regulations were introduced. Focusing more on autonomy and support than the regular regime (see Section 2.2), the expectation was that this trusting and positive approach might be reciprocated by the recipients (Putnam, 2000; Rothstein & Uslaner, 2005). To provide a broader picture, we not only study local political trust, but also whether the changes also spill over to national political trust and to society at large, i.e., generalized social trust (hereafter: social trust). Hence, our first main question: *To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes in the political and social trust among different groups of recipients?*

Besides shedding empirical light on how different social assistance designs relate to changing trust levels, the setup of this study also contributes in important methodological and theoretical ways to the literature on welfare policies and trust. First, methodologically, a fundamental question in this literature is about causality: do welfare policies induce trust or is trust the base for establishing such policies? (Bauer, 2015; Kumlin et al., 2018; Leenheer, Gesthuizen & Savelkoul, 2021; Nannestad 2008). As far as we are aware of, this study is the first to present a randomized controlled trial, which allows for stronger causal inferences in a real-world setting. As such, we can provide new insights into whether a causal influence runs from experiencing welfare policies to trust.

Second, we contribute theoretically by formulating and testing new and more specific expectations on *why* welfare policies might increase trust. As alluded to above, the literature generally works from the notion of experience and reciprocity: people who have positive experience with society, such as being trusted by others, including the state apparatus, will become more trusting themselves (Dinesen & Bekkers, 2017; Newton, 2009; Newton, Stolle & Zmerli, 2018; Paxton, 2007; Rothstein & Uslaner, 2005; Whiteley, 1999). Our experiment also starts from this understanding, but specifies this general mechanism by integrating the welfare policy and trust literature and the more general literature on economic strain and well-being (Haushofer & Fehr 2014; Butterworth et al., 2009; Skapinakis et al., 2006). Most importantly, the design of a policy does not only communicate a degree of trust which is reciprocated (as also highlighted by Rothstein & Uslaner, 2005), but it also has social and psychological implications. Taking a broader socio-psychological perspective draws attention to the idea that policies aimed at autonomy and support might not only be evaluated more positively and therefore increase trust, but also reduce stress or increase mental bandwidth (Mani et al., 2013; Shah et al., 2012) as well as reduce shame and allow for more social integration (e.g. Stewart et al., 2009). Whether these processes take place and to what degree is therefore central to our second research question: *To what extent are expected increases in political and social trust between recipients in different social assistance regimes explained by (a) policy evaluation, (b) social integration, and (c) psychological well-being?*

Thus, we contribute to existing literature by first examining the causal relation between welfare policy and trust, and second by theorizing and empirically testing what drives this trust.

3.2 Theoretical background and hypotheses

3.2.1 Welfare policies and the origins of trust: the current perspective

Most studies on the relation between welfare and trust assume a reciprocal relationship (e.g. Bergh & Bjørnskov, 2011; Edlund, 1999; Kumlin & Rothstein, 2005), but as mentioned above, the causal order is debated and theories on how welfare policies influence trust are limited.[75] Of these studies, Kumlin & Rothstein (2005) theorize the impact of the design of welfare-state policies on recipients' trust most specifically. They focus on the difference between universal welfare regulations and selective (means-tested) regulations. They argue that having experiences with the former increases political trust, because citizens feel treated fairly as they receive trust from public agencies instead of having to prove they deserve social benefits. This logic reflects the more general 'top-down' or 'experience-based' perspective on the origins of trust, as for instance distinguished by Newton et al., (2018), who focus on the influence of society and its institutions, and whether people have positive experiences with them. At its core, this approach supposes that persons' trust in both institutions and other people can grow or diminish, based on persons' experience with institutions and people. In this study, we will expand on this logic, but specify it in more detail to make it applicable to different regimes of welfare policy and expand it by theorizing different socio-psychological mechanisms underpinning this more general logic.

In collaboration with the national and municipal government, two experimental treatments with more trust-based alternative rules were designed. Both build on the concept of reciprocity, which brings together the experience-based perspective on welfare and trust and the insight from experimental economics that trust is a reciprocal concept, whereby an institution providing trust is likely

75 We are aware of the wider debate on the origins of trust, and the extensive literature on e.g. biological and psychological origins of trust. Without wanting do discard the important of those, our study revolved around experiences with government policies and consequently society, and thus we focus on experience based trust alone.

to receive trust (Fehr & Gächter, 2000; Frey & Jegen 2001; Bohnet et al., 2001). In our experiment this providing of trust manifests itself in two ways: less control on its recipients by the government and more self-chosen support and autonomy for its recipients.

3.2.2 The Dutch context and the Nijmegen experiment

Before turning towards our general theoretical propositions and specific expectations, however, the background of the experiment deserves some attention. The regular social assistance regime in the Netherlands (and thus also in the city of Nijmegen) is means-tested and control-focused. This regime is a last resort for people without an income or other means under condition that recipients try to find paid employment. The last few decades, the Dutch welfare system moved from a hybrid corporatist/social–democratic model towards a liberal model, whereby policy reforms were less based on collective solidarity, but on instituting a selective model involving privatization, individual responsibility and conditionality (Delsen, 2016), based on workfare and 'stick and carrot'-approaches (Groot et. al., 2018). The current system of social assistance has a large number of obligations for recipients as well as fines for those who do not comply. Recipients are supposed to end social assistance as soon as possible and are obliged to accept any job to do so. The possibility to earn extra money (from part-time or temporary work) in addition to an allowance, is strictly limited (Betkó et al., 2019).

The experimental treatments deviated from this (and thus from national law), explicit permission by the Ministry of Social Affairs was given to conduct them (based on a particular clause in the 'Participation Act')[76]. The experiment was held at the request of the Nijmegen City Council, who in majority was critical toward the Participation Act (that came into effect at January first 2015). The discussion on the topic started with the call of a political party which wanted to replace the Participation Act with a local basic income (Ranshuijsen & Westerveld, 2015) – though this aim got quickly replaced by a more unconditional form of social assistance, due to demands by the Ministry as well as the local political reality (Betkó, 2018). In the literature, the Nijmegen social assistance experiment, as well as similar experiments in other Dutch cities, is seen as part of a global wave of universal basic income experiments (Groot et al., 2017; Delsen, 2019; McFarland, 2017; De Wispelaere & Yemtsov, 2019). Details on the treatments follow below, where we discuss expected effects. This illustrates how social assistance has both a national and a local component. The broad framework is defined at the national level, in the Participation Act. The execution of the social assistance scheme takes place locally, at the level of the municipalities. Thus, the experiment we conducted, is an experiment with national social

76 More specific: Article 83. of the Participation Act, as effective from 1st of January 2015. All current Dutch laws can be found at www.wetten.nl.

assistance law, in one municipality, which is responsible for the local execution. So, while recipients are mainly in direct contact with the local government, they are affected by both national law and local implementation. Consequently, we study trust effects regarding local politics and regarding national politics, as the latter facilitated this policy.

Participation in the experiment was voluntary. Social Assistance recipients were asked to apply for the experiment, having been informed about it over several months in a number of different ways aimed to reach as many people as possible (e.g. personal contact, personal letters, digital news-letters, local media, gatherings in the neighborhood, and social media). Every participant gave explicit and active informed consent. For a schematic overview of the experiment: see Figure 3.1 below[77]. For more information on the ethical concerns on studying human subjects: see Appendix 4. The implementation of the experiments went according to plan, according to an external process evaluation (ZonMW, 2020), with the research report for the municipal government concluding that for most

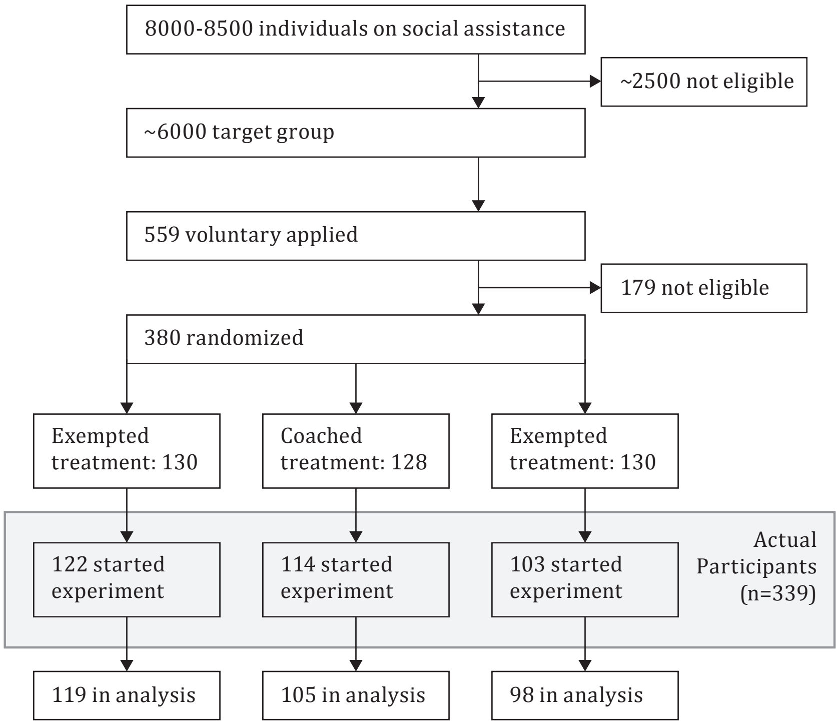

Figure 3.1 – Schematic structure of the experiment: trust

77 This lowest row (boxes 'in analysis') is relevant for the trust outcomes only, numbers differ for other outcomes in other chapters. See also Appendix 3 for further information about these schematics.

outcomes, no average difference effects exist between control group and experimental groups or such differences were small and often not statistically significant at conventional levels. The authors of the municipal research report concluded that, certainly for the step from social assistance to paid employment, the carrot does not work better than the stick, but also that the stick does not seem to outperform the carrot (Betkó et al., 2020). The report, however, does not cover all in the data included outcomes or subgroup analyses, and it does not assess the causal pathways or (potentially counteracting) mediation effects.

3.2.3 Theorizing the treatment effect of trust-based social assistance

Receiving social assistance is a core experience for economically marginalized people: the way in which social assistance is shaped is thus likely to shape people's trust. Crucial here, we expect, is the degree of trust the regime provides to the recipient. The experiment gave more trust to recipients by giving them more autonomy and less government control, with the general expectation to increase trust (Fehr & Gächter, 2000; Frey & Jegen 2001; Bohnet et al., 2001). Below we discuss the alternative treatments and expected effect on trust.

The first alternative treatment group is exempted from all obligations on re-integration (hereafter: 'exempted'). Under the regular treatment, social assistance recipients are obliged to actively seek employment, accept any job offered, commute up to 3 hours per day and can be required by the local authorities to perform labor without additional pay.[78] The exempted group was relieved of these obligations, so there was no government control regarding re-integration either. Participants were allowed though to make use of re-integration programs offered by the municipal government, on their own initiative: this stresses the self-support side of the treatment. The second alternative treatment group received a coaching-based re-integration scheme that was custom-made for the experiment (hereafter: 'coached'; see Appendix 1 for a description of this scheme in more detail). All participants in this group were obliged to participate in the coaching program, but the setup was not one of control, but of coaching and support, whereby participants had a high level of autonomy concerning their re-integration efforts. Participating in this group replaced all regular re-integration obligations, so also in this treatment there was less government control than in the regular regime, and tailor-made support was provided. Additionally, both treatments allowed recipients to keep a higher amount of money if they earned any by doing part-time or temporary work.[79]

78 A complete list of obligations related to labor integration can be found in article 9 of the Participation Act. They are obligations on the national level from which municipalities have not much room to deviate, though the municipality of Nijmegen doesn't make full use of the option to force social assistance recipients into unpaid labor.

79 Normally a social assistance recipient is allowed to keep 25% of money earned through work, up to € 200 per month maximum, for at most six months. During the experiment, participants were allowed to keep 50%, with the same € 200 per month maximum, for the duration of the experiment.

Effectively, these two treatments cover two aspects of a government providing trust: less control and more autonomy. Both the exempted and the coached treatments gave participants a higher degree of autonomy compared to the regular treatment and communicated that the government trusted the recipients to act responsibly and to contribute to seek an income, even if not forced and controlled. Additionally, both groups were able to set their own goals, highlighting self-chosen support. Specifically, the coached group was offered tailor made support here. The government more clearly invested in them (in a positive way), also compared to the exempted group. Consequently, based on the framework sketched above we expect both groups receiving alternative treatments to show an over-time positive development in their levels of trust. Additionally, the coached group can be expected to show an even more positive development as – they received intensive empowering support in reaching their self-chosen goals. Following the logic of reciprocity which is a core theoretical assumption, the coached group is not only likely to experience the same new autonomy as the exempted group, but also to experience this positive investment in them by the government, and thus there is more to be reciprocated.

Before formulating our explicit hypotheses, it should be noted though that we focused on trust in relatively general terms. As yet, our expectations are formulated with respect to political trust and more specifically with respect to trust in the local government, as the local government is the institution participants directly interacted with, organized the experiment, provided social assistance and was in this respect the 'face' of the government. However, trust in different levels of government as well as social trust are related and when it comes to welfare policies and trust, they feature in the same literature, engaging similar theories (Uslaner, 2018). In that respect, we might expect that the treatments impact local political trust most clearly, but could also impact trust in the national government given that social assistance is a shared responsibility between national and municipal government, and that the treatments could potentially result in positive experiences that even increase social trust. In Western democracies, national trust tends to be lower than local trust (Tang & Huhe, 2016), but given that we study the change in trust, this should not affect the results of this study. Generally, spill-over effects of one form of trust to the other have been shown empirically (Freitag & Traunmüller, 2009; Glanville & Paxton, 2007; Christensen & Lægreid, 2005). Consequently, similar albeit weaker effects could be expected for national trust level and social trust. Because of this theoretical relevance, and because the importance of these dimensions of trust for the cohesiveness of society (Putnam, 1996; Uslaner, 2018), we include these aspects of trust in this study, leading to our first set of hypotheses, related to Q1:

H1a: political and social trust will develop more positively in the exempted group compared to the control group
H1b: political and social trust will develop more positively in the coached group compared to the control group
H1c: political and social trust will develop more positively in the coached group compared to the exempted group

3.2.4 The political, social and psychological underpinning of potential treatment effects

Next, in order to provide answers to our second question, we contribute theoretically in a more in-depth way, by theorizing which particular mechanisms might mediate between the experimental treatments and trust. Besides focusing on the 'experience-based' perspective (Newton, 2009) that predicts trust to increase due to positive experiences with institutions and people, we integrate insights from the literature that links economic vulnerability with social integration and psychological well-being (Butterworth et al., 2009; Gallie et al., 2003; Pearlin, 1989; Stewart et al., 2009). The treatments might well have a substantial positive impact on the social integration and psychological well-being of social assistance recipients, yet so far these perspectives have been largely lacking. Below we thus argue for three potential mechanisms that might underpin the general reciprocity argument, which can be tested empirically in this study. These mediating mechanisms revolve around (a) policy evaluation, (b) social integration, and (c) psychological well-being, and we expect that these three factors are positively affected by the alternative treatments compared to the regular treatment, and that they in turn influence trust positively as well. The same remarks regarding the different dimensions of trust as discussed above apply here, whereby local political trust is at the core of our logic.

A. Policy evaluation – As is often implicitly assumed in the literature on welfare policies and trust, it can be expected that the increased trust of welfare recipients, particularly those who are under less control and more self-support, is partly due to the way participants *evaluate the policies* and their effectiveness, and whether they need to answer to a bureaucracy perceived as subjective. There are a number of substantially similar concepts, from different fields, that describe the mechanisms at work here, e.g. procedural justice (Lind & Tyler, 1988) and administrative burden (Moynihan, Herd & Harvey, 2014). In addition, several studies showed how notions on how institutional design and the way government deals with people relate to (social and political) trust (Kumlin & Rothstein, 2005; Rothstein & Uslaner, 2005; Kumlin et al., 2018). Core is that people, who are subject to institutional decision making and bureaucratic procedures, do not only want an outcome which is advantageous for them, they are at least as sensitive for being treated fairly (in a procedural just way), and that experiencing (unnecessary) administrative burdens leads to dissatisfaction. We argue that this is crucial for how people experience and evaluate policy, both if they are content with it (not too much burden and if it is fair) and if they deem it effective, and that these positive or negative experiences can influence how trusting people are. Specifically, regarding political trust, institutionalist theories argue that trust originates from evaluation of policy, which can refer to considerations regarding both the *output* of government and the perception of the fairness of the *process* (Christensen & Lægreid, 2005; Michler & Rose, 2001).

Translated to the issue at hand, if a government institution that has power over welfare recipients does not take their views and preferences into account and imposes high administrative burdens, this is likely to negatively affect the clients' trust in these public institutions, just as a positive approach is likely to have positive effects. In our experiment, both treatment groups received more autonomy and support from the government, with fewer obligations and sanctions. Therefore, we can expect that they considered these welfare policies more positively and consequently that this is reciprocated in increases in their trust. Phrased formally, we thus expect that:

H2: *political and social trust will develop more positively in the treatment groups compared to the control group, due to a more positive assessment of the social assistance policy in the treatment groups*

B. Social integration – Next, we build on both qualitative and quantitative sociological literature on economic vulnerability and more specifically on poverty and social isolation (Gallie et al., 2003; Stewart et al., 2009). We argue that a second mechanism might connect government's trust-based welfare policies and recipients' trust in the government, namely social integration. It has widely been shown that various conceptualizations of economic vulnerability and social isolation go hand in hand. Recently, Visser et al., (2018) showed that economically vulnerable groups (like the unemployed, permanently sick or disabled, and people having difficulties to cope with their household income) have relatively few social contacts and persons to discuss intimate matters with. Moreover, economically vulnerable people often tend to be looked down upon, stigmatized, rejected, and discriminated against (Gallie et al., 2003; Inglis et al., 2019; Lustig & Strauser 2007; Reutter et al., 2009) adding to the shame they often feel regarding their financial situation (Stewart et al., 2009). A lack of financial resources, negative social interactions and feelings of shame thus induce economically vulnerable people to be excluded and withdraw from social relationships and from society at large (Gallie et al., 2003; Stewart et al., 2009).
Applying these theoretical notions and empirical findings to the experiment, the treatments in this study are likely to increase social integration in several ways. First, due to less restrictive rules in the experimental conditions (no obligation

to actively seek employment for instance), these people may have had more time and energy to maintain and initiate social contacts. Second, the treatments might have made people less prone to rejection, shame, discrimination and stigma in social situations. Rejections that went hand in hand with the formerly obligatory job applications no longer negatively affect participants' self-esteem. Instead of having to prove worthiness for receiving social assistance by obliging to a strict set of rules, the received trust and autonomy might increase the feeling of being a valuable member of society. Third, in the regular regime, people on social assistance need explicit permission for volunteering, since it might interfere with their availability on the job market. In the experimental treatments, this was not so – meaning that participants in the treatments could create self-worth and social contacts from volunteering jobs. Finally, participants who made use of the opportunity to earn additional income had extra money, for instance to pay for a round of drinks or to purchase a birthday present which they earlier could not – thus improving their ability to have social contacts. In summary, the treatments are expected to induce social integration, both in terms of quantity and quality.

Repeated positive social interaction has been argued to develop trust (e.g. Putnam, 2000; Glanville et al., 2013; Spierings, 2019), as regular interactions with friends, family, neighbors and other network members facilitate the sense that these contacts will fulfill their obligations when the time comes to reciprocate, thus building trust (Welch, Sikkink, & Loveland 2007). This evolving trust from interactions within one's core network leads to positive experiences with society at large, which is said to translate to a more positive view of society, and its institutions.

In sum, a government that puts more trust in the recipients and allows them more autonomy is expected to increase peoples' social integration, which in turn translates to more trust:

H3: *political and social trust will develop more positively in the treatment groups compared to the control group, due to stronger social integration in the treatment groups*

C. Psychological well-being – Poverty has important consequences for people's psychological well-being, which also feeds into trust, and might explain the impact of more trust-based welfare policies, or the relation found between income and social trust (Brandt et al., 2015). Poverty has been shown to diminish psychological well-being over time (Butterworth et al., 2009; Skapinakis et al., 2006). The chronic stressor approach (Drentea & Reynolds 2015) tends to look at poverty as an enduring threat to the most basic human needs, causing ongoing (financial) stress, which in turn decreases psychological well-being (Haushofer & Fehr 2014; Pearlin 1989; Reading & Reynolds 2001), and increases the likelihood to suffer from anxiety and depression (Fitch et al., 2011; Frank et al., 2014).

Another stream of literature poses that the scarcity situation of poverty consumes a large portion of the mental bandwidth of people: a large part of poor people's cognitive resources is devoted to managing their immediate financial scarcity (Mani et al., 2013; Shah et al., 2012), making choices and displaying behavior to solve immediate problems, but which in the longer run aggravate problems. As Haushofer & Fehr (2014: 862) state: "poverty causes stress and negative affective states […] leading to short-sighted and risk-averse decision-making, […] constituting a feedback loop that contributes to the perpetuation of poverty." The stress that poverty causes is pivotal for the lack of psychological well-being that poor people on average display.

In several ways, the treatments in our experiment could reduce the (financial) stress levels experienced by the participants. First, the possibility to earn more extra income as compared to the regular regime could reduce the chronic (financial) stress and thus increase their mental bandwidth. Second, having fewer obligations and more autonomy could create more mental room for finding structural solutions for the causes of the problems that participants face in their lives. Consequently, it can be expected that the treatments lead to reduced stress and improved psychological well-being. Next, the experience-based logic predicts that positive experiences lead to trust, and feeling well psychologically might (subconsciously) translate to assessing society more positively, as also suggested in previous work (Kummlin & Rothstein, 2005).

Altogether, we thus theorize that the trust-based treatments in our experiment increase the psychological well-being of participants, which in turn translate to more political and social trust:

H4: *political and social trust will develop more positively in the treatment groups compared to the control group, due to better psychological well-being in the treatment groups*

3.3 Data & methods

3.3.1 Data

We collected data via surveying the participants of our social experiment. These surveys were conducted as a computer-assisted personal interview (CAPI), before the start of the experiment and approximately a year later. During that time, the participants were receiving the alternative social welfare scheme (with exception of the control group, which received the regular treatment). We combined these survey-data with data from the municipality on characteristics of people (which we used as control variables). The experiment ran for a little more than two years (December 2017 – January 2020), and there was a survey taken after years as well. However, given the highly reciprocal process involved in the subject of our study (trust), we cannot take the results of this second year into account. At the time of the final survey, the invested trust and autonomy are about to be taken away again, something which the participants were very much aware of, as they knew the experiment was about to end. After two years the participants were asked if they were worried about the experiments' ending and the return to the regular social assistance regime. It turned out that over 50% of the participants in the exempted group had worries about the upcoming end of the experiment, and almost 40% of the exempted group.[80] While worry about the end is not per se the same as experiencing a taking away of trust, the latter is highly likely. The experiment was framed in terms of trust and after it ending former participants, again had to fulfil obligations and were threatened with fines when not obliging., i.e. distrust was leading again. Therefore, measuring the change over two years, implies also measuring the impact of taking away the given trust, confounded with the treatment effects focused on in this study.

Participation in the experiment was voluntary. Out of the approximately 8.000 social assistance recipients, approximately 6.000 were eligible to participate.[81] Social assistance recipients were informed about the experiment in a 6 week long information campaign, consisting of, among others, letters, personal talks, information sessions in several community centers, an animation for social media, advertising in regular media, and flyers and posters. We started with 339 people as participants in the experiment. Due to drop-out and missing data, not everybody could be included in this study.[82] For each of the three dependent (trust) variables, we had approximately 300 valid answers in the baseline survey, and approximately 250 in the second survey.[83] The trust scores of the people of whom we missed the data on the second survey are hardly lower than on t0, but slightly lower for social trust – suggesting limited, if any, drop-out bias.[84] The descriptive statistics of the variables included in our models can be found in Table 3.1. It should be noted that our study contains three trust indicators as dependent variables, with a slightly different N. Table 3.1 is based on the cases for local political trust, since this is our primary focus.

80 Compared to a little over 20% for the control group (chi-square $p < 0.01$) – which is in itself interesting, since for the control group nothing changed so they already were in the regular social assistance regime.

81 The most important exclusion criteria was age.

82 People who left social assistance (due to for example finding a job) remained a participant. Drop-out from the experiment happened e.g. when a participant requested to leave the experiment, when the participant did not comply with the conditions the municipality set on participation, or when the participant died.

83 Political trust (municipality): 305 – 57, 248 left; political trust (national): 288 – 58, 230 left; social trust: 311 – 52, 259 left.

84 Political trust (municipality): drop-outs score 0.02 points lower on a 5-point scale in the baseline survey; political trust (national): drop-outs score 0.06 points lower on a 5-point scale in the baseline survey; social trust: drop-outs score 0.81 points lower on a 11-point scale in the baseline survey.

3.3.2 Dependent variables: trust

To test the impact of the treatments on trust, we investigate three dimensions of trust. First, we looked at political trust in the municipality (hereafter: local trust) and second we looked at political trust at the national level (e.g. government, parliament – hereafter: national trust). We used items that are standard in this type of research. For both levels of government, participants were asked “Do you want to list for each of the following organizations how much trust you have in them” ((1) no trust at all; (2) not too much trust; (3) a little bit of trust; (4) complete trust).[85] Social trust is measured with the widely used question (Bauer & Freitag, 2018): “Do you think that most people can be trusted, or that you cannot be too careful in dealing with people?”, measured on a 0-10 scale. As dependent variables we subtracted the individuals’ trust score at t = 0 (the baseline) from his or her trust score at t = 1 (measurement after one year), per type of trust. A positive score indicates trust has increased over time, a negative that trust has decreased.

3.3.3 Independent variable: treatments

The core focus here is on the different developments in trust between the people receiving different treatments. Our independent variable thus consists of the experimental group the participants were in. Group 1 pertains to the experimental ‘exempted’ group, Group, 2 to the experimental ‘coached’ group, and Group 3 to the control group. We created dummies for each group, with the control group as the point of reference. A negative sign shows the treatment influenced the difference in trust in a negative way, while a positive sign shows a positive treatment effect.

