Masaryk University

Faculty of Economics and Administration

# The impact of improved housing on economic decision-making, well-being and perceptions: Evidence from a randomized controlled trial

Habilitation thesis

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Brno, 2020

# Abstract

The habilitation thesis presents three studies that take advantage of a unique randomized controlled trial (RCT), a Brno's Housing First project for Families, which substantially improved housing conditions of treated families. The first study is motivated by growing evidence that poverty affects people's preferences and cognitive abilities in a way that may lead them to make bad decisions. In a lab-in-the-field experiment, it elicits risk preferences and time preferences of the participants of the Housing First project and measures their sustained attention. It finds that improved housing conditions do not impact any of these outcome variables, which is in line with the recent evidence from the US.

The second study combines the Housing First RCT with two laboratory experiments to study how the Housing-First participants' trustworthiness and ability to concentrate is perceived by students in the laboratory. The experimental design enables us to disentangle the effect of housing conditions from the effect of housing history. While low-quality housing has a negative effect on the expected trustworthiness, but no impact on the perceived ability to concentrate, people living in good-quality housing are perceived as less able to concentrate, but equally trustworthy, when their history of low-quality housing is revealed to students.

The third study is motivated by the evidence from cash-transfer RCTs which show that being untreated may have adverse effects on the affected participants' psychological well-being. The study finds that while the Housing First RCT resulted in a large increase in life satisfaction and psychological well-being for those treated, the values reported by the untreated remained stable. In addition, assignment to the control group did not have any negative effects on the participants' pro-social preferences or on their perceptions of others' pro-sociality. These results suggest that, at least in the context of rehousing experiments with scattered housing, being untreated in an RCT does not result in any substantial adverse effects on life satisfaction or pro-sociality.

## Keywords

Housing First, Housing Conditions, Risk Preferences, Time Preferences, Attention, Trustworthiness, Homelessness, Random Assignment, Life Satisfaction, Prosociality

# Declaration

I hereby declare that the habilitation thesis titled *The impact of improved housing on economic decision-making, well-being and perceptions: Evidence from a randomized controlled trial* is my own work and that I have cited all of the literature and other expert resources therein in accordance with applicable legal regulations, the Internal Rules and Guidelines of Masaryk University, and the internal managing acts of Masaryk University and the Faculty of Economics and Administration.

# Acknowledgements

I would like to thank Barča for her interest and support and Anička for being my joy. I am grateful to Rostislav Staněk for invaluable help with this research project. I would like to thank Štěpán Ripka and his colleagues from the *Platform for Social Housing* and the city of Brno for making the Housing First for Families in Brno possible. This thesis benefited from discussions with Maria Bigogni, Katarína Čellárová, Eliška Černá, Petr Kubala, Štěpán Mikula, Štěpán Ripka, Tommaso Reggiani, and Petra Závorková. I am grateful to Katarína Čellárová, Emília Ferencová, Lujza Gírethová, Jana Kočková, Eva Kopčíková, Martina Mrázová for excellent research assistance. Finally, my special thanks are extended to Zdeněk Tomeš and Jiří Špalek for providing stable and collaborative environment supporting research.

# Contents

# Chapter 1

# Introduction

## 1.1 Trapped in poverty

Ever since Adam Smith's *The Wealth of Nations*, the question of how individuals and nations become wealthy has been central to the economic profession. More recently, economists have become intrigued by the related puzzle as to why so many people remain so poor and unproductive despite being surrounded by an unprecedented rise in affluence and productivity (Banerjee and Duflo, 2007). One potential solution to this puzzle is provided by the concept of *poverty traps*, a set of self-reinforcing mechanisms which causally link *current* poverty to *future* poverty (Banerjee, Banerjee, and Duflo, 2011; Kraay and McKenzie, 2014; Barrett, Garg, and McBride, 2016; Haushofer and Fehr, 2014). The literature describes multiple mechanisms that work at macro, meso, and micro levels. This thesis focuses on behavioral (microeconomic) poverty traps.

One set of such mechanisms is based on the idea that preferences are endogenous (Bowles, 1998), i.e. may be shaped by various factors, including financial wealth or living standards. Tanaka, Camerer, and Nguyen (2010) show that poor Vietnamese farmers are more risk averse and have more present-oriented time preferences than their richer peers. Banerjee and Mullainathan (2010) employ a theoretical model to show that poor people spend a lower share of their income on temptation goods, which are goods that generate positive utility for the individual when consumed now, but not for the self that anticipates their consumption in the future. Shah, Mullainathan, and Shafir (2012) provide experimental evidence suggesting that poverty-induced scarcity makes people reallocate their attention in ways that justify overborrowing. Presenting a randomized controlled trial in Mosambique, Laajaj (2017) shows that an improvement in economic prospects motivates people to plan further ahead and, as a result, to accumulate more assets. All these mechanisms could result in insufficient investment in risky activities that involve current costs and future benefits. Most importantly, this category includes investment in the human or physical capital necessary to become productive enough to break out of the *vicious circle of poverty.*

Other mechanisms operate via the effect of poverty or scarcity on cognitive abilities or mental health. Mani et al. (2013) and Shah, Mullainathan, and Shafir (2012) provide experimental evidence in support of the theory that poverty impedes cognitive performance through capturing attention and triggering intrusive thoughts: People who think about how to satisfy basic needs in the short run have less mental capacity left for other mental processes. Lower cognitive abilities might generate unwise decisions, which keep the decision-maker poor. In a similar vein, Haushofer and Fehr (2014) document the role of poverty-induced stress and negative affective states and Barrett, Carter, and Chavas (2019) provide a theoretical case for the role of depression in keeping people trapped in poverty.

The impact of a shock in living standard on preferences and cognition is studied in *research question 1* (RQ1), which will be discussed in more detail in Section 1.3.

All these factors explain why poverty might be perpetuated by inefficient or erroneous decision-making. However, other people's decisions might contribute to marginalization of the poor, too. One area of interest is discrimination in the job market. The workhorse method for studying labor-market discrimination is the correspondence study. In correspondence studies researchers respond to real-life job openings by sending fictitious job applications, which differ only in one randomly assigned aspect, such as race, address, background, qualification, etc.

By measuring whether the callback rate differs between the experimental groups, these studies provide strong causal evidence of hiring discrimination (at least at the level of callbacks) based on minority race, religion, and citizenship status (Bertrand and Mullainathan, 2004; Baert, 2018), which are often correlated with wealth. The evidence on other correlates of wealth is mixed. While Bertrand and Mullainathan (2004) find that applicants living in less affluent neighborhoods are less likely to be contacted by potential employers, Tunstall et al. (2014) show that these effects might be driven by longer commuting distances from low-income neighborhoods and not by the neighborhood's affluence *per se*. Similarly, Siddique (2009) finds that upper-caste Indian applicants have higher callback rates than other castes, while Banerjee et al. (2009) finds no difference between castes.

Besides job prospects, discrimination could also impact poor peoples' success in the housing market. Bonnet et al. (2016) show that living in a deprived suburb is related to a lower chance of getting an appointment for a housing vacancy. Homelessness, in turn, could lead to lower chances in the labor market, as Golabek-Goldman (2016) suggests, observing that homeless applicants face discrimination in the labor market.

The *research question 2* (RQ2) of this thesis is whether poor people living in bad housing conditions are perceived as less trustworthy and less concentrated (see Section 1.3 for a detailed introduction).

The literature on behavioral poverty traps is still emerging, and the existence of many mechanisms is disputed. Nevertheless, this research agenda is of substantial importance for public policy, because it has the potential to improve the design of development strategies. If an improvement in financial wealth, living standards or housing conditions has the potential to break the behavioral shackles keeping people in poverty, a relatively small sum of money could set households or whole communities on growth trajectories. In line with this reasoning, the Millennium Villages project planned to spend \$6,000 per household to implement a complex package of interventions designed to lift selected African households out of extreme poverty (Clemens, 2012). It also provides additional justification for supporting institutions that improve incentives to save or improve access to credit markets (e.g. through microfinance loans).

## 1.2 Randomized controlled trials

Randomized controlled trials (RCTs) represent gold standard evidence in scientific research. They are extensively used in the medical and natural sciences and have recently gained prominence in social-science research. They have become commonplace in our field of interest, the study of microeconomic determinants of poverty, and more generally in development economics (Banerjee and Duflo, 2009).

The main benefit of this experimental method consists in its capacity to identify causal relationships reliably in situations in which program participants differ from nonparticipants in numerous ways. An RCT enables us to measure program effects that would be hard or impossible to evaluate otherwise. Interestingly, the existing literature has shown that our prior (theoretical) intuitions about the effects of particular components of a program (experiment) is often not very helpful. For example, it has been shown that cutting the teacher-student ratio in half has no effect on educational outcomes if implemented alone, but a significantly positive impact if the school committee receives money to monitor the extra teachers (Duflo, Dupas, and Kremer, 2007). The proper experimental assessment of program components is crucial for learning which interventions are helpful and which are not.

The RCT methodology has also raised several concerns (Banerjee and Duflo, 2009). The first of these relates the issue of *generalizability* or *external validity*, in other words whether the results of the experiment would hold if it was implemented in different circumstances, for example in a different country or with a different demographic group. The obvious solutions to this problem are to replicate the same design in various different settings or to run large-scale experiments.

Another issue related to the scale of the experiment is the *equilibrium effect* (Banerjee and Duflo, 2009). This implies that experimentally testing a particular program on a medium-sized representative sample might not very well predict the outcomes of the same program when scaled up to the national level. This is because the general equilibrium impacts of a nation-wide treatment might differ from the impacts of a medium-sized experiment. Take the example of an educational intervention that provides low-income families with vouchers for private-school education. Once the program is scaled up to the national level, private schools might not be able to provide the same quality of education if the number of the program students per school substantially increases, and the returns to education might be different if the schools increase the supply of students with better education (e.g. tougher competition for university places). One solution is to combine RCTs with micro studies of large-scale policy shifts (Acemoglu and Angrist, 2000; Duflo, 2004).

A third problem, which is most relevant for research question 3, is bias related to randomization. One form of randomization bias is the Hawthorne effect (Landsberger, 1958), according to which participants' behavior changes once they learn that they are observed. These effects, however, arise whenever people know that they are studied. People may behave differently on purpose, to preserve their self-image, to make the treatment a success or a failure, or for other reasons. For example, Levitt and List (2007) discuss possible effects of scrutiny on behavior in social-preference laboratory experiments.

Further instances of the Hawthorne effect are related to randomization itself. These effects have been observed, for example, in cash-transfer experiments. Baird, De Hoop, and Özler (2013) incentivized school attendance for randomly selected school girls in Malawi. They found that untreated girls living in areas where some girls were treated reported a substantial increase in psychological distress relative to girls in non-treatment areas. Haushofer, Reisinger, and Shapiro (2015), who offered unconditional cash transfers to poor households in rural villages in Kenya, observed a substantial reduction in life satisfaction among untreated neighbors. In both these cases, these negative effects disappeared after the cash transfer programs were terminated.

One way to mitigate this issue is to randomize at the level of locations, and justify the randomization by the budget or administrative capacity (Banerjee and Duflo, 2009). People in developing countries are used to the fact that only some areas receive treatment, while others do not. If the subjects receive an explanation that the treatment was not provided due to financial constraints on the side of the government or the NGO, the control areas typically consider the randomization a fair way of allocating limited resources. If, on the other hand, randomization is made within locations, as in Baird, De Hoop, and Özler (2013) and Haushofer, Reisinger, and Shapiro (2015), the absence of treatment is more painful for the control group participants. Not only could this have an impact on their well-being or health, it might also affect the treatment effects measured in the experiment.

This issue will be addressed in *research question 3* (RQ3), which investigates the effects of an RCT on the life satisfaction and pro-sociality of the untreated.

## 1.3 Research questions

This thesis focuses on one important aspect of poverty: substandard housing conditions. We use a housing project that provides higher quality housing to a randomly selected group of families. This allows us to identify the causal impact of housing on several outcomes of interest. Our data come from the Housing First project, which was launched in Brno (Czech Republic) in 2016. Out of the population of more than 400 families living in substandard housing, 50 families in were selected as the intervention group and 100 families as the control group. The intervention families were each awarded a long-term lease contract for a municipal flat, together with a package of social services. The control (or usual-care) families participated in the evaluation of the project but received no special housing assistance, only the standard range of social services provided by the government and municipality.

The rest of this section provides an introduction into the three research questions addressed in this thesis.

### RQ1: Do improved housing conditions impact preferences and attention?

As mentioned in the previous section, recent literature has reported that poverty affects preferences in a way that may contribute to people making the wrong economic choices. Compared to their more affluent peers, the poor are usually more risk-averse (Guiso and Paiella, 2008; Tanaka, Camerer, and Nguyen, 2010; Gloede, Menkhoff, and Waibel, 2015; Di Falco, Damon, and Kohlin, 2011), i.e. potentially less likely to engage in risky but profitable ventures, and less patient (Haushofer, Schunk, and Fehr, 2013; Tanaka, Camerer, and Nguyen, 2010), which could lead to lower investments in human and physical capital. Another suggested cause of poverty persistence is linked to cognitive abilities: Several studies have found that scarcity, whether due to actual or experimentally induced poverty, negatively affects cognitive function (Spears, 2011; Shah, Mullainathan, and Shafir, 2012; Mani et al., 2013). Lower cognitive ability could, in turn, lead to decision patterns that contribute to poverty.

This thesis uses the data from participants in the Housing First randomized controlled trial. The following chapter documents that the treatment significantly improved the quality and perceived stability of housing. The intervention also led to a significant increase in life satisfaction: Relative to the baseline levels, the treated participants' life satisfaction increased by 0.8 std. dev. This is a comparatively large effect. For example, unconditional cash transfers of an average value of $709 PPP to poor households in western Kenya (equivalent to two years of per-capita expenditure) increased recipients' life satisfaction by 0.17 std. dev. (Haushofer and Shapiro, 2016). This suggests that the treatment had a sizable impact on the living conditions of the families that participated in the project. This treatment can be seen as a permanent increase in income that is exclusively spent on housing.

The thesis measures the impact of improved housing conditions on risk preferences and time preferences. These preferences are elicited using multiple price lists adapted from Sutter et al. (2013), which can be easily administered in a field setting with time constraints and participants with potential cognitive limitations. In terms of cognitive abilities, sustained attention was measured for the following reasons: First, Mani et al. (2013) and Shah, Mullainathan, and Shafir (2012) consider attentional neglect (poverty capturing attention) to be the primary mechanism behind their results, so attention seems to be the main theoretical channel through which higher-order cognitive functions are influenced. Second, we believe that sustained attention is highly relevant in our setup, in which most of the participants are single mothers with small children. It is essential not only for activities related to the quality of upbringing but also for other important aspects of life, such as employment and contact with the authorities. Sustained attention is measured using the d2 test (Bates and Lemay, 2004).

Our data do not provide any evidence that improved housing conditions affect preferences or attention. While the thesis does not replicate the results of studies based on data from development economics (Tanaka, Camerer, and Nguyen, 2010; Mani et al., 2013), the results are in line with the evidence from the US (Carvalho, Meier, and Wang, 2016).

### RQ2: Are homeless parents perceived as less trustworthy or able to focus?

The homeless are one of the most economically disadvantaged groups in developed countries. Homeless families are especially relevant from the point of view of social policy, because of the potential adverse effects homelessness may have on children. With this research question, we study how homelessness affects how parents are perceived, which could be relevant for the economic and social well-being of the whole family. Homeless families in the Czech Republic either have no address or live in homeless shelters or private hostels, which may complicate their access to government services, housing and jobs (Golabek-Goldman, 2016; Poremski, Whitley, and Latimer, 2014).

The thesis investigates two different reasons for possible adverse outcomes of homelessness: people living in bad housing conditions could be perceived as less focused or less trustworthy. While these people's ability to focus could directly affect their labor-market outcomes, trustworthiness has a broader relevance: It will matter in all situations where the other side's outcome depends on the applicant or client's reciprocal actions. It might matter in the labor market, e.g. for gaining a supervisor's trust (Colquitt, Scott, and LePine, 2007) or in the housing market (Baldini and Federici, 2011).

We use laboratory experiments that exploit the exogenous variation in housing conditions created by the Housing-First RCT. In research quesion 2, we focus on a subset of families that lived in especially bad housing conditions before the start of the project; we refer to this housing as *private hostels* in this thesis.[1] The project families' *concentration performance* is measured using the d2 test of sustained attention (Brickenkamp, 1962) and *expected trustworthiness* as the amount the proposer expects to receive back from the receiver in a trust game (Berg, Dickhaut, and McCabe, 1995).

The task of the student subjects in the laboratory experiment is to guess the concentration performance and expected trustworthiness of an adult member of a family, which is characterized by its housing conditions and a few other variables (income, number of children). The pairing with the participants of the RCT enables us to separate the effect of housing quality from the effect of the bad signal provided by living in a hostel. We isolate the effect of better housing by comparing the expected outcomes of treatment- and control-groups participants from the same population. The students participating in the lab experiment are informed about typical housing conditions in the hostels (control group) and the municipal flats (treatment). In addition to that, they are informed that the treatment families used to live in private hostels and were then awarded housing in municipal flats without any additional conditions (screening). To measure the effect of the signal attached to their history of bad housing, we add an additional treatment group in which no information about the treatment families' housing history is provided.

The comparison of the participants who differ only in their actual housing

[1] In Brno, the hostels tend to be located in large buildings containing many housing units. The units typically consist of a single room, usually inhabited by one family. The kitchens, bathrooms, and showers are shared between many families. These hostels are typically run for profit by private firms.

conditions shows that moving people to municipal housing increases their expected trustworthiness, but not their perceived concentration performance. The comparison of the two groups living in flats (with and without information about their history of substandard housing) shows that withholding information about their housing history does not affect their perceived trustworthiness, but increases the concentration performance the student subjects expect them to have.

### RQ3: Does being untreated impact life satisfaction and pro-sociality

Research question 3 focuses on random allocation procedures in which participant assignment to treatment arms is not blind and in which participants would clearly prefer some treatment arms above others. Such procedures are frequently used in social science RCTs (see e.g. Fisher (2006), Harrison and List (2004), and Duflo and Kremer (2005)).

This thesis looks two possible outcomes of being assigned to the control (or business as usual) group. First, we test whether being untreated negatively affects the life satisfaction and psychological distress of the participants concerned. Second, we are interested in any effects the RCT might have on untreated participants' generosity and their perceptions of other people's pro-sociality.

The effects on life satisfaction and psychological distress might arise due to the inequality generated by the experiment. In the Housing-First RCT, the treated families experienced substantial improvement in their living standards, while control families did not. Several previous studies have found that a decline in relative economic status negatively affects psychological well-being (Mangyo and Park, 2011; Luttmer, 2005), and this has been shown to matter in the context of RCTs involving cash transfers (Baird, De Hoop, and Özler, 2013; Haushofer, Reisinger, and Shapiro, 2015). The impact on pro-sociality could be related to disappointment at being untreated: this leaves them relatively worse off. They might also consider the random allocation procedure to be unfair (Haushofer, Riis-Vestergaard, and Shapiro, 2019; Hillis and Wortman, 1976; Erez, 1985).

The Housing First RCT provides a suitable setting for our study: First, the existence of the project was public knowledge and the families in the control group were aware of the fact that other families were receiving housing and services through the project. Second, the treatment was substantial and clearly preferred to non-treatment. As documented in Section 2.3, it lead to significant improvements in housing quality, perceived housing stability, and life satisfaction.

The comparison between the values before and after the treatment does not reveal any effects on life satisfaction, psychological distress, or pro-sociality perceptions among the untreated. We also do not find any difference in generosity between the intervention and control families. Assuming that treatment does not reduce generosity, this provides suggestive evidence that there is no effect on generosity from being untreated. These results suggest that randomized interventions have no substantial adverse effects on the untreated, at least in the context of housing experiments.

This thesis continues as follows: Chapter 2 provides background information about the Housing First project in Brno and sets the project in a broader context by presenting some of the rationale behind housing interventions, introducing the staircase system and housing first interventions, and reviewing similar projects conducted in the developed world (mainly in the USA). The following three chapters provide detailed answers to research questions 1–3: Chapter 3 discusses the impact of housing on preferences and attention; Chapter 4 investigates the impact of housing on perceived trustworthiness and concentration performance; and Chapter 5 presents the impact of the Housing-First RCT on the untreated. The final chapter offers some closing remarks. The text is complemented by an appendix with a full set of experimental instructions and additional robustness checks.

