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How Welfare Policies Can Change Trust – A Social Experiment Assessing the Impact of Social Assistance Policy on Political and Social Trust

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Abstract: While there is a substantive literature on the link between welfare states and individuals' trust, little is known about the micro-linkage of the conditionality of welfare as a driver of trust. This study presents a unique randomized social experiment investigating this link. Recipients of the regular Dutch social assistance policy are compared to recipients of two alternative schemes inspired by the basic income and based on a more trusting and unconditional approach, testing the main reciprocity argument in the literature: a trusting government will harvest trust from welfare recipients in return. Particularly trust in local government – the level at which the experiment was implemented – increases among recipients of the alternative treatments. Subsequently, we innovatively theorize and test rigorously which mediating mechanisms might explain this increase. Policy evaluation, social integration, and psychological well-being are studied in this respect. Of these, the only underlying mechanism proven to mediate the treatment effect in local political trust, is citizens' satisfaction with policy.

Keywords: trust, welfare state, public policy, public sector reforms, basic income

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 et al., 2007).

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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 et al., 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; Christensen & Lægreid, 2005; Edlund, 1999; Kumlin & Rothstein, 2005; Rothstein & Uslaner, 2005)

Similarly, a positive connection between socio-economic security and trust has been established in industrialized democracies (Newton et al., 2018; Paxton, 2007; Whiteley, 1999; cf. Spierings, 2019). Poverty makes people less trusting (Newton, Stolle 2018; 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 who rely on welfare states' safety nets by receiving welfare benefits that should 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 et al., 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 (Betkó, 2018; Betkó et al., 2019; Groot et al., 2019). 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 relying more on autonomy for the recipients and less on control from the government than the regular regime 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 et al., 2017; 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 et al., 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 (Butterworth et al., 2009; Haushofer & Fehr, 2014; 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.

2 Theoretical Background and Hypotheses

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.[1] Of these studies, Kumlin and 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 to receive trust (Bohnet et al., 2001; Fehr & Gächter, 2000; Frey & Jegen, 2001). In our experiment this providing of trust manifests itself in two ways: less control on its

1 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.

recipients by the government and more self-chosen support and autonomy for its recipients.

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., 2019). 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').[2] 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 (Delsen, 2019; De Wispelaere & Yemtsov, 2019; Groot et al., 2019; McFarland, 2017). 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

2 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.

locally, at the level of the municipalities. Thus, the experiment we conducted, is an experiment with national social 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 Appendix 6. For more information on the ethical concerns on studying human subjects: see Appendix 7. 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 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 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.

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 (Bohnet et al., 2001; Fehr & Gächter, 2000; Frey & Jegen, 2001). Below we discuss the alternative treatments and expected effect on trust.

The first alternative treatment group is exempted from all obligations on reintegration (hereafter: 'exempted'). Under the regular treatment, social assistance recipients are obliged to actively seek employment, accept any job offered, commute up to 3 h per day and can be required by the local authorities to perform

labor without additional pay.[3] 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.[4]

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 from reciprocity which is a core theoretical assumption, the coached group is likely to not only experiences 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.

3 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.

4 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.

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 (Christensen & Lægreid, 2005; Freitag & Traunmüller, 2009; Glanville & Paxton, 2007). 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*

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 et al., 2015). 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; Kumlin et al., 2018; Rothstein & Uslaner, 2005). 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. Glanville et al., 2013; Putnam, 2000; 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 et al., 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 (Kumlin & 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 Data & Methods

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.[5] 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

5 A first wave of participants entered the experiment the 1st of December 2017, a second wave on 1st of April 2018. Both waves had the follow-up survey in November/December 2018.

(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.[6] 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[7] 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.[8] 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.[9] 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.[10] The trust scores of the people of whom we missed the data on the second survey are hardly lower than on t0, but slightly

6 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.

7 It was labeled the 'proef met vertrouwen' (colloquial Dutch for "experiment with trust-based socal assistance").

8 The most important exclusion criteria was age.

9 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.

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

lower for social trust.[11] Though there is little reason to assume drop-out bias, we did control for this (see Section 3.5). The descriptive statistics of the variables included in our models can be found in Table 1. It should be noted that our study contains three trust indicators as dependent variables, with a slightly different *N*. Table 1 is based on the cases for local political trust, since this is our primary focus.

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).[12] 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 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

11 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.

12 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 Statistics (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.