3.3.4 Mediating variables

To measure the first of our mediating variables, *policy judgement and effectiveness*, we used six survey items in which participants were asked to evaluate the rules and obligations of their current welfare regime, as is in line with the theoretical concepts discussed above. All of these items were ranked on a five-point scale, ranging from “totally disagree” to “totally agree”. Based on a factor analysis, (Appendix 5), we created two indices based on these six items, one labelled “policy satisfaction” and one “perceived policy effectiveness”. The policy satisfaction index is based on the theoretical logic of procedural justice (Lind & Tyler, 1988) and administrative burden (Moynihan, Herd & Harvey, 2014), and is a combination of the four items “I experience these rules and regulations as a

85 We used items that are standard in this type of research as much as possible, with small adjustments made to take the relative low average level of language skill of the target group into account. The questions about political trust are modelled after similar questions posed by the Dutch Bureau of Statictics (CBS) in their social cohesion (‘sociale samenhang’) monitor, which are similar to items in the European Social Survey, though there they use a 10 point scale.

burden", "These rules and regulations annoy me", "These rules and regulations allow me enough leeway to do what I want" and "These rules and regulations are fitting for my situation". The first three covering 'burden' and the latter 'fairness'. We recoded the first two of these items so that lowest score resembled the most negative opinion, allowing us to combine these items with the other two in the same index. The perceived policy effectiveness index covers the question if participants think the treatments are helpful to them, and is a combination of the two items "These rules and regulation help me to take part in society" and "These rules and regulations encourage me to find a paid job". Both indices rank from 1-5, with 1 being the most negative opinion and 5 the most positive. Again we calculated and include the difference score to determine the development on this indices. We subtracted the scores at t = 0 from the scores at t = 1. A negative sign means that people have become more negative about the experience or effectiveness, and a positive sign means people take a more positive outlook on it.

To measure our second mediating concept, *social integration*, we used four items, of which we combine the last three in a single index. The first is the question "how strong do you feel part of society", using a 0-10 scale. The other three items ask "how often do you have contact with …" 1) neighbours or people in your street, 2) friends and good acquaintances, 3) relatives and family. The options participants could choose from were "rarely or never", "less than once per month", "once per month", "twice per month", "thrice per month" or "at least once per weak", coded as 0-5, where 0 were the fewest contacts. Because we want to know whether the treatments lead to more social contacts, not the specific social relations underlying these contacts, we combined them into a single index. Once more, we calculated and used the difference score of the index, as well as for the loose item on feeling part of society.

To measure psychological well-being, our final mediating factor, we used two indices, one for subjective well-being and one for mental health. This is a common distinction in research, though the two constructs are closely related (see e.g. Petríková, 2018). Our choice to separate them here is furthermore based on a factor analysis (see Appendix 5). The items used in the subjective well-being index consists of the two items that are part of (among others) the EU-SILC 2013 dataset and the World Value Survey V23. They are: "Can you rate your current life satisfaction from 0 to 10?" and "Do you think the things you do in your life are meaningful?" (also on a 0-10 scale). The items for the mental health index are taken from the Mental Health Inventory-5. It is built up by combining the four following items: "the following questions are about how you felt the last 4 weeks. Give the answer for each question that best resembles how you felt. Were you very nervous? Were you so down that nothing could cheer you up? Were you calm and relaxed? Were you sad and dejected?"[86] These questions

86 In our survey we also asked "Were you happy" in this same set of questions, but factor analyses showed this item to be a double loader, on both subjective well-being as mental health, so it was removed.

were ranked on a 5-point scale from “never” to “always”. We recoded the items in a way that for each a low score means the most negative feeling, and (once again) used the difference scores.

3.3.5 Control variables

In a perfect RCT design control variables should not be necessary. After all, randomization should lead to an even spread of observed and unobserved characteristics that might influence the outcome over the different treatment groups, and should thus not be related to the independent variable in our models (see Pearl & Mackenzie, 2018). Nevertheless, as the randomization might not have been perfect and also taking into account the relative low number of participants, we take a cautionary approach and include a number of control variables that have been shown in past studies to be able to influence trust (e.g. Rothstein & Uslaner 2005; Hyggen 2006; Lee, 2012 This linkage to trust does not mean these are confounding per se, as for that also a causal effect on the treatment needs to exist (see e.g. Pearl, 2009; Pearl & Mackenzie, 2018), which, as said, might be the case if the randomization did not lead to completely comparable groups. Moreover, by including these control variables, we increase the precision of our estimates of the treatment effects. All control variables were measured at t=0, prior to the start of the different interventions. Thus there is no risk of over controlling: the treatment cannot have influenced the scores on the these control variables. We include: gender (m, f), age, education (basic, lower secondary, higher secondary, tertiary, and ‘unknown’), country of birth (recoded into: Dutch, other Western, non-Western) and household composition (single no kids, single with kids, couple no kids, couple with kids).

3.3.6 Analyses and models

To test our hypotheses, we use regression models estimating the difference score between the variable of interest on t=1 and t=0. In line with common practice, we rely on p-values to assess the degree of uncertainty of any found relationship to exist outside of the studied sample. However, we also acknowledge the limitation of this focus, particularly in terms of assessing effect size and shape (e.g. Gelman & Carlin, 2014; Ziliak & McCloskey, 2008). In this light, we assessed the patterns of the effects found, for instance assessing the impact of outliers and whether the effects are linear (as far as one can speak thereof in the cases of experiment group comparisons), particularly to prevent making Type-1 or Type-2 errors. For instance, we visually assessed the unexplained variance of the main models, without the treatment variable, depicted in box-and-whisker plots per treatment (see Appendix 8).

As mentioned, our dependent variable is a difference score, participants who already have a very high score at t0 will not or hardly be able to increase in their trust level. By adding the trust score on t=0, we control for such ceiling effects. Our first model, only including this t=0 variable, is:

Table 3.1 Descriptive statistics, N=248 (based on local trust).

Variable (N)	Min.	Max.	Mean / %	SD
Political trust: local	1	4	3.00	0.737
Political trust: local w2	1	4	3.09	0.700
Political trust: national (N=234)	1	4	2.52	0.870
Political trust: national w2 (N=234)	1	4	2.50	0.890
Social Trust	0	10	6.19	2.034
Social trust w2	0	10	6.17	2.032
Group	1	3	1.94	0.84
Index Policy Satisfaction	1.00	5.00	2.95	0.84
Index Policy Satisfaction w2	1.00	5.00	3.22	0.84
Policy satisfaction difference	-1.75	2.50	0.27	0.84
Index Perceived Effectiveness	1.00	4.50	2.52	0.88
Index Perceived Effectiveness w2	1.00	5.00	2.63	0.91
Perceived effectiveness difference	-2.00	3.50	0.11	0.86
Feels part of society	1	11	7.71	2.08
Feels part of society w2	1	11	7.80	1.87
Part of society difference	-7.00	8.00	.089	2.11
Index contacts	0.00	5.00	4.20	1.02
Index contacts w2	.67	5.00	4.27	0.99
Contacts difference	-3.33	3.33	0.07	0.94
Index subjective well-being	1.50	10.00	6.63	1.49
Index subjective well-being w2	1.00	10.00	6.85	1.46
Subjective well-being difference	-5.00	4.00	0.22	1.38
Index mental health	1.25	10.00	6.21	1.93
Index mental health w2	1.25	10.00	6.52	1.91
Mental Health difference	-5.63	6.88	0.32	1.78
Gender (ref=male)			0.55	0.50
male			0.45	
female			0.55	
Age	27	64	45.44	9.91
Country of birth				
Dutch			0.75	
Non-Western			0.22	
Western			0.04	
Household situation				
single, no kids			0.69	
single parent			0.25	
married, with_kids			0.06	
married,no_kids			0.00	

Table 3.1 Continued.

Variable (N)	Min.	Max.	Mean / %	SD
Education				
Lower education			0.10	
Lower middle education			0.11	
Higher middle education			0.34	
Tertiary education			0.37	
Education unknown			0.07	

Source: Surveys experiment Participation Act Nijmegen (October/November 2017, February/March 2018, November/December 2018); Register data Nijmegen municipal administration (December 2017, April 2018).

$$\Delta Y_{t1}\text{-}Y_{t0} = \beta_0 + \beta_1 Y_{t0} + \varepsilon \qquad (1)$$

In our second model, we add the independent variable (experimental treatment). This model shows the core effects of the experimental treatments on trust:

$$\Delta Y_{t1}\text{-}Y_{t0} = \beta_0 + \beta_1 Y_{t0} + \beta_2(group) + \varepsilon \qquad (2)$$

Next we add the other control variables, the model we use to test hypotheses 1a through 1c is thus as follows:

$$\Delta Y_{t1}\text{-}Y_{t0} = \beta_0 + \beta_1 Y_{t0} + \beta_2(group) + \beta_N(control) + \varepsilon \qquad (3)$$

For research question 2 and testing hypotheses 2 to 4, we assess whether mediation effects are taking place. We do so by taking several steps, reflecting the work by Baron and Kenny (1968) and by Hayes (2009) (see also Newsom, 2020; VanderWeele, 2015), which is common approach in explanatory sociology. First, we estimate the effect of treatment on outcome (the models discussed above; Step 1of 4 in Newsom, 2020). Additionally, we test whether the treatment has a significant impact on the mediator (in the appendices, Step 2 in Newsom). Next, we add the mediator to the model from Step 1, which should show a significant relationship between mediator and outcome (Step 3 of 4 in Newsom). Finally, we assess whether the coefficient of the treatment variables decreases after including the mediating variables, and a substantive decrease is needed to consider the mediation to actually mediate the original relationship established in Step 1 (Step 4a).

Our fourth step is not explicitly part of the Baron and Kenny / Newsom approach, but does reflect their Step 4 of calculating the indirect impact of treatment on outcome via the mediator. As we are mainly focused on whether the mediating factors explain an overall treatment effect, we take this akin but alternative fourth step. However, we do acknowledge that two mediation effects can cancel each other out. If that is the case the net effect of the treatments on trust is still absent, but the theoretically expected mediation might still exist. Therefore, in the case we found a mediator to be influenced by the treatment (Step 2) and to be influencing the outcome (Step 3; see Appendix 4), we also consider the indirect impact of treatment on the outcome (Step 4), which we assess by running a PROCESS model (Hayek, 2013) as suggested by Newsom (2020). This model helps to assess the significance of the effect of treatment on outcome via that mediator.

Our mediation analyses thus comprise a large set of models. Below, we focus on the mediations of the found overall effects, and those are central to the results section. The other outcomes are given in the appendices or can be obtained from the authors, and where they indicate indirect effect (while main overall effects of the treatments are absent) they are explicitly discussed in the text.

3.4 Results

3.4.1 Levels of trust

The results of our experiment on trust are described in Table 3.2 and 3.3. In Table 3.2, the descriptive outcomes are shown, while Table 3.3 shows the regression models. Our main conclusions on the changes in trust are primarily based on the full model including control variables (model 3).

The absolute raw scores we show in Table 3.2 – they seem to correspond with the current Dutch average (Dekker & Den Ridder, 2020)[87]. These numbers furthermore confirm that the strongest change in trust during the experiment is found regarding local trust, as expected. Local trust grows modestly in both treatment groups, and stays the same in the control group. The scores for national trust do not change much for any group. Regarding social trust, there is a modest growth for the exempted group, a modest decrease for the coached group and the control group stays mostly the same. This is an indication that our expectation that trust will increase might be confirmed for political trust in the municipal government, but expected spill-over effects do not manifest. If this is indeed the case will be tested in the regression analysis.

3.4.2 The treatments' impact on developments in trust

In Table 3.3 we show the impact of the treatments on the changes in social and political trust, controlled for potential ceiling effects and other potentially relevant characteristics. The results do indeed indicate ceiling effects which are

87 This study uses the same scale as our study for social trust – political trust is more difficult to compare.

Table 3.2 Trust scores on t=0 and t=1 per group.

		T=0		T=1		Δ trust
Dependent variable per group, [scale], (N)		mean	SD	mean	SD	
Local trust [1-4] (248)						
	Exempted (94)	2.98	0.70	3.12	0.67	+ 0.14
	Coached (74)	3.11	0.75	3.26	0.64	+ 0.15
	Control (80)	2.92	0.76	2.91	0.75	+ 0.01
National trust [1-4] (230)						
	Exempted (90)	2.49	0.78	2.44	0.86	- 0.05
	Coached (65)	2.60	0.93	2.57	0.88	- 0.03
	Control (75)	2.47	0.94	2.43	0.92	- 0.04
Social Trust [0-10] (259)						
	Exempted (98)	5.88	2.14	6.01	2.22	+ 0.13
	Coached (78)	6.33	2.05	6.14	1.99	- 0.19
	Control (83)	6.33	1.87	6.29	1.86	- 0.04

significant in all instances, as shown by the negative and significant effects of the trust levels at the first measurement: the higher the levels of trust were, the less they increase.

As for the main effect of the treatments on trust, we do find that participants in the treatment groups display a significantly stronger increase in trust as compared to participants in the control group, though indeed only for the type of trust most directly connected to the experiment: local trust. There is a significant increase for both the exempted group and the coached group ($p<0.05$). As for national trust level and social trust, the effects are not significant.

Hypotheses 1a, 1b and 1c are confirmed for local trust: both in the exempted and the coached group there is a stronger increase, and the increase seems strongest for the coached group. The hypotheses are rejected for the other types of trust: national trust and social trust. There appears to be no spill-over effect. These results are robust, and stay similar when the model is ran without the control variables (see Appendix 8), indicating that a lack of effects is not due to over-controlling.

3.4.3 Explaining the treatment effects

Having answered our first research question, we turn to the second question on which mechanism(s) connect the treatments and trust, whereby we will focus on local trust as the treatments do have a net positive effect on it. Theoretically, however, the potential positive effects of the treatments on national and social trust via the mediators is still interesting even if they are cancel out by other

Table 3.3 Change in social and political trust in the Nijmegen experiment, regressions with t=0 and control variables.

	Model 3.1 (local trust)		Model 3.2 (national trust)		Model 3.3 (social trust)	
	B	Sig.	B	Sig.	B	Sig.
Group						
Control	Ref		Ref		Ref	
Exempted	0.21	*	0.03		0.14	
Coached	0.26	*	0.04		-0.06	
T=0 trust value	-0.61	**	-0.26	**	-0.46	**
Gender						
Male	Ref		Ref		Ref	
Female	-0.03		-0.04		0.44	~
Age	0.00		0.00		-0.00	
Education						
Primary	Ref		Ref		Ref	
Lower secondary	0.28		0.06		1.45	**
Higher secondary	0.17		-0.08		1.44	**
Tertiary	0.26	~	0.05		1.58	**
Unknown	0.08		-0.01		1.48	**
Country of birth						
Netherlands	Ref		Ref		Ref	
Non-Western	0.29	**	0.26	*	-0.47	~
Western	0.21		0.03		-1.28	*
Household composition						
Single no kids	Ref		Ref		Ref	
Single parent	0.02		0.15		-0.21	
Couple no kids	-0.03		-0.70		-0.22	
Couple with kids	-0.16		0.03		0.42	
Intercept	1.33	**	0.53	*	1.68	*
N	248		230		259	
R	0.60		0.41		0.53	

** p < 0.01, * p < 0.05, ~ p < 0.10

elements of the treatments (as indicated by the negative overall effect we saw above). In our discussing of these results, we follow the steps as outlined in section 3.6 (see Baron & Kenny, 1986; Newsom, 2020; Vanderweele, 2015).

First, the treatment needs to affect the mediating variable. The result of the analyses testing this are given in Appendix 6. The model specification corresponds with model 3 above, now taking the mediators from Table 3.4 as dependent variable, including this mediator at t=0 to control for ceiling effects. Both the exempted group and the coached group show a sharper increase in policy satisfaction compared to the control group, and for the exempted group this effect was significant. Additionally, we unexpectedly found marginally significant *negative* effects for the exempted group on both 'feeling part of society' and 'subjective well-being' (Appendix 6). Though interesting, it falls beyond the scope of this paper to further theorize or examine why these effects in the opposite direction from our expectations are found; we simply acknowledge these results for now.

Second, the mediating variable should affect the difference in trust. To assess for local trust we consider models 4 through 7 (Table 4). There, we only see a positive and significant effect of a change in policy satisfaction on the change in local trust. In Appendix 7, and relevant to the additional results above, we also find a positive relationship between policy satisfaction and national trust and a marginal significant positive effect of feeling part of society on social trust.

Third, we should find that including the potentially mediating factor takes away part of the treatment effect on the change in trust compared to the model without the potentially mediating factor. First, focusing on the effects of the treatments on local trust, we compared models 4 through 7 to model 3. For policy satisfaction we see that the coefficients for the treatment groups ('exempted' and 'coached') are smaller compared to model 3. This particularly holds for the exempted group; for the coached group the decrease is more modest.[88] Also including subjective well-being partly decreases the initial effect of the exempted treatment; however no clear impact of the exempted treatment on subjective well-being was found, which makes us cautious on drawing a strong conclusion here on.[89]

From the steps above, it could also be derived that two potentially suppressed mediation effects are taking place, which we additionally assessed by calculating the indirect effect of the treatment on trust via the mediator, using PROCESS models (Hayes, 2013). First, the impact of the exempted treatment on social trust via feeling part of society was not found confirmed, which aligns with the rather weak (i.e. marginal significant) effects mentioned above. For the effect of the exempted treatment on national trust this was different as we do indeed also

88 The PROCESS model for the exempted group via policy satisfaction change also shows an indirect effect with a confidence interval above zero (0.01, 0.12); for the intensive treatment it was across zero (-0.01, 0.10).

89 Running the analysis with PROCESS also shows a clearly insignificant indirect effect (CI: -0.20, 0.07).

find an indirect effect via policy satisfaction.[90] As said, due to additional negative mechanisms connecting the exempted treatment to national trust, this effect is suppressed and the overall average exempted treatment effect on national trust is not significant.

These results imply for local trust that the (positive) change in how participants experienced the new policy in the treatments is likely to be part of the reason why local trust increased, most strongly for the exempted group. Similarly, via policy satisfaction we found a positive effect of the exempted treatment on national political trust, but this is suppressed by unidentified effects of the exempted treatment, leading to no average positive effect on national trust. Both effects discussed above indicate that relevant and substantive parts of the treatment effects found remain after including the potentially mediating factors, which means that the mechanisms behind the increase in political trust, particularly of the coached treatment, remains largely unknown.

Moreover, taking a step backwards to look at the larger question, we do find that both aspects of policy evaluation and effectiveness significantly influence the change in national political trust, and find one aspect of psychological well-being (i.e. mental health) and one aspect of social integration (i.e. feeling part of society) significantly influence the change in social trust, all in the expected direction. One could argue that it is not purely the theoretical logic not being at work, but the treatments not affecting change in the relevant mediating factors.

Further robustness tests confirm the results discussed above. For instance, one could argue that the value on the mediating variables at t=0 should be added to the mediation models as well. While we choose not do this for our main models, we did run these models as a robustness test. For local trust this model is given in Appendix 8 and this does not change the main substantial outcomes. Furthermore, in that model the effect of a change in subjective well-being on the change in local trust disappears.

To summarize: Hypothesis 2 is partly confirmed, indirect relations between the treatment and increased local trust[91] via policy satisfaction exist, though only substantially for the exempted group, while perceived effectiveness does not. Hypotheses 3 and 4 are fully rejected.

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90 The confidence interval of the indirect effect reported by PROCESS was fully above zero, albeit just (0.00, 0.12).

91 As stated earlier, using the PROCESS analysis there also was an indirect effect via policy satisfaction on national trust for the exempted group – however, since hypothesis 2 strictly speaking requires an overall positive effect of the treatment (which does not take place for national trust), this finding cannot be used to confirm the part of hypothesis 2 dealing with political trust on the national level, notwithstanding we do find an indirect positive treatment effect on national trust via policy satisfaction.

Table 3.4 Change in political trust (municipality) in the Nijmegen experiment, regressions with t=0, mediating and control variables.

	Model 3.1 (see Table 3)	*Model 4 Mediation 1 Pol judg.*	*Model 5 Mediation 2 Social int.*	*Model 6 Mediation 3 SWB&MH*	*Model 7 Full Mediation model*
Group					
Control	Ref	Ref	Ref	Ref	Ref
Exempted	0.21*	0.17~	0.19*	0.18~	0.14
Coached	0.26*	0.24*	0.26*	0.27**	0.25*
T=0 trust value	-0.61**	-0.61**	-0.61**	-0.63**	-0.64**
Policy evaluation					
Satisfaction		0.15**			0.17**
Perceived effectiveness		-0.01			-0.03
Social integration					
Part society			-0.03		-0.01
Contacts			0.02		0.04
Social & mental WB					
Subjective well-b.				-0.07*	-0.08*
Mental Health				-0.02	-0.01
Gender					
Male	Ref	Ref	Ref	Ref	Ref
Female	-0.03	-0.04	-0.03	-0.04	-0.06
Age	0.00	0.00	0.00	0.00	0.00
Education					
Primary	Ref	Ref	Ref	Ref	Ref
Lower secondary	0.28	0.27	0.30~	0.27	0.28
Higher secondary	0.17	0.11	0.18	0.16	0.10
Tertiary	0.26~	0.22	0.26~	0.27~	0.24~
Unknown	0.08	0.05	0.09	0.06	0.02
Country of birth					
Netherlands	Ref	Ref	Ref	Ref	Ref
Non-Western	0.29*	0.33*	0.28~	0.29**	0.34**
Western	0.21	0.23	0.22	0.22	0.25
Household composition					
Single no kids	Ref	Ref	Ref	Ref	Ref
Single parent	0.02	0.05	0.01	0.06	0.09
Couple no kids	-0.03	-0.25	-0.10	-0.12	-0.40
Couple with kids	-0.16	-0.23	-0.16	-0.13	-0.21
Intercept	1.33 **	1.40 **	1.39 **	1.48 **	1.58 **
N	248	248	248	248	248
R	0.60	0.62	0.61	0.62	0.65

*Fields shows the B and significance, where: ** p < 0.01, * p < 0.05, ~ p < 0.10*

3.5 Conclusion and discussion

We started this study with two questions. The first is how different social assistance regimes influence political and social trust, with the assumption that those less based on government control and more on recipients' autonomy have a positive effect. The second is which mechanisms are responsible for any occurring change in trust. We have answered these questions by using data of a unique randomized controlled trial with social assistance, which is particularly well suited to assess whether relationships are causal. Our main finding is that local trust among social assistance recipients grows among the groups that receive social assistance policy with less government control and more autonomy. However, we only find overall effects of the treatments on local trust, the level of government that was most actively involved in the experimental social assistance scheme. This impact on local trust does not clearly and strongly transfer to more trust in the national government, nor to trust in people in general (social trust).

Regarding our second question, we have shown that for the exempted group particularly, satisfaction (i.e. through less burden and more experienced fairness) with the social assistance policy drives increases in local trust, and also connects this treatment to national trust. Whether participants deem the policy more or less effective does not seem to matter in our experiment, nor do aspects of social integration or psychological well-being. We do find positive significant effects for several of these mediating factors on the change of some dimensions of trust though, but they do not link to the treatments we implemented. As such these results do support elements of more general theories on the relationships between these three factors (assessment of the policy; social integration; psychological well-being) and (some dimensions of) trust.

Our findings give thus a partial explanation for the increase in trust in the exempted group, but largely lack such an explanation for the coached group - while this group revealed the strongest increase in local trust. Though both groups were offered more autonomy and less government control, they do differ in a few important ways. These differences might help to theorize the combination of findings (strongest local trust increase for the coached group, yet no explanation of policy satisfaction).

The two biggest points of discrepancy between the coached and exempted groups are 1) that exempted has even less government control than the coached group, because the coached group has the obligatory group meetings, and 2) that the exempted group lacks all contact with government officials regarding re-integration, unless they chose to initiate it themselves, while the coached group has more contact than in the normal regime (again, due to the group meetings). In the light of our theoretical framework, we can speculate that the exempted group had even less burden, which might lead to policy satisfaction being a mediator. While the coached group did not perceive less burden, participants likely displayed a stronger reciprocal reaction because the municipality invested in them personally through those group meetings.

As for the effect of policy satisfaction, the results fit theories on how interactions with the government shape the way people experience policy, most notably the psychological perspective of the procedural justice theory of Lind and Taylor (1988), the administrative burden perspective of Moynihan, Herd and Harvey (2014), and the findings of Kumlin and Rothstein (2005) on how welfare state designs influences trust. More specifically, in selective welfare systems, civil servants (‘street level bureaucrats’) test citizens if they have the right to use something (and are not cheating). This causes distrust between official and citizen in a negative feedback loop, where the official professionally has to distrust the citizen, while the citizen is dependable and fears arbitrariness of the official. In selective systems, recipients are burdened with administrative efforts to proof that they are really eligible. While the alternative social assistance schemes in our experiment are not truly ‘universal’ in nature, they are no longer selective with regard to re-integration obligations of the labor market either. Recipients do not have the burdens regarding re-integration that regular social assistance users have. Moreover, Rothstein and Uslaner (2005) argue that a universal program signals to welfare recipients that they are important and can be trusted, whereby the concept of reciprocity (e.g. Fehr and Gächter, 2000) suggests this is reciprocated by citizens in trusting local government more. Our experiment is unique in presenting an RCT design to test this and provide strong evidence of a causal relation between the local government giving more trust and in turn being reciprocated.

Lastly, we showed that the above discusses result only manifests itself for the political level and then mainly the one most closely linked to the ‘more trusting’ policy, in our case municipal government. As such our study suggests that this linkage is an important condition that should be incorporated in the theoretical reasoning. Moreover, the fact that policy satisfaction is a relevant factor in the building of trust while the perceive effectiveness is not, is in line with the procedural justice theory that being treated fairly can be more important than the outcome itself (Lind & Taylor, 1988).

To put our results in the perspective of the debate on whether it is (rational choice, New Pubic Management based) ‘output’ or (more traditional) ‘process’ which drives satisfaction with government services (Christensen & Lægreid, 2005): for the group we studied, it all seems to be about process. Translating our findings to the benefits of trust for society, we can say that a social assistance scheme based more on trust and unconditionally might not affect social trust and (among others) the stability, cohesion and happiness associated with that, but it does increases local political trust, and can thus have a positive effect on issues like reducing tax evasion and effective implementation of public policy associated with that (Newton et al., 2018). In other words, our study lays bare specific policy feedback, underscoring that policies are political forces that shape the context in which the policies are made, a realization that has been claimed to be overlooked in the public administration and policy literature (e.g. Moynihan & Soss, 2014).