# Chapter 2

# Housing

## 2.1 The importance of good housing

Substandard housing affects people's lives through several channels. There is an increasing body of evidence linking quality of housing to various aspects of physical health. Bad housing often lacks some of the standard standard features of modern housing, such as safe drinking water, hot water for washing, effective waste disposal, adequate food storage and absence of disease vectors (e.g. insects and rats). This contributes to the spread of infectious diseases, such as tuberculosis and respiratory infections. Damp and moldy housing contributes to chronic diseases such as asthma, other respiratory conditions, recurrent headaches, fever and sore throats. Cold housing has been related with higher blood pressure and cholesterol, contributing to strokes in acute cases, and is associated with lower general health status. Dirt, old carpeting and pest infestation contribute to allergic, neurological and hematologic illnesses. Exposure to toxic substances, such as tobacco smoke, nitrogen dioxide, carbon monoxide, lead and asbestos, can also cause a variety of health issues. Many of these problems are intensified by overcrowding. Inadequate housing conditions increase the risk of injuries (burns and falls), especially for children. A lack of cooking facilities may contribute to poor nutrition and reduced physical growth among children (Krieger and Higgins, 2002; Adamkiewicz et al., 2014; Patino and Siegel, 2018). However, these associations are based on studies that are for the most part relatively small-scale and many of which focus on a specific populations without an adequate comparison group. Proving any causal links between these factors is inherently difficult (Shaw, 2004; Slopen et al., 2018).

More closely related to our research, several studies have looked into the associations between housing conditions and mental health. Damp and moldy housing units are related to anxiety and depression (Hyndman, 1990) and worse mental health (Hopton and Hunt, 1996; Bentley et al., 2018). Mental health and psychological distress are related not only to housing quality, but also to housing stability (Bentley et al., 2018). Inadequate housing has also been found to be related to sleep quality (Simonelli et al., 2013; Chambers, Pichardo, and Rosenbaum, 2016). Crowded housing reduces the probability of long duration sleep (Chambers, Pichardo, and Rosenbaum, 2016). Children in crowded housing (some of whom share beds with other children) have irregular sleep patterns and shorter sleep times, which contributes to lower cognitive performance and worse behavioral health (Solari and Mare, 2012; Liu, Liu, and Wang, 2003). The quality and duration of sleep is also affected if the home environment is perceived as unsafe (Simonelli et al., 2015).

## 2.2 From the "staircase system" to housing first

In developed countries, it is the homeless that live in the most dire housing conditions. Given the importance of good housing conditions, governments in developed countries have been engaged in housing intervention for decades (Balchin, 2013; Schwartz, 2014; Balchin and Rhoden, 2019).

Much of the European homelessness provision at the local level relies on a so-called "staircase system" or "staircase of transition"[1], in which individuals or households who comply with certain requirements move into higher standard housing with greater privacy and autonomy and a lower degree of supervision and control (Busch-Geertsema, 2013; Sahlin, 2005). For example, they might

[1] A similar system in the US context is called the "continuum of care".

start in traditional low-standard shelters or hostels and subsequently move to temporary accommodation for specific groups, before being allocated training flats and eventually awarded permanent rental contracts for municipal flats. At each step they are expected to solve some of their ongoing problems, such as paying off debt, finding work, or tackling a substance abuse problem.

The staircase system has come under increasing criticism in recent decades. According to Sahlin (2005), numbers of homeless people are growing rather than decreasing in cities that use the staircase system. This is related to the fact that only a small proportion of homeless people meet the conditions to move up to the next step, and many are even downgraded to lower steps. As new clients continue to enter the system, this leads to overcrowding at the lower levels, and insufficient promotion to higher levels and to independent housing. Furthermore, the system itself creates some problems (Sahlin, 2005; Busch-Geertsema and Sahlin, 2007): For example, the skills learned in the lower levels (in a structured congregate setting) are often not useful for independent living; the system is complicated, many people get lost between different steps; and the clients typically do not have control over when and where they are placed. Furthermore, the frequent moves into different housing leads to stress and a loss of relational capital for those involved.

Housing First approaches were developed in response to the low effectiveness of the staircase system in providing a long-term solution to homelessness. Housing First not only provides the homeless with long-term, self-contained housing without the requirements of the staircase system, but traditionally offers also substantial and multidisciplinary social support. This approach, pioneered by the Pathways to Housing organization in New York, was originally intended to help homeless individuals with multiple and complex needs, usually with serious mental health and substance abuse problems (Busch-Geertsema, 2013), but was also used in several settings with homeless (shelter) families (see Section 2.4 for a detailed review of Housing First programs for families). Evaluations of these programs have repeatedly shown that even homeless people with complex support needs can sustain long-term tenancies.

## 2.3 Housing First for Families in Brno

Before Brno's Housing First project began, in 2015, there were 68,500 homeless people (8,158 children) in the Czech Republic. A further 118,500 people were living in insecure or substandard housing (MPSV, 2016). An increasing number of these households were receiving government housing supplements: the number of recipient households tripled from 23,500 households in 2010 to 74,000 households in 2014 (MPSV, 2015). Most families in receipt of the housing supplement (80% in 2014) were living in temporary hostels, which were typically privately-owned (Kuchařová et al., 2015). The project was conducted in Brno, which is the second largest city in the Czech Republic with around 400,000 inhabitants. Prior to the Housing First project, the Brno authorities had relied on the staircase system to help homeless individuals and families. The main problem with that system was a very low number of successful transfers to stable housing (though there were also other problems of the sorts described by Sahlin (2005) and Busch-Geertsema and Sahlin (2007)).

Brno's Housing First project for families began with a Family Homelessness Registry Week in April 2016. During this week, families in need of housing in Brno and consisting of at least one primary caregiver and one dependent child were invited to register for the project. Out of almost 600 registered families, 421 families living in private hostels, shelters, or facing other forms of homelessness passed the initial eligibility screening. These families lived in substandard housing conditions. In terms of size, 50% of these families inhabited housing units with a total area of less than 30m$^2$; 70% of the housing units were smaller than 36m$^2$; in 50% of families this meant that the per-person area was less then 7m$^2$. Most of these families had long-term experience of homelessness: 92% of them had been homeless for more than six months. The median length of their homelessness periods was eight years. As for the reasons behind their homelessness, 21% of the families had lost stable housing after one of the parents experienced emotional, physical, psychological, sexual or other abuse (Ripka et al., 2018). In line with the literature described in Section 2.1, Ripka et al. (2018) found that substandard housing was related to self-reported health problems: 35% of the Brno families indicated that they (or their family members) suffered from chronic liver, kidney, stomach, lung or heart disease.

The 421 eligible families who signed up during the Family Homelessness Registry Week then participated in a treatment lottery, which took place in June 2016. The treatment status was determined by means of a random draw from a sample that was stratified with respect to number of children. 50 families were selected into the treatment group and were each awarded the long-term lease of a municipal flat and a package of social services related to their new housing situation (e.g. help with furnishing, assistance when children needed to change schools or kindergartens, mediation in case of problems with neighbors). Another 100 families were chosen to participate in the experiment as a control (or business-as-usual) group. These families participated in the evaluation of the project but did not receive any new housing or additional services: they continued to receive only the assistance that is available to all families in need though standard social services.

The randomization was successful. Table 2.1 presents the balance test, which uses baseline interviews conducted with the principal caregivers in all the families in summer 2016, before they learned their treatment status. The only value that is significantly different is the anomie index ($p = 0.05$), which reports the number of affirmative reactions to five statements measuring the level of disillusionment with the state of the world, society and authorities.[2] The measure of psychological distress is the average value of the six questions from the Kessler Psychological Distress Scale (K6). The statistical tests used are Fisher's exact test for binary outcomes and the Mann-Whitney test for continuous outcomes.

The treatment families moved into the municipal flats between September 2016 and May 2017.[3] Our after-treatment data were collected in two waves, approximately 6 months and 12 months after the treatment families moved into

[2]The text of one of these statements reads: "It does not makes sense to ask the authorities for help, because they are not very interested in ordinary people's problems."

[3]The RCT was registered at `http://www.isrctn.com/ISRCTN44050004`. For details of the experimental protocol, see Ripka et al. (2018).

(a) Hostel (control)

(b) Municipal apartment (intervention)

Figure 2.1: Housing in the treatment and control
Photos by Barbora Kleinhamplová

the municipal housing.[4] We will refer to these data sets as *baseline*, *6 months* and *12 months*, respectively. The lab-in-the field experiment was conducted in spring

[4]The interviews with treated families were spread over time such that the interviews took place 6 months or 12 months after the families had moved. The control families were interviewed in the same period as the treatment families.

Table 2.1: Balance test

| Variables | Control | Intervention | P-value |
|---|---|---|---|
| 1. Housing: | | | |
| – hostel (share YES) | 0.41 | 0.40 | 1 |
| – years without proper housing | 5.89 | 6.91 | 0.49 |
| 2. Family: | | | |
| – partner (share YES) | 0.57 | 0.48 | 0.30 |
| – number of children | 2.84 | 2.82 | 0.72 |
| 3 Work and income: | | | |
| – work in the last month (share YES) | 0.19 | 0.20 | 1 |
| – total family income (CZK) | 16,590 | 16,158 | 0.46 |
| 4. Life satisfaction and health: | | | |
| – life satisfaction (0: low – 10: high) | 4.51 | 4.22 | 0.70 |
| – overall health (1: very good – 5: very bad) | 2.63 | 2.32 | 0.30 |
| – psychological distress (lower, more distressed) | 3.15 | 3.42 | 0.22 |
| – smoker (share YES) | 0.80 | 0.74 | 0.53 |
| 5. Expectations about pro-sociality: | | | |
| – people can be trusted (0: disagree – 10: agree) | 2.71 | 2.26 | 0.39 |
| – people are fair (0: disagree – 10: agree) | 3.12 | 3.06 | 0.78 |
| – people are helpful (0: disagree – 10: agree) | 2.94 | 3.27 | 0.52 |
| – anomie index (0: low – 5: high) | 3.29 | 2.74 | 0.05 |

*Note:* The table compares average outcomes in the control and intervention groups using baseline questionnaire data collected before the participants knew their treatment status. That questionnaire was filled out by the primary caregivers in 148 families. The last column shows the p-values of the the Fisher's exact test (binary outcomes) or the Mann-Whitney test. We do not adjust for multiple hypothesis testing.

and summer 2018, roughly 12 months after the treatment families had moved.

Table 2.2 tests for treatment effects in several relevant areas using Fisher's exact test (binary outcomes) and the Mann-Whitney U test (continuous outcomes). It is based on the questionnaire data collected by the Housing-First project team 12 months after the treatment. By matching the questionnaire data with our experimental data, we obtain a sample of 120 families (with some missing variables). Segments 1–3 show that the treated families experienced a substantial change in housing quality. The floor area of the flats the families lived in increased by more than 60%. The proportion of families that expected to be able to stay in their current housing for as long as they wanted (*stay permanently*) increased by more than 50 %. Both these changes are highly statistically significant. Segments 2 and 3 document improvements in these families' housing standards. One year after their change of housing, the intervention families enjoy significantly better access to housing amenities and a significantly lower prevalence of various housing-related problems.

The intervention group families' financial situation seems somewhat better than that of the control families: Segment 4 shows that the treated families have higher incomes than the control families and run out of money later in the month. However, none of these differences are statistically significant. Segment 5 shows that the treatment increased participants' life satisfaction. Moreover, it reports the Kessler Psychological Distress Scale (K6) score. K6 contains six questions that measure psychological distress. These questions ask about frequency of bad psychological states using a Likert scale ranging from 1: "All the time" to 5: "None of the time"[5]. The index reports participants' average response values across the six questions. The results suggest that control group participants are significantly more distressed. Finally, the table shows that a higher proportion of intervention families can sleep as much as they need.[6]

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[5]This is the precise wording of the K6 questions: "During the past 30 days, how often did you feel: 1. nervous; 2. hopeless; 3. restless or fidgety; 4. so depressed that nothing could cheer you up; 5. that everything was an effort; 6. worthless?"

[6]Some of these questions were also included in questionnaire data collected after our lab-in-the-field experiment. With respect to the K6 results, we find that the intervention reduced the frequency with which our participants were nervous (Mann Whitney (MW) $p = 0.02$) and depressed (MW $p = 0.01$), but not unfocused (MW $p = 0.28$). Contrary to the evidence on sleep presented in Segment 5 of Table 2.2, we do not observe any difference between the number of hours slept the previous night by the primary caregivers in the control and intervention families (C: 6.81, I: 7.16; MW $p = 0.4$). The questionnaire also contained two questions related to the participants' financial situations at the moment of the experiment. First, we asked whether they lacked enough money to pay for anything important in the following three days. We do not find any significant difference in the primary caregivers' answers between the two treatment groups (share YES C: 48%, I: 37%; Fisher test $p = 0.24$). Second, we do not find any difference in the number of days left until the participants were due to receive their pay or benefits (C: 14.5; I:

Table 2.2: Differences between treatment groups 12 months after the treatment

| Variables | Control | Intervention | P-value |
|---|---|---|---|
| 1. Housing: | | | |
| – flat area ($m^2$) | 42.8 | 69.7 | < 0.001 |
| – stay permanently (share YES) | 0.41 | 0.93 | < 0.001 |
| 2. Shared or no access to (share YES): | | | |
| – running water | 0.32 | 0.00 | < 0.001 |
| – hot water | 0.35 | 0.00 | < 0.001 |
| – electricity | 0.23 | 0.00 | < 0.001 |
| – toilet | 0.32 | 0.02 | < 0.001 |
| – bathroom/shower | 0.32 | 0.00 | < 0.001 |
| – kitchen with a sink | 0.38 | 0.00 | < 0.001 |
| 3. Housing problems (share YES): | | | |
| – water-damaged walls, ceilings or floors | 0.41 | 0.12 | 0.001 |
| – damaged floors | 0.38 | 0.05 | < 0.001 |
| – large holes or cracks | 0.36 | 0.19 | 0.09 |
| – unpleasant odour | 0.46 | 0.12 | < 0.001 |
| – noise in the flat from outside | 0.58 | 0.50 | 0.49 |
| 4. Financial situation: | | | |
| – total family income (CZK) | 19,548 | 22,016 | 0.40 |
| – total family income net housing (CZK) | 10,177 | 13,285 | 0.14 |
| – low on money (days before payday) | 8.8 | 6.3 | 0.24 |
| 5. Well-being: | | | |
| – life satisfaction (Likert 0–10) | 4.17 | 7.14 | < 0.001 |
| – K6 index (Likert 1–5) | 3.07 | 3.93 | 0.004 |
| – sleep as much as you need (share YES) | 0.36 | 0.76 | < 0.001 |

*Note:* The table compares average outcomes in the control and intervention groups using questionnaire data collected approximately 12 months after the treatment by the Housing First project team. In order to be relevant for the outcomes of our experiment, the data is matched with our experimental data. The last column shows the p-values of Fisher's exact test (binary outcomes) or the Mann-Whitney test.

14.8; MW $p = 0.74$).

## 2.4 Related housing experiments

In this section, we will survey studies that have used the RCT method to study interventions addressing homelessness. Many different interventions have been tested in the literature; these include Intensity Case Management, Housing First, Critical Time Intervention, Abstinence-Contingent Housing, Housing Vouchers and Residential Treatment (see Menzies Munthe-Kaas, Berg, and Blaasvaer, 2018, for a meta-analysis). Of these, we are primarily interested in Housing First interventions.

Housing First interventions are usually directed at individuals with mental illness (Shern et al., 2000; Tsemberis, Gulcur, and Nakae, 2004; Aubry et al., 2015). While most of these experiments have been conducted in the US, Europe has also staged several notable Housing-First interventions (Busch-Geertsema, 2013; Bretherton and Pleace, 2015; Pleace et al., 2015; Bernad, Yuncal, and Panadero, 2016). In the rest of this section we will focus on studies that have tested the efficiency of such housing interventions on a sample of homeless families in an RCT framework.

Levitt et al. (2013a) tested the effects of time-limited housing subsidies and intensive case-management services on housing placement. Participants were families with at least one child, recruited through the New York City family shelter system. While the control group continued to receive standard shelter services, the treatment group participated in the Home to Stay program, which consisted of a time-limited housing subsidy and better case-management services, including more frequent client contact, flexible scheduling, financial literacy services, and continuity of services across the transitional period from shelter into housing. 138 families were randomly selected into the treatment group and 192 into the control group. The study's main finding was that families in the treatment group left shelters sooner, stayed out of shelters longer and spent fewer days in shelters overall.

Guo, Slesnick, and Feng (2016) studied the effect of an ecologically-based treatment (EBT) including housing (3 months of rent assistance of up to \$600) and supportive services (for up to 6 months) compared with the treatment as usual, which involved community-based housing and supportive services. The research was conducted on a sample of 60 mothers with small children, recruited at a shelter for homeless families. In order to be eligible for the study, each mother had to meet the DSM-IV criteria for substance abuse or dependence and have a child in her care. The study did not identify any statistically significant differences between the treatment groups, but did report an improvement in mothers' mental health problems in both groups, and a reduction in children's behavioral health problems in the EBT group.

Samuels et al. (2015) describes a case management model that targeted homeless mothers experiencing mental problems. Participants were recruited from single, female-headed households at family homeless shelters in a county neighbouring with New York City. In order to be eligible, each mother had to have at least one child between the ages of 18 months and 16 years living with them in the shelter and a mental illness and/or substance abuse problem. All eligible families entered the county homeless shelter system. This involved placing the families in shelters, transitional residences and emergency housing, but also providing them with a number of additional services (such as employment, child support, medical care and temporary financial services. In addition to that, half of the families received the Family Critical Time Intervention (FCTI) which was a community-based case management service lasting 9 months. The goal of the intervention was to establish long-term linkages to community-based resources and services and to extended family and friends. Compared to the control group, treatment families not only received more intensive case management but were also provided with scattered site housing without any time limits and without having to meet the housing readiness requirements typically imposed on the control group families (e.g. abstinence from substance use, engagement in mental health services). The final sample included 210 families (97 in the treatment group and 113 in the control group)—85% of the families were minority, and 85% of the mothers were currently unemployed. The study found that even though treatment families left shelters earlier on average than control families, there were no significant differences in their mental distress, which declined significantly over time in both treatments. Using the same experiment, Shinn et al. (2015) found that FCTI had positive effects on children's mental health and school outcomes.

Most of the projects we have mentioned so far were small-scale interventions that were primarily interested in the participants' housing outcomes and mental health. We now present two that were large-scale projects and also interested in economic outcomes: the Family Options Study and Moving to Opportunity.

The Family Options Study (FOS) was organized by the U.S. Department of Housing and Urban Development (Gubits et al., 2015, 2016, 2018). It assigned 2,282 families into three active intervention groups and one group receiving usual care in their communities. The evaluation showed that the most generous active intervention—a long-term rent subsidy—led to a significant difference in the participants' receipt of permanent housing subsidies, compared to the control group, which significantly reduced their measures of homelessness. The subsidy increased well-being, reduced psychological distress and intimate partner violence, although it increased separation between spouses. The intervention also had some effects on child development, since it reduced the number of schools attended, increased positive attitudes to school and resulted in fewer sleep problems and behavioral problems and more pro-social behavior. It had no effects on verbal or math ability. The study found a negative effect on children's executive functioning between 3.5 and 7 years. The treatment reduced work effort but, thanks to the subsidies, the treatment families appeared to be in a better financial position. This led to an improvement in food security and to a decrease in economic stress.

Moving to Opportunity (MTO) randomly assigned 4,604 families to three experimental groups: one group received housing subsidies (vouchers) that were valid only if they moved into low poverty neighborhoods, one group received similar vouchers valid in any neighborhood, and one control group received no vouchers. Relative to the control group, the first two groups experienced short- and long-term improvements in multiple measures of well-being (mental health) but no effects on labor market outcomes (Katz, Kling, and Liebman, 2001; Leventhal and Brooks-Gunn, 2003; Kling, Liebman, and Katz, 2007; Ludwig et al., 2013). Chetty, Hendren, and Katz (2016) found that moving to a better neighborhood as a child increased college attendance and earnings and reduced single parenthood rates, while a similar move during adolescence led to slightly worse outcomes.