Table 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	0.089	2.11
Index contacts	0.00	5.00	4.20	1.02
Index contacts w2	0.67	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	
Education				
Lower education			0.10	
Lower middle education			0.11	
Higher middle education			0.34	
Tertiary education			0.37	
Education unknown			0.07	

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

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.

3.4 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 2), 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 et al., 2015), and is a combination of the four items "I experience these rules and regulations as a 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 2). 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?"[13] These questions 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.

3.5 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. Hyggen, 2006; Lee, 2013; Rothstein & Uslaner, 2005). 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

13 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.

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).

3.6 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 9).

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:

$$\Delta Y_{t1} - Y_{t0} = \beta_0 + \beta_1 Y_{t0} + \varepsilon \tag{1}$$

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

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

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

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

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 (1986) 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 1 of 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 (Hayes, 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.

4 Results

4.1 Levels of Trust

The results of our experiment on trust are described in Table 2 and 3. In Table 2, the descriptive outcomes are shown, while Table 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).

Table 2: Trust scores on $t = 0$ and $t = 1$ per group.

Dependent variable per group, [scale], (*N*)	*T* = 0		*T* = 1		Δ trust
	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

The absolute raw scores we show in Table 2 – they seem to correspond with the current Dutch average (Dekker & den Ridder, 2020).[14] 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.

4.2 The Treatments' Impact on Developments in Trust

In Table 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 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.

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

Table 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	[b]	0.03		0.14	
Coached	0.26	[b]	0.04		−0.06	
$T = 0$ trust value	−0.61	[a]	−0.26	[a]	−0.46	[a]
Gender						
Male	Ref		Ref		Ref	
Female	−0.03		−0.04		0.44	[c]
Age	0.00		0.00		−0.00	
Education						
Primary	Ref		Ref		Ref	
Lower secondary	0.28		0.06		1.45	[a]
Higher secondary	0.17		−0.08		1.44	[a]
Tertiary	0.26	[c]	0.05		1.58	[a]
Unknown	0.08		−0.01		1.48	[a]
Country of birth						
Netherlands	Ref		Ref		Ref	
Non-western	0.29	[a]	0.26	[b]	−0.47	[c]
Western	0.21		0.03		−1.28	[b]
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	[a]	0.53	[b]	1.68	[b]
N	248		230		259	
R	0.60		0.41		0.53	

[a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.10$.

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 (Appendix 8), indicating that a lack of effects is not due to over-controlling.

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 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 3. The model specification corresponds with model 3 above, now taking the mediators from Table 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 3). 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 4, 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

Table 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[b]	0.17[c]	0.19[b]	0.18[c]	0.14
Coached	0.26[b]	0.24[b]	0.26[b]	0.27[a]	0.25[b]
$T = 0$ trust value	−0.61[a]	−0.61[a]	−0.61[a]	−0.63[a]	−0.64[a]
Policy evaluation					
Satisfaction		0.15[a]			0.17[a]
Perceived effectiveness		−0.01			−0.03
Social integration					
Part society			−0.03		−0.01
Contacts			0.02		0.04
Social & mental WB					
Subj. well-being				−0.07[b]	−0.08[b]
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[c]	0.27	0.28
Higher secondary	0.17	0.11	0.18	0.16	0.10
Tertiary	0.26[c]	0.22	0.26[c]	0.27[c]	0.24[c]
Unknown	0.08	0.05	0.09	0.06	0.02
Country of birth					
Netherlands	Ref	Ref	Ref	Ref	Ref
Non-western	0.29[b]	0.33[b]	0.28[c]	0.29[a]	0.34[a]
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[a]	1.40[a]	1.39[a]	1.48[a]	1.58[a]
N	248	248	248	248	248
R	0.60	0.62	0.61	0.62	0.65

Fields shows the B and significance, where: [a]$p < 0.01$, [b]$p < 0.05$, [c]$p < 0.10$.

exempted group; for the coached group the decrease is more modest.[15] 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.[16]

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 find an indirect effect via policy satisfaction.[17] 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.

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15 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).

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

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

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 5 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[18] via policy satisfaction exist, though only substantially for the exempted group, while perceived effectiveness does not. Hypotheses 3 and 4 are fully rejected.

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

18 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.

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 et al. (2015), 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

& 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): 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 & Shroder, 2004) and we should be careful in generalizing the found policy effects to all people on social assistance, as well as other types of welfare in other (national) context. 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 & Shroder, 2004). 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 and Rothstein (2005) – 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 et al., 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.

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