Considering the specific group we focused on, our results also add to the literature that indicate that social policy design can have a empowering political impact for economic vulnerable groups particularly. Earlier research, for instance on the U.S., has found relatively little political empowerment among these groups, while also suggesting that caring policy designs can feed into an new sense of political de-alienation among vulnerable citizens (see Michener, 2018), while controlling and policing policies decreases a feeling of equal citizenship leading to less trust in the political system (see Lerman & Weaver, 2014). Our study suggests this holds beyond that specific context and adds that particularly procedural justice is a crucial element of the causal chain that connect social policies to political empowerment. Moreover, citizens seem well aware in which political domain the design is rooted, here the municipality, to which they reciprocate their trust.

Our randomized design allows for relatively strong, internally valid, conclusions on the causal impact of the treatments. Moreover, our focus on social assistance recipients is unique in testing the impact of welfare states policies directly on those citizens who are subjected to it. However, the strengths of this design are mirrored by a more restricted external validity (e.g. Greenberg & Schroder, 2003) and we should be careful in generalizing the found policy effects to all people on social assistance, as well as to other types of welfare or to other countries. One thing to consider is that the Netherlands already has a relative large welfare state and is a relative high trust society – therefore, the possible gains are smaller than if similar policy as the ones tested in this experiment would be implemented in a more low trust country with a more modest welfare state. Also of relevance is that it might be that overall the social assistance treatments tested here actually lead to lower trust among other, non-recipient, people. Especially people who believe in notions as 'the undeserving poor' might lose trust when a welfare scheme is introduced with less government control and more personal autonomy. This is a matter of further research though, at this point, our experiment underlines the importance of policy satisfaction for the impact on trust and as such the experience-based logic on trust (e.g. Newton, 2009), and the population at large will have little direct experience with social assistance regardless of the exact rules and regulations. Regarding further research, more studies are required to confirm if our findings are valid in other contexts. Preferably future experiments have more participants (the size of our experiment was sufficient, but on the small side), and a longer duration – especially if you take our finding into account that a measurement just before the ending of an experiment like this is not useful for a topic like trust, when the treatment that is supposed to foster trust is about to be taken away again. Possibly future experiments can be held without voluntary participation, to avoid any possible Hawthorne-effects (people behaving different due to partaking in an experiment). On the other hand, very little is known at the moment on whether Hawthorn-effects occur differently in treatment groups compared to control – and if they do occur in equal measure, this removes possible bias (Greenberg & Schroder, 2003). Moreover, while self-selection due to voluntary participation can lead to selection bias, an earlier assessment of selection bias in this particular experiment showed that there were some differences between participants and non-participants on objective characteristics, but if existing at all these were modest (Betkó et al., 2019).

The outcomes of our experiment require some reflection on the crucial study of Kumlin & Rothstein (2003) – does our outcomes detract from their observations, that contacts with (non-selective, less bureaucratic) universal welfare institutions increases social trust? We think not, for several reasons. Our experiment was modest in scale: number of participants, duration, but mostly how far the experimental treatments deviated from the regular regime. The experimental treatments were less conditional and selective (and therefore less bureaucratic), but not truly universal welfare – let alone the universal basic income the discussion started with in Nijmegen. The fact that participants knew the experiment was limited in time (and thus the trust put in them was as well), will if anything have reduced the effects found on trust in this study. We therefore consider it entirely possible that the linkage we found for local trust can be found in future studies for social trust as well.

Given the advantages of social and political trust for the functions of society and politics (e.g. Newton, Stolle & Zmerli, 2018), this study's outcomes are relevant both academically and socially. Our results indicate that political trust, particularly trust in the municipal government, indeed can be reciprocal among social assistance recipients. Changing social policy to start from a notion of trust can thus influence the level of local trust of a difficult to reach part of the population, whose trust is hard to win. Hyggen (2006, 507) concluded on social trust: "From a policy point of view, one solution to develop trust, or at least not to break it down may thus be to restructure parts of the system of social assistance. (…) by developing the universality of the welfare system as opposite to making it even more discretionary." While more work on various trust-based policies is needed to draw more definite conclusions, our experiment does suggest that if one wants to give political trust a boost, it is also worthwhile to considering trust as a basis for welfare policies.

4

Influencing health through social assistance

A slightly different version of this chapter is at the moment of writing in a revise and resubmit procedure at an international peer reviewed journal in the domain of public health. See the section Contributions of authors for information on co-authors.

4.1 Introduction

It is a well-established fact that the way social welfare is organized strongly impacts health. In current research in several fields (including medicine, sociology, psychology and economy) it is an important object of study, and as an editorial of a public health journal stated: "*Evaluating social welfare policies from a public health perspective is essential if we are to avoid prolonging or worsening inequalities*." (The Lancet, 2020). For decades, social welfare experiments have been conducted, ranging from active labor market policy experiments (Kluve, 2010; Card et al., 2018; Greenberg & Schroder, 2003) to more basic income-like experiments (Greenberg & Schroder, 2003; Gibson et al., 2020). Most studies (especially those on active labor market polices) focus exclusively on labor market outcomes. Conner et al. therefore notice in their study *Randomised studies of income supplementation: a lost opportunity to assess health outcomes* that "Very few health outcome data were collected in these trials and even fewer reported" (Connor et al., 1999). Governments that implement stricter welfare systems sometimes explicitly do not want health outcomes evaluated, preferring to look *only* at labor market outcomes (Wickham et al., 2020). So while few studies include health as outcome, some do. A recent and relevant study in the light of our research, showed how a social welfare regime with increased conditionality (universal credit in the UK) influenced mental health negatively (Wickham et al., 2020). More generally, a recent scoping review in which basic income-related welfare experiments were assessed on health shows mixed, though mainly positive effects (Gibson et al., 2020). Positive results include birthweight, reduced infant obesity, improved nutrition and mental health, which are ascribed to reduced financial stress and the escape from an intrusive bureaucracy. On the negative side, experiments with this type of welfare sometimes show more substance abuse, indirectly causing other harm like accidents (Gibson et al., 2020).

Particularly, experiments with a more unconditional approach to welfare are relatively scarce. The earlier mentioned scoping review from 2020 counted 10 experiments with a Negative Income Tax, and 17 with unconditional cash transfers. The review "*highlights the paucity of evidence on universal basic income and the need for more extensive trials and modelling to understand its wider health, economic, and social implications."* (The Lancet, 2020). This study contributes to providing such additional evidence. We tested the effect of a (randomized) social assistance field experiment on health (namely: self-rated health, subjective well-being and mental health), and we assessed in which groups the experimental welfare regimes have most impact. We take a 'resource perspective' to assess the latter, with the proposition that people with more (personal, social and economic) resources gain stronger health benefits than those with less resources. Literature on stress and coping shows that having certain resources can dampen negative effects of stress (Thoits, 1995; McKee-Ryan et al., 2005). We argue that the health of people with more resources could also benefit more from the extra opportunities (given by a more unconditional or less strict social assistance regime). However, some studies suggest the contrary: stronger (positive) health effects for more disadvantaged groups (Gibson et al., 2020). So based on the current literature, there is ground to assume that those with more resources could benefit either more or less, and we provide an empirical study of these contradictory claims. We systematically analyze how participants' resources shape the expected health benefits from the experimental welfare treatments.

In our experiment we study the regular workfare based (Groot et al., 2019) Dutch social assistance regime compared to two alternative regimes (the treatments). These have in common they are less conditional, offer the opportunity to earn additional income (up to ~€200 per month) next to the social assistance benefits, and have fewer rules and regulations. Assumptions are that these treatments stimulate both intrinsic and extrinsic positive motivation, and lead to a more reciprocal relation with the government (Fehr & Gächter, 2000). With specific regard to health, the (potential) additional income could alleviate financial stress, while the reduced bureaucracy should lead to less administrative burden and therefore less psychological costs, and allow more mental bandwidth and less stress (Moynihan et al., 2015; Mullainathan & Shafir, 2013; Haushofer & Fehr, 2014). Based on the stress and coping literature we thus consider not having a job and living in poverty – which is applicable to most social assistance recipients - to be a chronic strain type of stressors, while the bureaucracy that social assistance recipients have to undergo is considered to be a 'daily hassle' type stressor (Thoits, 1995). The treatments are assumed to diminish these stressors, thereby yielding overall positive health effects. The differences between the treatments are detailed in the methods section under "study design". Though not a universal basic income experiment, the experimental treatments are inspired by it, and part of the academic discourse on the current wave of basic income experiments around the globe (Delsen, 2019; Groot et al., 2019; McFarland, 2017; De Wispelaere & Yemtsov, 2019).

To summarize, we primarily aim to contribute to the evidence on how the design of welfare policies (in particular social assistance) influences health – conceptualized in terms of self-rated health, subjective well-being and mental health. And secondarily, to determine whether specific groups that vary in their resources, benefit more or less from the tested treatments.

4.2 Data & methods

4.2.1 Study design, participants and randomization

We conducted a randomized field experiment in collaboration with the municipality of Nijmegen and under auspices of the national government. Core of the experiment is having two treatment groups experiencing different than normal social assistance regimes, and comparing these with a control group for whom the usual rules remain in effect.

The regular social assistance rules, as laid down in the nation-wide Participation Act, are based on a 'work first'-approach. Rules emphasize that people have to leave social assistance as soon as possible, have a strict set of obligations regarding re-integration in the labor market, and are sanctioned and fined if they do not comply with these rules. Recipients have to ask permission to do voluntary work (because time spend doing voluntary work cannot be spend looking for jobs), and if they earn extra income in addition to their allowance that amount of money is deducted from their allowance. In contrast, both experimental treatments are characterized by less government control and participants having more autonomy, fewer obligations, and being allowed to keep some income in addition to their allowance if they have a part-time or temporary job. The difference between the two experimental treatments is that for the 'exempted' group all obligations regarding re-integration are simply abolished, while the 'coached' group has an (obligatory) intensive but supportive coaching scheme which replaces regular obligations. In the coached group, participants can (contrary to regular recipients) set their own goals, like whether it is part-time, full-time or voluntary work, and self-employed or not. (For further details about both the regular regime as well as the treatments see Appendix 1).

Participation in the experiment was voluntary. Over a period of several months, participants were recruited for the experiment in a way to maximize participation and avoid selection bias where possible, taking into account that a significant part of the population has limited digital skills and reading ability. At the start of the study, the municipality provided 7,500-8,000 social assistance allowances, of which approximately 1,000 to couples (leading to approximately 8,000-8,500 social recipients). Approximately 6,000 recipients met the eligibility criteria (see Section 2.4.1 on recruitment). A little over 550 people applied (not all of whom actually met the eligibility criteria), from which 380 were evenly randomized over the treatment and control groups, after which some dropped out. In total, 339 social assistance recipients enrolled in the trial, who gave their consent (see Appendix 4 on research with human subjects and consent).

Most of the data were obtained from surveying the participants. Surveys were conducted before the start as a baseline (t=0), after approximately 1 year (t=1), and after 2 years (t=2). We combined these with data from the municipal registry, which include information on personal characteristics like country of birth, part-time work and household composition. For this study, we have data on 259 recipients from before the start and after the first year (76.4%), and 224 (66.1%) recipients from before the start and after two years). Figure 1 below shows a flow diagram of the study, see Appendix 3 for more information.

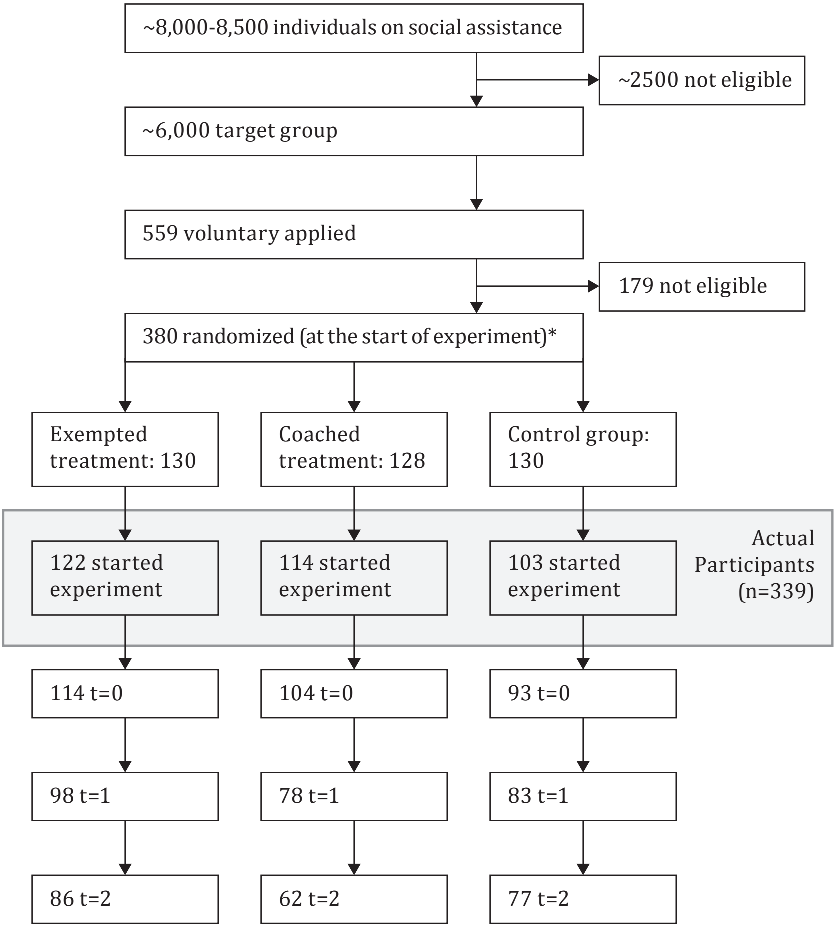

Figure 4.1 Study flow diagram for health outcomes.

**At a later point a few extra participants joined, who were denied participation by the municipality, who objected against that decision and won the appeal.*

4.2.2 Measurements

As outcome measures, we include three dimensions of health. First, *self-rated health* measured by a single item: "how would you rate your health", with answers on a 5-point scale ranging from "bad" to "excellent". This is generally considered to be a valid indicator for both mental and physical health, and valid for different (ethnic and socio-economic) groups (DeSalvo et al., 2006; Jylhä, 2009; Huijts, 2011). Second, *subjective well-being* is measured by combining two items - life satisfaction and the meaningfulness of one's life, both measured on a 0-10 scale - in a single index based on the arithmetic mean. Finally, *mental health* is measured using the five MHI-5 questions (Ware & Sherbourne, 1992; Driessen, 2011) in an additive index, ranging from 0-100. (See Appendix 9 for additional information on the indices/items used.)

The predictor variable is the treatment: we distinguish the exempted group, the coached group and the control group.

There are four types of resources we include as moderators: personal resources, social resources, economic resources, and a category in which these are conjoined. Self-confidence is the personal resource we study; it was measured with four items combined in a single index.

The number of social contacts and whether the participant is single or living with a partner indicate social resources. The latter is included as a dichotomy (1=partnered). For the former, participants were asked how often they had contact with 1) neighbors or people in your street, 2) friends and good acquaintances, and 3) relatives and family. Answers were combined in an index. Economic resources were operationalized in three ways. Education is measured in four categories (basic, lower secondary, higher secondary, and tertiary education). Having debts is included as a dichotomous variable (no debts/having debts – thereby assuming that the 9 people who answered 'don't know' will have debts). Finally, we included if somebody held a part-time job prior to the experiment as a dichotomy, representing recent labor market skills and professional contacts, with 'yes' meaning that any income was earned in 2017 before the start of the experiment (December 2017), irrespective of the amount. Finally, we take into account migration background as a category tapping into the likely absence of multiple resources. Natives are generally more resourceful in multiple aspects compared to migrants, also including unmeasured resources. We established whether a participant has no direct migration background (Dutch), people with a Western migration background and people with a non-Western migration background (given that in comparison people from a non-Western background are on average in a more disadvantageous position). This is operationalized through 'country of birth', so only first-generation migrants are distinguished from others.

Gender (male/female), age (in years), and having children (yes/no) are used as control variables.

The descriptive statistics are below in Table 4.1.

Table 4.1 Descriptive statistics

Descriptive statistics, N varies between 225 and 322*

Variable (N)	Min.	Max.	Mean / %	SD
Self-rated health t1-t0	-2.00	2.00	-0.02	0.83
Self-rated health t2-t0	-3.00	3.00	0.03	0.82
Self-rated health t2-t1	-2.00	3.00	0.06	0.73
Subjective well-being t1-t0	-5.00	5.50	0.26	1.42
Subjective well-being t2-t0	-7.50	5.00	0.21	1.44
Subjective well-being t2-t1	-5.00	3.00	-0.06	1.14
Mental health t1-t0	-50.00	65.00	2.84	16.61
Mental health t2-t0	-55.00	60.00	1.20	17.55
Mental health t2-t1	-60.00	45.00	-2.20	15.27
Index self-rated health t=0	1.00	5.00	3.33	0.88
Index self-rated health t=1	1.00	5.00	3.28	0.91
Index subjective well-being t=0	2.50	11.00	7.60	1.53
Index subjective well-being t=1	2.00	11.00	7.79	1.50
Index mental health t=0	5.00	100.00	61.05	19.32
Index mental health t=1	10.00	100.00	63.54	18.25
Group				
Exempted			37	
Coached			31	
Control			32	
Gender (ref=male)				
male			47	
female			53	
Age	27	64	45.06	9.98
Kids	0.00	1.00	0.33	0.47
Index self-confidence	0.50	4.00	2.49	0.61
Contacts	0.00	5.00	4.13	1.07
Couple	0.00	1.00	0.07	0.25
Education				
Lower education			11	
Lower middle education			12	
Higher middle education			41	
Tertiary education			36	
No debts	0.00	1.00	0.43	0.50
Additional income	0.00	1.00	0.40	0.49
Country of birth				
Dutch			73	
Non-Western			23	
Western			4	

Source: Surveys experiment Participation Act Nijmegen (October/November 2017, February/March 2018, November/December 2018, September/August 2019); Register data Nijmegen municipal administration (December 2017, April 2018).
*Descriptive statistics per model can be requested through the corresponding author.

4.2.3 Statistical Analysis

To estimate the effects of the treatments on health, we used ordinary least squares regression models. For the dependent variables, we calculated the difference between t=0 and t=1, between t=1 and t=2, and between t=0 and t=2. The changes in health are the outcomes. We add the health score at t=0 (or t=1 when modelling the difference between t=1 and t=2) to account for ceiling and floor effects. On the right-hand side, the models include which treatment the participants got (the control group being the reference category), and the control variables (age, kids and gender). This leads to the following model (example for the difference between t=0 and t=1):

$$\Delta Y_{t1}\text{-}Y_{t0} = \beta_0 + \beta_1 Y_{t0} + \beta_2(group) + \beta_N(control) + \varepsilon$$

Group differences are tested by adding the moderator variables and interaction terms between moderator variable and treatment variables. The moderator variables that we use are the scores as they were at t=0. These models do not include the control variables as the main models show they have little effect but do reduce power. This moderator model is specified as follows:

$$\Delta Y_{t1}\text{-}Y_{t0} = \beta_0 + \beta_1 Y_{t0} + \beta_2(group) + \beta_3(moderator) + \beta_4(moderator*group) + \varepsilon$$

To assess the potential moderation effects, we consider whether the interaction term is statistically different from 0 (indicating that the effect for that particular subgroup deviates from the effect in the reference group on de moderating variable) and, where relevant, whether the treatment effects for the different subgroups are statistically significant in themselves.

Given the relatively small sample sizes (and therefore low power), we ran our models with all cases for which we have data on the included time points, leading to more cases when we compare t=0 with t=1, than t=0 with t=2 (due to attrition). We consider $p<0.05$ to be statistically significant, and $p<0.10$ to indicate marginal significance.

4.3 Results

4.3.1 Average treatment effects

Table 4.2, 4.3 and 4.4 show the average treatment effects for self-rated health, subjective well-being, and mental health respectively. As the dependent variable shows the difference between a point in time and 1 or 2 years later, the treatments work if the group variable (either exempted or coached) has a positive and significant effect, meaning that the growth in health is stronger (or the decline in health is less severe) compared to the reference category (treatment as usual). First, as Table 1 shows, changes in self-rated health appear to be similar across both the treatment and 'care as usual' groups. Second, within the exempted group a substantive decline in subjective well-being is found after one year as compared to the control group, while there is no effect from the coached treatment in this timeframe. After the second year, this relative decline in subjective well-being for the exempted group is compensated due to a marginally significant positive effect in the second year, and thus proves to be temporary. A robustness check showed this is not due to drop-out bias.[92] Thirdly, for mental health we find no significant effects for the treatment groups, though the coached treatment approaches marginal statistical significance. Overall, the average effects are limited and clearly not consistently positive over two years, and there are differences between the effects of the two treatment groups.

Table 4.2 Changes in self-rated health in the Nijmegen experiment, regressions with t=0 and control variables, 1-5 scale.

	Change after 1 year		Change between year 1-2		Change after 2 years	
	B	P-value	B	P-value	B	P-value
Group						
Control	Ref	Ref	Ref	Ref	Ref	Ref
Exempted	0.10	0.37	-0.03	0.79	0.05	0.67
Coached	0.08	0.50	-0.07	0.52	0.08	0.53
T=0 self-rated health value	-0.42	<0.01	-0.36	<0.01	-0.46	<0.01
Gender						
Male	Ref	Ref	Ref	Ref	Ref	Ref
Female	0.18	0.08	0.00	0.98	0.10	0.37
Age	0.01	0.19	-0.00	0.85	0.00	0.82
Kids						
No	Ref	Ref	Ref	Ref	Ref	Ref
Yes	-0.03	0.82	0.12	0.24	0.14	0.20
Intercept	0.92	0.02	1.27	<0.01	1.33	<0.01
N	259		232		224	
R	0.44		0.47		0.51	

92 As a robustness check, we ran the model between t=0 and t=1 with the 224 participants on which analysis between t=0 and t=2 is ran, The outcome of this extra model gave virtually the same result as the model with 259 cases, (B=-0.47, p=0.05). If the initially found effect for the exempted treatment was driven by drop-out bias, the effect should be smaller in the robustness test; it was not.

Table 4.3 Changes in SWB in the Nijmegen experiment, regressions with t=0 and control variables, 0-10 scale.

	Change after 1 year		Change between year 1-2		Change after 2 years	
	B	P-value	B	P-value	B	P-value
Group						
Control	Ref	Ref	Ref	Ref	Ref	Ref
Exempted	-0.43	0.02	0.32	0.06	-0.01	0.98
Coached	-0.04	0.82	0.23	0.21	0.20	0.35
T=0 SWB value	-0.86	<0.01	-0.24	<0.01	-0.45	<0.01
Gender						
Male	Ref	Ref	Ref	Ref	Ref	Ref
Female	-0.05	0.78	-0.10	0.50	-0.14	0.44
Age	-0.01	0.30	0.00	0.82	0.00	0.79
Kids						
No	Ref	Ref	Ref	Ref	Ref	Ref
Yes	0.36	0.04	-0.03	0.83	0.31	0.50
Intercept	4.37	<0.01	1.67	<0.01	3.46	<0.01
N	259		232		224	
R	0.56		0.35		0.47	

4.3.2 Moderation and treatment effects for specific groups

Several of the interaction effects we included in our analysis show statistically significant relationships, indicating that the effects of the treatments differ between societal groups. We mainly find such effects regarding self-rated health, which we present in Table 4.5, 4.6 and 4.7 below. These show the results of seven models, each with a different moderator variable (or: interaction term). The results for the other two outcomes variables - subjective well-being and mental health - can be found in Appendix 10; for these outcomes we found none to hardly any significantly different effects between groups.

Regarding self-rated health, interaction terms with the number of contacts, level of education and part-time work show no effect for any treatment group or time period. As a robustness check, we also ran the model with an item measuring whether participants felt part of society, as an alternative measurement of social resources focusing more on the subjective dimensions than the objective number of contacts. This did not show a significant interaction effect either. On social contacts as well as level of education and part-time work, we thus conclude that these indicators of resources have no moderation effects on self-rated health.

Table 4.4 Changes in MH in the Nijmegen experiment, regressions with t=0 and control variables, 0-100 scale.

	Change after 1 year		Change between year 1-2		Change after 2 years	
	B	P-value	B	P-value	B	P-value
Group						
Control	Ref	Ref	Ref	Ref	Ref	Ref
Exempted	-1.19	0.59	2.34	0.27	2.30	0.34
Coached	0.71	0.60	2.67	0.25	3.64	0.16
T=0 MH value	-0.45	<0.01	-0.36	<0.01	-0.47	<0.01
Gender						
Male	Ref	Ref	Ref	Ref	Ref	Ref
Female	-2.10	0.30	-2.71	0.17	-2.63	0.24
Age	0.06	0.52	-0.06	0.57	0.03	0.77
Kids						
No	Ref	Ref	Ref	Ref	Ref	Ref
Yes	3.16	0.13	-3.05	0.14	-3.07	0.19
Intercept	27.49	<0.01	24.40	<0.01	29.09	<0.01
N	259		232		224	
R	0.50		0.46		0.52	

On the contrary, we do find effects for indicators of other resources. Our analysis shows that participants with more self-confidence benefit less from both treatments in terms of self-rated health (B=-0.45/-0.38; p<0.05) after one year, and a significant positive treatment effect is found for people with less self-confidence in both treatments (i.e. the main effects only hold for participants with less self-confidence: B=1.64, p=0.02; B=1.40, p=0.03 respectively). An additional model[93] shows that this treatment effect stays significant for the approximately 30% of participants with the lowest self-confidence. This effect is significant by the conventional threshold (p<0.05) for exempted group, and marginally significant (p<0.10) for the coached group. The effect disappear in the second year of the treatment, to which we will return to in the conclusion.