These experiments differ from our setup in two essential aspects. First, while the families in FOS and MTO were offered housing subsidies that were valid only if the families moved (to a better neighborhood), all the treated families in our project moved. Second, the treated and untreated families in FOS and MTO differed in their access to housing subsidies that changed their net income. Our treatment consists of moving families into better housing conditions without substantially affecting their financial resources. Most of the families who registered for the project in Brno have access to housing benefits, which can be used for covering their housing costs, and this is true for the control families as well as the treatment families.

# Chapter 3

# Preferences and cognition

## 3.1 Introduction

The aim of this chapter is to test the effect of improved housing conditions on preferences and cognitive abilities. As discussed in Section 1.1, shifts in preferences and cognitive abilities might be one of the causes of poverty persistence, forming one of the so-called poverty traps (Banerjee, Banerjee, and Duflo, 2011; Kraay and McKenzie, 2014; Barrett, Garg, and McBride, 2016; Haushofer and Fehr, 2014). Recent literature suggests that poverty may affect preferences and cognition in a way that may contribute to suboptional economic decisions. Compared to their more affluent peers, the poor are usually more risk-averse (Guiso and Paiella, 2008; Tanaka, Camerer, and Nguyen, 2010; Gloede, Menkhoff, and Waibel, 2015; Di Falco, Damon, and Kohlin, 2011). This could make them less likely to engage in risky ventures, such as investment into human or physical capital. The same outcome could be related to differences in time preferences: Haushofer, Schunk, and Fehr (2013) and Tanaka, Camerer, and Nguyen (2010) find that less wealthy people seem to be less patient. As for cognitive abilities, several studies have found that the actual or experimentally induced scarcity negatively affects cognitive function (Spears, 2011; Shah, Mullainathan, and Shafir, 2012; Mani et al., 2013).

The literature linking poverty to preferences and cognition faces several problems. The first concerns the measurement of discount rates. The fact that the poor are more inclined to prefer payments sooner rather than later might not only be a sign that they have lower patience but also of the fact that their liquidity constraints are more severe (Frederick, Loewenstein, and O'donoghue, 2002; Dean and Sautmann, 2014; Ambrus et al., 2015; Epper et al., 2015). This problem can be solved if, instead of monetary payments, participants allocate real effort in time (Augenblick, Niederle, and Sprenger, 2015). Carvalho, Meier, and Wang (2016) measures time preferences using both monetary and real-effort choice tasks. They compare the discount rates of low-income U.S. households before and after payday and find evidence to support a present bias for choices in money but not for choices in real effort. This result suggests that the link between poverty and time preferences might be due to liquidity constraints, not due to poverty shaping preferences.

Another challenge is to demonstrate a causal relationship between wealth (or income) and preferences or cognitive abilities. Many studies report a correlation between wealth or income with risk or time preferences (e.g., Donkers, Melenberg, and Van Soest, 2001; Dohmen et al., 2011; Falk et al., 2018; Dohmen et al., 2015; Lawrance, 1991; Sullivan, 2011; Stephens Jr and Krupka, 2006) but do not identify causality. Other studies address the problem of endogeneity of income or wealth using instrumental variables (Guiso and Paiella, 2008; Tanaka, Camerer, and Nguyen, 2010), semi-random weather shocks (Gloede, Menkhoff, and Waibel, 2015; Di Falco, Damon, and Kohlin, 2011), or variation in income due to payday or harvest (Carvalho, Meier, and Wang, 2016; Mani et al., 2013). Others solve the problem of endogeneity by experimentally inducing a variation in income or perceived scarcity. This approach, though, creates new and equally serious concerns about external validity (Haushofer, Schunk, and Fehr, 2013; Spears, 2011; Shah, Mullainathan, and Shafir, 2012; Mani et al., 2013). As Tanaka, Camerer, and Nguyen (2010, p. 557) put it, an ideal yet hard to achieve approach to causal inference would be to randomly assign individuals to different economic circumstances.

We study the impact of random assignment to better-quality housing provided by the Housing First randomized controlled trial (RCT) that took place in Brno (Czech Republic). As detailed in Chapter 2.3, the intervention group consisted of 50 families; these were each awarded the long-term lease of a municipal flat and a package of social services primarily related to their new housing. The control (or usual-care) group consisted of 100 families, which participated in the evaluation of the project but received no new housing or extra services. As documented in the Section 2.3, the treatment significantly improved the quality and perceived stability of housing. In addition, it significantly increased life satisfaction among the treated group.[1] This suggests that the treatment had a sizable impact on the living conditions of the families that participated in the project. This impact can be seen as a permanent increase in income that is exclusively spent on housing.

This chapter reports the results of lab-in-the-field experiments we conducted with participants of the RCT Housing First. We use incentivized multiple price lists adapted from Sutter et al. (2013) to elicit risk and time preferences and a d2 test to measure sustained attention (Bates and Lemay, 2004). We do not find that the improvement in housing conditions had any effect on our measures of risk preferences, time preferences, or sustained attention.

Our findings contribute to the literature documenting the effect of poverty on preferences (Tanaka, Camerer, and Nguyen, 2010; Carvalho, Meier, and Wang, 2016). As Carvalho, Meier, and Wang (2016) argue, the effect of wealth on time preferences might be driven by other factors, such as liquidity constraints. In contrast to previous studies, our design does not create significant differences in the financial situations of the treated and untreated families. While each of the treated families is granted long-term lease of a municipal flat, they still have to cover their own housing costs, as they did before. Most of the families in both the treatment and control groups cover their housing costs through social benefits, whose value is typically equal to the rent they pay. Our findings are in line

[1] Relative to the baseline levels, the treated participants' life satisfaction increased by 0.8 std. dev. This is a comparatively large effect. For example, unconditional cash transfers of an average value of $709 PPP to poor households in western Kenya (equivalent to two years of per-capita expenditure) increased recipients' life satisfaction by 0.17 std. dev. (Haushofer and Shapiro, 2016).

with the results presented in Carvalho, Meier, and Wang (2016), in which time preferences are measured by a choice between real-effort tasks.

Our findings are closely related to those of Mani et al. (2013) and Carvalho, Meier, and Wang (2016), who study the impact of real-life variation in financial resources on cognition. Our results resemble those of Carvalho, Meier, and Wang (2016), who find no difference in the cognitive function of participants from low-income US households before and after their payday, and differ from those by Mani et al. (2013), who observe that poor Indian farmers perform better at cognitive tests after the harvest than prior to the harvest. This suggests that the zero result might be related to the fact that, like in Carvalho, Meier, and Wang (2016), our treatment takes place in a developed country, where poverty may generate lower cognitive demands than in developed countries.

This chapter is also related to studies that look at the outcomes of Housing First RCTs involving families with small children, as discussed in Section 2.4. Most of these projects are small-scale interventions interested in housing outcomes and mental health (Levitt et al., 2013b; Samuels et al., 2015; Shinn et al., 2015; Guo, Slesnick, and Feng, 2016). More interestingly, this study also relates to two large-scale projects that are interested in economic outcomes: the Family Options Study (FOS) (Gubits et al., 2015, 2016, 2018) and Moving to Opportunity (MTO) (Katz, Kling, and Liebman, 2001; Leventhal and Brooks-Gunn, 2003; Kling, Liebman, and Katz, 2007; Ludwig et al., 2013; Chetty, Hendren, and Katz, 2016). None of these papers, however, investigated the impact of housing on preferences and cognition.

This chapter is structured as follows. Section 3.2 provide a detailed review of the relevant literature. Section 3.3 introduces our experimental design. Section 3.4 presents the data. Section 3.5 reports the results of our study. Finally, Section 3.6 concludes.

## 3.2 Literature review

Recent evidence suggests that cognitive abilities can be seen as limited resources that can be exhausted by the hardships of the life in poverty (Schilbach, Schofield, and Mullainathan, 2016). People have limited capacity to engage in deliberate, effortful and costly decision-making processes. When mentally burdened, e.g. by poverty-related scarcity or stress, they have less "bandwidth" to solve other tasks requiring cognitive effort (Mullainathan and Shafir, 2013). There are two components of bandwidth: cognitive capacity—the ability to reason and solve problems—and executive control—the ability to control and manage our cognitive abilities. Both components can be burdened with the demands resulting from life in poverty. The main mechanisms through which poverty impacts bandwidth are nutrition (Schofield, 2014), monetary concerns (Mani et al., 2013), alcohol (Steele and Josephs, 1990; Ben-David and Bos, 2017; Schilbach, 2019). Other more speculative factors include sleep deprivation (Lim and Dinges, 2010; Bessone et al., 2019), stress and negative affective states (Chemin, De Laat, and Haushofer, 2013; Haushofer and Fehr, 2014), pain (Moriarty, McGuire, and Finn, 2011) or environmental factors as noise, heat, or pollution (Tzivian et al., 2015; Simmons et al., 2008).

The effects of reduced bandwidth have been widely studied in experimental psychology. By experimentally imposing cognitive load, many studies have tested what is the impact of a reduction in bandwidth on outcomes relevant for the poor such as self control and present bias (Hinson, Jameson, and Whitney, 2003), choices between risks and benefits (Finucane et al., 2000), risk aversion and monetary discounting (Deck and Jahedi, 2015), food choice (Shiv and Fedorikhin, 1999; Ward and Mann, 2000). In the rest of this survey, we concentrate on aspects that measured by this study which are risk and time preferences and aspects cognitive capacity, mainly related to attention.

### 3.2.1 Preferences

The correlational evidence about the association between wealth and risk and time preferences is mixed. Negative association between income or wealth and risk aversion are found by Donkers, Melenberg, and Van Soest (2001) in the Netherlands and Dohmen et al. (2011) in Germany. On the other hand, Binswanger (1980), Miyata (2003), Yesuf and Bluffstone (2009) or Guiso and Paiella

(2008) do not find any correlation between wealth and risk preferences. The literature also finds that poor individuals and countries have higher discounting than others (Falk et al., 2018; Dohmen et al., 2015; Lawrance, 1991; Sullivan, 2011; Stephens Jr and Krupka, 2006). However, these results cannot be interpreted causally since there might be also reverse causality at play, i.e. people who are more risk loving or patient could be more successful at getting a better paid work, and accumulating more wealth.

One way how to deal with the endogeneity are instrumental variables. Guiso and Paiella (2008) use the 1995 wave of the Bank of Italy Survey of Household Income and Wealth, which collects data for a representative sample of 8,135 of Italian households, to construct a measure Arrow-Pratt index of absolute risk aversion. This measure is related to different proxies for consumers' resources. They find that risk aversion is an increasing function of consumers' resources. This finding is robust once resources are instrumented a set of instruments including among others education and year of birth of household head's father and windfall gains. These include a dummy for the family house being acquired as a result of a bequest or gift, estimate of a gain in value since the family house was acquired, and value of insurance settlements and other transfers received by the family.

Another strategy is used by Gloede, Menkhoff, and Waibel (2015) and Di Falco, Damon, and Kohlin (2011) who study the effect of negative (income) shocks on preferences of poor rural populations. Gloede, Menkhoff, and Waibel (2015) administer a survey representative of rural populations in North East Thailand and Vietnam covering 2,000 households in each country. Risk attitudes are measured using a simple non-incentivized item (see e.g. Dohmen et al., 2011). Replicating some of the previous correlational studies, they find that poorer respondents were more risk averse. They focus more on the impact of shocks, such as bad weather causing lower harvest or (unexpected) death of a family member, some of which can be considered as random. The shocks are categorized in four dimensions: demographic or agricultural; with high, medium, or low impact; idiosyncratic or covariate; and expected or unexpected. They find a relationship between negative shocks and risk aversion. With respect to the categorization, both demographic and agricultural shocks matter, and larger and more unexpected shocks matter more.

Di Falco, Damon, and Kohlin (2011) uses a panel data from 1,720 houshold to test whether negative environmental (income) shocks, such as severe droughts, have any effect on time preferences measured in a hypothetical choice experiment. They find that households that experience negative shocks are less patient. Interestingly, they also find that patience is negatively correlated with adoption of soil conservation measures.

Tanaka, Camerer, and Nguyen (2010) administered incentivized experimental measures of risk aversion, loss aversion and time preferences to a sample of 181 farmers from 9 villages. The choice of these villages was based on a 2002 survey of 25 households in each of 279 villages. The choice was done in order to retains villages with substantial differences in mean village income and market access. They construct two income variables: mean village income and relative income within village from the household survey conducted previously. After instrumenting the endogenous income variable with rainfall and a dummy variable showing whether the head of household can work, they find a marginally significant effect of mean income on curvature of the value function, indicating that individuals living in wealthier villages are less loss averse and less risk averse. They also find that household income and mean village income negatively correlated with discount rate, indicating that wealthier individuals, or individual living in wealthier villages, are more patient. They find no effect of income on present-bias.

Studies that use weather shocks to instrument income or wealth (Di Falco, Damon, and Kohlin, 2011; Tanaka, Camerer, and Nguyen, 2010) suffer from additional problems Haushofer, Schunk, and Fehr (2013). First, these shocks are impossible to disentangle from psychological effects of these shocks. It is therefore not clear whether differences in preferences can be attributed to different levels or to changes. Second, poor people might appear more impatient not because they might be liquidity constrained (Dean and Sautmann, 2014; Ambrus et al., 2015; Epper et al., 2015). Haushofer, Schunk, and Fehr (2013) uses laboratory experiment to deal with both issues in order to provide a causal relationship between a negative income shock, or lower income levels, on time preferences. The size of income shocks and the initial income levels were such that it was possible to compare the discount factors of subjects who earn the same income and differ only in whether they experienced a (positive or negative) income shock. They find that negative income shock leads to lower patience (higher present bias) and that positive income shock weakly decreases discounting. Income levels are found to have no effect on discounting.

Carvalho, Meier, and Wang (2016) study the impact of short-lived variation in financial resources on payday on participants' choices in incentivized risk and intertemporal choice tasks and measures of cognitive function. They administered an online survey to a sample of 3,821 participants from low-income households in the U.S. These people were randomly assigned to a group surveyed shortly before the payday and to a group surveyed shortly after the payday. Risk preferences were measured in two tasks by Eckel and Grossman (2002) and Choi et al. (2014). Part of the sample also received two related tasks: an unincentivized loss-aversion task (Fehr and Goette, 2007) and an incentivized simplicity seeking simplicity seeking task (Iyengar and Kamenica, 2010). The do not find any significant difference between the groups in these tasks. So financial situation does not seem to have any effect on risk preferences.

Subjects were also administered two intertemporal choice tasks, one with monetary and one with non-monetary rewards. The first task consisted in convex time budget tasks, in which participants allocate an experimental budget to two payments at two dates, with the later payment containing interest (Andreoni and Sprenger, 2012). Participants answered 12 tasks that differed in the interest rate. In the second task, participants made intertemporal choice between real effort: they choose whether they prefer a shorter survey within 5 days or a longer within 35 days. They answered 10 choices with different time requirements of the earlier survey, and different times for deadlines. A small share of participants has one of the 10 choices implemented—the survey was sent, and if completed, participants received monetary reward. The time of the reward was fixed, so it did not depend on when participants completed the survey. They find that before-pay day group made choices consistent with more present-biased preferences. However, the difference disappeared once time preferences were measured in real-effort rather than money. This suggests that these choices can be attributed to liquidity constraints rather than lack of attention to future. Differences in liquidity constrained between poor and richer Vientameese farmers might explain the finding of Tanaka, Camerer, and Nguyen (2010).

We contribute to this literature by studying the effect of living conditions on preferences in an experimental setup where the randomly assigned intervention has no significant impact on liquidity constraints.

### 3.2.2 Congitive abilities

There is a growing evidence of cognitive functions being limited resources that can be strained by life in poverty. Spears (2011) conducts a lab-in-the-field, and a field experiment in rural India and analyzes the American Time Use Survey (ATUS). In all three studies poverty is connected with lower behavioral control. In the lab-in-the field experiment, the treatment variation between rich and poor was induced experimentally in a store game. Participants imagined that the experimental room was a store with three items available. Participants were randomly assigned a smaller or a larger budget, they either got two of these items for free (rich), or received only one of them (poor). They were also randomly either given the option to choose their items (choice) or the item or items were chosen for them (no choice). Then their behavioral control was measured in the handgrip task (e.g. Muraven, Tice, and Baumeister, 1998) and the Stroop task (e.g. Flowers, Warner, and Polansky, 1979). The experiment was conducted with 57 adult men. Spears finds that the participants who were poor and at the same time had to choose have lower performance in these tasks.

In the field experiment, they conducted 216 interviews with males aged between 18 to 65. They visited participants from poorer and richer villages according to the census. They also collected additional survey questions. The experiment consisted of three tasks: squeezing a handgrip (same as in the lab-in-the-field experiment), a working memory test used to measure cognitive control, and an economic decision. In the working memory test, the surveyor read a list of five simple words, then asked a set of irrelevant questions, after which the participants were asked to remember the words. In the economic decision, the participants have an option to buy soap at a 60% discount compared to the market price (43% purchased the soap). The decision was either before (soap first) or after the squeezing task and working memory task. Spears (2011) finds that the interaction between soap first and poverty negatively affects the endurance in the squeezing task.

The final study in Spears (2011) analyzes the ATUS data, which record the activities of a representative sample of Americans during one day (24 hours). The information is matched to household economic and demographic data from the Current Population Survey. The variable of interest is secondary eating, i.e. eating during another activity, which may be interpreted as a failure of behavioral control. Consistently with the previous results, the paper finds that the poor engage in more secondary eating during shopping events (for other items than food) than rich.

Shah, Mullainathan, and Shafir (2012) pursues similar aim as Spears (2011), but the mechanism through which scarcity impacts cognitive abilities shifts from behavioral control to attention and cognitive control. They show that scarcity changes how people allocate attention by engaging more deeply in some problems while neglecting others. They conduct five experiments. In experiment 1, participants (60 MTurk workers) play a version of Wheel of Fortune. Scarcity is manipulated by giving participants different numbers of chances to guess letters in word puzzles. The task might lead to cognitive fatigue, that is measured using a version of the Dots-Mixes task, which assesses attention and cognitive control. The find that poor participants perform worse than rich. Scarcity seems to create a greater engagement with the guessing task, so that even with less time played, poor people were more fatigued.

In experiment 2, participants (68 MTurk workers) play a video game similar to Angry Birds with the goal of earning points by shooting on target. The game consists of several rounds. Poor has an overall budget of 30 shots (3 per round) compared to 150 shots by the rich (15 per round). Some participants could borrow, i.e. make more than the assigned number shots in each round at a cost of 2 shots from the overall budget. All participants are allowed to save shots for later rounds. The game is played until the budget was exhausted. They find that poor participants engaged more in counterproductive borrowing (borrowed more shots as a fraction of their budget), and the amount of borrowing was significantly correlated with individual's average amount of time spent aiming. This suggests that scarcity motivates people to focus more on current round and inefficiently borrow from future rounds. These results are replicated in a different contexts in experiment 3 (143 students), with more salient consequences of borrowing in experiment 4 (118 MTurk workers), and with design structure that offered more direct support for scarcity generating attentional neglect in experiment 5 (137 MTurk workers).

Mani et al. (2013) use two studies to test the hypothesis of Shah, Mullainathan, and Shafir (2012) that poverty impedes cognitive performance through capturing attention and triggering intrusive thoughts. The first study consisted of four lab-in-the-field experiments. In experiment 1, 101 shoppers at a New Jersey mall were presented with four hypothetical scenarios. These scenarios described a financial problem, such as a problem with car that requires a specific expenditure to be fixed, and asked about preferred solution, such as paying in full, taking a loan, or taking a chance and leaving the car unfixed at the moment. These scenarios are intended draw participants' attention to their own financial concerns, the hypothesis being that poorer participants are in a comparatively more worrisome financial situation than the rich. Before allowing them to provide answer to the previous scenario, participants are asked to take two computerized measures of cognitive function: Raven's progressive matrices measuring "fluid intelligence" and a spatial compatibility task measuring cognitive control. They find is that rich and poor achieve similar scores in both tests in the easy condition, but perform significantly worse compared to rich when confronted with the hard condition. The size of these effects is large, with Cohen's $d$ ranging from 0.88 to 0.94.