Turning to being partnered, as indicator of social resources, we found the following differences. Being exempted from obligations resulted in a stronger increase in self-rated health among participants being part of a couple for the first year (interaction coefficient 0.74; p<0.10), compared to single participants. Additional analyses, in which the reference category was changed, show that the

93 All additional analyses can be obtained from János Betkó.

positive treatment effect for partnered participants (0.77) is marginally significant (p=0.07). The effect remains positive between t=1 and t=2 and between t=0 and t=2, but is no longer significant – the effect size after 2 years is similar to that after 1 year, but has a p-value of 0.12, implying that the loss of statistical power suppressed statistical significance.

Regarding the economic resource of not having debts, we found a stronger effect of both treatments on self-rated health compared to care as usual among participants without debts. This effect is marginally significant for the exempted group and significant for the coached group. Further analyses show that the main effect for participants without debts is (marginally) significant in the exempted and in the coached group (B=0.30, p=0.08, and B=0.38, p=0.04 respectively). On the other hand, for people with debts no significant effect of the treatments is found. In the second year, we find no effect for the coached group, while the exempted group shows a comparable effect after 1 year (B=0.31), but which is no longer (marginally) significant for this smaller sample. Again, statistical power seems to be an issue after two years.

Last, country of birth is shown to play an important role in determining the effect of the exempted treatment. After one year, participants with a non-Dutch Western background are shown to benefit significantly more in terms of self-rated health. When we take both non-Dutch groups in an additional analysis as one combined reference group, we find that both in the exempted and the coached treatment non-Dutch have a marginally significant stronger treatment effect (respectively B = 0.40, p = 0.09;B = 0.38, p = 0.09). In the second year of the treatment, we see a significant increase in the self-rated health of participants with a non-Western background, and (consequently) after two years, both Western and non-Western non-Dutch participants show (marginally) significant higher self-rated health. An additional analysis, in which we contrast all participants with a migration background to those without, shows a significant positive treatment effect for participants with a migration background (B = 0.58, p < 0.01). All coefficients for the interaction term with the coached group are positive, after 1 year, after 2, and between 1 and 2, but not statistically significant. Altogether these analyses on migration background point in the same direction: people with a migration background benefit more from the alternative social assistance regimes.

Concluding, the exempted social assistance regime seems to have a relatively positive impact on self-rated health for people who are part of a couple, do not have debts, are of non-Dutch origin, and have low self-confidence, and the coached treatment has a relatively positive impact for those without debts, of a non-Dutch origin, and have low self-confidence.

Table 4.5 Self-rated health including moderations between t=0 and t=1.

Activities	Model 1	Model 2	Model 3
Group			
Control	Ref	Ref	Ref
Exempted	1.19 (0.02)**	0.39 (0.39)	0.03 (0.83)
Coached	1.02 (0.03)**	0.57 (0.27)	0.05 (0.67)
Baseline value activities	-0.44(0.00)**	-0.43(0.00)**	-0.41(0.00)**
Self-confidence	0.14 (0.31)		
**exempted*	-0.45 (0.02)*		
**coached*	-0.38 (0.04)**		
Contacts		-0.41 (0.63)	
**exempted*		-0.07 (0.49)	
**coached*		-0.12 (0.32)	
Couple			-0.49 (0.10)~
**exempted*			0.74 (0.09)~
**coached*			-0.65 (0.29)
Education			
**exempted*			
**coached*			
No debts			
**exempted*			
**coached*			
Part-time work			
**exempted*			
**coached*			
Migration Background (ref=NL)			
Western			
Non-western			
*Western*exemp*			
*Western*coach*			
*Non-west*exemp*			
*Non-west*coach*			
Intercept	1.04**	1.53**	1.31**
R	0.46	0.45	0.45

N=259, except model X (part-time work), there N=258
*~ p<0.10, * p<0.05, **p<0.01*

Model 4	Model 5	Model 6	Model 7
Ref	Ref	Ref	Ref
0.48 (0.20)	-0.09 (0.54)	0.08 (0.61)	-0.02 (0.91)
0.29 (0.50)	-0.17 (0.28)	0.08 (0.62)	-0.05 (0.72)
-0.41(0.00)**	-0.40(0.00)**	-0.41(0.00)**	-0.39(0.00)**
-0.03 (0.77)			
-0.14 (0.25)			
-0.07 (0.58)			
	-0.28 (0.10)~		
	0.39 (0.09)~		
	0.55 (0.02)**		
		-0.03 (0.85)	
		0.01 (0.97)	
		-0.04 (0.88)	
			Ref
			-0.42 (0.44)
			-0.26 (0.20)
			1.32 (0.05)*
			0.69 (0.33)
			0.23 (0.43)
			0.41 (0.15)
1.35**	1.39**	1.29**	1.30**
0.45	0.45	0.43	0.46

Table 4.6 Self-rated health including moderations between t=1 and t=2.

Activities	Model 1	Model 2	Model 3
Group			
Control	Ref	Ref	Ref
Exempted	-0.36 (0.58)	-0.33 (0.45)	-0.05 (0.67)
Coached	-0.05 (0.94)	-0.54 (0.28)	-0.08 (0.47)
Baseline value activities	-0.37(0.00)**	-0.38(0.00)**	-0.36(0.00)**
Self-confidence	-0.04 (0.73)		
**exempted*	0.09 (0.61)		
**coached*	0.00 (0.99)		
Contacts		-0.12 (0.12)	
**exempted*		0.07 (0.48)	
**coached*		0.12 (0.30)	
Couple			-0.10 (0.71)
**exempted*			0.19 (0.61)
**coached*			0.30 (0.53)
Education			
**exempted*			
**coached*			
No debts			
**exempted*			
**coached*			
Part-time work			
**exempted*			
**coached*			
Migration Background (ref=NL)			
Western			
Non-Western			
*Western*exemp*			
*Western*coach*			
*Non-west*exemp*			
*Non-west*coach*			
Intercept	1.38**	1.81**	1.27**
R	0.47	0.48	0.46

For the analyses between t=1 and t=2, the N is as follows.
Self-confidence: 224, Contacts: 224, Couple: 232, Education: 224, No debts: 232, Part-time work: 231, Country: 232.
*~ p<0.10, * p<0.05, **p<0.01*

Model 4	Model 5	Model 6	Model 7
Ref	Ref	Ref	Ref
-0.24 (0.49)	-0.07 (0.60)	-0.01 (0.93)	-0.14 (0.20)
-0.44 (0.28)	0.12 (0.44)	-0.14 (0.33)	-0.03 (0.79)
-0.36(0.00)**	-0.36(0.00)**	-0.37(0.00)**	-0.36(0.00)**
-0.01 (0.91)			
0.07 (0.52)			
0.13 (0.32)			
	0.06 (0.66)		
	0.11 (0.60)		
	0.39 (0.07)~		
		-0.22 (0.13)	
		-0.06 (0.78)	
		0.18 (0.41)	
			Ref
			-0.73 (0.11)
			-0.15 (0.39)
			0.25 (0.66)
			0.32 (0.59)
			0.60 (0.02)
			-0.09 (0.74)
1.28**	1.24**	1.40**	1.30**
0.47	0.48	0.48	0.51

Table 4.7 Self-rated health including moderations between t=0 and t=2.

Activities	Model 1	Model 2	Model 3
Group			
Control	Ref	Ref	Ref
Exempted	0.48 (0.36)	0.06 (0.90)	-0.02 (0.86)
Coached	0.53 (0.29)	-0.09 (0.87)	0.05 (0.72)
Baseline value activities	-0.37(0.00)**	-0.47(0.00)**	-0.45(0.00)**
Self-confidence	0.13 (0.34)		
**exempted*	-0.18 (0.38)		
**coached*	-0.18 (0.35)		
Contacts		-0.09 (0.29)	
**exempted*		-0.01 (0.94)	
**coached*		0.04 (0.76)	
Couple			-0.40 (0.17)
**exempted*			0.65 (0.12)
**coached*			0.19 (0.75)
Education			
**exempted*			
**coached*			
No debts			
**exempted*			
**coached*			
Part-time work			
**exempted*			
**coached*			
Migration Background (ref=NL)			
Western			
Non-Western			
*Western*exemp*			
*Western*coach*			
*Non-west*exemp*			
*Non-west*coach*			
Intercept	1.38**	1.93**	1.52**
R	0.47	0.51	0.51

For the analyses between t=0 and t=2, the N is as follows.
Self-confidence: 224, Contacts: 224, Couple: 224, Education: 224, No debts: 224, Part-time work: 223, Country: 224
*~ p<0.10, * p<0.05, **p<0.01*

Model 4	Model 5	Model 6	Model 7
Ref	Ref	Ref	Ref
0.05 (0.90)	-0.10 (0.51)	0.03 (0.84)	-0.14 (0.29)
-0.13 (0.77)	0.07 (0.69)	0.02 (0.92)	0.02 (0.89)
-0.45(0.00)**	-0.46(0.00)**	-0.46(0.00)**	-0.45(0.00)**
-0.03 (0.72)			
-0.01 (0.96)			
0.07 (0.64)			
	-0.06 (0.70)		
	0.31 (0.18)		
	0.01 (0.97)		
		-0.20 (0.24)	
		-0.02 (0.95)	
		0.13 (0.61)	
			Ref
			-0.99 (0.05)~
			-0.29 (0.14)
			1.16 (0.07)~
			0.76 (0.26)
			0.70 (0.01)*
			0.20 (0.50)
1.58**	1.52**	1.60**	1.57**
0.50	0.51	0.51	0.53

For subjective well-being and mental health, we hardly find treatment effects for subgroups. The main finding is that higher educated participants benefit less from the exempted treatment (marginally significant) regarding subjective well-being, after 2 years. For mental health, we find no moderating effects at all.

4.4 Conclusion and discussion

Aim of this study was to test in an experiment if less-conditional and more trust-based welfare leads to positive health outcomes, and if results differ for specific subgroups varying in resources. We found several important results after testing the impact of the treatment on three different health outcomes, being self-rated health, subjective well-being and mental health, after 1 and 2 years.

For subjective well-being we found a negative average effect for the exempted group after 1 year, that disappeared after 2 years of treatment. Furthermore, it seems that higher educated participants benefit less from the exempted treatment (after two years).

For mental health, we did not find any unambiguous effects, for either the entire group or for subgroups. Nevertheless, the positive effect for the coached group after 2 years approaches marginal significance. Though not conclusive, a positive effect from a less conditional regime does corresponds with earlier findings that *more* conditionality has a negative effect on mental health.

Self-rated health did not improve across the experimental treatment groups compared to the control group. We did find several moderation effects indicating the treatments did work as intended for certain subgroups. In the exempted group, people who are part of a couple, without debts, with a migration background, and low self-confidence were better off after one year (as compared to similar subgroups in the control group). In the coached group people without debts and with low self-confidence were better off (as compared to similar subgroups in the control group). Some of these effects fade out after two years, but this is likely due to the lower statistical power. Additionally, surveys showed that participants started to worry about returning to the regular regime towards the end of the experiment, which could have increased stress toward the end of t=2 (and thus reduced effectiveness).

Using surveys provides detailed information on subjects that cannot be found in administrative data (like indications of (mental) health, the number of contacts one has, or self-confidence). The trade off, however, is that surveys fall short in following all participants who started the experiment (cf. ITT-analysis) and introduce the risk of drop-out and randomization bias due to the people quitting the experiment or deciding not to partake between randomization and start of the experiment (Heckman et al., 2000; Smith, 2000). Regarding drop-out bias, our additional analysis on the effects of the treatment on subjective well-being (see footnote 92) suggest that drop-out in our study is not selective. Regarding randomization bias: as the flow diagram in Figure 4.1 shows, there was more drop-out from the coached and the control group than the exemption group. However, additional analysis shows that on key demographic indicators (age, gender, social assistance spell, migration background), the three groups are comparable. Therefore, we are reasonably confident that our experiment presents internally valid results. External validity is always an issue with social experiments, especially when voluntary participation is required (Shadish et al., 2002; Greenberg & Schroder, 2004). From earlier research on these experiments, we know that there was some difference between participants and non-participants (Betkó et al., 2019). However, given the target group and what was at stake in the experiments, we consider voluntary participation the ethical option here.

On the organizational side, it is important to note that this experiment faced clear treatment restrictions from the national government. The municipality and the researchers would have preferred to deviate more from the current social assistance laws: to have higher additional income for working participants, to have a longer experimentation period, and to have not only a relaxation of rules regarding re-integration, but also on other (demanding) aspects of social assistance rules (described in Appendix 1). For this the Ministry of Social Affairs denied permission. Given that the deviation from 'treatment as usual' was fairly small, it is expected that the effects are rather small too. In other words, finding an effect with these treatments is a strong indication an effect of such treatment exists.

To conclude, whereas earlier studies suggest that people in a disadvantageous position would benefit more from regimes that are less conditional and more trusting (Gibson et al., 2020), and the coping literature suggests that people with resources could benefit more (Thoits, 1995; McKee-Ryan et al., 2005), reality is more complex. Neither having resources or being in disadvantageous position seems key – while it is striking that several marginalized groups, like non-Dutch and those with little education, are worse of in the regular regime. Our results suggest the impact of trusting and supportive social assistance regimes on health is context dependent. We show that resources matter for 'who benefits', particularly regarding self-rated health, but the effects for each resource should be studied and theorized on its own merit. Given our design, we cannot give definite answers on *why* different resources have different effects (Heckman et al., 2000); however, we suspect that part of the explanation lies in the broader context of (in our case: Dutch) welfare policies. For example, when people with high debts earn additional income, this additional income goes directly to the debtors. This means that participating in the treatments, which would usually allow the option to keep some additional income which in turn could alleviate (financial) stress, works less for the debtor's subgroup. So, while this study provided evidence on for *whom* more trusting and supporting welfare is beneficial from a health perspective, further research is needed to establish *why* some groups benefit more than others. As for the effect of the coached treatment on mental health after two years, more research is needed. Statistically we cannot confidently claim that there is an effect – but it is close enough that it deserves more study, in experiments with a longer duration and which deviate more from 'treatment as usual' than this study did. Finally, we interpret the temporary negative effect on subjective well-being for people in the exempted group as a potential warning for policy makers who want to experiment with more unconditional welfare. A major difference between our two experimental treatments was that the 'exempted' were left to their own devices, while the 'coached' got guidance. A relative vulnerable group like people on social assistance might require some help when they are given more autonomy, and the responsibility that comes with it.

5

Fewer obligations for welfare recipients, more social and economic activities?

A slightly different version of this chapter is at the moment of writing under review at an international peer reviewed journal. See the section Contributions of authors for information on co-authors.

5.1 Introduction

The current paradigm in social welfare policy in many Western countries, including the Netherlands, is ‘workfare’ and ‘work-first’. This includes strict conditionality for social services: people on welfare, including social assistance, are expected to get any job as soon as possible, moving to something (potentially) better from there (Delsen, 2016; Groot, Muffels & Verlaat, 2019; Arts, 2020). This line of thought is grounded in the idea that people are rational actors, and thus when given the chance to receive ‘money for nothing’, recipients will feel an incentive to stay on social assistance. The result of this paradigm is a welfare system with a relative high amount of obligations, restrictions and fines for those who do not comply. The underlying idea is that when welfare recipients are not forced to be active, they will remain passive (Edzes et. al, 2021).

This workfare paradigm also dominates the Dutch social assistance law, the Participation Act (Groot, Muffels & Verlaat, 2019). Because of this, both before and after the Participation Act came into effect in January 2015, it drew criticism from policy makers and experts, expressing concerns about the emphasis on conditionality for, lack of autonomy of and distrust towards welfare recipients, the strict regime of control and sanctions, the required bureaucracy to enforce this (e.g. Tinnemans, 2014; Westerveld, 2015; Vliegenthart, 2016). Many critics considered the traditional workfare paradigm with the accompanying ‘stick and carrot’ approach –the stick growing bigger and bigger– particularly ill-fitting for vulnerable groups in society, with numerous social problems such as having a low education, mental health problems, and a prolonged distance from the labor market; characteristics that fit the group of social-assistance recipients (e.g. Delsen, 2021; Delsen, 2016; Einerhand & Ravesteijn, 2017).

Also in academic literature, questions are raised about the wisdom and effectiveness of this paradigm: can and should a social assistance recipient be considered a rational choice actor as described above? To what extent would an approach grounded more in behavioral sciences, acknowledging the concept of bounded rationality work better (e.g. WRR, 2017; Kampen et. al, 2020; Edzes et. al 2021)?

Somewhat paradoxically, due to the Participation Act’s stringent focus on finding (any) job for the unemployed (like people on social assistance usually are), it discourages several activities that could function as a bridge to the labor market. Schooling is a well-known contributor to human capital that increases one’s chances to find a job, and setting up a self-employed business to make a living is, if successful, of course a direct way to end unemployment and, if the income is high enough, social assistance dependency. Under the Participation Act though, recipients are required to get explicit permission from the municipality before they are allowed such activities. Volunteering is another activity which is sometimes considered to improve employability, though this is contested (Paine et. al, 2013). Regardless if it is effective, it serves a wider purpose of doing good for a collective and it stimulates social integration (Wilson, 2000). Nevertheless, volunteering is limited in the Participation Act: official permission is required as well.

Additionally and interestingly, while the social-assistance regime pinpointed finding any job as soon as possible as the core goal, in the same period the Dutch government increasingly issued an appeal on citizens, including those receiving social assistance, to become more active in society under the flag of 'self-reliance' and contributing to the 'participation society'. For instance, the government scaled back formal care, in favor of informal care: people needing care should rely in the first place on their own network (Bredewold, 2018). Similarly, volunteering is stimulated by both local and national governments, because both the volunteer as well as society as a whole are supposed to benefit (Movisie, 2014).

So while there are separate and at points even contradictory ideas of 'workfare' and the 'participation society' in the welfare system, and while restrictions in the Participation Act might cut off activities that could lead to work in the longer run, empirical knowledge lacks on how the strictness or conditionality of the social-assistance regime influences the behavior of recipients: to what extent do they, indeed, perform less social and economic activities other than work?

This study addresses this knowledge gap, by offering causal empirical evidence from a social experiment in which the regular workfare based social-assistance regime is used as a control group for two alternative, less conditional treatments. Besides including control and treatment groups to which participants were randomly assigned, the experiment contains a pre- and post-measurement, allowing for statements on the causal impact of these less conditional treatments. This type of real world experiments is rare, and as far as they have been conducted in the past, the focus is usually on economic outcomes, particularly 're-integration in the labor market' (Greenberg & Schroder, 2004). To the best of our knowledge, no experiment of this kind has been conducted in which social and economic activities other than work (from here on 'social and economic activities', thus not mentioning 'other than work' anymore) are studied as the outcome: volunteering, informal care, schooling and setting up a business as self-employed.

Our social experiment allows us to answer the following research question (Q1): *To what extent do social assistance policies which, compared to the regular regime rely more on unconditionality and autonomy for recipients and less on control from the government, induce changes on time spent by recipients on activities beneficial for society and/or further employment?* Additionally, little is known about *for whom*, that is, for which subgroups, different types of social assistance work and why. With qualitative data which were gathered among participants and officials involved in this experiment, we first generate theoretical insights on this issue, and then empirically test them to answer our second research question (Q2): *Do the treatments have different effects on time spent on social and economic activities for different subgroups of participants, and if so, for which groups are the effects stronger?*

Our study contributes to the existing knowledge in at least three ways. First, we provide robust evidence from a randomized social experiment that allows for causal inference, showing the effects of less conditional welfare on hours spent by welfare recipients on several social and economic activities. Second, we generate theoretical insights based on qualitative data about who benefits most from a more unconditional welfare approach. And third, we put these newly generated theoretical insights to the test and provide quantitative evidence on which groups are more effected by the treatments.

5.2 Context, theory and general proposition

5.2.1 Context: the Dutch Social Assistance system and Participation Act

The regular Dutch social-assistance regime, under the Participation Act, is based on a workfare / work first approach, as we addressed briefly in the introduction (Delsen, 2016; Groot, Muffels & Verlaat, 2019; Muffels & Gielens, 2019). Recipients face many obligations enclosed in the Participation Act, that can be broadly divided into two categories. The first is related to the necessity to prove that recipients are at all times eligible for social assistance, which obliges them, among others, to communicate changes in income to the government, receiving gifts of any kind (so that their benefit will be reduced by the value of the gift), and changing living situations.

The second set of obligations regards re-integration to work. Social-assistance recipients are expected, among others, to actively search for employment, to prove they do so (which could be specified in e.g. a minimum number of job applications per week), to accept any job they are offered, to move away or commute up to one and a half hour for a job, and to dress appropriately. Specifically regarding activities that we focus on in this contribution: recipients usually need to ask for permission for any activity which could interfere with job-searching efforts – including schooling and volunteering. Critics of the Participation Act argued that recipients of social assistance are helped better with an alternative system, based less on conditionality and more on giving recipients trust and autonomy (e.g. Vliegenthart, 2016). This would give social-assistance recipients the opportunity to spend time on other social and economic activities, and as such make stronger and more long-lasting efforts towards social and economic re-integration. In this respect, some critics even called for an approach more like a universal basic income as an alternative for the Participation Act (e.g. Ranshuijsen & Westerveld, 2015).

5.2.2 Theoretical background and general proposition

This criticism on the Participation Act, and more broadly on the workfare paradigm, has a theoretical foundation grounded in behavioral research, which goes beyond welfare-focused studies. Insights from this behavioral branch of science are highly relevant for questions on the functioning of welfare regimes. For instance, studies on mental bandwidth and stress showed that financial and psychological stress from obligations and insecurity can impair people's executive functions and long-term perspective, thus diminishing their decision making ability (Mullainathan & Shafir, 2013; Haushofer & Fehr, 2014; WRR, 2017). Furthermore, it is known that when people are confronted with administrative hurdles and obligations, these experienced burdens can lead to stress and reduced mental bandwidth – affecting those with lower financial resources more strongly (Moynihan, Herd & Harvey, 2014; Mullainathan & Shafir, 2013).

These general findings from psychological and economic behavioral research have strong implications when related to the functioning of the Dutch social assistance system: they imply less than optimal results towards persistent integration, exactly due to these negative by-effects of administrative burdens and (too many) obligations. Many social-assistance recipients live below the poverty line, thus having little financial resources and experiencing financial stress (Delsen, 2016; Goderis, 2020). Moreover, empirical findings show the 'stick and carrot' regime to cause considerable experienced administrative and psychological burden for recipients, due to its obligations and restrictions regarding re-integration (Arts, 2020; Eleveld, 2020). Experienced stress and reduced mental bandwidth negatively influence will power and self-control, causing risk aversion and the tendency to be less perceptive towards long term routes towards persistent re-integration. Presumably, this creates a downward spiral, in which it is getting harder and harder for social-assistance recipients to take the effective steps –which might ask for openness towards alternative routes, a longer term effort, and stepping away from the work-first paradigm– to eventually find a job and escape unemployment and poverty permanently (e.g. Mullainathan & Shafir, 2013; Haushofer & Fehr, 2014; Moynihan, Herd & Harvey, 2014).

All this leads to a general proposition, that an alternative social-assistance regime which reduces administrative burdens as well as psychological stress due to poverty, and which thus creates more mental bandwidth and increases executive functions like will power and self-control, would possibly enable social-assistance recipients to make more effective choices – a logic which has been laid down in more detail in Groot et al. (2019; see also Edzes et al., 2021 and Betkó et al., 2019). While the general proposition is that alternative social-assistance regimes could improve a wide array of outcomes for social-assistance recipients on a number of outcomes, we focus in this study on the time spent on several social and economic activities, which are of importance for both individuals and their communities and moreover to society at large, and which are likely be influenced by the treatments, as laid out in the following section.

5.2.3 The experiment's general proposition

The criticism on the Participation Act and the general proposition that a more unconditional social assistance system, based on trust and autonomy, could be a general improvement compared to a workfare approach, led to a number of similar randomized social experiments in Dutch municipalities - sometimes seen as part of the wave of experiments over the globe with welfare based on or inspired by the universal basic income (e.g. Delsen 2020; Groot et al. 2019). In these Dutch real-life experiments, the Participation Act regime functions as control group and is compared with alternative, less conditional treatments that give recipients more autonomy (Sanders et. al., 2020; Groot, Muffels & Verlaat, 2019; Betkó et al., 2019; Edzes et al. 2021). In the experiment in Nijmegen, which took place from December 2017 to January 2020, two alternative treatments were compared with the treatment as usual under the Participation Act. Participation was voluntary and open for most people on social assistance (see Chapter 2, Section 2.4.1 for more information on recruitment and eligibility). All participants gave their consent to participate in the experiment and could end their participation at any moment. In Appendix 4 more can be found about the ethical considerations about studying human subjects in an experiment, as well as the informed consent signed by the participants. The people who applied to participate were, after screening if they indeed were eligible, randomly divided over three groups, one of which was the control group.

The first treatment group (hereafter: the exempted group) was given an exemption from all obligations regarding re-integration and associated fines. Thus, their administrative burden was reduced potentially increasing mental bandwidth, and they were given trust and autonomy by the municipal government. They were allowed to contact the municipal social service or the regional re-integration office for assistance if they wanted to, but were under no obligation to do so.

The second treatment group (hereafter: the coached group) had all re-integration obligations as mentioned in the previous paragraph replaced with intensive (monthly) group coaching. Participation in this coaching was the single obligation participants had, reducing their administrative burden and potentially increasing bandwidth. They had full autonomy on whether they wanted to look for a job, part-time job, start as a self-employed, or participate in volunteer work and where and how they were going to do this.

Additionally, participants in both groups were allowed to keep a little bit of extra income compared to treatment as usual, if they would find temporary or part-time work, giving them an opportunity to reduce stress due to financial difficulty, if any existed. In Appendix 1 more details can be found on both treatment as usual and the two experimental groups.

Having described the broad theoretical framework and the treatments, we can see how the treatments are connected to the specific activities that we study as an outcome in this paper: volunteering, informal care, schooling and setting up as self-employed. In one way, this is very direct: since participants in the experimental treatments are no longer required to spend an amount of time looking for jobs, they can spend this time on other activities, including the four we study here. Furthermore, for all these activities except for informal care, permission is required under the regular rules, and permission is not given if the re-integration office thinks such an activity might hinder job application efforts. Additionally, both treatments have the possibility to alleviate financial stress with a small job in addition to the allowance, and are less conditional and demanding than the regular regime, having no or fewer obligations regarding reintegration and no threatening fines in case of non-compliance – something which obviously has profound impact on people living on a minimum income. This should decrease the psychological burden (Moynihan, Herd & Harvey, 2014) and increase mental bandwidth (Mullainathan & Shafir, 2013), enabling people to spend their time and energy on more long term planning benefiting themselves and/or society, be it either through volunteering, informal care, schooling or setting up a self-employed business.