In order to address potential concerns with external validity of the laboratory experiments, the second study uses a natural variation in pre- and post-harvest income of 464 Indian in the sugarcane-growing areas in Tamil Nadu, India. These farmers are randomly drawn from the population of small farmers (with 1.5 to 3 acres of land) who earned at least 60% of their income from sugarcane. All the farmers in the sample were interviewed twice within a 4-months period, once before the harvest and once after the harvest. They show that harvest indeed significantly relieved the financial pressures the farmers face. Fluid intelligence was again measured using Raven's matrices, cognitive control was measured with numeric version of Stroop task instead of spacial incompatibility task, which could not be administered in the field. In the Stroop task consisted of 75 trials. In a typical trial farmers that are shown "5 5 5" should respond 3, i.e. the number of characters, instead of 5, i.e. the character itself. The outcome measures are total response speed and number of errors. They observe significantly higher test scores after the harvest than before the harvest. They also need significantly less time to complete the Stroop task, and commit less errors while doing so. They also discuss the potential mechanisms responsible for these results. They are able to exclude training effects (farmers take the test twice), the role of nutrition and stress. This all suggests that poverty reduces cognitive function, because it leads to attentional capture.

Using similar temporal variation in financial resources as in Mani et al. (2013), Carvalho, Meier, and Wang (2016) study the impact of payday on participants' choices on quality of decision making and measures of cognitive function. Quality of decision-making is investigated based on the intertemporal and risk choice tasks (see Subsection 3.2.1 for details on these tasks and general design of the study). In the intertemporal choice task with monetary rewards, they measure the fraction of times subjects increase the later reward in response to a rise in the interest rate (Giné et al., 2018). In the task with non-monetary rewards they measure the fraction of subjects with one or zero switching points (as e.g. in Burks et al., 2009). In risk-preference choice tasks, they detect violations to general axiom of revealed preference (GARP) and first-order stochastic dominance (FOSD). They find no statistically significant differences in any of these measures between the before-pay and after-pay group. Cognitive function is assessed by four different tasks: Flanker task measuring inhibitory control (Eriksen and Eriksen, 1974) and Stroop task measuring cognitive control (as in Mani et al., 2013), working memory task and cognitive reflection test, which assesses the tendency to override an incorrect intuitive response and instead to give a reflective correct answer (Frederick, 2005). As with the quality of decision making, they find no differences between the before-pay and after-pay group in any of these measures.

Carvalho, Meier, and Wang (2016), therefore, do not replicate previous results of Mani et al. (2013) and Shah, Mullainathan, and Shafir (2012). One possible reason could be that the difference between financial wealth of poor US households before and after the payday is rather small, especially when compared to the difference in financial wealth of poor farmers before and after the harvest (Mani et al., 2013). Our design contributes to this literature by looking at the impacts of a large variation in living standards (e.g. as measured by the change in life satisfaction), but still using a sample of poor households living in a developed country.

## 3.3 Experimental design

### 3.3.1 Risk and time preferences

To elicit risk- and time-preferences, we use multiple price lists (MPLs) adapted from Sutter et al. (2013). In the risk-preference MPL, participants choose 20 times between a risky lottery (random draw from a bag) and receiving a specified amount of money. While the lottery remains the same in all 20 lines of the MPL (an equal chance of getting 0 CZK or 220 CZK on each draw[2]), the specified amount increases in steps of 11 CZK from 11 CZK to 220 CZK (see Figure A.1 in the appendix for the MPL). Risk preferences are measured by the lowest right-hand side choice in the MPL, which corresponds to the certainty equivalent to

[2]At the time of the experiment 220 CZK was worth 8.5 euro and was roughly the price of two hours of unqualified work in the Czech Republic.

the lottery.

We elicit time preferences using choices between earlier and later monetary payments. Since there are no significant differences in the financial situations of the treated and untreated families (see Section 2.3), our treatment effects should not be biased by differences in liquidity constraints (Frederick, Loewenstein, and O'donoghue, 2002; Stahl, 2013; Dean and Sautmann, 2014). As in the risk-preference elicitation, we use multiple price lists to measure time preferences. Our measures are switching points, indicating the lowest amount to be paid at a later date that is preferred over a fixed amount payable earlier. We have four MPLs in total, each containing 20 lines with the earlier payment equal to 98 CZK throughout and the later payment ranging from 118 CZK to 498 CZK (see Figure 3.1). As in the risk preference MPL, this resulted in a list of prices that did not contain any round numbers that might particularly attract participants' attention.

| | amount **the following business day Date:** | | or | | amount **in 2 weeks after the following business day Date:** |
|---|---|---|---|---|---|
| [1] | 98 CZK | **O** | **or** | **O** | 118 CZK |
| [2] | 98 CZK | **O** | **or** | **O** | 138 CZK |
| [3] | 98 CZK | **O** | **or** | **O** | 158 CZK |
| [4] | 98 CZK | **O** | **or** | **O** | 178 CZK |
| [5] | 98 CZK | **O** | **or** | **O** | 198 CZK |
| [6] | 98 CZK | **O** | **or** | **O** | 218 CZK |

Figure 3.1: Segment of the time-preference MPL

The four MPLs differed only in the earlier and later dates. The earlier date was set as either the next working day (Delay = 0) or a week after the next working day (Delay = 1). The variation in the earlier date can be used to test present bias. The later date was set as either 2 weeks (Long period = 0) or 10 weeks (Long period = 1) after the earlier date. Different waiting periods were used to test the robustness of the outcome. When Delay = 0, the MPLs are coded as *Tmrw2* for the 2-week wait and *Tmrw10* for the 10-week wait. For MPLs with the earlier date delayed by a week, the MPLs are labeled as *Week2* and *Week10*. The order in which participants filled out the MPLs was randomized. To keep the effort required to collect the promised money constant across the different MPLs, the resulting payments were always sent to a post office of the participant's choice (see Subsection 3.3.2 for details about the payment procedure).[3]

Choices in risk preference and time preference elicitation were incentivized. In each task, one line was randomly selected for real payment to the participant (see Appendix A.1.1 and A.1.2 for details). In all risk- and time-preference MPLs, the research assistants filled out the MPL together with the participants line by line, starting with line 1. They also adjusted the wording of the task to the amounts pertinent to each line. We do not have multiple switching in our risk- and time-preference MPLs. In rare cases, when our subjects tried to switch back to the left-hand side, we explained why this is not a sensible strategy and asked them to reconsider their answers.

### 3.3.2 Payment procedures in time-preference elicitation

All payments from time-preference elicitation were sent in envelopes bearing the participant's name, postal code specifying their chosen post office, and the term "poste-restante", which instructs the post office to keep the letter for 14 days after it is received. To collect their letter, each participant needed to present their ID card and ask to collect a poste-restante letter. In order to help them to do this, we left them a card stating the amount they were to receive, the postal code of their chosen post office, and instructions for how to collect the letter. 37 out of the 162 letters sent (23%) were not collected within the 14-day window, and were therefore subsequently returned to us by the post offices.

Table 3.1 presents linear probability models with the dependent variable *Let-*

[3]Our participants usually use the post office to collect their social benefits, so all of them know the location of their preferred post office. Sending money to the post office, rather than directly to their home addresses, also kept the incentives constant between the control and intervention group, because participants in the control group were more likely to change their address during the waiting period. This could have made future payment more uncertain for the control group, which might have resulted in a higher preference for earlier payments.

Table 3.1: Time preferences – letters returned

| *Dependent variable:* | Letter returned | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Constant | 0.255*** | 0.217** | 0.232 |
| | (0.044) | (0.105) | (0.166) |
| Intervention | −0.073 | −0.070 | −0.046 |
| | (0.079) | (0.079) | (0.078) |
| Time wait | | 0.001 | 0.007 |
| | | (0.007) | (0.010) |
| Time wait$^2$ | | −0.00000 | −0.0001 |
| | | (0.0001) | (0.0001) |
| Payoff (time) | | | −0.0003 |
| | | | (0.0003) |
| Days to pay | | | 0.003 |
| | | | (0.004) |
| Missing money | | | 0.003 |
| | | | (0.076) |
| Male | | | 0.098 |
| | | | (0.077) |
| Experimenter FE | No | Yes | Yes |
| Observations | 161 | 161 | 155 |
| $R^2$ | 0.007 | 0.012 | 0.110 |
| Adjusted $R^2$ | 0.001 | −0.007 | 0.034 |

*Note:* Linear probability model with standard errors clustered at the household level. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

*ters returned*, which takes the value of one if the letter was not collected by the participants. It seems that the probability of not collecting the letter is not related to the number of days the participant waited (*Time wait*). It is also not related to the amount in the envelope (*Payoff (time)*), nor to our variable measuring financial the subjects' financial situations (*Days to pay* and *Missing money*). Indeed, it seems that this outcome is not systematically related either to the version of the MPL used (the payoff date) nor to any participant-specific variables.

### 3.3.3 Attention

We use the d2 test to measure sustained attention (Brickenkamp, 1962; Bates and Lemay, 2004). Out of all the possible cognitive functions, we selected sustained attention for the following reasons: First, Mani et al. (2013) and Shah, Mullainathan, and Shafir (2012) consider attentional neglect (poverty capturing attention) to be the primary mechanism behind their results, so attention seems to be the main theoretical channel through which higher-order cognitive functions are influenced. Because of time constraints, we decided to measure attention directly. Second, we believe that sustained attention is highly relevant in our setup, in which most of the participants are single mothers with several small children. It is essential not only for activities related to bringing up children but also for other important aspects of life, such as employment or contact with authorities.

We used standard procedures to conduct the d2 test (see Appendix A.1.3 for details). In this study, we report concentration performance (d2CP), calculated as the total number of correctly marked minus the total number of incorrectly marked symbols in the test. This measure was incentivized; participants were paid their d2CP in CZK. We prefer d2CP to the total number of correctly processed symbols because it is more resilient to test-taking strategies (Bates and Lemay, 2004).

### 3.3.4 Power analysis

The Housing First project provides enough power to detect medium effects. With 50 observations in the treatment group and 100 observations in the control group, $\alpha = 0.05$, and power of 0.8, the effect size (Cohen's d) in a two-sample, two-sided t-test is 0.49.[4] Several of the studies mentioned above report effects that would be detectable with this power. Shah, Mullainathan, and Shafir (2012) report

[4] As we show in Section 3.4, our experimental data only covers 123 families: 43 in the intervention group and 80 in the control group. Cohen's d based only on the number of households in our sample is 0.53. In some families, though, we conducted interviews with both partners. If these observations are considered independent, we have 55 intervention and 106 control observations, which results in Cohen's d of 0.47.

that their treatments had medium or large effects on measures of cognition. For example, the effect size (Cohen's d) on correct responses in experiment 1 is 0.54, the effect size on time aiming in experiment 2 is 0.86, and the effect size on the comparison between the performance of those who could not borrow with those who borrowed with interest is 1.03. Mani et al. (2013) finds large effects with Cohen's $d$ ranging from 0.88 to 0.94 in their first study. Tanaka, Camerer, and Nguyen (2010) does not report the sizes of the effect of wealth on risk- and time-preferences, but can detect effects on a sample size of 188 farmers.

## 3.4 Data

### 3.4.1 Descriptive statistics

The data was collected in 161 individual experimental sessions (55 treatment, 106 control) in spring and summer 2018. We have data from at least one participant from 123 out of the total 150 households (82%), with similar participation rates in both the intervention group (80%) and the control group (86%). Several participants refused to take part in the d2 test (mainly due to bad eyesight), and a few refused to answer some of the survey questions.

Table 5.2 presents summary statistics. The variables *Risk*, *Tmrw2*,*Tmrw10*, *Week2*, and *Week10* are the switching points (s.p.) in the respective risk- and time-preference MPLs. These measures range from 1, which means that the subjects switched on the first line, to 21 if they did not switch in any of the 20 lines. Higher *Risk* corresponds higher certainty equivalent to the lottery, and hence lower risk-aversion. Higher values of the time-preference variables mean that subjects were willing to wait longer only for relatively high amounts of money, indicating lower patience (higher discount rates). The comparison of the switching points across the MPLs in Table 5.2 shows that, on average, more money was needed to compensate a 10-week wait, compared to a 2-week wait (*Long period* 0 vs 1). Delay in the earlier payment (Week vs. Tmrw) makes people more patient, suggesting present-biased preferences. However, these differences are small. *Payoff (risk)* and *Payoff (time)* show the payoff (in CZK) in the risk-elicitation task. *Time wait* measures the number of days between the date of the experimental session and the date when the participant could collect the payoff at the post office.

Table 3.2: Summary statistics

| Variable | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| Risk (s.p. MPL) | 161 | 9.32 | 5.43 | 1 | 21 |
| Payoff (risk) (CZK) | 161 | 137.74 | 84.95 | 0 | 220 |
| Tmrw2 (s.p. MPL) | 161 | 4.04 | 4.83 | 1 | 21 |
| Tmrw10 (s.p. MPL) | 161 | 7.22 | 7.23 | 1 | 21 |
| Week2 (s.p. MPL) | 161 | 3.73 | 4.71 | 1 | 21 |
| Week10 (s.p. MPL) | 161 | 6.71 | 6.88 | 1 | 21 |
| Time wait (days) | 161 | 37.07 | 30.09 | 1 | 82 |
| Payoff (time) (CZK) | 161 | 268.05 | 134.24 | 98 | 498 |
| d2CP | 153 | 43.18 | 25.23 | −34 | 104 |
| d2 correct answers | 153 | 46.02 | 21.84 | 7 | 104 |
| d2 wrong answers | 153 | 2.84 | 8.10 | 0 | 54 |
| Payoff (d2) (CZK) | 153 | 43.81 | 23.80 | 0 | 104 |
| Male (dummy) | 161 | 0.25 | 0.43 | 0 | 1 |
| Origin hostel (dummy) | 161 | 0.39 | 0.49 | 0 | 1 |
| Days to pay (days) | 159 | 15.04 | 8.75 | 0 | 30 |
| Missing money (dummy) | 156 | 0.48 | 0.50 | 0 | 1 |

The following segment of Table 5.2 displays the d2 test scores. Concentration performance (*d2CP*) is calculated as the number of correctly marked symbols (*d2 correct*) minus the number of incorrectly marked symbols (*d2 wrong*). Five participants achieved negative scores. We did not allow negative payoffs (these participants received 0 CZK from the d2 test), as can be seen from the payoff statistic (*Payoff (d2)*).

The final segment shows that only 25% of our sample ($N = 40$) are men, and almost 40% ($N = 62$) were living in hostels at the time of the treatment lottery (*Origin hostel*).[5] In our analysis, we use two measures of participants' financial

[5]In Brno, the hostels tend to be located in large buildings containing many housing units.

situations, based on the post-experimental questionnaire. *Days to pay* measures the number of days until the participants receive their next paycheck or benefits. *Missing money* indicates whether participants will lack enough money to pay for something important over the next three days. Their next payment was due in about 15 days (*Days to pay*), and 48% predicted they would lack money for important items in the next three days (*Missing money*).

Table 3.3 provides a comparison of the two treatment groups and tests the differences for selected variables. The differences are tested using either the Fisher test (for dummy variables) or Mann-Whitney U test (MW). None of these variables are statistically different in the intervention group compared with the control group.[6] The table also shows that the differences in preferences and attention are small (effect sizes ranging from 0.025 to 0.08).

Table 3.3: Summary statistics by treatment

| Treatment | Intervention | | | Control | | | Difference |
|---|---|---|---|---|---|---|---|
| Statistic | N | Mean | SD | N | Mean | SD | P-value |
| Risk | 55 | 9.38 | 5.67 | 106 | 9.29 | 5.34 | 0.79 (MW) |
| Tmrw2 | 55 | 4.15 | 5.17 | 106 | 3.99 | 4.67 | 0.89 (MW) |
| Tmrw10 | 55 | 7.49 | 7.69 | 106 | 7.09 | 7.02 | 0.85 (MW) |
| Week2 | 55 | 3.91 | 5.02 | 106 | 3.63 | 4.56 | 0.68 (MW) |
| Week10 | 55 | 6.95 | 7.10 | 106 | 6.59 | 6.80 | 0.74 (MW) |
| d2CP | 51 | 41.82 | 26.23 | 102 | 43.85 | 24.81 | 0.42 (MW) |
| Male | 55 | 0.18 | 0.39 | 106 | 0.28 | 0.45 | 0.18 (Fisher) |
| Origin hostel | 55 | 0.38 | 0.49 | 106 | 0.39 | 0.49 | 1 (Fisher) |
| Days to pay | 55 | 15.49 | 8.59 | 104 | 14.81 | 8.87 | 0.68 (MW) |
| Missing money | 54 | 0.43 | 0.50 | 101 | 0.50 | 0.50 | 0.40 (Fisher) |

They typically consist of one room, usually inhabited by one family. The kitchens, bathrooms and showers are shared between many families.

[6] These tests are likely to understate the p-value for two reasons. First, the choices of participants from the same family are often not independent, especially in questions on family-level variables such as *Missing money*. Second, we do not correct for multiple-hypothesis testing. These tests should, therefore, be considered descriptive.

### 3.4.2 Histograms of the outcome variables

In this section we present histograms of the outcome variables. Figure 3.2 presents the distribution of the switching points in the risk-preference MPL. 10% of control subjects and 13% of intervention subjects switched in the very first row, preferring a payment of 11 CZK over the lottery option. One subject did not switch at all (value 21), meaning she preferred the lottery of either 0 or 220 CZK above a safe payment of 220 CZK. All of these choices might be considered censored.

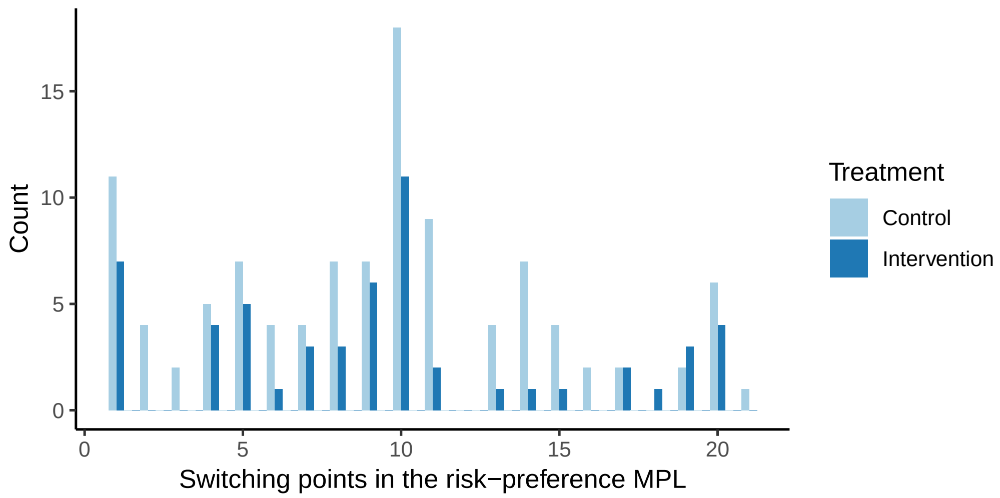



Figure 3.2: Risk preferences: histogram of switching points in the MPL split by treatment group

The border choices are even more prevalent in the time-preference MPLs (see Figure 3.3). Around 60% of participants switched either on the very first line, indicating their willingness to wait for 20 CZK or less, or did not switch at all, meaning they were not willing to wait even for 400 CZK. In the Tmrw10 variant, the share of border choices was around 40%. To our surprise, the vast majority of those border choices consisted of switching on the first line.

The regressions on risk- and time-preferences presented in this chapter use OLS models. To show the robustness of our results, we account for censoring in Tobit regressions in Appendix B.1.