All this leads to the hypothesis H1: *participants in both treatment groups will spend more hours on social and economic activities compared to the control group.*

There could be a difference between the effects of the exempted and the coached group. When we look at the differences between the exempted and the coached treatments, the former mainly creates more mental bandwidth and opportunity, for both volunteering, informal care, schooling and setting up a business as self-employed. The coached group is also given opportunity and also tailor made support, for volunteering, schooling and setting up as self-employed – but is not focused on supporting somebody who gives informal care. For the exempted group, all obligations regarding re-integration are removed, while in the coached group they are replaced with a single obligation, i.e., participating in the group coaching, which is much less intrusive than the regular regime, but which is an obligation nevertheless. Based on this, we are insecure to argue which one is expected to perform best compared to the other, and therefore will not hypothesize about this.

5.3 Qualitative exploration: who might benefit more?

Besides presenting a unique test of how social-assistance regimes influence the time recipients spent on social and economic activities, we set out to generate and test new theoretical insights on which subgroups these alternative regimes might affect most. Reasoning that it is unlikely that the treatments affect all social-assistance recipients in the same way, we explored if there are subgroups, based on personal characteristics, that benefit more from the treatments. People on social assistance are a very specific (and vulnerable) group, who are generally not envisioned when formulating theories on social and economic participation, let alone that it is theorized how different subgroups of these people respond to welfare policy.

However, before and during the experiment, we collected qualitative data among participants and officials. This allows us to generate theoretical insights 'bottom up', from what people themselves say on why they did or did not spend time on volunteering, schooling, informal care or setting up as self-employed, and from what officials involved in the experiment saw happening to participants regarding these activities. In the following section of this paper we therefore, first, present our analyses of the qualitative data sources for clues on which subgroups might be affected more by the treatments.

5.3.1 Sampling & analytical strategy

Our qualitative sources are broadly divided into two categories. One source consists of interviews with professionals who had contact with a large number of (potential) participants. The focus of these interview was to collect general information on the implementation, success and failures of the experiment. These sources, including how many people were interviewed and at what time during the experiment, are described in Appendix 2, where also an example of an interview guide can be found. The second category of qualitative data is derived directly from the participants: each survey had an 'open question' at the end, where participants could mention anything of interest to them. About two-third of the participants made use of that option each of the surveys, where responses varied between a single line remark up to hundreds of words.

To analyze these data, we used hybrid iterative coding (e.g. Vennix, 2011). We used closed coding, in the sense that we studied the data looking specifically for mentions of sociodemographic subgroups as well as participation activities as operationalized in this paper: volunteering, informal care, schooling and setting up as a self-employed. And we used open coding, in the sense that we did not know beforehand which socio-demographic subgroups would turn up in the data.

We studied all the sources in detail, systematically reading the texts line by line, coding all text segments about activities included in our dependent variables, and then read the texts once more coding all parts about how different subgroups related to the experiment (giving each such subgroup a code of its own). We did not only code the literal words (e.g. 'self-employed') but also any synonyms and descriptions of the concepts we were looking for (e.g. 'entrepreneur', 'start working for myself'). This resulted in a selection of the data that dealt with both our outcomes of interest, as well as specific socio-demographic subgroups. After in-depth reading this selection once more, we distilled new theoretical mechanisms and formulated hypotheses. The results of our analyses and hypothesis are discussed in the following section.

5.3.2 Generating new hypotheses

Of the potential moderators, age and education came up frequently. Recruiter 3 mentioned in her interview that the reasons why people participated differed by age and education: *"… the somewhat higher educated people, they were not mainly in for the* [opportunity to earn extra] *money. More like '…, we have the opportunity for some self-development, and more opportunities in general'. I also saw that a lot with the older people…".*[94] Also the contact person from the municipal social service stated that in general most people participated for the freedom from restrictions, but specifically the younger group was interested in working part-time to earn additional income in addition to the social assistance allowance.

Similarly, in the first focus group with interviewers, who conducted the face-to-face interviews with participants, it was said that *"being above a certain age … was mentioned a lot"* by participants as a reason for not being able to find work. This was repeated in the open questions in wave 1 and 2, and one participant explicitly argued this was a reason to look for volunteer work. These indications are relevant as they suggest that higher educated and older people are strongly interested in the opportunities for self-development indicating a stronger intrinsic drive to perform social and economic activities (regardless of whether it makes them money). Furthermore, older people, who have a hard time finding a job due to their age, might be looking for alternatives for work for that reason. This leads to the following exploratory hypotheses:

H2a: When in treatment, older participants will show stronger increases in doing social and economic activities than younger, compared to participants in the control group.

H2b: When in treatment, higher educated participants will show stronger increases in doing social and economic activities than lower educated, compared to participants in the control group.

In the focus groups with the interviewers, the spotlight was set on two other subgroups of the participants. In the first place, people with a migration background were mentioned multiple times. For instance, it was mentioned that

94 Translation from Dutch to English by authors.

"... often with people with a migration background, it was indeed due to this migration background" [not being able to find a job] - *"I had a respondent who was 14 years in the Netherlands, who spoke fluently Dutch... but still. And a girl with a headscarf. She spoke Dutch very well. And that was not the problem." "[interviewer] Did they experience discrimination when looking for a job?" "Yes, yes.".*

In the second focus group (after one year) another problem was mentioned for people not being born in the Netherlands: foreign degrees are not always recognized in the Netherlands, making it more difficult to find a job. If people with a migration background have a harder time to find a job due to discrimination on the labor market and unrecognized certificates, it makes sense to expect them to turn their efforts to other societal and economic activities, like volunteering or self-employment, or schooling to obtain a recognized degree. The open questions confirmed this line of thought, and provided individual cases of people with a migration background being specifically interested in setting up as self-employment and investing time in schooling. We thus hypothesize:

H2c: When in treatment, participants not born in the Netherlands will show stronger increases in doing social and economic activities than those born in the Netherlands, compared to participants in the control group.

Also people having problems with their mental health came up in the focus group, who got the opportunity in the less conditional and restrictive regimes to focus on other activities: *"...I had a conversation with a woman who ended up on social assistance, because she had mild burn-out symptoms, she experienced a lot of pressure, and was now part of the exempted treatment. She experienced a lot more rest without all the obligations, started doing things she enjoyed, more design, and she noticed that because she had this freedom she got orders with which she could make money...".*

Although the above is a specific mental health example, a number participants confirm the idea that people with a bad health in general (including physical health) spend less time looking for regular work (fulltime or part-time), sometimes explicitly mentioning spending their time on other activities which we study instead: *"I have chronic pain and that impairs me enormously when looking for a job, and led to social isolation. I am thinking about starting as a self-employed."* This leads to the following hypothesis:

H2d: When in treatment, participants with a weaker health will show stronger increases in doing social and economic activities than those with a better health, compared to participants in the control group.

Finally, in the open questions one other subgroup appeared that was not mentioned in the interviews with professionals: people with children. In both surveys a number of people mentioned that taking care of their (young) child(ren) was a reason they could not be (fully) employed, several mentioning that as the reason they had other activities, as the ones we study in this paper: *"I study and I take care of my children, so I have no time to apply for jobs"*. This leads to the following hypothesis:

H2e: When in treatment, participants who are a parent will show stronger increases in doing social and economic activities than those without children, compared to participants in the control group.

5.4 Quantitative design: data and methods

Having supplemented our general hypothesis with hypotheses about which socio-demographic groups could benefit most from the experimental treatments, we turn to the quantitative analysis. Data on the outcomes during the experiment were collected via a panel-design survey, with a baseline survey before the experiment started and one after approximately 2 years - a few months before the end of the experiment. The surveys were conducted as personal, computer assisted interviews (CAPI). The number of cases included in this study is 163 (see the study flow diagram below in Figure 5.1 and the additional information in Appendix 3). The survey data were supplemented by data from the municipal administration of Nijmegen for information about personal characteristics, like age and parenthood.

The dependent variable in our models is the difference score in social and economic activities between the baseline and the end survey, based on the number of hours per week a participants spent on volunteering, informal care, schooling or setting up a company. We add all these hours together in a combined variable for two reasons. In the first place because we are primarily interested in the answer to the question whether a more unconditional approach activates people into spending more time on social and economic activities - more than in which specific activity this is. And secondly, because these activities are up to a point a zero sum game: for instance, one cannot (or is highly unlikely) to spend 25 hours per week on volunteering, while providing 20 hours of informal care and 20 hours on schooling as well. In the survey, participants were asked for each of these activities how much hours per week they spent on them. These hours recorded for these separate items were summed to create a variable considering the number of 'hours spent per week on social and economic activities'. As this variable was not normally distributed, we recoded the hours into 5 categories, running from -2 to 2. This variable does show an approximate normal distribution. The recoded variable was as follows: a difference between baseline and end survey of -11 or fewer hours per week spent on these activities was labelled '-2'; between the -10.5 and -0.5 was labeled '-1'; 0 was labeled '0'; +0.5 and +10.5 was labeled '1'; and +11 and higher was labeled '2'.

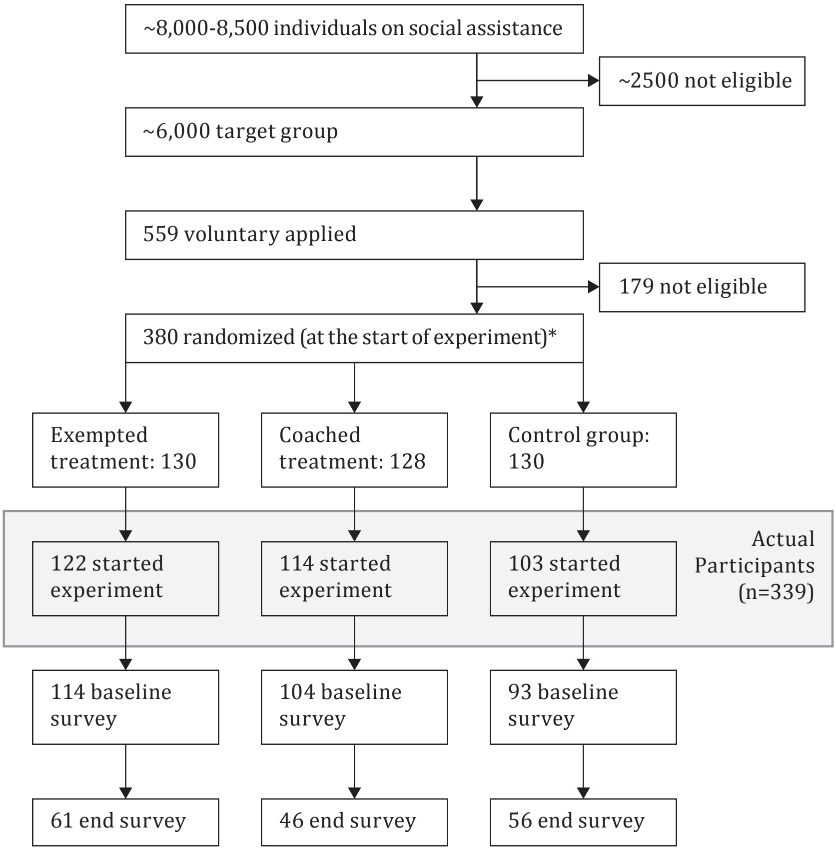

Figure 5.1 Study flow diagram for social and economic activities.

**At a later point a few extra participants joined, who were denied participation by the municipality, who objected against that decision and won the appeal.*

We add the score from the baseline survey as a control variable to the model to capture for ceiling effects. After all, participants already spending a lot of hours per week on social and economic activities are less likely to increase that number. For this control variable, we capped the number of hours spent at 40: numbers higher than that are both less realistic, and would get a disproportional weight in the statistical analysis.

The core predictor variable is the group a participant was in, either ‘exempted’, ‘coached’ or ‘control’.

Table 5.1 Descriptive statistics.

Variable	Min.	Max.	Mean / %	SD
Social and economic activities end - baseline	-2,00	2,00	0,04	1,17
Social and economic activities baseline (hours)	0,00	40,00	7,30	10,73
Group				
Exempted			37.4	
Coached			28.2	
Control			34.4	
Age				
Younger than 50			62.6	
50+			37.4	
Education				
Lower education			9,8	
Lower middle education			12,3	
Higher middle education			35,6	
Tertiary education			42,3	
Country of birth				
Dutch			75	
Non Dutch			25	
Self-rated health	1	5	3,27	0,94
Mental Health Index (0-100)	15,00	100,00	62,33	18,14
Kids				
Yes			37	
No			63	

Source: Surveys experiment Participation Act Nijmegen (October/November 2017, February/March 2018, November/December 2018, September/August 2019); Register data Nijmegen municipal administration (December 2017, April 2018).
N = 163

Following the outcomes of the qualitative section, we test for moderating effects of age, education, country of birth, health, and single parenthood. These are operationalized as follows.

Age is measured in two categories: younger than 50 and 50 and older (50+ being a common category used in studies on older age and work, e.g. CBS, 2006). Education is divided in four categories: basic, lower secondary, higher secondary and tertiary. Country of birth is measured as 'Netherlands' or 'other'. Parenthood is measured binary (yes/no).

Health is measured in two complementary ways. The first is self-rated health, which is known to be a good measure for both mental and physical health, and which is valid for different socio-economic groups (e.g. DeSalvo et. al., 2006; Jylhä, 2009; Huijts, 2011). Participants were asked to rate their own health on a 1-5 scale, where 1 is 'bad' and 5 is 'excellent'. The second is mental health. For this we used the five questions of the Mental Health Inventory-5 (e.g. Ware & Sherbourne, 1992). It contains the following five items: "the following questions are about how you felt the last four weeks. Give the answer for each question that best resembles how you felt. Were you very nervous? Were you so down that nothing could cheer you up? Were you calm and relaxed? Were you sad and dejected? Were you happy" These questions were ranked on a 5-point scale from "never" to "always". We recoded the items in a 0-100 index in such a way that a low score means the most negative feeling, following the way the Dutch Bureau of Statistics works with this indicator (Driessen, 2011).

Given that we used a robust experimental design where people with different characteristics are randomized over the different treatment groups, and we added the DV-score from the baseline survey, we do not add additional control variables.

5.5 Results

The results of the average effect of the treatments on changes in social and economic activities are presented below in Table 5.2.

Table 5.2 Changes in social and economic activities in the Nijmegen experiment, regressions with baseline score as control variable.

	Change after 2 years	
	B	**P-value**
Group		
Control	Ref	
Exempted	0.27	0.21
Coached	0.36	0.11
Baseline self-rated health value	-0.03	<0.01
Intercept	0.08	0.60

N = 163; R = 0.31; scale between -2 and 2.

Both the coached and the exempted treatment show effects in the expected direction, but they cannot be distinguished statistically from 0, refuting hypothesis H1. Given the modest sample size in this experiment, we can conclude no large and highly consistent effect is present. Still the positive effects might indicate that the amount of time recipients spent on social and economic activities might be modestly influenced or only among some groups, which will not be picked up in the analyses presented in Table 5.2.[95]

When we zoom in on the specific subgroups derived from the qualitative analysis, we do indeed find that for some subgroups the time spent on these

Table 5.3 Social and economic activities in the Nijmegen experiment (moderations).

Activities	**Model 1**	**Model 2**	**Model 3**
Group			
Control	Ref	Ref	Ref
Exempted	0.08 (0.81)	0.65 (0.36)	0.74 (0.09)*
Coached	0.57 (0.13)	-0.19 (0.82)	0.95 (0.03)**
Baseline value activities	-0.03 (0.00)	-0.03 (0.00)	-0.03 (0.00)
Age (ref=50+)	0.18 (0.54)		
**exempted*	0.40 (0.34)		
**coached*	-0.34 (0.47)		
Education		0.04 (0.83)	
**exempted*		-0.13 (0.56)	
**coached*		0.17 (0.49)	
Country (ref=migrant)			0.36 (0.31)
**exempted*			-0.61 (0.11)
**coached*			-0.81 (0.11)
Self-reported health			
**exempted*			
**coached*			
Mental health			
**exempted*			
**coached*			
Having Children (ref=no)			
**exempted*			
**coached*			
Intercept	-0.03	-0.04	-0.20
R	0.32	0.33	0.34

95 Adding work and part-time work changes effect size only marginally; the treatment effects found here are thus separate from and additionally to any treatment effect on work. Adding them to the moderation models does not lead to different conclusions either.

activities increases, as is shown in Table 5.3 below. In all cases where we find statistically significant effects, the treatment effect of the experiment is positive in terms of the over time development in hours spent on social and economic activities.

Model 4	Model 5	Model 6
Ref	Ref	Ref
0.47 (0.58)	1.70 (0.02)**	0.25 (0.34)
0.97 (0.23)	0.52 (0.51)	0.23 (0.44)
-0.03 (0.00)	-0.03 (0.00)	-0.03 (0.00)
-0.05 (0.76)		
-0.06 (0.79)		
-0.19 (0.40)		
	0.00 (0.84)	
	-0.02 (0.06)*	
	-0.00 (0.82)	
		-0.16 (0.62)
		0.05 (0.91)
		0.32 (0.49)
0.27	-0.04	0.14
0.34	0.37	0.32

The first subgroup for which we find a more positive development in time spent on social and economic activities are people not born in the Netherlands. Table 5.4 shows a positive effect for participants in both the exempted (0.74) and coached (0.95) treatment group, though in the exempted group the effect is only marginally significant. These are the treatment effects for those group members that are labeled “0” in the dummy variable ‘country of birth: Dutch’ - which are those participants with a migrant background. The interaction terms show the treatment effect of the people not having a migration background: -0.61 for the exempted and -0.81 for the coached, both having a p-value of 0.11. This shows that almost the entire average treatment effects found are due to people with a migration background. As the treatment effect for Dutch-born is not significant, and for migrants it is with both interaction terms, we consider this an indication that people with a migration background, when exempted from re-integration obligations or given supportive coaching instead, spent more time on social and economic activities. Consequently we do not reject Hypothesis H2c and cautiously consider it supported.

We also find a positive treatment effect for people in the exempted group who have a relatively low mental health. The main effect (1.70) is significant for this exempted group, meaning that this is the effect for people who score a 0 on the variable ‘mental health index’. The interaction term is -0.02 meaning that for every point of mental health increase, the main effect gets 0.02 lower. This suggest that people with a relative low mental health, when exempted entirely from re-integration obligations, spent more time on social and economic activities, and that this effect gets smaller when people have a better mental health. Given the significant main effect and marginally significant effect of the interaction term for the exempted group, we consider hypothesis 2d partly supported. We only find a significant effect when testing for mental health and not for self-rated health, and we find the effect only for one of the two treatments. We furthermore find that age, educational level, and having kids do not seem to affect hours spent on social and economic activities, refuting hypotheses H2a, H2b, and H2e.

5.6 Conclusion and discussion

Contributing answers to the broader question whether people on social assistance will become active when they are given trust and autonomy – which goes against the traditional workfare paradigm – we asked: is there an effect of a less conditional social-assistance regime on time spent by recipients on social and economic activities? Moreover, we wanted to know whether effects differ across recipients’ subgroups of people.

To answer these questions, we compared the time social-assistance recipients spent on social and economic activities in three social-assistance regimes in a randomized controlled social experiment. Next to the Dutch default social-assistance regime, two alternative, less-conditional treatments were devised, based more on autonomy and trust, less on conditionality. In the 'exempted group', people were exempted from all obligations regarding re-integration. In the 'coached group', all obligations regarding re-integration were replaced by coaching towards self-chosen goals. For both groups, administrative burden was lower compared to the control group. Additionally, using qualitative data gathered during the experiment, we generate new insights on subgroup effects of less conditional and less burdensome social assistance, which were tested quantitatively using the experiment data.

5.6.1 Core findings

Our analysis showed that in both treatment groups the amount of time spent on social and economic activities increased more, but these average treatment effects were not statistically significant, although for the coached group it was close. Our qualitative analysis indicated that less-conditional and less-burdensome social assistance might work better for participants who are older, higher educated, have a migration background, have relatively bad (mental) health, or have children. Of these, for two subgroups the alternative treatments also showed the expected stronger increase in hours spent on social and economic activities in the quantitative data: people not born in the Netherlands (exempted and coached treatment) and people with a relative low mental health (exempted treatment).

The qualitative analysis helps to understand the found effects. People not born in the Netherlands are mentioned to face discrimination on the labor market (see e.g. Van den Berg et al., 2017) and often their certificates are not accepted. This pushes non-native people in the direction of other social and economic activities instead of trying to find a job. In the default Dutch social-assistance regime, recipients are discouraged to take up such activities as this is said to interfere with finding a job quickly. Both alternative treatments, however, do allow and support this focus and given their labor market hardship, participants not born in the Netherlands are more likely to spent more hours on such social end economics activities. This reasoning aligns with and expands on administrative burden theory, stating that such burdens most strongly affect those with little human capital (Moynihan, Herd & Harvey, 2014), such as people with a migrant background.

The qualitative data indicated that people with a relative bad physical and mental health can be stimulated by an alternative, less conditional regime to spend more time on social and economic activities. For these people, a regular job is close to unobtainable, but other social and economic activities are a way to escape social isolation, which was allowed in the alternative treatment, while the regular regime only increases stress. The statistical analyses only show the difference in effects for mental health and only among those in the exempted group (versus the control group). The latter aligns with the stress mechanism particularly as only the exempted group's treatment is really unconditional regarding re-integration: all obligations are scrapped. In the coached treatment, there still is a conditional part albeit with more autonomy. It seems that people who suffer from a lower mental health benefit most from a truly unconditional regime; obligations, no matter how soft, cause additional mental pressure reducing their available bandwidth, which impairs them becoming active, as argued by theories on mental bandwidth (Mullainathan & Shafir, 2013).

We did not find evidence that age, education or having children influence the treatments effects. Given the indications from the qualitative study and our modest sample size in the experiment, we do recommend further research on these issues.

5.6.2 Limitations

The results, discussed above, should be considered in the context of our study. As already mentioned, the most obvious limitation is the relatively small sample size of 163 participants, which makes smaller effects less likely to be statistically significant. Given the average treatment effects results, we consider it quite possible that a modest average treatment effect does exist. Moreover, the results strongly indicate that the alternative treatments are not harmful compared to the control group.

Also, the treatments consisted of two components due the political and practical context is which the social experiment was designed. Next to fewer obligations, the treatments offered the opportunity to keep a little extra income from labor, in addition to the allowance. This could have stimulated people to take a small part-time job, thus artificially *downward-biasing* our results.

More generally, selective participation can be an issue in social experiments, especially when participation is voluntary, which affects the generalization towards the wider population (Greenberg & Schroder, 2003). Earlier analysis of the data we used showed some differences between those who applied for the experiment and the population on social assistance indeed: recipients in more advantageous positions (including those who already had a part-time job, who were higher educated or had no migration background) were more likely to join the experiment, while qualitative evidence showed recipients experiencing considerable stress participated less (Betkó et al., 2019). While we cannot determine the exact impact hereof, it could actually lead to underestimating the impact on social and economic activities (not work) as the treatments particularly benefit those with low mental health and because the more advantageous are more likely to find work, rather than increasing other social and economic activities. This might raise the question whether fewer obligations actually crowded out the transition to work. Controlled for (full-time and part-time) work in the robustness checks does not support that reasoning. It seems more likely that people, who are normally obliged to spend time on job applications that are unlikely to succeed, can spend their time now on more useful activities in the alternative treatments.

Similarly, drop-out bias (Heckman et al., 2000) might occur when a selective group leaves the experiment. However, robustness checks showed that it is highly unlikely that we found false positives due to drop-out bias.[96] Altogether and if anything, this study's limitations seems to suggest that effects' underestimation is more likely than finding false positives.

5.6.3 Implications

In terms of further research and policy, we have shown that a less conditional welfare regime, based on trust and autonomy, can stimulate some groups of social-assistance recipients to spend more time on volunteering, informal care, setting up their own business and schooling/training. Accordingly, local and national governments could consider to give social-assistance recipients with mental health problems 'a break'. Obligations and (threats of) fines might hinder their mental health recovery and thus be counterproductive for social and economic reintegration. Additionally, these governments could look into the question why the current social-assistance regime works less well for people with a migration background – and into the labor market discrimination which drives people to other activities than paid work, which came up clearly in our qualitative material.

Several of the activities - particularly schooling and setting up a business, and arguably also volunteering (Paine et. al, 2013) - might function as steps towards work (and thus reduce governments' welfare costs in the long run) by creating social and human capital. Moreover, volunteering and informal care have value in themselves, for instance increasing health (e.g. Piliavin & Siegl, 2007) and social integration (e.g. Wilson, 2000). In other words, it is in the (financial) interest of governments to stimulate these economic and social activities and more research should be directed to the impact of social assistance regime on these activities, while the literature is predominantly focused on direct labor market reintegration. This study could be springboard for this.

96 If the results found would be completely due to drop-out bias, the effects found after two years should be absent among those who left the treatment after one year in that first year, but present in that first year among the people for who did not leave the treatment. Rerunning our analyses for the subgroups and treatments showing significant results after two years, but then after 1 year, only including those that participated in the two full years, does not yield significant results however. In other words, the absence of significant effects after one year and the presence of them after two indicates our results after two years are unlikely to be (completely) due to drop out bias.

In this light, our study could be replicated in other welfare contexts. For instance, it is striking that some relatively vulnerable groups fare relatively poorly in the normal regime and benefit more from the alternative treatments. While this overlaps with results from basic income-like experiments (i.e. another form of more unconditional welfare) regarding health benefits (Gibson et. al., 2020), it still has to be established how broad this pattern holds and to what extent it varies with the context. It deserves more investigation whether current welfare paradigms often work less well for disadvantaged groups, as we found.

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Of course, any errors and misjudgments in this thesis are on the expense of the author, and on the author alone.

Contributions of authors

Chapter 2, 3, 4 and 5 consist of co-authored papers, for which János Betkó is first author, and to which Niels Spierings, Maurice Gesthuizen and Peer Scheepers contributed as well. The contributions are as follows. János, Niels and Maurice designed the experiment. János led the data collection. János, Niels, Maurice and Peer designed the statistical analyses. János analyzed the data, interpreted the results, and wrote the manuscripts. Niels, Maurice and Peer provided feedback on the interpretation of the outcomes and made contributions to and revisions of the manuscripts.