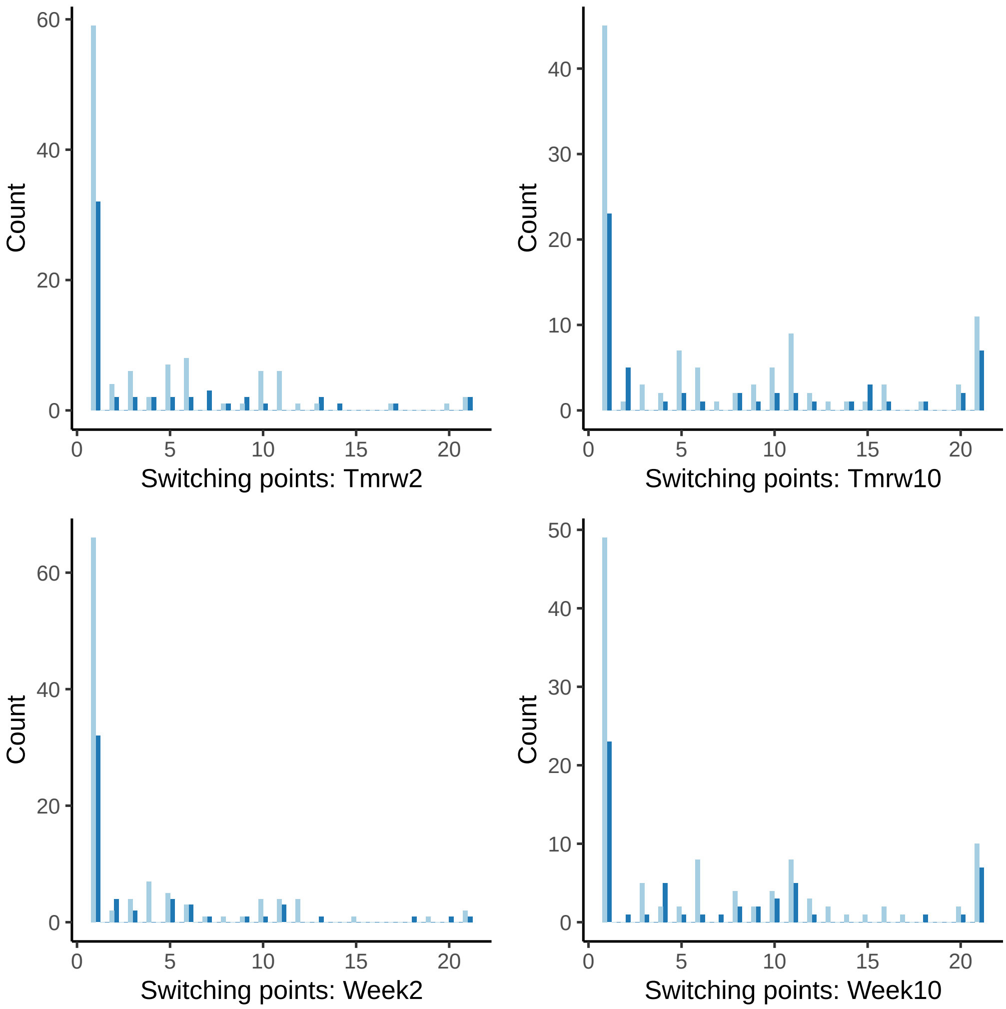



Figure 3.3: Time preferences: histogram of the switching points in the four MPLs split by treatment groups: the darker ink represents the intervention group and the lighter ink the control group. The four MPLs contain choices between different monetary amounts tomorrow or in two weeks (Tmrw2), tomorrow or in 10 weeks (Tmrw10), in a week's time or in three weeks (Week2) and in a week's time or in 11 weeks (Week10).

Figure 3.4 presents distributions of all the d2 outcomes. While the wrong answers are clearly censored at zero points, we do not observe any censoring of the outcome variable d2CP, which is calculated as the difference between the number of correct answers and the number of incorrect answers.

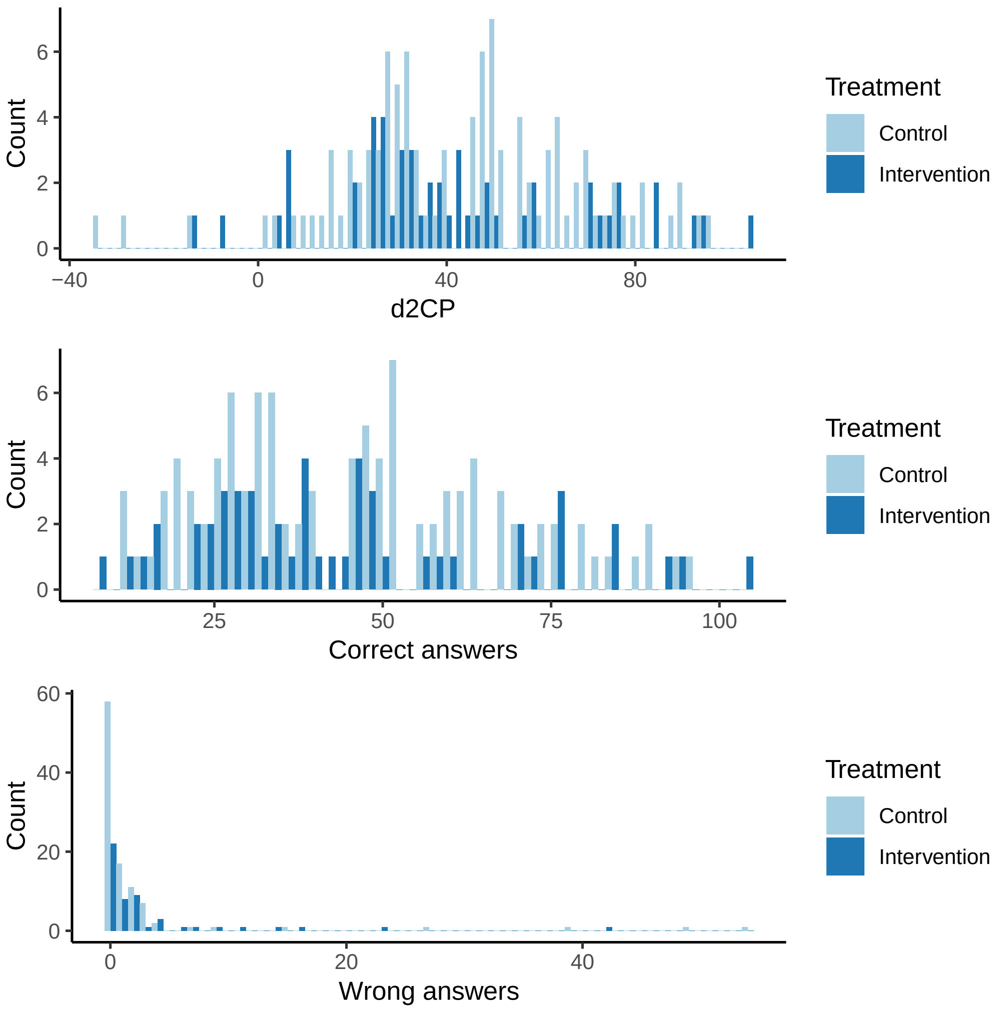



Figure 3.4: Attention: histograms of correct answers, wrong answers and total score (correct − wrong answers) in the d2 test.

## 3.5 Results

This section presents the effects of treatment on risk preferences (Table 3.4), time preferences (Table 3.5), and sustained attention (Table 3.6). The tables report OLS regressions with standard errors clustered at the household level. All the tables share the same structure: Model 1 only includes the treatment variables. Model 2 splits the treatment by sex. Model 3 looks at the effect for a subgroup of families that were living in hostels before the RCT started. For these families, moving to municipal flats represented a more substantial improvement in living standards, so larger treatment effects could be expected. Finally, Model 4 adds all the remaining control variables.

Table 3.4 presents the effect of the intervention on the switching point in the risk-preference MPL (ranging from 1 to 21), with a higher switching point corresponding to more risk-loving preferences. The effect of the intervention is consistently small and not significantly different from zero. Models 2–4 show that men are more risk-loving, which is consistent with the latest evidence (Falk et al., 2018). There are no significant gender differences in the impact of the intervention ($Intervention \times Male$). Models 3 and 4 show that neither initially living in a private hostel nor any other control variables are significantly related to risk preferences. These results remain robust when we deal with censoring in our data by estimating Tobit models instead of OLS models (see Appendix B.1).

The regressions in Table 3.5 explain the switching point in the time-preference MPLs: the higher the value, the more impatient the subjects are. We run OLS models using data from all four MPLs (observations) per subject, with standard errors clustered at the household level. The MPLs differ in their earlier payment date and periods between the payment dates: the dummy variable *Delay* shows whether the earlier payment date was six working days away rather than just one; the dummy variable *Long period* indicates whether the waiting period lasted for 10 weeks rather than 2 weeks. Model 1 does not reveal that the intervention had any effect on participants' time preferences, nor do we find evidence of present bias (*Delay*)[7], nor on the interaction between the intervention and the present

[7]The negative sign indicates that people with the delayed early payment are more patient.

Table 3.4: OLS regressions – risk preferences

| *Dependent variable:* | Risk seeking (switching point) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Constant | 9.292*** | 8.513*** | 8.702*** | 8.380*** |
| | (0.533) | (0.583) | (0.666) | (1.687) |
| Intervention | 0.089 | 0.065 | 0.336 | −0.190 |
| | (0.941) | (0.976) | (1.166) | (1.089) |
| Male | | 2.754** | 2.787** | 2.215* |
| | | (1.140) | (1.151) | (1.310) |
| Intervention × Male | | 1.669 | 1.663 | 1.291 |
| | | (2.332) | (2.393) | (2.310) |
| Origin hostel | | | −0.512 | −0.482 |
| | | | (1.155) | (1.178) |
| Intervention × Origin hostel | | | −0.707 | −0.122 |
| | | | (1.945) | (2.132) |
| d2CP | | | | 0.003 |
| | | | | (0.019) |
| Days to pay | | | | −0.040 |
| | | | | (0.057) |
| Missing money | | | | 0.650 |
| | | | | (0.923) |
| Experimenter FE | No | No | No | Yes |
| Observations | 161 | 161 | 161 | 148 |
| $R^2$ | 0.0001 | 0.068 | 0.074 | 0.139 |
| Adjusted $R^2$ | −0.006 | 0.051 | 0.044 | 0.056 |

*Note:* Risk preferences are measured by switching points in the risk-preference MPL (ranging from 1 to 21), with a higher switching point corresponding to more risk-loving preferences. Standard errors clustered at the household level in parentheses. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

bias ($Delay \times Intervention$). The only parameter that clearly impacts patience is *Long period*: participants faced with the choice of a 10-week wait are less patient. All these results also hold after including the control variables in Models 2 – 4.

Models 2 and 3 do not reveal that sex or housing origin have any effect, nor

In the Tobit model, presented in Appendix B.1, the *Delay* variable is marginally significant.

Table 3.5: OLS regressions – time preferences

| *Dependent variable:* | Impatience (switching point) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Constant | 3.997*** | 3.701*** | 3.704*** | 4.068** |
| | (0.512) | (0.502) | (0.652) | (1.889) |
| Intervention | 0.280 | 0.899 | −0.130 | 0.471 |
| | (1.075) | (1.058) | (1.239) | (1.261) |
| Delay | −0.429 | −0.429 | −0.429 | −0.500 |
| | (0.284) | (0.284) | (0.285) | (0.326) |
| Intervention × Delay | 0.038 | 0.038 | 0.038 | −0.343 |
| | (0.618) | (0.618) | (0.619) | (0.537) |
| Long period | 3.081*** | 3.081*** | 3.081*** | 3.287*** |
| | (0.458) | (0.459) | (0.460) | (0.498) |
| Male | | 1.049 | 1.049 | 1.042 |
| | | (1.101) | (1.106) | (1.130) |
| Intervention × Male | | −2.818 | −2.879 | −2.555 |
| | | (1.933) | (1.913) | (1.953) |
| Origin hostel | | | −0.008 | 0.639 |
| | | | (1.066) | (1.029) |
| Intervention × Origin hostel | | | 2.724 | 1.271 |
| | | | (1.991) | (1.789) |
| LetterReturn | | | | −1.485 |
| | | | | (1.085) |
| Tomorrow | | | | −0.539 |
| | | | | (1.027) |
| Days to pay | | | | 0.001 |
| | | | | (0.045) |
| Missing money | | | | 2.005** |
| | | | | (0.799) |
| Risk | | | | 0.078 |
| | | | | (0.080) |
| d2CP | | | | 0.021 |
| | | | | (0.014) |
| Experimenter FE | No | No | No | Yes |
| Observations | 644 | 644 | 644 | 592 |
| $R^2$ | 0.063 | 0.071 | 0.087 | 0.176 |
| Adjusted $R^2$ | 0.057 | 0.062 | 0.075 | 0.149 |

*Note:* Time preferences are measured by switching points in time-preference MPLs (ranging from 1 to 21), with a higher switching point corresponding to higher impatience. Standard errors clustered at the household level in parentheses. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

that there was any difference in the treatment effect in these subgroups. Finally, Model 4 includes additional controls. In line with our expectations, we find that people who claim that they will lack enough money for something important over the next three days are less patient. These results hold when Tobit models are used to account for censoring in the data (see Appendix B.1).

Lastly, the regressions in Table 3.6 explain concentration performance in the d2 test (d2CP). The effect of the intervention is small and insignificant. Furthermore, there are no differences between the attention scores attained by men and women, nor between people who previously lived in hostels and others. We do not find that financial constraints have any effect on performance in the attention test.

## 3.6 Conclusion

This chapter sets out to contribute to the debate on poverty, preferences and cognitive abilities (Guiso and Paiella, 2008; Tanaka, Camerer, and Nguyen, 2010; Shah, Mullainathan, and Shafir, 2012; Mani et al., 2013). We have exploited random variation in housing conditions, created by a housing-first experiment, to study the causal impact of improved living standard on risk preferences, time preferences, and sustained attention. Studying the effects of housing quality is essential not only for understanding the impact of poverty on peoples' behavior but also for evaluating public housing policies.

We ran a lab-in-the-field experiment to collect incentivized measures of risk preferences, time preferences and sustained attention. Our data do not show that improved housing conditions have any effects on these variables. While we do not replicate the results of many of the field studies (Tanaka, Camerer, and Nguyen, 2010; Guiso and Paiella, 2008; Mani et al., 2013), our results are in line with those reported by Carvalho, Meier, and Wang (2016). It seems that our results are similar to theirs for two reasons: First, our measures of time preferences are not affected by differences in liquidity constraints. Second, both of our studies focus on poor inhabitants in developed countries (OECD members).

There are some caveats to our conclusions. First, our sample size of around

Table 3.6: OLS regressions – d2 concentration performance

| *Dependent variable:* | d2CP | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Constant | 43.843*** | 44.562*** | 42.924*** | 45.040*** |
| | (2.631) | (2.861) | (2.876) | (4.781) |
| Intervention | −2.039 | −0.657 | −0.167 | 1.115 |
| | (4.761) | (5.141) | (6.512) | (6.274) |
| Male | | −2.527 | −2.951 | −4.493 |
| | | (5.192) | (5.314) | (5.722) |
| Intervention × Male | | −9.378 | −9.145 | −4.703 |
| | | (8.287) | (8.277) | (7.524) |
| Origin hostel | | | 4.598 | 6.438 |
| | | | (5.967) | (5.744) |
| Intervention × Origin hostel | | | −1.586 | −4.507 |
| | | | (9.551) | (8.811) |
| Days to pay | | | | −0.452 |
| | | | | (0.285) |
| Missing money | | | | −4.433 |
| | | | | (4.627) |
| Experimenter FE | No | No | No | Yes |
| Observations | 153 | 153 | 153 | 148 |
| $R^2$ | 0.001 | 0.014 | 0.020 | 0.190 |
| Adjusted $R^2$ | −0.005 | −0.006 | −0.013 | 0.118 |

*Note:* Standard errors clustered at the household level in parentheses. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

150 observations is sufficient only for detecting large or medium-sized effects. We are not able to rule out the existence of small effects. Second, due to the limited attention of our experimental subjects, we were not able to use multiple measures of cognitive abilities and the quality of decision making, like Carvalho, Meier, and Wang (2016). Our findings might therefore not be informative for other aspects of cognition and decision making. Finally, the treatment we study affects the subjects' economic situations though their consumption of housing services. Our approach does not capture the financial aspects of wealth. This might limit the

external validity of our setup. On the other hand, it makes it more relevant in situations where improved housing quality is an important aspect of development.

# Chapter 4

# Trustworthiness and concentration

## 4.1 Introduction

Homelessness, which manifests in having either no address or in living in a homeless shelter or hostel, may complicate poor people's access to government services, housing and jobs. Using qualitative and survey evidence from homeless individuals, employment specialists, service providers and employers, Golabek-Goldman (2016) concludes that homeless applicants face discrimination in the labor market when they provide the address of a shelter or do not have any address to provide. Poremski, Whitley, and Latimer (2014) also identify job-market barriers related to homelessness. A similar adverse effect is likely to arise in other contexts, such as when applying for housing or to use various government services.[1]

[1] On the other hand, the evidence from housing projects suggests that moving to better housing does not positively impact employment. For example, Gubits et al. (2018) show that a long-term rent subsidy for families recruited from homeless shelters in the Family Options Study decreased their homelessness but resulted in lower employment. However, these effects could be linked to the subsidy's direct effect on the participants' incomes. Poremski et al. (2016) find that moving adults with mental illness from homeless shelters to scattered-site housing and providing them with assertive community treatment or intensive case management does not impact their employment rates.

In this study, we look at two different possible reasons for these adverse outcomes of homelessness. First, people living in substandard housing conditions could be perceived as less able to concentrate or less trustworthy. While the ability to sustain attention is directly linked to these people's labor-market outlooks, trustworthiness has a broader relevance; it will matter in all situations where the homeless person's own actions affect another party's outcome. It might matter in the labor market, e.g. for gaining a supervisor's trust (Colquitt, Scott, and LePine, 2007). In the housing market, people will be reluctant to rent a property to a potential tenant they do not trust, since bad behavior on the part of the tenant could affect their revenue: the tenant might not pay rent, might refuse to move out, or might damage the furnishings.[2]

The effect of homelessness on expected concentration performance and trustworthiness may be caused by two mechanisms: 1) people with a lower ability to focus and lower trustworthiness might be more likely to end up homeless or in inadequate housing (a *history of inadequate housing* might provide signals about the low level of these qualities); 2) *inadequate housing conditions* might be considered to have a direct adverse effect on people's trustworthiness and ability to concentrate.

The design of our laboratory experiment enables us to separate these two mechanisms. To identify the causal effect of housing on the expected concentration and trustworthiness, we use the exogenous variation in housing conditions created by a Housing-First RCT, which started in Brno (Czech Republic) in 2016. Out of the population of 421 families in need of housing, 50 families were randomly selected to receive rental contracts for municipal flats and a package of social services related (mostly) to this new housing, while 100 families were selected into a control group and received neither the housing nor the services.[3] We

[2]Baldini and Federici (2011) find that ethnic discrimination in the Italian housing market appears stronger when immigrants apply for small or medium-sized flats, which might indicate lower trustworthiness.

[3]As documented in Section 2.3, the treatment significantly improved the quality and perceived stability of housing. In this chapter we focus on a subset of families that lived in private hostels before the start of the project. In Brno, private hostels tend to be located in large buildings containing many housing units. These units typically consist of one room, usually inhabited by

can isolate the effect of *inadequate housing conditions* by comparing the expected outcomes of treatment and control group participants from the same population. The students who participate in the lab experiment are informed about typical housing conditions in hostels (control group) and municipal flats (treatment), and that the treatment families used to live in hostels before being awarded the long-term lease of a municipal flat without any additional conditions (screening). To measure the effect of the *history of inadequate housing*, we create an additional treatment group in which the information about the treatment families' housing history is not provided. This effect is isolated by comparing the perceived outcomes of the project participants living in flats, with and without a known history of inadequate housing.

We ran two laboratory experiments with about 160 student participants each, one to measure expected concentration performance and the other to elicit expected trustworthiness. In experiment 1, students were asked to estimate the *expected concentration performance* of the adult members of families enrolled in the Housing-First RCT. These participants' concentration performance had been previously measured using the d2 test of sustained attention (Brickenkamp, 1962) in individual lab-in-the-field sessions with the participants of the Housing First project themselves. In experiment 2, we paired students and project participants in a trust game (Berg, Dickhaut, and McCabe, 1995), with students in the role of senders and the adult members of the project families in the role of receivers. The *expected trustworthiness* is defined as the amount the sender expects to receive back from the receiver. It reveals the sender's expectations about the receiver's willingness to give up financial benefits in order to reciprocate the proposer's investment.

We find that trustworthiness is causally linked to housing conditions, but not to housing history. The student senders believe that people living in inadequate housing conditions would be less reciprocal and send less money back. Moreover, they expect less money from families with more children. These results suggest

one family. Kitchens, bathrooms, and showers are shared between many families. These hostels are typically run for profit by private firms.

that they base their expectations on the families' perceived neediness. Assuming that the families' incomes do not differ, the students expect that larger families and families with a greater need for housing will keep higher shares of the investment, presumably because they have a better use for the money. In contrast to trustworthiness, we find that expected concentration performance is not affected by housing conditions but is linked to housing history. The students expect that people with a history of inadequate housing will differ in trustworthiness from people without such a history. This result is in line with the above-mentioned mechanism according to which people with lower cognitive abilities might be expected to end up in substandard housing more often.