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Summary

The origin of this study is a social experiment with social assistance, held between 2017 and 2020. The municipality of Nijmegen (The Netherlands), dissatisfied with the strict policy of the Participation Act which regulates social assistance, organized the experiment. In the experiment, the regular social assistance policy was compared with two alternatives, both which were more unconditional and based on a trusting approach which allowed participants more autonomy. In both alternative treatments, participants were allowed to earn more income from work than normal, in addition to their allowance, which created the opportunity to improve their financial situation. Additionally, both alternatives had different rules regarding reintegration. One treatment (hereafter: the exempted group) received full exemption from the reintegration obligations (and all associated fines for any non-compliance). The other treatment (hereafter: the coached group) received intensive coaching towards a self-chosen goal, which replaced the regular reintegration obligations.

When it comes to experiments with social assistance and reintegration, the first (or sometimes: only) focus is often on obtaining employment, which ends social assistance. While important, other outcomes are equally so interesting and socially relevant, but the impact of the social assistance policies or welfare regimes on them is far less often the subject of academic study. Therefore, the focus of this study was on several of those other outcomes. Additionally, the treatment effects on subgroups are analyzed, for example based on migration background or education, and the functioning of mechanisms is studied (why does a certain effect take place?). On both these subjects there are gaps in the current available literature.

These gaps are address in the four studies in this dissertation. The first of these is methodological in nature. It examines which participants (based on personal characteristics, such as age and migration background) chose to participate in the experiment. The second study zoomed in on the effects of the experiment on political and social trust, and the mechanisms responsible for any increase or decrease. In the third study, the object of study was ‘health’, and it was examined whether there were different effects for subpopulations, based on available resources. The fourth and final study looked at the effect of the experimental treatments on social participation (such as voluntary work and informal care), and again looked at effects on subpopulations (such as migration background and education).

The first study showed that there were indeed groups that were over- and under-represented in the experiment. The participants were compared with non-participants on nine characteristics, divided into three categories: individual, household, and benefit situation. There turned out to be an over-representation of participants without a migration background, with a relatively higher level of education, without an official exemption from compulsory re-integration, singles and people who had temporary or part-time work in the year before the experiment started. There appeared to be no statistical significant difference based on age, gender, cost sharing situation or benefit duration. Qualitative analysis showed that recipients who experienced a lot of stress were also less likely to participate. These results were taken into account when interpreting the results of the following studies.

The second study analyzed impact of the experimental treatments on social and political trust, and the latter for both national and local politics. In the exempted group as well as the coached group, there is no increase in social trust or political trust in the national government after one year. Both groups do show an increase in political trust at the local level. It was analyzed whether there is an indirect effect, via 1) the way in which participants assessed the policy, 2) increased social participation or 3) psychological and mental well-being. There appears no evidence for this, with one exception: the rise in local political trust in the exempted group is partly due to the way they assessed policy. This can be broken down into the extent to which they considered the policy effective, and the extent to which they were satisfied with it (if it was perceived as fitting, limiting or annoying). The supposed effectiveness was not of influence, while policy satisfaction was. The crux therefore seems to be whether participants feel they have been treated fairly and decently. Theoretically, it is plausible that the rest of the increase in local political trust can at least partly be explained by reciprocity: the government gives trust in the experiment, and then gets it back from recipients. However, this could not be tested empirically.

The third study is about the effects of the experiment on the health of the participants. Three dimensions of health are distinguished: self-rated health, mental health and subjective well-being, and effects are tested for several subgroups of participants. The only effect that can be determined with certainty at the treatment level is a temporary dip in the well-being of participants in the exempted group after one year, which then disappears again after two years. When looking at health effects for subgroups, a perspective was used that considers what resources people have: personal (self-confidence), social (living together, social contacts) and economic (education, debt, part-time work) resources; and one other characteristic in which several of the aspects above play a role (migration background). Here we find a number of effects, all but one of which have to do with self-rated health. In the exempted group, there was a positive effect after one year for people without debts, who live together, have a migration background, or relatively low self-confidence, with the effect on people with a migration background remaining significant after two years. In the coached group, there was a positive effect after one year for people with no debts or relatively low self-confidence. Another effect was found for well-being: in the exempted group, the higher educated did relatively worse after two years. Taking this all together, it seems to be the more vulnerable groups that are over all better off in the alternative, experimental treatments - especially since people in debt often cannot benefit from the extra income component of these treatments, which likely also diminished their benefit from the treatment.

The last study is about the extent to which people participate in non-work social-economic activities. This concerns providing informal care, doing voluntary work, setting up as a self-employed or following training or education. All these activities have been combined into one variable for this study, which was measured after two years. The first question is whether the extent to which these activities are done depends on the treatment. The answer to that is that no significant effect can be found for either of the experimental groups. In a qualitative analysis, it was then examined whether there is reason to assume that subgroups of participants experience more or less effect from the treatment, and if so, which groups these are. It appeared that a greater effect might be expected for the elderly, the higher educated, people with a migration background, people with relatively poor health, and parents of children still living at home. Finally, this was tested quantitatively: it showed that two groups indeed benefited relatively more from the alternative treatments: people with poor mental health (in the exempted group), and people with a migration background (in both groups). This fits the pattern that more vulnerable groups are helped better by the experimental treatments, and have less benefit from the regular regime.

The general conclusion is that overall, there are limited general effects at the treatment level that are statistical significant. Specifically, we have found a rise in political trust at the local level in both treatments, and a temporary dip in subjective well-being for the exempted group. The rise in trust in the exempted group can be partly explained by the increase of satisfaction with the policy – it was considered more fair and less burdensome. Looking at effects for subgroups, we see that the treatments are effective for a number of subgroups, predominantly more vulnerable groups. This is especially striking when these results are compared with the results of the first study, on who participated. The groups that benefit most from the alternative forms of assistance, are also the groups that are under-represented in the experiment. In other words: it is reasonable to assume that if the alternative treatments were rolled out over the entire social assistance population, the effects would be larger and more positive, simply because the entire population contains more people from these vulnerable groups. The results of this study point to some interesting angles of possible new research. It seems sensible to look in other contexts of social policy, like in the other municipalities in which a similar experiment took place, if current policies seem detrimental for the more vulnerable groups. And where this is the case, to study the reason for this. Furthermore, new experiments with social assistance offer an opportunity to advance our knowledge, especially when new experiments are improved with the lessons learned of the previous wave (of which the Nijmegen experiment was part). Among these lessons are: aim for a experiments with a longer duration, which differ more than the past wave from the default policy, which have more participants, among others though having the same experiment in all participating municipalities – so we can measure effects more accurately. Furthermore, the results could be reason for some considerations among policymakers. The question whether the current policy is not detrimental for the most vulnerable citizens is something which asks attention from them, as well as from researchers. This is also the case for the effects found at the treatment level. The results of the, more generous, and more trust-based treatments, are for the outcomes studied here over the entire line about the same as they are for the default policy. This means that 'effectivity' is no argument to keep the current policy as it is – especially because the policy of the Participation act is relative strict and is considered harsh by many involved.

Nederlandstalige samenvatting

De basis voor deze studie is een sociaal experiment met de bijstand, dat is gehouden tussen 2017 en 2020. De gemeente Nijmegen, uit onvrede met het strenge beleid van de Participatiewet, organiseerde een experiment waarbij het reguliere bijstandsbeleid werd vergeleken met twee ruimhartigere alternatieven, die meer op onvoorwaardelijkheid waren gestoeld, en waarbij deelnemers meer vertrouwen kregen en autonomie hadden. In beide alternatieve behandelingen ('treatments') mochten deelnemers meer geld bijverdienen naast hun uitkering dan normaal, wat de mogelijkheid gaf voor meer financiële armslag. Daarnaast hadden beide alternatieven een andere regels rondom re-integratie, en de begeleiding daarbij. Eén, de ontheffingsgroep, kreeg volledige ontheffing van de re-integratieplicht (en alle daarbij behorende boetes voor eventueel niet-naleven). De ander, de gecoachte groep, kreeg intensieve begeleiding richting een zelfgekozen doel, die de reguliere re-integratieplicht verving.

Als het gaat om experimenten met de bijstand en re-integratie, wordt vaak in eerste instantie (of: alleen maar) gekeken naar het effect op de uitstroom uit de uitkering naar werk. Hoewel belangrijk, zijn andere uitkomsten dat minstens evenzeer, maar de impact van sociaal beleid daarop is veel minder vaak het onderwerp van wetenschappelijk onderzoek. De interesse in dit proefschrift ging uit naar enkele van deze andere uitkomsten. Ook is er bijzondere aandacht voor effecten op subgroepen, bijvoorbeeld op basis van migratieachtergrond of opleidingsniveau, en de werking van enkele mechanismen (waarom vindt een bepaald effect plaats?). Over deze onderwerpen zijn er nog lacunes in de bekende literatuur, waarbij dit proefschrift bijdraagt aan het opvullen daarvan.

De studie bestaat uit vier deelstudies, waarbij de eerste methodologisch van aard is. Hierin is gekeken wat voor bijstandsgerechtigden (op basis van persoonskenmerken, zoals leeftijd en migratieachtergrond) er voor kozen om deel te nemen aan het experiment. In de tweede studie werd ingezoomd op de effecten van het experiment op politiek en sociaal vertrouwen, en welke mechanismen verantwoordelijk zijn voor een eventuele stijging of daling. In de derde studie was het onderwerp van onderzoek 'gezondheid', en is gekeken of er andere effecten waren op deelpopulaties, op basis van beschikbare hulpbronnen. In de vierde en laatste studie werd gekeken naar het effect van het experimentele beleid op maatschappelijke participatie (zoals vrijwilligerswerk en mantelzorg), en werd wederom gekeken naar effecten op deelpopulaties (zoals migratieachtergrond en opleidingsniveau).

Het eerste deelonderzoek wees uit dat er inderdaad groepen waren die over- en ondervertegenwoordigd waren. De deelnemers zijn vergeleken met niet-deelnemers op negen kenmerken, verdeeld over drie categorieën: individueel, huishouden, en uitkeringssituatie. Uit de analyse bleek dat er een oververtegenwoordiging was van deelnemers zonder migratieachtergrond, met een relatief hogere opleiding, zonder ontheffing van de arbeidsplicht, singles en van mensen die tijdelijk werk of deeltijdwerk hadden in het jaar voor het experiment begon. Er bleek statistisch geen significant verschil op basis van leeftijd, geslacht, kostendeelsituatie of uitkeringsduur. Een kwalitatieve analyse toonde tevens aan dat ook mensen die veel stress ervaarden ook minder geneigd waren om mee te doen.

Het tweede deelonderzoek analyseerde het effect van het experiment op sociaal en politiek vertrouwen, en dat laatste zowel in de landelijke als de lokale politiek. Voor zowel de ontheffingsgroep als de gecoachte groep is er na één jaar geen stijging van sociaal vertrouwen of politiek vertrouwen in de landelijke overheid. Bij beide groepen is wel een stijging waarneembaar in het vertrouwen in de lokale politiek. Hiervoor is geanalyseerd of er sprake is van een indirect effect, via 1) de manier waarop mensen het beleid beoordelen 2) toegenomen sociale participatie of 3) psychisch en mentaal welbevinden. Hiervoor is geen bewijs, op één uitzondering na: een deel van de stijging van lokaal politiek vertrouwen in de ontheffingsgroep is te herleiden tot de manier waarop deelnemers het beleid beoordelen. Dit kan worden opgesplitst in de mate waarin ze het beleid effectief achten, en de mate waarin ze er tevreden over zijn (of men de regels ervaart als passend, beperkend of irritant). De veronderstelde effectiviteit heeft geen invloed, dat laatste wel: crux lijkt dus te zijn of mensen zich eerlijk en fatsoenlijk behandeld voelen. Op basis van de theorie ligt het in de rede dat het effect verder ten dele verklaard kan worden op basis van wederkerigheid: de overheid geeft (meer) vertrouwen in het experiment, en krijgt dit vervolgens terug. Dit kon echter niet empirisch getest worden.

Het derde deelonderzoek gaat over de effecten van het experiment op de gezondheid van de deelnemers. Er worden drie dimensies van gezondheid onderscheiden: hoe mensen hun eigen gezondheid beoordelen, mentale gezondheid en welbevinden. Het enige effect dat met zekerheid is vast te stellen op treatment-niveau, is een tijdelijke dip in het welbevinden van deelnemers aan de ontheffingsgroep na één jaar, die vervolgens weer verdwijnt na twee jaar. Bij het kijken naar effecten op gezondheid voor subgroepen, is een perspectief gebruikt dat in ogenschouw neemt welke hulpbronnen mensen hebben: persoonlijke (zelfvertrouwen), sociale (samenwonen, sociale contacten) en economische (opleiding, schulden, deeltijdwerk) hulpbronnen; en één overige eigenschap waarbij meerdere van bovenstaande aspecten meespelen (migratie-achtergrond). Hierbij vinden we een aantal effecten, die op één na allen te maken hebben met de eigen waardering van de gezondheid. In de ontheffingsgroep was er een positief effect na één jaar voor mensen zonder schulden, die samenwonen, een migratieachtergrond hebben, of relatief weinig zelfvertrouwen, waarbij het effect op mensen met een migratieachtergrond ook na twee jaar significant bleef. In de gecoachte groep was er een positief effect na één jaar voor mensen zonder schulden of relatief weinig zelfvertrouwen. Een ander effect werd gevonden voor welbevinden: in de ontheffingsgroep deden na twee jaar de hoger opgeleiden het relatief slechter. Het lijken dus vooral de meer kwetsbare groepen te zijn die beter af zijn in de alternatieve, experimentele regimes – vooral omdat mensen met schulden vaak niet kunnen profiteren van de bijverdien-component van deze treatments en daardoor waarschijnlijk mede daardoor een verminderd effect van de treatment ervaren.

Het laatste deelonderzoek gaat over de mate waarop mensen participeren in sociaaleconomische activiteiten die geen werk zijn. Het gaat om het verlenen van mantelzorg, het doen van vrijwilligerswerk, het opzetten van een eigen onderneming en het volgen van trainingen of scholing. Al deze activiteiten zijn voor dit onderzoek samengevoegd tot één variabele, die is gemeten na twee jaar. De eerste vraag is of de mate waarin deze activiteiten worden gedaan afhankelijk zijn van de treatment. Het antwoord daarop is dat er voor geen van de experimentele groepen een statistisch significant effect (stijging of daling) te vinden is. In een kwalitatieve analyse is vervolgens gekeken of er aanleiding is te veronderstellen dat subgroepen van deelnemers meer of minder effect ondervinden van de treatment, en zo ja, welke groepen dat zijn. Daaruit is gebleken dat er wellicht een groter effect zou kunnen zijn voor ouderen, hoger opgeleiden, mensen met een migratieachtergrond, mensen met een relatief zwakke gezondheid, en ouders van thuiswonende kinderen. Ten slotte is ook dit kwantitatief getoetst: hieruit bleek dat er twee groepen bovengemiddeld veel baat hadden van de alternatieve treatments: mensen met een zwakke mentale gezondheid (in de ontheffingsgroep), en mensen met een migratieachtergrond (in beide groepen). Dit past in het patroon dat mensen behorend tot de meer kwetsbare subgroepen beter geholpen zijn in de experimentele treatments, en minder tot hun recht komen in het reguliere regime.

De algemene conclusie is dat er, op treatment-niveau, enkele statistisch significante effecten zijn gevonden. Dit zijn een toename in vertrouwen in de lokale politiek, en een tijdelijke dip in welbevinden voor de ontheffingsgroep. De toename in vertrouwen kan, voor de ontheffingsgroep, deels verklaard worden door een grotere tevredenheid over het beleid, dat als meer passend en minder belemmerend werd ervaren. Wanneer gekeken wordt naar de effecten op specifieke subgroepen binnen de treatments, zien we dat deze effect hebben op verschillende subgroepen, en dan vooral op meer kwetsbare groepen. Dit is extra interessant wanneer we dit vergelijken met de resultaten van de eerste studie, over wie meededen aan het experiment. Groepen die het meeste baat hebben bij de alternatieve aanpak, zijn ook groepen die ondervertegenwoordigd zijn. Met andere woorden: het ligt in de rede te veronderstellen dat wanneer de alternatieve treatments zouden worden uitgerold over de hele bijstandspopulatie, effecten groter en positiever zijn, simpelweg om dat de hele bijstandspopulatie meer mensen uit deze kwetsbare groepen bevat. De gevonden resultaten bieden aanknopingspunten voor interessant en relevant vervolgonderzoek. Zo ligt het voor de hand om breder te kijken, bij andere experimenteergemeenten en in andere contexten, waar nog meer het de meest kwetsbaren zijn voor wie het beleid minder goed werkt. En om daar waar dat het geval is de reden daarvan te achterhalen. Daarnaast zouden we veel kunnen leren van nieuwe bijstands-experimenten, rekening houdend wat we geleerd hebben van de vorige serie (waar het Nijmeegse experiment er één van was). Geleerde lessen die daarbij in de praktijk kunnen worden gebracht zijn onder andere: een langere experimenteerperiode, meer afwijking van het reguliere beleid, en meer deelnemers, onder andere door het houden van gelijke experimenten in de verschillende deelnemende gemeenten, om zo nauwkeuriger te kunnen meten wat er gebeurt. De resultaten bieden eveneens een aantal zaken ter overweging voor beleidsmakers. De vraag of het huidige sociaal beleid niet averechts werkt voor juist de meest kwetsbare groepen, is iets dat (los van eventueel academisch vervolgonderzoek) aandacht behoeft. Dat geldt ook voor de resultaten op treatmentniveau: de resultaten van de alternatieve (ruimhartigere, meer op vertrouwen gebaseerde) behandelingen, op de hier onderzochte uitkomsten, wijken weinig af van de resultaten van het reguliere beleid. Dat betekent dat ‘effectiviteit’ geen argument is om het huidige beleid in stand te houden – des te meer omdat het beleid van de Participatiewet relatief streng is, en door veel berokkenen als hard wordt ervaren.

List of publications related to the project

The following (academic & professional) (co)authored publications are not included in this dissertation but are also related to this experiment:

- Betkó, J. (2018). Het Nijmeegse experiment met de Participatiewet. *Sociaal Bestek, 80*(3), 34-36.
- Betkó, J. G. (2019). Sociaal experimenteren met de bijstand, *Sociologie Magazine*, 27, 4, (2019), pp. 22-24
- Betkó, J. G., Spierings, N., Gesthuizen, M. J. W., & Scheepers, P. L. H. (2020). *Rapportage experiment Participatiewet gemeente Nijmegen*. Nijmegen: Radboud Universiteit.
- Betkó, J.G. (2020). Experimenten met een 'basisinkomenachtige' bijstand, *Wetenschappelijk Bureau GroenLinks*, 20 juli 2020, https://www.wetenschappelijkbureaugroenlinks.nl/artikel/experimenten-met-een-basisinkomenachtige-bijstand
- Sanders, M. W. J. L., Betkó, J. G., Edzes, A., Gramberg, P. J., Groot, L. F. M., Muffels, R. J. A., ... & Verlaat, T. L. L. (2020). De bijstand kan beter. *Me Judice*, 28.
- Edzes, A., Muffels, R., Zulkarnain, A., Jongen, E., Sanders, M., Verlaat, T., ... & Venhorst, V. (2021). Perspectieven voor interventies in de bijstand: Experimenten Participatiewet. *Tijdschrift voor arbeidsvraagstukken, 37*(3): 327–356.
- Scholten, L., Betkó, J., Gesthuizen, M., Fransen-Kuppens, G., de Vet, R., & Wolf, J. (2023). Reciprocal relations between financial hardship, sense of societal belonging and mental health for social assistance recipients, *Social Science & Medicine*, 115781, https://doi.org/10.1016/j.socscimed.2023.115781.

A series of blogs on the subject (all in Dutch) can be found @ https://sargasso.nl/series/proef-bijstand/

Appendices

Appendix 1 – about the treatments and care as usual (control)

Care as usual in the Dutch Social Assistance system is recorded in the Participation Act ('Participatiewet'). It is based on a workfare approach: people on social assistance are expected to leave it as soon as possible, and any job (or combination of jobs) is considered better than receiving social assistance. The person's own preferences are not taken into account.

This approach comes with a large number of obligations regarding re-integration. Social Assistance recipients are required to fulfil re-integration obligations set by social services (e.g. apply for a certain number of jobs per week; accept any job; commute 3 hours/day or move to another house if required for a job opportunity; make sure that the way they dress, present and take personal care of themselves do not interfere with the opportunity to get a job; et cetera.) If recipients want to do volunteer work, they have to get permission from social services, because that might interfere with searching for a job. And if people on social assistance work part-time, their allowance is reduced by the amount of money they make. The major exception is that if recipients earn additional income in the first six months, they are allowed to keep 25% of that without their allowance being reduced, up to a maximum of ~€200,- per months.

In addition, people on social assistance are to comply to a set of obligations related to the legality of them being on social assistance. This requires them to inform the municipality on every part of their life that might influence their eligibility for social assistance, like living together with somebody, or receiving any income (whatever the source, including a heritage, selling personal items second hand, or being brought groceries by family).

The treatments are, as stated in the paper, based on an approach that gives recipients more autonomy, takes trust in their capacities and intention as a starting point more, and allows them to keep more money from part-time or temporary work to facilitate steps forward. This extra income was part of both experimental treatments: the maximum per month stayed ~€200,- but that was prolonged for the entire duration of the experiment (maximum 25 months) instead of 6 months. Moreover, they were allowed to keep 50% of earned income, instead of 25%. Also, for both treatments the regular re-integration obligations (such as applying for a certain number of jobs) were set aside.

The most important difference between the two treatments is in that participants in the exempted group could freely choose their own path towards re-integration. They could do whatever they wanted, and any and all contact with re-integration services was voluntary and on their own initiative. There was no government control on the activities in this respect. Participants in the coached group could also choose their own path, but they got tailor made support for this, mainly through (on average) monthly group coaching sessions. Participation in the group coaching was the only obligation regarding re-integration, but still not control focused. Participants set their own goal, whether it was work (full-time or part-time), volunteering work, or entrepreneurship, and were offered support in obtaining those goals.

Appendix 2 – Qualitative data sources & interview guide

Table A2.1 Interviews with professionals who had contact with a large number of (potential) participants.

Role interviewee(s)	Number	Date	Main focus
Recruiters (civil servants of the municipality)	3 individual interviews	August and September 2017	Establishing whether selection bias took place (who participates and why)
Coaches of the coached treatment (civil servants of the regional re-integration service)	1 focus group with 3 coaches	June 2018	Their experiences with the coached group, implementation of the treatment and their observations about the participants
Contact person social service Nijmegen for participants in experiment	1 individual interview	April 2019	Because the rules and regulations for participants of the experiment were different, a few employers (customer managers) were designated to be familiar with these rules, and all questions from participants came to these people. This was the first contact person, majority of questions came from people in the exempted group.
Interviewers who did the survey fieldwork	2 focus groups with 5-10 people[97]	January 2018 and February 2019 (after field work was completed)	Interviewers talked with often dozens participants, mostly in their own house (the interviews were face to face), for about an hour on average. This means interviewers have the opportunity to have a good impression on how the experiment affects the participants.

Interview guide

Below is an example of an interview guide used. This was used when interviewing the three municipal civil servants who did the recruitment, and thus spoke with a large number of potential participants. The other interview guides can be requested from the authors.

97 Due illness, the second focus group was lead not by one of us, the researchers, but another employer from the company who did the fieldwork (surveying).

Topic-list interviews wervers stadswinkel

DOEL INTERVIEW
Welke factoren hebben een rol gespeeld in wie zich wel en niet opgegeven hebben voor de proef met de bijstand? (Het genereren van mechanismen.)

TOPICS
Motivatie mee te doen
- Financieel
- Regeldruk
- Kans uitstroom

Motivatie niet mee te doen
- Wantrouwen
- Vertrouwen eigen kunnen / geen effect verwacht
- Inbreuk privacy/controle
- Taalvaardigheid

Reacties inhoud proef
Reacties bij niet in doelgroep
Reacties random indeling

VOORAF
-consentformulier
-opnamecheck

INTERVIEW GUIDE

Intro
Voorstellen Doel: -- Achtergrond informatie voor de proef met de bijstand -- Interview wordt gebruikt in onderzoek, maar altijd **geanonimiseerde:** functie, niet naam. (Naam = vertrouwlijk) -- ervaring + perspectief; geen foute antwoorden; onduidelijk dan aangeven. -- Doorvragen: graag veel informatie willen en details Heeft u nog vragen?

Onderwerp 1: Setting
Algemeen

Vraag 1:
Zou je me eerste wat feitelijk informatie kunnen geven; wanneer je werving in de Stadswinkel gedaan hebt, voor hoe lang, hoeveel mensen je hebt aangesproken, de opstelling, dat soort dingen.

- Folders en materiaal
- Alleen
- Aantal gesprekken; totaal aantal mensen voorbij kwamen

Vraag 2:
Hoe heeft u de dag ervaren?

- Deed met persoon (houding)
- Verloop over de dag
- Verwachting vooraf

Notities

Onderwerp 2: Motivaties
Algemeen

Vraag 3:
Als U zo terugdenkt aan de gesprekken, wat waren dan de motivaties van mensen om mee te doen of een folder mee te nemen?

- Uitgesproken of impliciet
- Wat meer en minder / ook als maar 1 persoon noemde interessant
- Opening richting mensen (argumenten voor)
- Op welke manier proberen over te halen
- Potentiele antwoorden
    - Financieel
    - Regeldruk
    - Uitstroom

Vraag 4:
Wat kreeg u terug waarom mensen geen interesse hadden?