This chapter contributes to the literature on the persistence of poverty (Banerjee, Banerjee, and Duflo, 2011; Kraay and McKenzie, 2014; Barrett, Garg, and McBride, 2016), as poverty is often linked with bad-quality housing. If inadequate housing conditions affect trustworthiness, this might contribute to the vicious circle of poverty (e.g. Haushofer and Fehr, 2014). On the other hand, if improved housing conditions engender trust, or even affect expected concentration performance, this might be another reason to promote housing that prioritise providing homeless people with good quality housing.

Our results also indirectly contribute to the literature on discrimination based on address, which offers mixed results so far. Bonnet et al. (2016) show that living in deprived suburbs is related to a lower chance of getting an appointment for a housing vacancy. Using a fictitious-résumé method, Bertrand and Mullainathan (2004) find a lower callback rate for resumes with addresses in less affluent neighborhoods of Boston and Chicago. On the other hand, Tunstall et al. (2014) do not find any such neighborhood effects in the UK. Phillips (2018) shows that distance to employment, not the neighborhood's affluence, is related to the callback rate. This could explain the difference in the previous studies' findings, since the applications sent by Tunstall et al. (2014) were matched to have similar commute distances, while those in Bertrand and Mullainathan (2004) were not. While particular neighborhoods are often linked to a certain quality of housing, it is hard for these studies to distinguish between bad signals from less affluent neighborhoods and the direct effects of housing quality.

The rest of this chapter is structured as follows. Section 4.2 presents the design and procedures of the laboratory experiments measuring the expected concentration performance and trustworthiness. Section 4.3 reports the data. Section 4.4 discusses the results. Section 4.5 concludes.

## 4.2 Experimental design and procedures

We conduct two laboratory experiments with a student population to elicit their expectations about concentration performance measured by sustained attention (experiment 1) and trustworthiness measured by an incentivized trust game (experiment 2).

### 4.2.1 Experiment 1

In experiment 1, students were asked to estimate the average concentration performance of different groups of participants in Brno's Housing First project in a d2 test of sustained attention (Brickenkamp, 1962; Bates and Lemay, 2004).[4] In the test, the Housing First participants received two sheets of paper containing a large number of letters *p* and *d* with 0, 1, or 2 vertical lines above and below the letters (see Figure A.5). Their task was to find and mark all *d* characters with a total of two vertical lines above and or below them. The time limit was 280 seconds, which means that they had 20 seconds for each of the 14 lines, which contained 49 characters each. Concentration performance is one of the overall performance measures in this test and is calculated as the number of correctly marked symbols minus the total number of incorrectly marked symbols. We incentivized concentration performance by paying the participants 1 CZK for each correctly marked symbol and deducting 1 CZK for each incorrectly marked symbol. We corrected the test at the end of the experimental session.

Before the start of the task, each participant had 40 seconds to complete a trial sheet. The research assistants then went through the trial test with them,

[4]See Appendix A.1.3 for the experimental instructions.

showing the participants where they had missed characters and made mistakes, and counting their correctly marked symbols. If everything was clear, the subjects were then free to start the actual test.

The experiment with students consisted of two sessions, the first of which took place 14 days before the second (see Appendix A.2 for the experimental instructions). In the first session, students took a short version of the d2 test to enable them to assess the difficulty of the test. The procedure resembled that used in the field experiment. Students were given 40 seconds to complete the trial test. They corrected the test themselves; research assistants were available to resolve any doubts and answer any questions. Once the rules of the test were clear, they were given 70 seconds (1/4 of the time given to participants in the field) to complete the actual test. Both the trial test and the actual test were administered on paper, to mimic the procedures of the field experiment. After the end of the test, the students were allowed to self-correct their tests using pencil only. They received one point for each correctly marked symbol and lost one point for each incorrect marking. They were then paid 1 CZK per point according to their final score. They were asked to input their final score into the experimental computer environment. In the 14 days between the sessions, we checked their corrected tests. If their reported score was correct, they received an additional bonus of 10 CZK, to motivate the participants to find out the actual score they achieved. Out of 159 students, 11 made a mistake in the correction.

Once the tests were completed, each student was asked to make four choices on four decision screens. The task was to choose the expected average concentration performance (d2CP) achieved by four different groups of families in the Housing First project. We took the information about the family groups from questionnaires collected six months after the treatment families were moved. Each decision screen contained the following information (the exact wording on the decision screen is given in italics):

*The receiver will be randomly selected from a group of families with the following average characteristics:*

- *Number of children: 2 or 3*

- *Total family income: 20 000 CZK*
- *Family income net of housing costs: 11 000 CZK*
- *Detailed description of housing: 3 types (see below for full descriptions)*

Since the sample families had between 2 and 3 children, we used two versions of the family description. Variation in the number of children was also intended to reduce the potential demand effect that might result from the within-subject variation of housing conditions (Haushofer, Riis-Vestergaard, and Shapiro, 2019).

The three housing types were described as follows:

- Hostel (H)[5]

  *The family lives in a hostel.*

  *The whole family lives in one room. The toilet, bathroom and kitchen facilities are shared with other residents of the hostel. Occasionally toilet and electricity are not working. There are large holes or cracks in the walls through which cold air or rain penetrates. The walls, ceilings or floors are damp, moldy, or damaged by water. In some cases, cockroaches, bugs or rats appear in the hostel. Sometimes there are noisy or otherwise problematic neighbors.*

- Municipal flat + no info (F NI)

  *The family rents a municipal flat.*

  *The municipal flat is a standard multi-room apartment with a kitchen and bathroom. The apartment has no major defects.*

- Municipal flat + from hostel (F FH)

  *The family lives in a municipal flat. They previously lived in a hostel but the city then awarded them a lease for this municipal flat; this was not subject to any other conditions.*

[5] The information about hostel standards was also taken from the questionnaire collected six months after the treatment families were moved.

*The municipal flat is a standard multi-room apartment with kitchen and bathroom. The apartment has no major defects.*

*In the hostel, the whole family lived in one room. The toilet, bathroom and kitchen facilities were shared with other residents of the hostel. Occasionally toilet and electricity are not working. There were large holes or cracks in the walls through which cold air or rain penetrated. The walls, ceilings, or floors were damp, moldy, or damaged by water. In some cases, cockroaches, bugs, or rats appeared in the hostel. Sometimes there were noisy or otherwise problematic neighbors.*

Table 4.1 shows all six treatments given by the variation. Each student was given four of these treatments—the two versions of the municipal flat, *no info* (NI) and *from hostel* (FH), were given to different subjects. Hence, there are two between-subject treatments, each consisting of four within-subject treatments: H2, H3, F2 NI, F3 NI and H2, H3, F2 FH, F3 FH. The order is randomized in four ways: H2–H3–F2–F3, F2–F3–H2–H3, H3–H2–F3–F2, or F3–F2–H3–H2. In these orders, the number of children changes first in order to limit possible demand effects related to housing.

Table 4.1: Overview of treatments

| | Hostel | Municipal flat | |
|---|---|---|---|
| | | No info | From hostel |
| 2 children | H2 | F2 NI | F2 FH |
| 3 children | H3 | F3 NI | F3 FH |

The laboratory experiment took place at the Masaryk University Experimental Economics Laboratory (MUEEL) in Brno during October and November 2019. The experimental environment was created in zTree (Fischbacher, 2007). We recruited 159 students through hroot (Bock, Baetge, and Nicklisch, 2014) for an experiment that contained several tasks administered in two sessions held 14 days apart. The other tasks were not related to the aim of this paper and thus will not be discussed here. This task was the second task in the first session. Students were not informed about the payoffs from the individual tasks until the end of the second session, when they received payoffs from all the tasks.

In addition to the payment for the completion and correction of the d2 test, the students received a payoff based on one randomly selected choice. If their guess was five points or fewer from the Housing-First families' actual performance score, they received 50 CZK. The whole task took approximately 15 minutes. Only 11 out of 159 students guessed correctly. The average payoff, including the payment for the d2 test and the correction bonus was 41 CZK (= 1.5 EUR).[6] Six of the students did not participate in the second session and therefore did not receive any payment from the experiment at all. All students were informed several times that they would not receive any payoffs if they failed to show up for the second session. We made sure that there were no participants who did not intend to participate in the second session. Judging from the excuses we received from these participants by email, their absence was related to unexpected situations such as hospitalization or illness. We therefore use all the data we had collected in the first session.

### 4.2.2 Experiment 2

In experiment 2, we used a standard trust game (Berg, Dickhaut, and McCabe, 1995) to elicit expected trustworthiness. In the trust game, the students took on the role of senders and the adult members of the families participating in Brno's Housing First project played the role of receivers.[7] As in experiment 1, we limited our sample to families that had initially stayed in hostels (about 1/3 of the total sample). Each sender was given an endowment of 150 CZK[8] and could choose an amount to send, $s \in [0, 30, 60, 90, 120, 150]$. The receiver got $3s$ and decided which amount $r \in [0, 3s]$ to return to the sender. In addition to that, we elicited the

[6] This amount corresponds to 20 minutes hours of unqualified student labor.

[7] See Appendix A.1.5 for experimental instructions for the receivers (participants of the Housing First project), and Appendix A.3 for the experimental instructions for the senders (students in the laboratory experiment).

[8] At the time of the experiment, this amount was roughly equivalent to 6 euro, which corresponded to the market price of 60–80 minutes of unqualified student labor.

*expected trustworthiness*: We asked what amount of CZK 180 received ($s = 60$) the sender expected the receiver would send back. The sender's payoff equaled to $150 - s + r$, the receiver's payoff was $3s - r$. The reward from the expected trustworthiness elicitation was 200 CZK if the guess was 10 CZK or less from the actual amount returned by the receiver.

Each student made 8 choices on 8 decision screens. The task was to choose amounts to send (send) and to predict amounts sent back by the receivers (expect) for four different families. We used the same descriptions of families and ordering of the treatments as in Experiment 1 (see Section 4.2.1). The only difference compared to Experiment 1 is that in Experiment 2 each subject made two choices instead of one. Subjects input their sending choices to screens 1–4 and their expectations to screens 5–8. Each student was matched with an adult family member from the corresponding group of families, and the payoff was calculated from one randomly selected choice.

We collected the data from student senders in February and March 2018. The experiment was conducted in MUEEL using a computerized environment programmed in zTree (Fischbacher, 2007). Subjects were recruited using hroot (Bock, Baetge, and Nicklisch, 2014). The trust game was the first of several parts of the experimental session. Students received the payments from the other parts at the end of the session. The payoffs of the trust game were paid out in summer 2018 after the decisions from project families had been collected. The average payoff from the trust game was 93 CZK (= 3.5 EUR). Only 5 students failed to provide us with their bank account number or collect the money in person.

## 4.3 Data

### 4.3.1 Experiment 1

Table 4.2 provides summary statistics from Experiment 1. The upper part shows the d2CP scores of the Housing First families. Out of the total of 160 data points (in some families, both parents or partners participated), we included only the 59 participants who had lived in hostels before the project started. The participants had 280 seconds to complete the test. The average score in the control group is slightly higher than the score in the intervention families that were moved to municipal flats. However, the difference is not statistically significant (t-test $p = 0.62$, MW $p = 0.59$). Housing First participants achieved around 0.15 points per minute, which is substantially lower performance compared to the 0.4 points per minute obtained by students (see d2CP 70s below).

Table 4.2: Summary statistics: subject specific variables

| Variable | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| *Housing First participants* | | | | | |
| d2CP 280s | 59 | 45.46 | 24.66 | −27 | 96 |
| – intervention | 20 | 43.35 | 21.30 | −7 | 85 |
| – control | 39 | 46.54 | 26.42 | −27 | 96 |
| *Students* | | | | | |
| d2CP 70s | 159 | 28.51 | 6.66 | 2 | 57 |
| Age | 159 | 21.52 | 2.20 | 19 | 31 |
| Female | 159 | 0.45 | 0.50 | 0 | 1 |
| Czech | 159 | 0.50 | 0.50 | 0 | 1 |
| Econ | 159 | 0.55 | 0.50 | 0 | 1 |
| WorkExp | 159 | 0.68 | 0.47 | 0 | 1 |
| KnowHF | 159 | 0.06 | 0.24 | 0 | 1 |

The lower part of the table provides information about the characteristics of our student subjects. Half of our students were of Czech nationality and half of Slovak nationality, 55% studied economics and business (Econ), 68% had some work experience (*WorkExp*). Only 6% of them had already heard about Brno's Housing-First project, which was covered in the local and national media (*KnowHF*).

Table 4.3 presents the means and standard deviations of the expected CP scores based on first choices (the first out of four screens) and all data. The scores suggest the number of children (2 or 3) had no impact, housing (hostel H vs. flat F) had a limited impact, and origin (F NI vs. F FH) had a high impact. Figure 4.1 presents a histogram of the expected concentration performance based on all data.

Table 4.3: Summary statistics: expected d2CP

| Variable | H2 | H3 | F2 | F3 | F NI | F FH |
|---|---|---|---|---|---|---|
| *First choices* | | | | | | |
| Mean exp d2CP (SD) | 67.32 | 64.55 | 64.89 | 77.27 | 80.29 | 63.25 |
| | (25.46) | (27.82) | (28.93) | (26.68) | (25.74) | (28.36) |
| Observations | 43 | 42 | 37 | 37 | 34 | 40 |
| *All data* | | | | | | |
| Mean exp d2CP (SD) | 66.14 | 64.20 | 74.86 | 75.20 | 81.29 | 68.69 |
| | (27.10) | (27.08) | (27.10) | (27.46) | (25.71) | (27.34) |
| Observations | 159 | 159 | 159 | 159 | 160 | 158 |

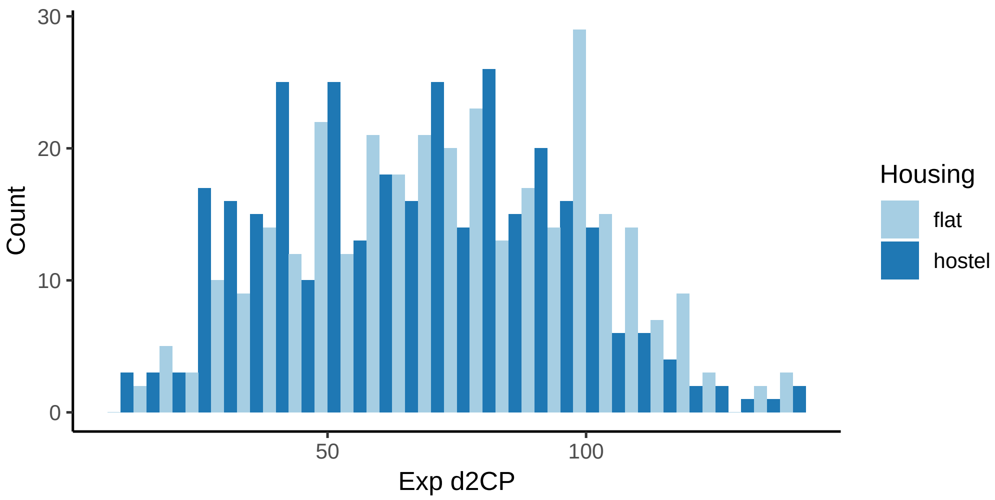



Figure 4.1: Histogram of expected concentration performance in the d2 test based on all data

### 4.3.2 Experiment 2

Table 4.4 summarizes the data from Experiment 2. Most of our subjects were Czech (61%), female (60%) students of economics and business (65%) with work experience (72%). Only 9% of them had heard about Brno's Housing First project (*KnowHF*).

As for the outcome variables, we will present the money sent (*send*) as well as the expected amounts returned (*expect*), even though our main variable of interest is *expect*. We are not going to discuss the actual amounts returned by the RCT participants, because we do not have enough power to test for any differences between treatment and control (we have less than 1/3 of the sample and five levels of amounts sent). Table 4.5 presents the means and standard deviation of *send* and *expect* based on first choices (the first out of four screens) and all data for the main treatments: H stands for hostel and F for flat; FH indicates that students were informed that participants originally lived in a hostel, and NI means that they did not receive this information. Table 4.5 does not reveal any treatment effects on sending behavior. *Expect* provides a clear pattern: Students expect more money from smaller rather then larger families (H2 and F2 vs. H3 and F3) and from families living in hostels rather than flats (H vs. F). The impact of the families' origin (F NI vs F FH) seems small. Figure 4.2 presents histograms of send and expect.

Table 4.4: Summary statistics: subject specific variables

| Variable | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| Age | 160 | 22.30 | 2.27 | 18 | 30 |
| Female | 160 | 0.60 | 0.49 | 0 | 1 |
| Czech | 160 | 0.61 | 0.49 | 0 | 1 |
| Econ | 160 | 0.65 | 0.48 | 0 | 1 |
| WorkExp | 160 | 0.72 | 0.45 | 0 | 1 |
| KnowHF | 160 | 0.09 | 0.28 | 0 | 1 |

Table 4.5: Summary statistics: send and expect

| Variable | H2 | H3 | F2 | F3 | F NI | F FH |
|---|---|---|---|---|---|---|
| *First choices* | | | | | | |
| Mean send (SD) | 65.71 | 65.71 | 58.42 | 68.68 | 57.43 | 68.78 |
| | (51.00) | (47.89) | (37.53) | (43.51) | (34.41) | (45.12) |
| Mean expect (SD) | 45.05 | 26.64 | 64.32 | 53.11 | 61.40 | 56.42 |
| | (41.15) | (27.00) | (34.33) | (28.00) | (27.79) | (34.74) |
| Observations | 42 | 42 | 38 | 38 | 35 | 41 |
| *All data* | | | | | | |
| Mean send (SD) | 63.00 | 67.50 | 56.63 | 60.94 | 58.69 | 58.88 |
| | (47.84) | (50.36) | (42.82) | (42.75) | (39.72) | (45.75) |
| Mean expect (SD) | 37.24 | 31.84 | 63.03 | 53.98 | 59.38 | 57.63 |
| | (31.87) | (30.34) | (38.94) | (33.55) | (33.67) | (39.34) |
| Observations | 160 | 160 | 160 | 160 | 160 | 160 |

## 4.4 Results

### 4.4.1 Experiment 1

The main effects on expected concentration performance (d2CP) are summarized in Figure 4.3. This shows that better housing conditions do not increase expected concentration performance: the difference between hostel and flat FH is not statistically significant. When information about the participants' housing history is not revealed to the students (F NI), the expected concentration performance increases significantly compared to flat FH (MWU $p = 0.01$) and hostel (MWU $p = 0.008$).