- Verschillen mensen langer of niet gesproken
- Impressies wat meespeelde
- Verrassende antwoorden
- compleet
- Hoe ze werver leken te zien
- Potentiele antwoorden
    - Wantrouwen
    - Vertrouwen eigen kunnen / geen effect verwacht
    - Inbreuk privacy/controle
    - Taalvaardigheid

Notities

Onderwerp 3: Beeld van Proef **Algemeen**
Vraag 5: Over welke onderdelen van de inhoud en opzet van de proef hoe U onder andere gesproken en wat vonden de mensen die u sprak daarvan? Indeling in groepen Voorkeur groep Sollicitatieplicht Bijverdienen In welke woorden / waar had men moeite mee Vraag 6: Waren er ook mensen die eigenlijk wel mee wilden doen maar buiten doelgroep vielen en hoe reageerden ze? Voorkomen Waarom meedoen Waarom uitvallen
Notities

Afsluiting
Vraag 7: Zijn er zaken opgevallen die we niet besproken hebben of andere punten die u wilt delen? Slot: Danken Op de hoogte van resultaten? (Duurt wel even)

Appendix 3 – about the flowcharts

The flowcharts in this thesis (Figure 3.1, 4.1 and 5.1) deserve some additional explanation. The studies in Chapter 3, 4 and 5 are based on survey data. That means, only follow individuals can be traced who actually started the experiment, and complied with filling in the survey at both the pre-treatment moment and after one year. This inevitably leads to some drop-out, and potentially to drop-out bias. As can be seen, especially in the control group drop-out after randomization was higher than in the treatment groups.

Regarding the first three boxes (people on social assistance, people not eligible, target group), the scheme includes estimations instead of definitive numbers. This is the result of working in a real-life setting: the numbers are based on administrative data from a municipality that are stored for practical purposes, and not with research in mind, and which are retro-actively updated. The number of social assistance claimants changes daily.

The experiment had two starting moments: 1 December 2017, and 1 April 2018. To know exactly how many people were on social assistance at those moments, one has to look at the two municipal datasets at that time, would have to know which people were receiving benefits as a couple or as an individual, and if this household situation was the same a few months later. The municipal datasets do not have that level of detail. Furthermore, to know how many people exactly were invited, one had to look at the records a few months earlier, when people were invited to participate. Even if we could find the exact date, records will have been retro-actively changed since then. Giving a broad estimate is thus better than giving an exact number that gives a false sense of accuracy.

The same goes for the people who were eligible to participate: it is impossible to know exactly who was eligible. Part of the screening procedure was that people who applied were checked by the regional re-integration organization, if they were not halfway or further in a re-integration trajectory, or had obligations with a third party, that might be crossed by the experiment. This being work-intensive, they only did it for people who applied, not for the entire population. So part of the population we thought eligible, was non-eligible as turnout after applying.

So, instead of giving exact numbers, an estimate is given in these three categories. And even in the other categories that seem straightforward (like the number of applications, or the number of people who started the experiment), numbers were sometimes difficult to give exact. There were for instance applications of people who did not have a social assistance benefit or were not living in the municipality of Nijmegen, there were people allowed to start the experiment from which it later turned out that they were not supposed to have an allowance, and people who were denied participation by the municipality in the first place, but who won their appeal and entered in a later stage.

Appendix 4 - ethical research with human subjects and consent

For this study we conducted a social experiment with human subjects. We did everything to make sure the experiment and study were conducted ethically, complying with all relevant standards.

- All subjects gave active informed consent, for which the form can be found below.[98] Participants were informed about the goal of the study, the duration, the number of interviews they were expected to partake in, that other (administrative) data might be linked with the survey data, the fact that they could end their participation at any moment without negative consequences, and a guarantee that their data would be treated with utmost confidentiality, whereby all publications would be fully anonymous and untraceable to individual participants.
- In addition to the fact that participation was voluntary, participants could end their participation at any moment they deemed fitting, and were informed thereof.
- The overall hypothesis was that both treatments would be preferable (from the participants point of view) compared to the regular social assistance regime, so that it is unlikely that participants suffer from overall negative consequences during the experiment.
- The experiment has been approved by both the local and the national government, and supervised by the Dutch scientific advisory council ZonMW, which promotes and subsidizes health research and care innovation. This experiment falls under the experimentation and innovation clause of the Participation Act (article 83). Explicit permission was given by the Ministry of Social Affairs on November 3rd, 2017 (reference number 2017-0000172924). ZonMW participated through an academic guidance committee that gave feedback on the experiment while it was ongoing. They furthermore advised the Ministry of Social Affairs about permitting the experiment to start (reference number 2017t141 2|ZONMW).

98 It carries the logo of the Municipality of Nijmegen, since the municipality was in charge of data collection and is the owner of the data.

The informed consent:

INFORMATIE- EN TOESTEMMINGSFORMULIER: PROEF BIJSTAND

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Hartelijk dank voor uw deelname aan deze studie. In dit formulier wordt de doelstelling van de proef beschreven, uw rol en uw rechten als deelnemer.

Het doel van deze proef is: onderzoeken hoe mensen zich gedragen, wanneer andere regels gelden voor de bijstand (Participatiewet), waarbij mensen meer vrijheid krijgen bij het zoeken naar werk en meer mogen bijverdienen naast de uitkering.

Als onderdeel van deze proef wordt vier keer een één op één interview met u gehouden aan de hand van een vragenlijst, tussen november 2017 en oktober 2020. Deze interviews zijn verplicht voor alle deelnemers aan de proef bijstand. Voor de interviews wordt u verzocht om zo volledig mogelijk te antwoorden op de gestelde vragen. Dit interview kan bij u thuis plaatsvinden. Indien u dit liever niet heeft, kan het interview op een andere locatie plaatsvinden.

De methode die we hanteren om dit doel te bereiken is een: één-op-één interviews en analyse van de data in de administratie van de gemeente en (indien van toepassing) het WerkBedrijf. Voor de interviews wordt u verzocht om de interviewer zo volledig mogelijk te antwoorden op gestelde vragen. Dit interview kan bij u thuis plaatsvinden. Indien u dit liever niet heeft kan een andere locatie gekozen worden.

U (de deelnemer) kunt gedurende het hele onderzoek vragen stellen over de aard van de studie, de onderwerpen die worden besproken of de methode die wordt gebruikt.

Van de interviews met u maken wij een verslag in de vorm van een vraag- en antwoordlijst. Deze gegevens worden vertrouwelijk behandeld en alleen in het kader van dit onderzoek verwerkt. Uw persoonsgegevens worden alleen gebruikt om te kunnen verifiëren dat u het bent. Uiteindelijke verwerking van de resultaten van het onderzoek, zal anoniem gebeuren.

U heeft het recht om op elk moment uw medewerking aan deze proef te beëindigen. Vanaf het moment dat u zich terugtrekt, valt u weer onder het reguliere regime van de Participatiewet.

Inzichten die vergaard worden middels de interviews met u en de andere deelnemers zullen worden gebruik voor een wetenschappelijk onderzoek. Directe citaten zullen niet aan uw naam verbonden worden, ook uw naam of andere identificerende informatie zal niet worden verstrekt en uw gegevens zullen ten alle tijden anoniem blijven.

Mocht u nu of na afloop nog vragen hebben, neem dan contact op met [redacted]

Ik ben naar tevredenheid over het onderzoek geïnformeerd. Ik heb de schriftelijke informatie goed gelezen. Ik ben in de gelegenheid gesteld om vragen te stellen. Mijn vragen zijn naar tevredenheid beantwoord. Ik heb goed over deelname aan het onderzoek kunnen nadenken. Ik snap dat ik het recht hem om mijn toestemming op ieder moment weer in te trekken zonder dat ik daarvoor een reden hoef op te geven.

Ik stem toe met deelname aan het onderzoek en geef toestemming voor de daarvoor noodzakelijke verwerking van mijn persoonsgegevens.

Naam :
Geboortedatum :

Handtekening : Datum:

Einde van document

Appendix 5 – factor analyses for the trust chapter

Table A5.1 Mental health / SWB, oblique rotation, N=259 (w1) 270 (w2).

	Wave 1			Wave 2		
	h^2	Mental health	Subjective Well-being	h^2	Mental Health	SWB
Life satisfaction	0.76		-0.81	0.60	0.78	
Life meaningful	0.51		-0.75	0.42	0.65	
Were you nervous?	0.32	0.60		0.27	0.52	
Were you down?	0.63	0.77		0.49	0.70	
Were you (not) calm/ relaxed?*	0.28	0.53		0.38	0.62	
Were you sad/dejected?	0.73	0.82		0.58	0.76	
Were you (un)happy?*	0.57	0.46	-0.38	0.59	0.77	
Eigenvalue		3.64	1.02		3.84	0.99

h^2: communalities; correlation Mental health - SWB, wave 1: -0.60; wave 2: not applicable

Table A5.2 Mental health / SWB minus “happy”, oblique rotation, N=259 (w1) 270 (w2).

	Wave 1			Wave 2		
	h^2	Mental health	Subjective Well-being	h^2	Mental Health	SWB
Life satisfaction	0.83		-0.87	0.51	0.72	
Life meaningful	0.46		-0.70	0.40	0.63	
Were you nervous?	0.33	0.60		0.32	0.56	
Were you down?	0.63	0.76		0.54	0.73	
Were you (not) calm/ relaxed?*	0.25	0.51		0.40	0.63	
Were you sad/dejected?	0.76	0.83		0.59	0.77	
Eigenvalue		3.07	1.01		3.27	0.92

h^2: communalities; correlation Mental health - SWB, wave 1: -0.59; wave 2: not applicable

Table A5.3 Mental health / SWB minus "happy" forcing 2 factors, oblique rotation, N=259 (w1) 270 (w2).

	Wave 1			Wave 2		
	h^2	Mental health	Subjective Well-being	h^2	Mental Health	SWB
Life satisfaction	0.83		-0.87	0.82		-0.91
Life meaningful	0.46		-0.70	0.53		-0.69
Were you nervous?	0.33	0.60		0.44	0.74	
Were you down?	0.63	0.76		0.60	0.73	
Were you (not) calm/ relaxed?*	0.25	0.51		0.39	0.50	
Were you sad/dejected?	0.76	0.83		0.59	0.66	
Eigenvalue		3.07	1.01		3.27	0.92

h^2: communalities; correlation Mental health - SWB, wave 1: -0.59; wave 2: -0.62

Table A5.4 Policy judgement & effectiveness, oblique rotatie, N=221 (w1) 270 (w2).

	Wave 1			Wave 2		
	h^2	Policy contentment	Policy effectiveness	h^2	Policy contentment	Policy effectiveness
Rules are a burden	0.62	0.84		0.53	0.75	
Rules help me to take part in society	0.62		0.78	0.53		0.70
Rules annoy me	0.52	0.72		0.69	0.83	
Rules encourage me to find a job	0.37		0.60	0.48		0.71
Rules allow me leeway to do what I want	0.32	0.53		0.48	0.72	
Rules are fitting	0.40	0.52		0.51	0.63	
Eigenvalue		2.74	1.10		3.09	1.07

h^2: communalities; correlation policy exp – policy eff, wave 1: -0.50; wave 2: -0.54

Appendix 6 - Main effects mediating variables Trust

Table A6.1 Change in policy contentment and social integration in the Nijmegen experiment, regressions with t=0 and control variables.

	Policy contentment		*Perceived policy effect.*		*Part of society*		*Number of contacts*	
	B	Sig.	B	Sig.	B	Sig.	B	Sig.
Group								
Control	Ref		Ref		Ref		Ref	
Exempted	0.25	*	0.13		-0.45	~	-0.18	
Coached	0.15		0.14		-0.18		-0.21	
T=0 value	-0.47	**	-0.49	**	-0.59	**	-0.47	**
Gender								
Male	Ref		Ref		Ref		Ref	
Female	0.03		0.00		0.06		0.27	*
Age	0.00		0.00		-0.02		-0.00	
Education								
Primary	Ref		Ref		Ref		Ref	
Lower secondary	0.14		0.11		0.79	~	0.10	
Higher secondary	0.28		0.10		0.33		-0.00	
Tertiary	0.15		0.29	~	0.30		0.17	
Unknown	0.30		0.21		0.20		0.16	
Country of birth								
Netherlands	Ref		Ref		Ref		Ref	
Non-Western	-0.16		0.43	**	-0.60	*	-0.32	*
Western	-0.24		0.00		1.04	~	0.03	
Household composition								
Single no kids	Ref		Ref		Ref		Ref	
Single parent	-0.11		0.06		-0.11		0.12	
Couple no kids	1.13		-0.40		-1.94		-0.57	
Couple with kids	0.51	*	0.42	~	0.44		0.52	*
Intercept	1.13	**	0.76	*	5.31	**	2.02	**
N	259		259		259		259	
R	0.55		0.53		0.63		0.59	

*** p < 0.01, * p < 0.05, ~ p < 0.10*

Table A6.2 Change in mental health and subjective well-being in the Nijmegen experiment, regressions with t=0 and control variables.

	Subjective Well-Being		*Mental Health*	
	B	Sig.	B	Sig.
Group				
Control	Ref		Ref	
Exempted	-0.34	~	-0.04	
Coached	0.02		0.16	
T=0 value	-0.51	**	-0.45	**
Gender				
Male	Ref		Ref	
Female	0.00		-0.22	
Age	-0.01		0.01	
Education				
Primary	Ref		Ref	
Lower secondary	0.54		-0.33	
Higher secondary	0.22		-0.19	
Tertiary	0.51		0.05	
Unknown	-0.08		-0.57	
Country of birth				
Netherlands	Ref		Ref	
Non-Western	-0.14		-0.29	
Western	0.29		-0.20	
Household composition				
Single no kids	Ref		Ref	
Single parent	0.35		0.13	
Couple no kids	-2.2	~	-1.34	
Couple with kids	0.62	~	0.27	
Intercept	3.66	**	3.00	**
N	259		259	
R	0.59		0.51	

*** p < 0.01, * p < 0.05, ~ p < 0.10*

Appendix 7 – Regression tables for national political trust & social trust

Table A7.1 Change in national trust in the Nijmegen experiment, regressions with t=0, mediating and control variables.

	Basic model	*Mediation 1 Pol judg.*	*Mediation 2 Social int.*	*Mediation 3 SWB&MH*	*Mediation total*
Group					
Control	Ref	Ref	Ref	Ref	Ref
Exempted	0.03	0.01	0.01	0.02	-0.01
Coached	0.04	0.02	0.03	0.04	0.00
T=0 trust value	-0.26**	-0.28**	-0.26**	-0.26**	-0.27**
Policy evaluation					
Contentment		0.12*			0.12*
Perceived effectiveness		0.09*			0.10*
Social integration					
Part society			-0.03		-0.03
Contacts			0.00		-0.01
Social & mental WB					
Subjective well-being				-0.01	0.01
Mental Health				-0.01	0.00
Gender					
Male	Ref	Ref	Ref	Ref	Ref
Female	-0.04	-0.05	-0.04	-0.04	-0.05
Age	0.00	-0.00	-0.00	-0.00	-0.00
Education					
Primary	Ref	Ref	Ref	Ref	Ref
Lower secondary	0.06	0.05	0.07	0.06	0.06
Higher secondary	-0.08	-0.13	-0.08	-0.08	-0.13
Tertiary	-0.05	-0.02	0.05	0.05	-0.02
Unknown	-0.01	-0.09	0.00	-0.02	-0.07
Country of birth					
Netherlands	Ref	Ref	Ref	Ref	Ref
Non-Western	0.26*	0.27*	0.23*	0.26*	0.24*
Western	0.03	0.04	0.04	0.03	0.04

Table A7.1 Continued.

	Basic model	*Mediation 1 Pol judg.*	*Mediation 2 Social int.*	*Mediation 3 SWB&MH*	*Mediation total*
Household composition					
Single no kids	Ref	Ref	Ref	Ref	Ref
Single parent	0.15	0.17	0.14	0.15	0.16
Couple no kids	-0.70	-0.85	-0.76	-0.71	-0.92
Couple with kids	0.03	-0.06	0.02	0.03	-0.07
Intercept	0.53 ~	0.64 *	0.56 *	0.54 ~	0.67 *
N	230	230	230	230	230
R	0.41	0.46	0.42	0.41	0.47

*Fields shows the B and significance, where: ** p < 0.01, * p < 0.05, ~ p < 0.10*

Table A7.2 Change in social trust in the Nijmegen experiment, regressions with t=0, mediating and control variables.

	Basic model	*Mediation 1 Pol judg.*	*Mediation 2 Social int.*	*Mediation 3 SWB&MH*	*Mediation total*
Group					
Control	Ref	Ref	Ref	Ref	Ref
Exempted	0.14	0.11	0.21	0.13	0.11
Coached	-0.06	-0.07	-0.02	-0.11	-0.10
T=0 trust value	-0.46**	-0.47**	-0.47**	-0.46**	-0.47**
Policy evaluation					
Contentment		0.13			0.15
Perceived effectiveness		0.02			-0.02
Social integration					
Part society			0.11		0.10~
Contacts			-0.01		-0.03
Social & mental WB					
Subjective well-b.				-0.04	-0.10
Mental Health				0.19**	0.18*

Table A7.2 Continued.

	Basic model	*Mediation 1 Pol judg.*	*Mediation 2 Social int.*	*Mediation 3 SWB&MH*	*Mediation total*
Gender					
Male	Ref	Ref	Ref	Ref	Ref
Female	0.44~	0.44~	0.44~	0.43~	0.41~
Age	0.00	0.00	-0.01	0.00	0.01
Education					
Primary	Ref	Ref	Ref	Ref	Ref
Lower secondary	1.45**	1.45**	1.39**	1.59**	1.54**
Higher secondary	1.44**	1.41**	1.44**	1.49**	1.45**
Tertiary	1.58**	1.57**	1.57**	1.62**	1.61**
Unknown	1.44**	1.41**	1.37**	1.63**	1.53**
Country of birth					
Netherlands	Ref	Ref	Ref	Ref	Ref
Non-Western	-0.47~	-0.44~	-0.38	-0.44~	-0.35
Western	-1.28*	-1.26*	-1.31*	-1.20*	-1.24*
Household composition					
Single no kids	Ref	Ref	Ref	Ref	Ref
Single parent	-0.21	-0.19	-0.17	-0.24	-0.16
Couple no kids	-0.22	-0.39	0.04	-0.34	-0.43
Couple with kids	0.42	0.37	0.46	0.44	0.44
Intercept	1.68 *	1.73 *	1.50 *	1.57 **	1.54 *
N	259	259	259	259	259
R	0.53	0.53	0.54	0.59	0.57

*Fields shows the B and significance, where: ** p < 0.01, * p < 0.05, ~ p < 0.10*

Appendix 8 – Robustness checks trust

Visual magnitude and shape analysis, using box-and-whisker plots.
The residuals are derived from the regression models excluding the variable for the treatment group. Subsequently, visualizing the residuals per treatment provided insight in whether the median of the outcome are different per treatment, whether the spread in residuals show particular non-linear relationships (for instance, whether the effect is caused by a select group or outliers), and whether the absence of an effect in the regression analyses is accompanied by a differences in dispersion or such non-linearities.

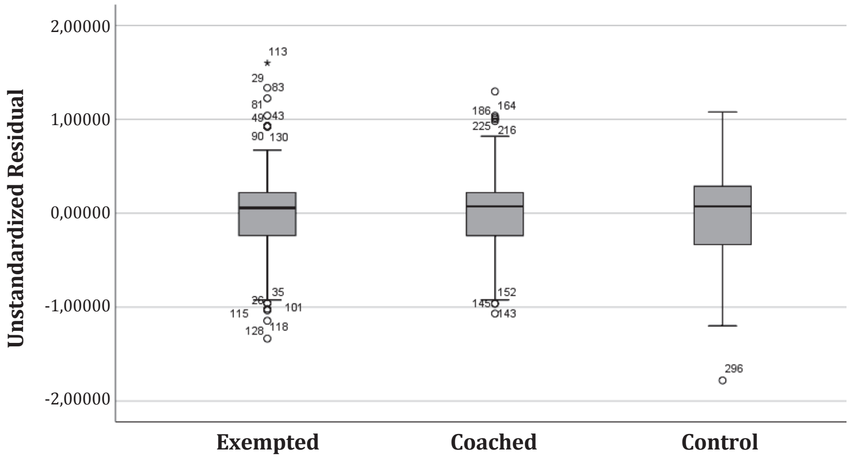

Figure A8.1 Residuals per treatment for national political trust.

Figure A8.1 shows that the median residual are very much the same. No clear non-linearities are indicated as for all groups the lower middle quartile is bigger that the upper middle quartile. The dispersion is somewhat wider for the control group, but the mathematical outliers for both treatment groups show considerable spread in the other groups and these are not skewed. The absence of a statistical effect does not seem to be cause by magnitude errors or non-linearities, which reduces the likelihood of a Type-1 error on the impact of the treatments on national political trust.

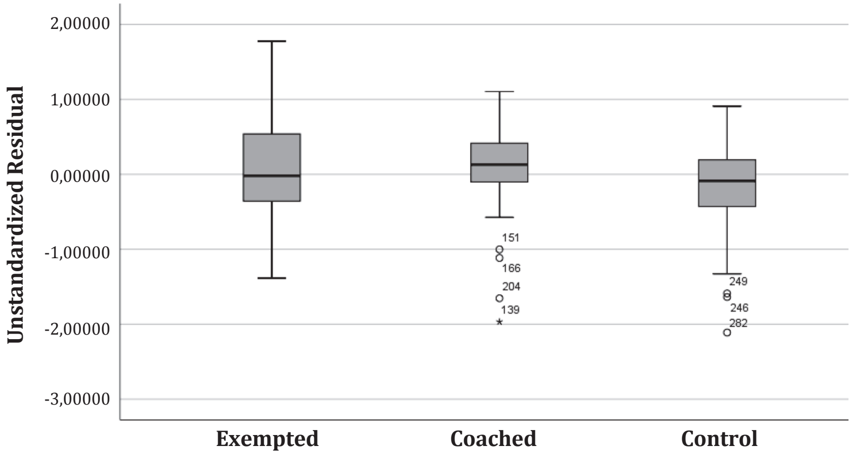

Figure A8.2 Residuals per treatment for municipal political trust.

Figure A8.2 clearly indicates higher median respondents in the treatment, particularly the coached, and also the upper bound of the middle two quartiles is clearly higher for both treatment groups. For the coached groups three outlier are reported, but this is largely a function of the rather tight variation around the median overall. Noteworthy is also the higher variation found for the exempted group. This seems to indicate that the exempted treatment, although leading to more trust on average, has a relatively diverse effect: for most respondents it works positively, but for some it work distinctly more negatively in terms of trust than found for the coached group (which is intuitively in line with the hands-off treatment versus the empowering treatment). To summarize, these analyses show that the conclusion that the treatments have a positive effect of local political trust is very unlikely to be due to a Type-2 error.

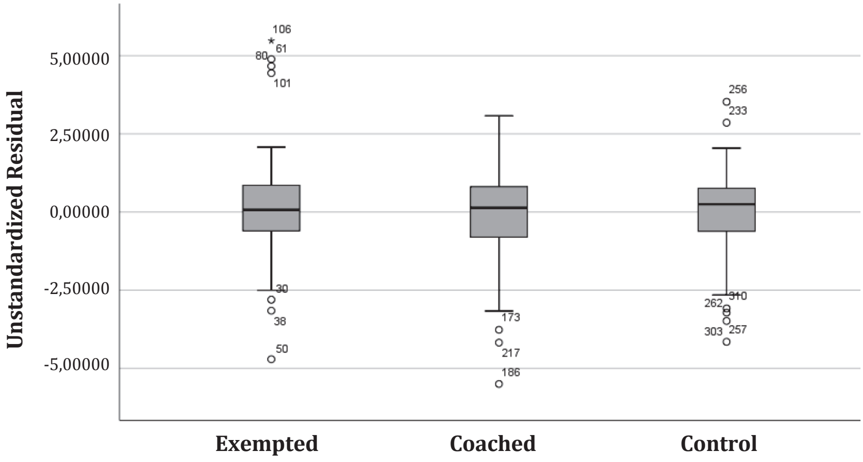

Figure A8.3 Residuals per treatment for social trust.

Figure A8.3 confirms the main effects found in the regression model for social trust: there are no clear differences across the groups. The medians are highly similar as are the quartile scores. Based on these results a Type-1 error is highly unlikely.

Table A8.1 Change in local trust, regressions with t=0, mediating and control variables.

	Basic model (3.1)	*Full mediation model (7)*	*Mediation total + t=0*
Group			
Control	Ref	Ref	Ref
Exempted	0.21*	0.14	0.16~
Coached	0.26*	0.25*	0.23*
T=0 trust value	-0.61**	-0.64**	-0.69**
Policy evaluation			
Contentment t=0			0.15*
Contentment		0.17**	0.23**
Perceived effect. t=0			-0.05
Perceived effect.		-0.03	-0.06
Social integration			
Part society t=0			-0.00
Part society		-0.01	-0.01
Contacts t=0			-0.02
Contacts		0.04	0.02
Social & mental WB			
SWB t=0			0.09*
Subjective well-b.		-0.08*	-0.03
Mental Health t=0			-0.02
Mental Health		-0.01	-0.02
Gender			
Male	Ref	Ref	Ref
Female	-0.03	-0.06	-0.07
Age	0.00	0.00	0.00
Education			
Primary	Ref	Ref	Ref
Lower secondary	0.28	0.28	0.21
Higher secondary	0.17	0.10	0.08
Tertiary	0.26~	0.24~	0.21
Unknown	0.08	0.02	0.04
Country of birth			
Netherlands	Ref	Ref	Ref
Non-Western	0.29*	0.34**	0.37**
Western	0.21	0.25	0.23

Table A8.1 Change in local trust, regressions with t=0, mediating and control variables.

	Basic model (3.1)	*Full mediation model (7)*	*Mediation total + t=0*
Household composition			
Single no kids	Ref	Ref	Ref
Single parent	0.02	0.09	0.06
Couple no kids	-0.03	-0.40	-0.27
Couple with kids	-0.16	-0.21	-0.26
Intercept	1.33 **	1.58 **	1.06 **
N	248	248	248
R	0.60	0.65	0.67

*Fields shows the B and significance, where: ** p < 0.01, * p < 0.05, ~ p < 0.10*

Appendix 9 – Health index construction

Regarding *subjective well-being*, two items are used to create an index in a similar way to the EU-SILC 2013 dataset, and the World Value Survey V23. The questions are:

1) Can you rate your current life satisfaction from 0 to 10?
2) Do you think the things you do in your life are meaningful?