The same picture emerges from OLS regressions presented in Table 4.6. We use two data sets. In models 1–2, the sample is restricted to first choices; models 3–5 are based on all data. All regressions control for the number of children and the students' performance in the d2 test (*d2CP 70s*). Across the models, expected performance increases with the students' actual performance: one point increase in d2CP 70s performance increases the expected performance of the project participants (d2CP 280s) by roughly 1.3 points (see Table 4.2 for summary

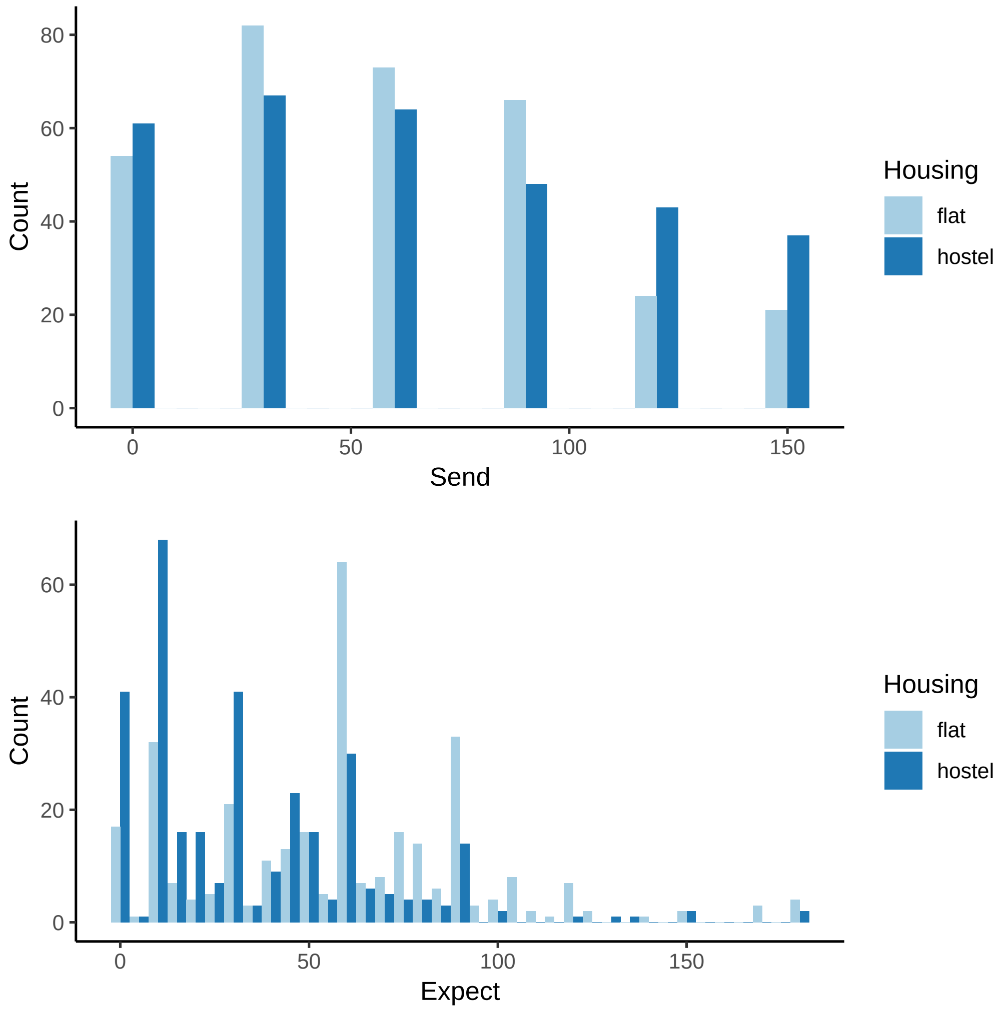



Figure 4.2: Histogram of send and expect based on all data

statistics of these variables). The effect of larger families (3 vs. 2 children) is small and positive for the first choices but disappears when all data are taken into account.

The variables of interest are *FlatFH* and *FlatNI*: *FlatFH* takes the value of one in the F2 FH and F3 FH treatments; *FlatNI* is equal to one in F2 NI and F3 NI treatments. The contrast are families living in private hostels (H2 or H3). Models 1 and 3 show no consistent effect of FlatFH compared to hostel.

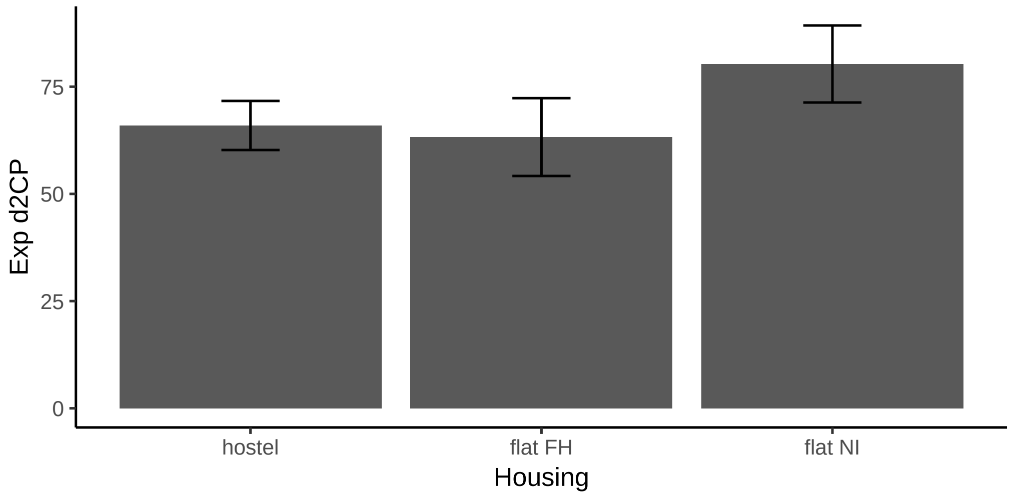



Figure 4.3: Expected concentration performance in the d2 test (d2CP) in the data on first choices ($N = 159$). The data contain only the first choices made in the experiment. The bars display 95% confidence intervals.

The expected performance is somewhat lower in the between-subject data (first choices) and somewhat higher in the data as a whole. When the information about housing origin is not provided (*Flat NI*), the expected concentration performance is higher and this effect highly statistically significant in both the first-choices data and across all the data.

The main results hold after additional control variables are included in models 2 and 4. The estimated parameters in the control variables are not significantly different from zero. The only exception is the variable *KnownHF*, a dummy variable that takes the value of one in the ten cases when our subject had heard about the Housing First project in Brno. A similar picture emerges when we exploit within-subject variation using subject-specific fixed effects in model 5.

### 4.4.2 Experiment 2

This section presents the outcomes of the trust game. Figure 4.4 presents the main results based on the first-choices data. Sending behavior does not depend on the type of housing nor on the number of children. In this context, however,

Table 4.6: OLS regressions of concentration performance in a d2 test

| *Dep. var.:* | Expected concentration performance in a d2 test (Exp d2CP 280s) | | | | |
|---|---|---|---|---|---|
| | First choices | | All data | | |
| | (1) | (2) | (3) | (4) | (5) |
| Constant | 27.545*** | 44.382** | 27.180*** | 41.952*** | |
| | (8.079) | (19.489) | (0.647) | (11.597) | |
| 3Children | 1.214** | 0.968* | −0.805 | −0.805 | −0.805 |
| | (0.507) | (0.511) | (0.585) | (1.966) | (0.675) |
| FlatFH | −3.999* | −4.972** | 3.544*** | 3.281 | 4.557*** |
| | (2.049) | (1.978) | (1.133) | (2.430) | (0.958) |
| FlatNI | 14.948*** | 14.997*** | 16.100*** | 16.360*** | 15.100*** |
| | (2.301) | (2.285) | (1.831) | (2.419) | (0.952) |
| d2CP 70s | 1.333*** | 1.385*** | 1.347*** | 1.310*** | |
| | (0.303) | (0.307) | (0.151) | (0.152) | |
| Age | | −0.720 | | −0.548 | |
| | | (0.910) | | (0.502) | |
| Female | | −3.550 | | −2.062 | |
| | | (4.040) | | (2.078) | |
| Czech | | −4.898 | | −2.438 | |
| | | (3.893) | | (2.049) | |
| Econ | | 3.837 | | 3.602 | |
| | | (4.223) | | (2.212) | |
| WorkExp | | −0.034 | | 1.677 | |
| | | (4.413) | | (2.251) | |
| KnowHF | | −8.557 | | −13.767*** | |
| | | (7.033) | | (4.320) | |
| Order FE | | | No | Yes | |
| Subject FE | | | No | No | Yes |
| Observations | 159 | 159 | 636 | 636 | 636 |
| $R^2$ | 0.160 | 0.194 | 0.164 | 0.208 | 0.929 |
| Adjusted $R^2$ | 0.138 | 0.139 | 0.158 | 0.191 | 0.905 |

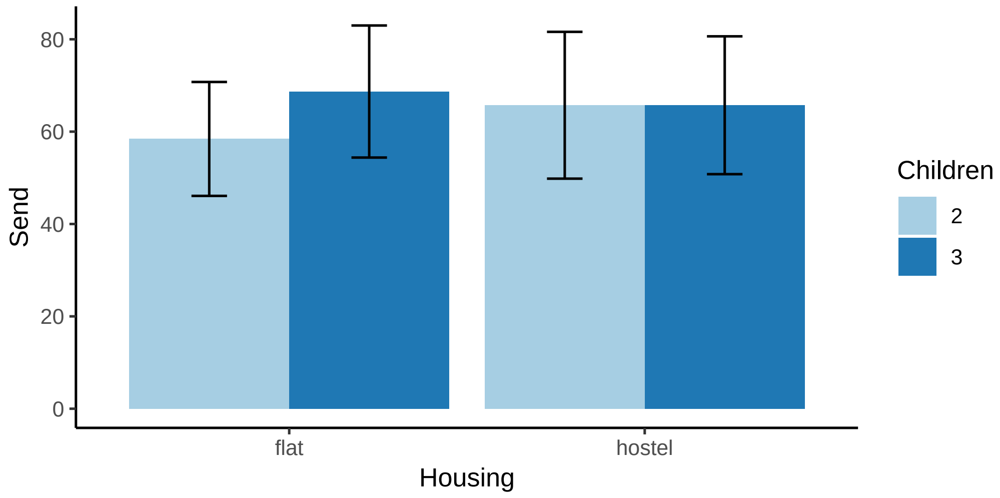
*Note:* Columns 1–2 show the first choices in the experiment (between-subject decisions), columns 3–5 show all data (4 choices per participant). Standard errors are shown in parentheses. Standard errors in columns 3–5 are clustered at the level of subject. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

sending choices are likely to be affected by both expected trustworthiness and altruism. For this reason, we elicited expectations about the receivers' choices, to gain a direct measure of their expected trustworthiness. The data on *expect* show a clear pattern: Recipients living in flats are considered more trustworthy than recipients living in private hostels (MWU $p < 0.0001$), and those with two children are expected to send back more than those with three children (MWU $p = 0.01$).

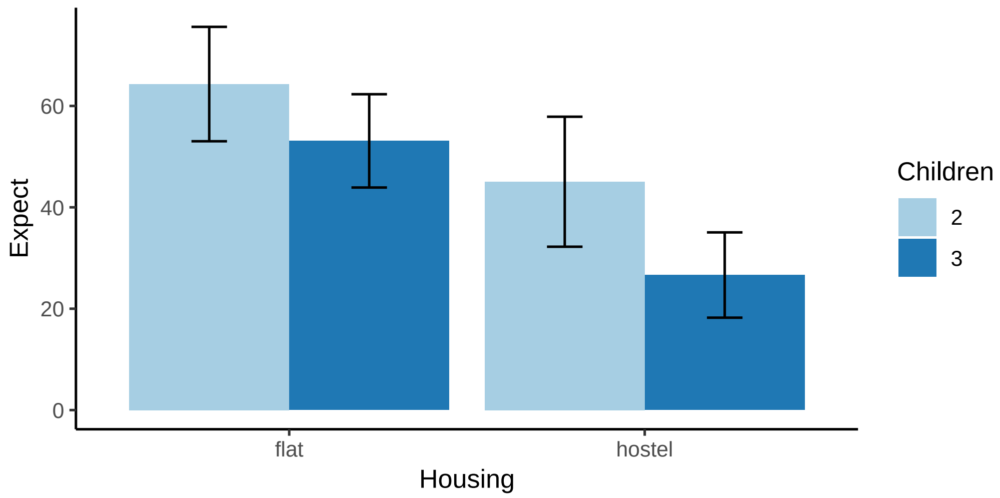
These findings about expected trustworthiness are confirmed in regression analyses presented in Table 4.7. The table has the same structure as Table 4.6: Models 1–2 use data from first choices only, models 3–5 take advantage of all the data available. Models 1 and 3 contain variables *3Children* (as opposed to 2 children), *Flat FH* and *Flat NI* (as opposed to hostel). Models 2 and 4 control for other possible confounds, and model 5 adds subject-specific fixed effects. The regressions shows that the student senders expected to receive more money back from recipients with smaller families (as opposed to larger families) and from families living in flats (as opposed to hostels). It seems that the senders expected to get less money from receivers in need. Larger families and families living in provisional housing conditions could be seen as having a more serious shortage of money.

On the other hand, we do not find any significant difference between the Flat FH and Flat NI variants, except in model 5, which uses only within-subject variation. As we show in Figure 4.5, this effect is not driven by differences between *Flat FH* and *Flat NI*, but in the parallel hostel questions: our students expected to receive less money from people living in hostels in the between-subject session with *Flat NI* than in the session with *Flat FH*, even though the choices were the same. By focusing only on the within-subject variation, Model 5 captures differences in levels of *expect* in hostels that have no reasonable interpretation. In line with the results of models 1–4 and evidence shown in Figure 4.5, we conclude that a history of substandard housing does not affect the expected trustworthiness of our project participants.

Table 4.8 presents the same regressions as in Table 4.7, but for sending be-




(a) The amount sent (send)




(b) The amount expected to be returned out of 180 CZK (expect)

Figure 4.4: The amount sent and the expected amount returned in the data on first choices ($N = 160$). The bars display 95% confidence intervals.

havior. The model results confirm the conclusions drawn from Figure 4.4. While students seem to send somewhat lower amounts to participants living in flats than to those living in hostels, this finding does not appear in all specifications

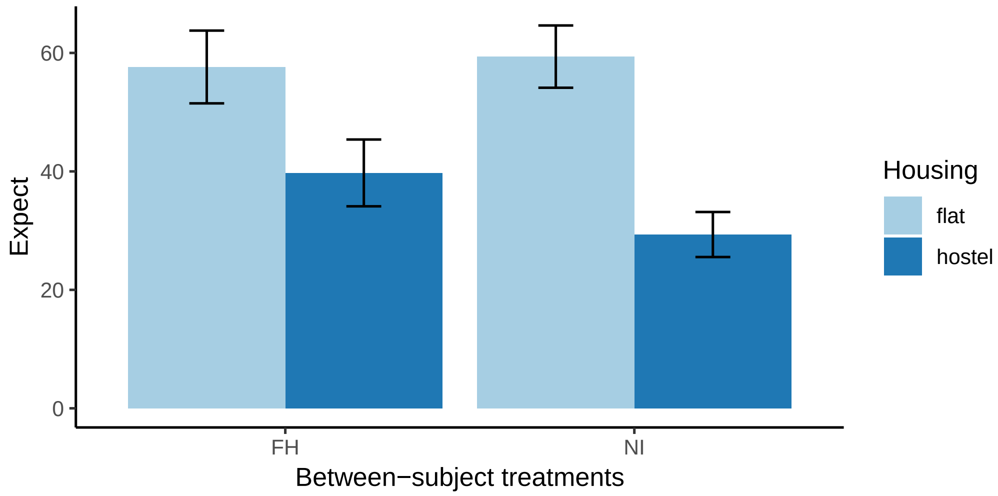



Figure 4.5: The within-subject comparison of the expected amount returned (expect) based on all data ($N = 640$). The data are differentiated by the between-subject treatments: the left pair of columns show amounts returned by participants living in hostels and flats, when the students were provided with information about their history of inadequate housing. The right pair of columns show the expected amounts returned in the between-subject treatment when no additional information about the flat inhabitants' housing history was provided. The bars display 95% confidence intervals.

(see models 3 and 4), and the difference is quantitatively small. Interestingly, regressions 2 and 4 also show that Czech students send less than Slovak students. This effect could be driven by differences in altruism, or by the Czech students' knowledge of local housing conditions.

## 4.5 Conclusion

In this chapter, we have seen that homeless parents are perceived as less trustworthy and less able to focus. Lower trustworthiness is causally linked to housing conditions but not to housing history. Student-senders believe that people living in inadequate housing conditions would be less reciprocal and sent less money

Table 4.7: OLS regressions of expected trustworthiness in a trust game

| *Dep. var.:* | Expected amount returned (expect) | | | | |
|---|---|---|---|---|---|
| | First choices | | All data | | |
| | (1) | (2) | (3) | (4) | (5) |
| Constant | 43.455*** | 85.443*** | 38.156*** | 95.259*** | |
| | (2.434) | (23.878) | (2.176) | (14.632) | |
| 3Children | −15.220*** | −16.955*** | −7.225*** | −7.225*** | −7.225*** |
| | (1.335) | (1.345) | (1.542) | (2.614) | (1.757) |
| FlatFH | 20.013*** | 23.210*** | 23.087*** | 23.032*** | 17.887*** |
| | (3.752) | (3.778) | (4.309) | (3.223) | (2.485) |
| FlatNI | 26.207*** | 24.634*** | 24.837*** | 24.893*** | 30.037*** |
| | (3.779) | (3.769) | (4.042) | (3.223) | (2.485) |
| Age | | −1.089 | | −1.811*** | |
| | | (1.020) | | (0.646) | |
| Female | | −6.429 | | −2.093 | |
| | | (4.568) | | (2.754) | |
| Czech | | −15.407*** | | −7.729*** | |
| | | (4.628) | | (2.824) | |
| Econ | | −1.160 | | −3.948 | |
| | | (4.426) | | (2.954) | |
| WorkExp | | −4.475 | | −4.676 | |
| | | (5.172) | | (3.025) | |
| KnowHF | | −0.395 | | −5.135 | |
| | | (4.982) | | (4.749) | |
| Order FE | | | No | Yes | |
| Subject FE | | | No | No | Yes |
| Observations | 160 | 160 | 640 | 640 | 640 |
| $R^2$ | 0.151 | 0.225 | 0.121 | 0.173 | 0.716 |
| Adjusted $R^2$ | 0.135 | 0.179 | 0.117 | 0.157 | 0.619 |

*Note:* Columns 1–2 show the first choices in the experiment (between-subject decisions), columns 3–5 show all data (4 choices per participant). Standard errors are shown in parentheses. Standard errors in columns 3–5 are clustered at the level of subject. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

back. Moreover, they expect less money from families with more children. These

Table 4.8: OLS regressions of sending behavior in a trust game

| *Dep. var.:* | Amount sent (send) | | | | |
|---|---|---|---|---|---|
| | First choices | | All data | | |
| | (1) | (2) | (3) | (4) | (5) |
| Constant | 63.056*** | 56.410* | 63.047*** | 62.077*** | |
| | (3.762) | (32.711) | (2.127) | (20.175) | |
| 3Children | 5.317*** | 4.885*** | 4.406** | 4.406 | 4.406** |
| | (1.494) | (1.505) | (1.725) | (3.604) | (2.074) |
| FlatFH | 3.261 | 5.280 | −6.375 | −5.905 | −7.312** |
| | (4.279) | (4.280) | (3.918) | (4.444) | (2.933) |
| FlatNI | −8.514* | −10.494** | −6.562 | −7.032 | −5.625* |
| | (4.459) | (4.539) | (5.672) | (4.444) | (2.933) |
| Age | | 0.785 | | 0.130 | |
| | | (1.485) | | (0.891) | |
| Female | | 10.154 | | 9.458** | |
| | | (6.566) | | (3.797) | |
| Czech | | −13.671** | | −11.085*** | |
| | | (6.804) | | (3.894) | |
| Econ | | −6.332 | | −0.698 | |
| | | (6.856) | | (4.073) | |
| WorkExp | | −6.261 | | −2.935 | |
| | | (7.678) | | (4.171) | |
| KnowHF | | 1.899 | | 0.380 | |
| | | (14.187) | | (6.548) | |
| Order FE | | | No | Yes | |
| Subject FE | | | No | No | Yes |
| Observations | 160 | 160 | 640 | 640 | 640 |
| $R^2$ | 0.012 | 0.051 | 0.007 | 0.041 | 0.758 |
| Adjusted $R^2$ | −0.007 | −0.006 | 0.003 | 0.023 | 0.676 |

*Note:* Columns 1–2 show the first choices in the experiment (between-subject decisions), columns 3–5 show all data (4 choices per participant). Standard errors are shown in parentheses. Standard errors in columns 3–5 are clustered at the level of subject. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

results suggest that people base their expectations on the families' perceived neediness. Assuming that the families' incomes do not differ, students expect that larger families and families in greater need of housing will keep higher shares of the investment, presumably because they have a better use for the money.

Interestingly, the amounts sent are equal for both levels of housing quality. This is consistent with a rise in altruism compensating for lower trustworthiness. It is not clear, however, what happens to the relative sizes of these effects once the stakes are high. In such a case, the increase in the charitable component might not entirely balance out the impact of reduced trustworthiness. People are perhaps more willing to help the homeless if the cost is small, for example by giving them some change, but would think twice before giving them a job or allowing them to move into a property they own.

These findings suggest that people in inadequate housing conditions might find it harder to gain the trust of other people in society, many of whom might be essential for their prosperity and that of their families: teachers, police, health workers, current or potential employers, landlords, and so on. In some contexts, this lower perceived trustworthiness might be mitigated by a greater disposition to help people in need.