For *mental health*, we combine the five used from the MHI-5 indicating whether a respondent is nervous, down, calm & peaceful, downhearted, and happy. All are answered on a 5-point scale, ranging from 'never' to 'always'. These are linearly recoded and combined additively and rescaled to run from 0 through 100 as described in the manuscript. The questions asked are:

1) In the last 4 weeks, did you feel very nervous?
2) In the last 4 weeks, were you so down that nothing could cheer you up?
3) In the last 4 weeks, did you feel calm and peaceful?
4) In the last 4 weeks, did you feel gloomy and depressed?
5) In the last 4 weeks, did you feel happy?

The items measuring *self-confidence* are developed for a number of social experiments in The Netherlands with social assistance (E. J. de Bruijn, et al., Experiments Participation Act Questionnaires and documentation), covering themes regularly used in the measuring of self-confidence. For each question, there were five standard possible answers: "completely disagree", "disagree", "neither agree nor disagree", "agree" and "completely agree". The questions used were:

1) Usually I can control my emotions
2) I can do the things I find important
3) I can deal with difficult situations myself
4) Setbacks in life I can overcome myself.

Having debts is measured in six categories: 'no debts', 'less than € 1000,-', 'between € 1001,- and € 15.000,-', 'between € 15.001,- and € 50.000,-', 'over €50.000,- ' and 'don't know', and recoded here into a dichotomous variable (no debts / having debts – thereby assuming that the 9 people who answered 'don't know' probably will have debts).

Regarding *social contacts*, the answers for the three different types of possible contacts were administered on a scale ranging from 'never' (0) to 'at least once per weak' (5). The index is constructed by taking the arithmetic mean.

Appendix 10 – Subjective well-being and mental health incl. moderations

The tables below show the effects of the treatments on subjective well-being (Table A10.1) and mental health (Table A10.2). They provide the same information as tables 4.5, 4.6 and 4.7 provide in Chapter 4, which is effects of the analysis with a health-related variable using a model that includes interactions between treatment groups and potential moderators.

For the sake of brevity the three tables showing the differences between t=0 and t=1, t=1 and t=2, and t=0 and t=2, are compressed into a single table. The Each model is shown over two lines, in the same color (either grey or white). To illustrate how the table works, take as an example model 3 (interaction model including 'part of a couple') for the DV 'change in subjective well-being' in the timeframe between t=0 and t=1. In table A10.1, line 7 and 8 show the results in column 2, 3 and 4. Line 7 column 3 shows the results for "exempted group", line 7 column 4 the results for "coached group", line 8 column 2 the results for "couple" line 8 column 3 results for the interaction between "exempted group and couple" and finally line 8 column 4 the results for the interaction "coached group and couple". Since "couple" can be either 0 (not part a couple) or 1 (part of a couple), line 7 column 3 and 4 show the results for participants not part of a couple for respectively the exempted and the coached group, while line 8 column 3 and 4 show the results for the participants who are part of couple.

Table A10.1 Subjective well-being including moderations.

	T0-T1 (N=259)*			T1-T2 (N see note 1)			T0-T2 (N see note 2)		
	B (p)	Exemption	Coached		Exemption	Coached		Exemption	Coached
**		-0.43 (0.58)	0.51 (0.49)		-0.56 (0.46)	-0.42 (0.57)		-1.05 (0.24)	0.05 (0.95)
Self-confidence	0.50 (0.02)	0.02 (0.96)	-0.21 (0.47)	0.04 (0.83)	0.38 (0.20)	0.26 (0.34)	0.25 (0.30)	0.45 (0.20)	0.07 (0.83)
		-0.43 (0.59)	-0.02 (0.98)		0.94 (0.18)	0.68 (0.40)		0.62 (0.46)	0.77 (0.43)
Contacts	0.04 (0.79)	-0.00 (0.99)	-0.00 (0.99)	0.25 (0.05)	-0.13 (0.42)	-0.11 (0.57)	0.15 (0.33)	-0.15 (0.45)	-0.13 (0.55)
		-0.39 (0.04)	0.03 (0.86)		0.29 (0.10)	0.15 (0.42)		0.01 (0.96)	0.21 (0.48)
Couple	0.65 (0.17)	-0.60 (0.39	-0.63 (0.52)	-0.44 (0.30)	0.41 (0.50)	1.27 (0.10)	0.01 (0.99)	-0.06 (0.93)	0.01 (0.99)
		0.12 (0.84)	0.10 (0.88)		0.69 (0.22)	0.82 (0.24)		1.13 (0.09)	1.04 (0.19)
Education	0.17 (0.24)	-0.18 (0.34)	-0.04 (0.86)	0.08 (0.55)	-0.10 (0.59)	-0.18 (0.40)	0.27 (0.08)	-0.36 (0.08)	-0.27 (0.27)
		-0.45 (0.06)	0.07 (0.78)		0.53 (0.03)	0.15 (0.54)		0.15 (0.58)	0.24 (0.42)
No debts	0.28 (0.29)	0.07 (0.84)	-0.18 (0.63)	0.05 (0.84)	-0.43 (0.20)	0.18 (0.62)	0.14 (0.63)	-0.32 (0.43)	-0.05 (0.90)
		-0.51 (0.03)	-0.09 (0.72)		0.29 (0.18)	0.14 (0.56)		-0.03 (0.91)	0.19 (0.66)
Part-time work	0.00 (1.00)	0.17 (0.65)	0.16 (0.68)	0.13 (0.58)	0.14 (0.68)	0.25 (0.49)	0.10 (0.73)	0.16 (0.70)	0.24 (0.58)
		-0.40 (0.05)	-0.07 (0.75)		0.50 (0.01)	0.12 (0.57)		0.14 (0.53)	0.08 (0.74)
Country: west	0.01 (0.99)	0.29 (0.78)	0.65 (0.56)	1.19 (0.12)	-1.69 (0.07)	-0.53 (0.60)	1.02 (0.26)	-1.21 (0.28)	-0.09 (0.94)
Country: n-west	-0.00 (0.99)	-0.28 (0.54)	0.10 (0.83)	0.01 (0.98)	-0.64 (0.12)	0.44 (0.33)	-0.08 (0.82)	-0.58 (0.25)	0.47 (0.36)

* Except Part-time work, there: N=258.
** For each moderator we tested, the first line in the table shows the treatment effect, which is the outcome for the treatment if the moderator variable equals zero.
For the analyses between t=1 and t=2, the N is as follows.[1] Self-confidence: 224, Contacts: 224, Couple: 232, Education: 224, No debts: 232, Part-time work: 231, Country: 232.
For the analyses between t=0 and t=2, the N is as follows.[2] Self-confidence: 224, Contacts: 224, Couple: 224, Education: 224, No debts: 224, Part-time work: 223, Country: 224.

Table A10.2 Mental health including moderations.

	T0-T1 (N=259)*			T1-T2 (N see note 1)			T0-T2 (N see note 2)		
	B (p)	Exemption	Coached		Exemption	Coached		Exemption	Coached
**		0.83 (0.93)	9.50 (0.30)		-9.02 (0.35)	-9.30 (0.31)		-11.76 (0.28)	-3.22 (0.76)
Self-confidence	5.31 (0.05)	0.58 (0.88)	-3.52 (0.32)	-2.36 (0.36)	5.08 (0.18)	5.00 (0.16)	-1.75 (0.56)	5.85 (0.17)	2.96 (0.50)
		-0.89 (0.92)	-1.04 (0.92)		8.10 (0.36)	5.55 (0.59)		6.25 (0.54)	6.46 (0.58)
Contacts	0.63 (0.70)	-0.44 (0.98)	0.39 (0.87)	2.25 0.16)	-1.05 (0.61)	-0.55 (0.82)	1.30 (0.48)	-0.84 (0.72)	-0.69 (0.80)
		-0.76 (0.74)	0.43 (0.86)		2.52 (0.26)	2.61 (0.28)		2.68 (0.29)	3.26 (0.23)
Couple	0.93 (0.87)	-4.64 (0.58)	11.85 (0.32)	1.39 (0.80)	1.80 (0.82)	4.35 (0.66)	0.21 (0.97)	0.64 (0.94)	11.64 (0.35)
		8.36 (0.25)	4.28 (0.61)		-3.19 (0.65)	6.69 (0.43)		5.01 (0.53)	11.25 (0.24)
Education	1.95 (0.26)	-3.06 (0.17)	-1.13 (0.66)	-0.09 (0.96)	2.26 (0.30)	-1.05 (0.67)	1.58 (0.39)	-0.71 (0.78)	-2.44 (0.41)
		-2.45 (0.40)	0.13 (0.97)		4.33 (0.15)	3.18 (0.33)		2.23 (0.50)	3.07 0.39)
No debts	0.19 (0.95)	3.40 (0.44)	1.38 (0.77)	1.16 (0.71)	-3.62 (0.40)	-0.80 (0.86)	-1.96 (0.58)	0.84 (0.86)	1.04 (0.84)
		-2.56 (0.38)	-2.37 (0.44)		1.92 (0.49)	0.33 (0.92)		2.17 (0.49)	0.09 (0.98)
Part-time work	-1.85 (0.57)	3.14 (0.48)	6.64 (0.15)	1.63 (0.60)	1.98 (0.65)	5.58 (0.24)	1.51 (0.67)	1.54 (0.75)	8.56 (0.10)
		-1.01 (0.68)	1.92 (0.47)		3.53 (0.14)	1.95 (0.47)		2.52 (0.36)	0.38 (0.29)
Country: west	-1.59 (0.88)	-15.56 (0.22)	14.05 (0.30)	9.55 (0.33)	7.55 (0.53)	-5.15 (0.69)	7.90 (0.47)	-1.77 (0.90)	4.28 (0.76)
Country: n-west	-1.05 (0.78)	3.18 (0.56)	-6.18 (0.25)	0.53 (0.89)	-7.45 (0.16)	3.14 (0.57)	-0.77 (0.86)	0.45 (0.94)	-0.07 (0.99)

* Except Part-time work, there: N=258.
** For each moderator we tested, the first line in the table shows the treatment effect, which is the outcome for the treatment if the moderator variable equals zero.
For the analyses between t=1 and t=2, the N is as follows.[1] Self-confidence: 224, Contacts: 224, Couple: 232, Education: 224, No debts: 232, Part-time work: 231, Country: 232.
For the analyses between t=0 and t=2, the N is as follows.[2] Self-confidence: 224, Contacts: 224, Couple: 224, Education: 224, No debts: 224, Part-time work: 223, Country: 224.

Appendix 11 – Data management

Declaration Data Management PhD Thesis
Radboud Social Cultural Research, Radboud University

Section A. Primary data / information

For my thesis I have collected **primary** data / information.

Yes X → Complete section A.
No ☐ → Go to section B.

I declare that

A1.	The data for my thesis are obtained with the consent of informants / respondents.	Yes/~~No~~
A2.	Privacy sensitive data / information is encrypted and is stored on a protected computer or server environment.	Yes/~~No~~
A3.	The data / information is securely stored for reasons of scientific integrity at least for 10 years after finishing PhD research.	Yes/~~No~~
A4.	Anonymized data / information is registered in a well-known data repository system (Research Data Repository, DANS-KNAW).	~~Yes~~/No/~~n.a~~.
A5.	Access to anonymized data / information is arranged referring to the FAIR principles of data management.	~~Yes~~/No/~~n.a.~~

Section B. Secondary data / information

For my thesis I have used data / information **collected by other researchers.**

Yes ☐ → Complete section B.
No X → Go to section C.

I declare that

B1.	The data / information is obtained legitimately.	Yes/No
B2.	Non-public or secured data / information is stored on a protected computer or server during research.	Yes/No
B3.	The data / information is not shared with third parties, and has been treated in accordance with the agreements made with the information provider	Yes/No

Section C. General

I declare that

C1.	A short methodological justification, and/or the syntax and method of data / information processing is deposited in a so-called 'publication package'.	Yes/~~No~~
C2.	It is not possible to link data / information in publications to individuals (except with explicit consent).	Yes/~~No~~
C3.	The data / information is analyzed in a trustworthy manner and is not been deliberately manipulated toward certain outcomes.	Yes/~~No~~

Signature

Name PhD Candidate	János Betkó
Title Thesis	Effects of welfare policies based on trust and unconditionality: a social experiment with recipients of social assistance in Nijmegen
Date	May 22, 2022
Signature:	

Additional statement on the data management declaration:
In the declaration above both A4 and A5 cannot be confirmed. In accordance with the 'comply or explain' principle, the explanation for this is given here. The data used in this thesis is both owned by a third party (the municipality of Nijmegen) as well as both confidential and sensitive. The aim is to anonymize the dataset as much as possible to make it public. This will require a substantial time investment as well as the cooperation (and ultimately: permission) from the municipality of Nijmegen, and is not expected to be finalized before 2023. Researchers who want to inspect the dataset with a legitimate purpose before it has been made public can contact j.betko@nijmegen.nl to see if partial access can be arranged.

Na- en dankwoord

Nawoord

Pfff, wat een trip. Het voordeel van een onverbeterlijke optimist zijn die vrijwel alles leuk en interessant vindt, is dat je dan “ja, leuk, ga ik doen!” roept tegen, bijvoorbeeld, promoveren, naast je baan, in een heel ander vakgebied dan waar je ooit, lang geleden, in bent afgestudeerd. En dan zit je opeens weer in de collegezaal. Tussen de eerstejaars, ‘Statistiek A’ te leren. Leer je onderzoek doen, en schrijf je wetenschappelijke publicaties, en werk je met mensen in het hele land samen om kennis op te doen over ‘de bijstand’.

Het nadeel van een onverbeterlijke optimist zijn die alles leuk en interessant vindt, is trouwens dat je bij veel te veel dingen “ja, leuk, dat ga ik doen!” roept. Zoals promoveren, deels naast het werk, in een vakgebied waarin je geen vooropleiding hebt, en terwijl je een jong gezin hebt, en weinig slaap. Daarmee kun je je soms meer werk op de hals halen dan, strikt genomen, verstandig is.

Aanleiding voor deze hele rit was mijn op dat moment nieuwe baan bij de gemeente Nijmegen, die startte in het voorjaar van 2015. Ik werd beleidsmedewerker op het gebied van de bijstand, en dat was op zichzelf al spannend want ik wist niets van die bijstand[99], en was ook nooit beleidsadviseur geweest. Eén van de redenen waarom ik solliciteerde was dat ik toevallig had gehoord (dank aan toenmalig raadslid Lisa Westerveld) dat er wellicht interessante dingen gingen gebeuren in Nijmegen op dit vlak, omdat de gemeenteraad wilde experimenteren met een ‘lokaal basisinkomen’. En hoewel ik geen stellige mening over het basisinkomen had, of heb, vond ik dat wel een fascinerend onderwerp om me mee bezig te houden.

Dus ik solliciteerde, werd aangenomen en werd kort daarna ‘uitgeleend’ aan enkele raadslieden. Voor hen mocht ik informatie bij elkaar gaan scharrelen: wat mag van de (Participatie)wet, hoe duur zou zo een experiment ongeveer zijn, welke onderzoekers zouden kunnen en willen helpen bij het wetenschappelijk onderzoek, wat vinden het ministerie en andere gemeenten er van, et cetera. De bal ging rollen, van beleidsmedewerker werd ik (ook) projectleider van het experiment, en omdat ik ook verantwoordelijk werd voor de begroting werd “hoe duur wordt het onderzoek” opeens ook een relevante vraag. En toen bleek zo’n buitenpromotie, deels in eigen tijd, opeens ook erg aantrekkelijk voor de

99 Nou ja, wel van het gelijknamige Kollektief Kafé op de Van Welderenstraat, waar ik vroeger op woensdagavond regelmatig te vinden was.

gemeente vanuit financieel perspectief. Goedkoper voor de burger, handig voor de gemeente om nieuw ontwikkelde kennis direct in huis te hebben, leuk voor mij (en natuurlijk *fantastisch* voor de afdeling sociologie die gratis en voor niets mij vier jaar in huis kregen – al viel dat laatste enigszins in het water door een pandemie, die noopte tot thuiswerken).

En zo geschiedde. Ondanks de soms lastige data, en het regelmatig tot 's avonds laat achter de computer zitten: het was het waard. In de ruim vijf jaar dat ik hier mee bezig ben geweest heb ik heel veel mogen leren, ben ik fantastische mensen tegengekomen met wie ik heb mogen samenwerken, en heb ik met dat alles bij mogen dragen aan 'the greater good' - in dit geval kennisontwikkeling én een aantal mensen in de bijstand het leven iets makkelijker maken.

Dan ben je zo een paar jaar verder, en heb je opeens een boek geschreven vol met onderzoek. Misschien is het mooiste nog wel: het is nog niet eens helemaal voorbij. Aangezien ik één dag in de week nog steeds aan de universiteit ben verbonden, kan ik, hopelijk in een iets relaxter tempo, nog steeds onderzoek blijven doen naar (onder andere) dit mooie en belangrijke onderwerp.

Dankwoord

En dan rest me het bedanken van heel veel mensen. En dat is heel lastig, want er zijn zó verschrikkelijk veel mensen en organisaties met wie ik heb samengewerkt, die betrokken waren bij dit experiment en bij de soortgelijke experimenten in het land, dat ik geheid mensen ga vergeten. Dus vooraf een algemene "dankjewel" aan alles en iedereen die heeft geholpen en/of me heeft gesteund op deze trip, en die heeft bijgedragen aan het mogelijk maken van dit experiment, hoe bescheiden je bijdrage ook was.

Om het toch iets specifieker te maken, wil ik beginnen met het bedanken van mijn (co)promotoren: Peer Scheepers, Niels Spierings en Maurice Gesthuizen. Jullie hebben best een gok genomen mij hier aan te laten beginnen, wetende dat ik qua sociaalwetenschappelijke methoden nog wel één en ander bij te spijkeren had (zeg maar, vanaf het nulpunt). Maar het is goed gekomen. Ik vond het reuze interessant om van jullie te leren, onder andere over hoe je onderzoek doet en hoe je een wetenschappelijk artikel schrijft. Hartelijk dank! Een extra shout-out naar Niels voor het 5 jaar lang, vrijwel 24/7 bereikbaar en beschikbaar zijn voor vragen via de chat – en als het moest ook niet te beroerd was thuis samen achter een beeldscherm te gaan zitten met kop koffie of (als het echt moest) een glas whisky. Als iedere promovendus een dergelijk begeleider had zou er een stuk minder tijd aan promoveren worden besteed, vermoed ik zo.

Om in de categorie ‘wetenschappers’ te blijven, ook dank aan de andere collega’s bij de afdeling Sociologie van de RU met wie ik heb gesproken, geluncht, gekletst over van alles en nog wat, en van wie ik het nodige heb geleerd. Inmiddels ben ik begonnen met één dag in de week werken bij de afdeling Bestuurskunde. Ook dank aan de collega’s daar met wie ik heb kunnen sparren over mijn onderzoek, specifiek Stéfanie André met wie ik ook naar de ACES 2022 conferentie ben geweest om (samen met Lincy Scholten) wat resultaten van dit experiment te presenteren. Verder uiteraard ook veel dank aan alle mensen van het Landelijk Overleg Experimenten Participatiewet (LOEP), waarbij ik Timo Verlaat speciaal wil noemen omdat hij zo vriendelijk was mijn eerste paper (The Who and the Why) tegen te lezen en van nuttig commentaar te voorzien: thnx man! Ook dank voor de fijne samenwerking aan de mensen van het CPB, de begeleidingscommissie van ZonMw, en de onderzoekers en medewerkers van Labyrinth onderzoek en advies, die het veldwerk verzorgd hebben.

En dan was er ook nog heel veel ‘overheid’ betrokken bij dit onderzoek. Ik wil graag beginnen met mijn eigen gemeente, en werkgever, de gemeente Nijmegen. Toen ik de eerste keer voorzichtig opperde bij een leidinggevende dat het een optie was dat ik het wetenschappelijk onderzoek deels zelf zou kunnen doen, als deel van een buitenpromotie, was de reactie: “wat leuk dat je jezelf zo wil ontwikkelen!” En hoewel ik sindsdien toch een flink aantal leidinggevenden heb gehad: de support was en bleef onvoorwaardelijk. Het tekent de gemeente, en hoe er met werknemers en professionalisering wordt omgegaan. Ik wil dus graag alle programmamanagers, afdelingshoofden, bureauhoofden en directeuren bedanken die hier aan bijdroegen en - dragen. Het experiment zelf is opgezet in een projectgroep Experiment Participatiewet: alle leden daarvan ook heel hartelijk bedankt, ik vond de samenwerking fantastisch. Zonder jullie inzet en bijdragen (en de regelmatig gemaakte extra uren in avonden en weekenden) was het experiment überhaupt nooit van de grond gekomen. Ook wil ik alle betrokken wethouders en raadsleden bedanken die zich voor dit experiment hebben ingezet; mijn collega’s in team Werk & Inkomen; alle mensen uit de uitvoering van de afdeling Zorg & Inkomen die hebben bijgedragen onder andere aan de werving van deelnemers; de stagiairs die afstudeeronderzoek hebben gedaan naar dit experiment; de financials die hebben bijgedragen aan het krijgen van de Europese subsidie voor dit onderzoek; en alle anderen die al dan niet zijdelings betrokken waren.

Niet helemaal ‘gemeente Nijmegen’, maar toch daar heel dichtbij: dank aan de collega’s van het WerkBedrijf Rijk van Nijmegen. Jullie meedenken in het begin, de professionele uitvoering van de intensieve begeleiding en het enthousiasme waren heel waardevol. Ook wil ik de collega’s bij de andere gemeenten bedanken: alle beleidsadviseurs, projectleiders, en anderen die in de andere experimenteergemeenten hard gewerkt hebben om eenzelfde experiment te organiseren. Ik heb veel gehad aan het onderling sparren en het elkaar op de hoogte houden, en plezier beleefd aan de samenwerking. Dat geldt ook voor de collega's bij SZW: daar hebben ook veel mensen zich met dit onderwerp beziggehouden, en ook die samenwerking was goed.

Heel veel dank aan beide paranimfen, Lincy Scholten en Marieke Selten: niet alleen voor het vervullen van deze rol en het helpen bij de voorbereiding van de verdediging, en alles wat daar bij komt kijken, maar ook voor jullie bijdragen aan het experiment en het onderzoek. Marieke vooral aan het begin, bij het nadenken over een goed design, maar ook later in het experiment heb ik er veel aan gehad om met je te sparren over onder andere statistiek. En Lincy voor het excellente werk tijdens je onderzoeksstage, en de interviews die je daarvoor hebt afgenomen - deze zijn een verrijking geweest voor het onderzoek.

Verder ook dank aan alle familie, vrienden en kennissen, die de afgelopen jaren regelmatig hebben moeten luisteren naar mijn geklets over mijn onderzoek - want als je bezig bent met zo'n proefschrift vind je dat al gauw Heel Belangrijk, en kun je er al dan niet abusievelijk van uit gaan dat anderen dat ook vinden. Bij een vrij intensief traject als een buitenpromotie is het des te belangrijker om te ontspannen, of dat nou is door uitjes met familie, door te gamen, te sporten, een concert of festival te bezoeken, of een pilsje of bak koffie te drinken met leuke mensen. Extra shout-out naar Judith, voor het nalezen van mijn synthese op 'fatsoenlijk Engels'.

Tot slot wil ik mijn gezin bedanken. Hester, Ingmar, Erwan. Jongens: jullie hebben geloof ik niet héél veel last gehad van wat ik 's avonds allemaal nog op de zolderkamer zat te doen, omdat jullie sliepen. Maar nu heeft papa weer tijd om als jullie slapen mooie Lego- of Duplobouwwerken voor jullie te maken. Hester, eh, ja, jij hebt daar wél soms wat last van gehad, omdat ik de neiging heb vrij hard op mijn toetsenbord te meppen, zeker wanner ik enthousiast of geïrriteerd ben. Dat matcht in een gehorig huis niet altijd met 'slapen onder de studeerkamer', bij dezen, sorry! Het was voor de wetenschap enzo. Als ik nu weer heel hard op zolder op mijn toetsenbord zit te meppen, is dat waarschijnlijk niet meer omdat een reviewer iets lelijk heeft gezegd over een paper, maar omdat ik in een game met een monster aan het vechten ben.

Dit proefschrift draag ik op aan de deelnemers: zonder hen geen experiment. Ook al waren de voorwaarden van het experiment gunstiger dan onder het reguliere beleid: het gaat over je inkomen, waarvan je moet voorzien in je levensonderhoud. Daarmee willen experimenteren is toch best een ding, want je weet wat je hebt en niet wat je er precies voor terugkrijgt. Dus voor hen nog een keer een welgemeende "dank!", en ik hoop dat de informatie uit dit experiment de overheid in staat stelt beter bijstandsbeleid te maken in de toekomst, voor iedereen.

About the author

János Betkó graduated as MA in history in 2007. Since then, he worked as executive board member of the National Union of Students, auditor in higher education quality assurance and policy advisor on the subject of welfare for the municipality of Nijmegen. In his free time he, among others, wrote blogs, co-authored a book on the Dutch higher education act and participated in a collective that trained people and organizations on how to write and to communicate. Writing a PhD was not on his bucket list, but when the municipality of Nijmegen decided to do this social assistance experiment, for which extensive academic research was mandatory, the opportunity was too good to pass up. He wrote this dissertation as an external PhD student at the department of Sociology (Radboud Social Cultural Research, Radboud University). At the moment of writing, he coordinates the broad topic of 'countering social insecurity' for the municipality of Nijmegen, and works one day per week as an Assistant Professor of Practice for the department of Public Administration, Nijmegen School of Management, Radboud University.

This study provides an analysis of a social experiment with social assistance, conducted in the city of Nijmegen (The Netherlands), between 2017 and 2020. The particular focus of this study was on the impact of alternative, less conditional policies. As a reaction on national social policy deemed too strict, the local government designed an experiment with more unconditional social assistance, giving recipients more trust and autonomy relative to the normal regime. It analyzes how the experimental treatments effected participants health, (social and political) trust and participation in society (for instance through volunteering and informal care). Special attention is given to the question if specific effects can be found for subgroups, based on characteristics like education level or migration background. Among the findings are positive treatment effects on political trust at the local level, and that it are predominantly the more vulnerable groups which are helped better by the alternative treatments, compared to the regular regime.