In the second experiment, we asked students to estimate the d2-test concentration performance of adult members of the project families. In contrast to trustworthiness, we found that expected concentration performance is not affected by housing conditions but is linked to housing history. The student-evaluators assumed that people with histories of sub-standard housing differ in their abilities from people without such history. This shows that people interpret homelessness differently in the domains of cognitive abilities (sustained attention) and trustworthiness. Unlike trustworthiness, people perceive cognitive abilities as given and they believe that cognitive abilities determine living conditions but not the other way around.

These results suggest that people living in inadequate housing conditions face adverse expectations from the rest of society. Our results provide additional rationale for housing projects that move people out of substandard housing. Improving these people's housing conditions generates positive effects on trustworthiness even when the 'trustors' are informed about their housing history. This is important, since in many interventions, such as job-market or community-based programs, participants' histories are known to the project team or its partners. On the other hand, once participants go on to apply for labor market positions through standard channels, their history of homelessness will not be revealed (it is not a standard part of a CV or covering letter), so the effect that history has on expectations about concentration performance should not affect such an application process.

# Chapter 5

# The impact on the untreated

## 5.1 Introduction

Randomized controlled trials (RCTs) are gold standard evidence in the natural sciences. More recently, this method was also adopted in research in the social sciences.[1] Random assignment into the treatment group, which leaves some participants untreated, raises several ethical concerns. Some of these concerns apply even in the case of double-blind, randomized, placebo-controlled trials, in which neither the participants themselves nor the researchers are informed about which treatment group the individual participants are in. In medical experiments, researchers' concerns include whether administering a placebo instead of active treatment could potentially harm the participants, e.g., by exposing them to higher levels of pain or by increasing the risk of death (Nardini, 2014). Ideally, researchers running an RCT would like to have an honest disagreement in the expert medical community over whether a treatment is beneficial; this is called *clinical equipoise* (Freedman et al., 1987).[2] In many social science experiments,

[1]In social sciences, RCTs are commonly used in the evaluation of social programs (Fisher, 2006; Harrison and List, 2004; Duflo and Kremer, 2005).

[2]Clinical equipoise has been criticized as irrelevant (see e.g., Miller and Brody, 2003; Veatch, 2007). Veatch (2007) argues that what matters is the subject's evaluation of the research. Furthermore, even if the participants are not indifferent concerning the treatment arms, the research can be considered morally justifiable so long as the participants have given their adequately in-

the treatment arms are not equally good for the subjects, mainly because of resource limitations (e.g, no project can provide a sizable cash-transfer to all poor families in Kenya). Moreover, it is often difficult or even impossible to ensure that participants are *blind* to their treatment assignment. This means that the participants in the control group are aware that they remain untreated as a result of bad luck in the random treatment lottery.

This chapter focuses on random allocation procedures in which participant assignment to treatment arms is not *blind* and in which participants would clearly prefer some treatment arms to others. Such procedures are frequently used in social science RCTs; another example is the random assignment of grant funding (see e.g., Fang and Casadevall, 2016). We use data from the Housing First RCT, which started in Brno (Czech Republic) in 2016. Out of the population of 421 families in need of housing, 50 families were randomly selected to receive rental contracts for municipal flats and social services related (mostly) to this new housing, while 100 families were selected into a control group and received neither the housing nor the services. This experiment provides a suitable setting for our study: First, the existence of the project was public knowledge and the families in the control group were aware of the fact that other families were receiving housing and services through the project. Second, the treatment was substantial and clearly preferred to non-treatment. It lead to significant improvements in housing quality, perceived housing stability (see Section 2.3 for more details), and life satisfaction.[3]

The aim of this study is to test whether assignment to the control group negatively affects the life satisfaction, psychological distress and pro-social behavior of the participants concerned. Effects on life satisfaction and psychological distress might arise due to the inequality that the experiment generates. In the

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formed, free, and unexploited consent.

[3]The life satisfaction of the treated participants increased by 0.8 std. dev. This is a large effect compared to other studies. For example, unconditional cash transfers with an average value of $709 PPP (equivalent to two years of per capita expenditure) made to randomly selected poor households in western Kenya increased the recipients' life satisfaction by 0.17 std. dev. (Haushofer and Shapiro, 2016).

case we examine, treated families experienced substantial improvement in their living standards, while control families did not. Since the participants in both groups were drawn from the same population, control families might consider the treatment families as their reference group. Consistent with this explanation, several studies have found that a decline in relative economic status negatively affects psychological well-being (Mangyo and Park, 2011; Luttmer, 2005), and this has been shown to matter in the context of RCTs involving cash transfers. Baird, De Hoop, and Özler (2013) offered monthly cash transfers to randomly selected school-age girls and their parents or guardians in Malawi. They found that untreated girls living in areas where some girls were treated reported a substantial increase in psychological distress relative to girls in non-treatment areas. Similarly, Haushofer, Reisinger, and Shapiro (2015) used an RCT in which poor households in Kenya received unconditional cash transfers and found that it resulted in a substantial reduction in life satisfaction for untreated neighbors. In both these cases, these negative effects disappeared after the cash transfer programs were terminated.[4] In contrast to these studies, the intervention we look at does not involve cash transfers. Instead, it provides improved housing conditions and intensive case management. Additionally, the treated subjects are not the immediate neighbors of the untreated and thus are not in intensive social contact with them.

Similarly, we are also interested in any effects the RCT might have had on untreated participants' generosity and their perceptions of other people's prosociality. Not winning in the treatment lottery might cause disappointment: it leaves the untreated participants relatively worse off, and untreated participants often perceive random assignment into treatment groups as unfair (Haushofer, Riis-Vestergaard, and Shapiro, 2019; Hillis and Wortman, 1976; Erez, 1985). Be-

[4]On the other hand, development programs often have positive effects on other outcomes for the untreated. Angelucci and De Giorgi (2009) report positive spillovers from cash transfers. They found that consumption increased among the neighbors of families treated within the Mexican Progresa program. Miguel and Kremer (2004) showed that a randomized program of treatment with deworming drugs reduced the rate of absence among untreated children in the same Kenyan primary schools as those treated.

sides negatively affecting their life satisfaction and psychological well-being, these feelings could also turn them against other people (for example, those who were selected to receive better housing). The project's existence raised their hopes but in the end it did not help them. In this chapter we tests whether this affected their willingness to help others and their feelings about how trustworthy, helpful, or fair other people are. To the best of our knowledge, we are the first to study pro-sociality in this context.

We use data from two sources: 1) questionnaire data collected on three occasions: before the lottery assignment was known to the participants, and then 6 and 12 months after families in the intervention group had moved into their new housing, and 2) an incentivized dictator game played roughly 12 months after the treated families had moved. In the questionnaire data, we compare the values of the European Social Survey (ESS) general questions on life satisfaction, trust, fairness, and helpfulness perceptions before and after the treatment within both the control and intervention groups. We also look at the impact on psychological distress using the the Kessler Psychological Distress Scale. In the lab-in-the-field experiment, participants played two dictator games against charities helping ill children. While the donation was observed by the research team in one game, it was anonymous in the other game. In the anonymous condition, a special procedure was used to guarantee that the experimenter did not observe the donation and that the research team cannot match it with the identity of the participant. Since repeating the dictator game both before and after the intervention might compromise the after-intervention data, we ran the experiment only once, roughly 12 months after treated families had moved.

While the treatment led to a substantial improvement in the measures of life satisfaction and psychological distress among those treated, we do not find any significant effects when comparing the values reported by those who were not treated. Neither do we observe any impacts on the perceived pro-sociality of the untreated: their perceptions of others' trustworthiness, fairness and helpfulness remain constant over time. Furthermore, we find no significant difference between the charity donations made by the treated families and the control families, either in the observed version or the anonymous version of the game. Since it is unlikely that receiving better housing would have reduced our participants' generosity toward ill children, we interpret this zero result as evidence that the RCT had no negative impact on the pro-social preferences of the untreated participants. These results suggest that randomized interventions have no substantial adverse effects on the life satisfaction and pro-sociality of the untreated, at least in the context of housing experiments. It must however be noted that our sample size of 150 families is only sufficient for detecting medium-sized effects and we are therefore only able to exclude negative effects of this size or larger.

In addition to the studies on the potential adverse effects of randomized cash transfer programs mentioned above, this chapter is related to several other strands of literature. First and foremost, we contribute to the literature on the costs and benefits of random treatment allocations. In several survey-based studies, respondents have raised objections to randomization: they do not perceive randomization as permissible for scientific purposes when some people are excluded from the intervention group because of insufficient resources (Hillis and Wortman, 1976), or when the treatment is life-saving (Johnson, Lilford, and Brazier, 1991). Erez (1985) find that out of four different mechanisms that could be used to allocate beneficial programs (need; merit; first come, first served; randomization), prison inmates perceive need as the fairest and randomization as the least fair. Only Innes (1979) finds that college students approve random assignment for placing juvenile offenders into institutionalization or family therapy. However, these results might have been consistent with the findings by Erez (1985) if the respondents had been juvenile offenders themselves. Haushofer, Riis-Vestergaard, and Shapiro (2019) address similar questions using a laboratory experiment. They find that people prefer randomization if the potential recipients have equal endowments, but randomization is not popular among relatively disadvantaged recipients. These results suggest that untreated participants, especially those who are relatively more in need, might consider the treatment lottery unfair.

Our study is also related to the literature studying the effects of emotions on pro-sociality. Several studies have found that pro-sociality is affected by exposure to bad news (Hornstein et al., 1975; Han et al., 2019). Other research has shown that people in neutral moods are less helpful than people in positive moods (Carlson, Charlin, and Miller, 1988; Isen, Clark, and Schwartz, 1976; Lay, Waters, and Park, 1989). Finally, this chapter contributes to the literature that links preferences for equality to pro-social behavior (see e.g. Schmidt and Sommerville, 2011), since our treatment created substantial differences in living standards of previously-equal families.

Consistent with this explanation, several studies have found that a decline in relative economic status negatively affects psychological well-being (Mangyo and Park, 2011; Luttmer, 2005), and this has been shown to matter in the context of RCTs involving cash transfers. Baird, De Hoop, and Özler (2013) offered monthly cash transfers to randomly selected school-age girls and their parents or guardians in Malawi. They found that untreated girls living in areas where some girls were treated reported a substantial increase in psychological distress relative to girls in non-treatment areas. Similarly, Haushofer, Reisinger, and Shapiro (2015) used an RCT in which poor households in Kenya received unconditional cash transfers and found that it resulted in a substantial reduction in life satisfaction for untreated neighbors. In both these cases, these negative effects disappeared after the cash transfer programs were terminated.[5] In contrast to these studies, the intervention we look at does not involve cash transfers. Instead, it provides improved housing conditions and intensive case management. Additionally, the treated subjects are not the immediate neighbors of the untreated and thus are not in intensive social contact with them.

In addition to the studies on the potential adverse effects of randomized cash transfer programs mentioned above, this chapter is related to several other strands of literature. First and foremost, we contribute to the literature on the costs

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The rest of the chapter is organized as follows: Section 5.2 presents our methodology, and Section 5.3 describes the data. Section 5.4 presents the results, and Section 5.5 discusses the implications and limitations of our findings.

## 5.2 Experimental design and procedures

### 5.2.1 Questionnaire and experimental measures

We use questionnaire data on life satisfaction, psychological distress, and perceptions of trust, fairness and helpfulness. The questions are taken from two sources. To measure changes in psychological distress we use the Kessler Psychological Distress Scale (K6). K6 contains the following questions (labels of the variables in brackets):

*During the past 30 days, how often did you feel*

1. *nervous?* (K6 nervous)
2. *hopeless?* (K6 restless)
3. *restless or fidgety?* (K6 depressed)
4. *so depressed that nothing could cheer you up?* (K6 effort)
5. *that everything was an effort?* (K6 effort)
6. *worthless?* (K6 worthless)

The value is measured by a Likert scale ranging from 1: "All the time" to 5: "None of the time". The average value across all six questions gives the K6 index.

In addition to that, we use the following four European Social Survey (ESS) questions:[6]

- ESS life satisfaction: *All things considered, how satisfied are you with your life as a whole nowadays?* [Likert scale where 0 means *extremely dissatisfied* and 10 means *extremely satisfied*]

[6]Source: `https://www.europeansocialsurvey.org/docs/round8/fieldwork/source/ESS8_source_questionnaires.pdf`, accessed on 19/9/2019. We did not use the cards, only a range of answers in the questionnaire.

- ESS people trusted: *Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?* [Likert scale where 0 means "You can't be too careful" and 10 means "Most people can be trusted"].
- ESS people fair: *Do you think that most people would try to take advantage of you if they had the chance, or would they try to be fair?* [Likert scale where 0 means "Most people would try to take advantage of me" and 10 means "Most people would try to be fair"].
- ESS people helpful: *Would you say that most of the time people try to be helpful or that they mostly look out for themselves?* [Likert scale where 0 means "People mostly look out for themselves" and 10 means "People mostly try to be helpful"].

We measured pro-social attitudes using a lab-in-the-field experiment. The experiment was conducted in individual sessions at the participants' homes or other locations of their choice (see Appendix A.1 for the experiment instructions). Each participant played two dictator games, one in a observed version and one in an anonymous version. In both games, participants were given a small plastic box containing coins worth 150 CZK ($\approx$ 6 EUR), and were offered the opportunity to choose an amount $c \in [0, 10, ..., 150]$ to send to charities that help children with serious illnesses.[7] The participants were asked to put their intended donation back into the box, and to keep the rest of the 150 CZK. When the donation was observed, we asked them to write their name and surname and the amount they contributed on the answer sheet (see Panel 5.2a). They were informed that the size

[7]We have several reasons for deciding that the contributions would go to a charity helping seriously ill children. First, our sample consists of low-income families and most of our participants regularly receive assistance from the government and NGOs. We needed to choose a recipient whom they can identify with, and whom they may consider even more deserving than themselves; otherwise, a large proportion might opt for zero-donations. Second, we avoided choosing a charity that helps homeless people, since participants in the control group might give less to this type of charity, not because they are less generous, but because they might consider themselves in greater housing need than participants in intervention families whose housing needs have just been attended to.

of their donation, along with their name, would be shared with the research team but would not be shared with anyone outside the project, and certainly not with the officials in charge of municipal housing strategy in Brno. In the anonymous version, participants were informed that how much they donated would be known only to them, and that a procedure was in place to ensure the research team could not find out whose box was whose (see the following subsection for details).

We selected this dictator game because, contrary to other commonly used games such as trust games or public goods games, it measures pro-sociality isolated from strategic considerations. It also has several other logistical advantages: First, it is comparatively simple to explain, which is crucial when participants are recruited from socially and economically disadvantaged families. Second, the contributions can be made anonymously, which enables us to test for robustness to reputational concerns that might arise if the contribution was observed (Bénabou and Tirole, 2006). Third, participants do not need to be matched with other people and can thus be paid immediately at the end of their individual experimental session. Otherwise, participants in the treatment groups might have different levels of trust in being paid later, which could contaminate the results.

While the dictator game is criticised by several authors as artificial (Levitt and List, 2007; List, 2007; Bardsley, 2008), there are also several studies finding support, although sometimes week or mixed, for the external validity of laboratory dictator game. Benz and Meier (2008) find week correlation between laboratory and field student donations to two funds at the University of Zurich supporting students in financial difficulties or foreigners. The size of the donations in the lab and in the field in the four semesters before and after the lab experiment was similar. Franzen and Pointner (2013) find a week correlation between dictator game and returning a misdirected letter. Barr and Zeitlin (2010) find a week negative correlation between dictator-game giving and teacher absenteeism in Uganda. Carpenter, Connolly, and Myers (2008) find that the dictator game or charity dictator game is related to self-reported training hours of volunteer firefighters and the probability of being non-volunteer firefighter, but unrelated to self-reported call hours and recorded response to calls. Holm and Danielson

(2005) find some correlation between dictator-game giving and survey of trusting attitudes in Sweden, but no correlation with trusting attitudes in Tanzania and trusting behavior (lending money in the past) in Tanzania and Sweden. Galizzi and Navarro-Martínez (2018) finds a week correlation to self-reported measure of past social behaviors, but not to a subset of three items related to money. They do not find any significant correlation to field measures of pro-social behavior such as opportunity to donate for children's or environmental charity.

### 5.2.2 Experimental procedures of the dictator game

The experiment consisted of several tasks. The dictator game was the fourth task, after risk- and time-preference elicitation tasks and the d2 test. Participants did not receive any feedback about their payoffs from the previous tasks before the dictator game. At the beginning of the dictator game, each participant was shown a blue box (for observed donation) and a red box (for anonymous donation) and informed that each contains 150 CZK in a combination of coins that enable them to select any amount from 0 to 150 CZK in multiples of ten i.e. 0, 10, 20, 30, 40, 50, ... to 150 CZK. The red box also contained two additional coin-shaped pieces of metal.

The order of the tasks was randomized. Participants learn about the specific rules of each task only just before undertaking it. The boxes did not contain any identifiers apart from the letters K and I, which denoted control participants and intervention participants, respectively. The letter was stuck to the box that was selected to be used in the first of the two tasks. Figure 5.1 shows all four sets of boxes. For example, the set of boxes on the left comes from a control-group participant who took the blue-box task first. The donations from the first task were to be sent to "Rafael dětem" (Raphael for kids), the donations from the second task to "Dobrý anděl" (Good angel). Participants were provided with a short description the missions of the two charities, which are almost identical (see Figure 5.2).[8]

[8]Even the names of the organizations are very similar: both of them contain an angel or the name of an angel. Anecdotally, we believe the organization "Dobrý anděl" is somewhat better

The task itself consisted of several steps. First, the research assistant opened the box and showed the participant the money inside. The participant could verify that the box did not contain any identifier apart from the letter I or K, denoting intervention and control, and was informed that the 150 CZK in their box belonged to them. They were then offered the opportunity to send any amount between 0 and 150 CZK (in multiples of 10) to the selected charity that helps severely ill children. A description of the charity was read aloud to the participant from the experimental sheet.


Figure 5.1: Boxes used in the dictator game – the labels "I" and "K" denote intervention and control, respectively. The box with the label on it was always used in the first task, the one without the label in the second task.

The rest of the procedure differed between the two tasks. In the observed donation variant, participants were asked to take all the money into their hand, put any donation back into the box, and keep the rest of the 150 CZK. They were also asked to write down their first name, surname and amount donated on the answer sheet (see Panel 5.2a). They were informed that their name and the information about the value of their donation would be shared with members of the research team, but that it would not be shared with anyone outside the project, and certainly not with any officials in charge of municipal housing in Brno. The research assistant then checked that the written amount on the response sheet corresponded with the money actually put in the box. The box was then sealed shut with adhesive tape.

known to the Czech general public than "Rafael dětem".

In the anonymous donation variant, the participants were informed that the amount they chose to donate would remain known only to them and that the research team would not be able to find out which box was theirs. They were asked not to tell the research assistant present about how much they had decided to donate. The research assistant did not watch while the participant decided how much money to place in the red box; in most cases, the assistant left the room or, if that was not practical, went to the opposite side of the room and turned to face away from the participant. The participant was then asked to take all the money out of the box, put both the additional pieces of metal and their intended donation back into the box, and put the rest of the money away out of sight, so that the research assistant would not see it. After this, the participant closed the box and the assistant returned only once the participant had confirmed that it was shut. The research assistant avoided picking up the red box except when strictly necessary, and made no attempts to shake or weigh the box. The metal pieces were included to make any inference about the size of the donation difficult. After the second donation was complete, the two boxes were stuck together with adhesive tape and the box package was then placed into a bag containing several other packages from previous participants.[9] Our research assistants used two separate bags for boxes from male and female participants so that in addition to the treatment group and task order, our data contains also information about the sex of the participants in the donation game.

[9]For the first participants, we placed dummy packages into the bag that contained only the non-coin metal pieces; none of the boxes were opened until many more boxes had been added to the bag, so that it was impossible to establish which boxes were from which participants.

*Answer sheet: game 4, page 1*

**Donate to the foundation Rafael dětem**

Rafel helps children with serious illnesses such as cancer, cerebral palsy, epilepsy, autism, microduplication syndrome, developmental brain diseases, central brain atrophy, spinal muscular atrophy, acute respiratory failure or neuropathy.

________________________(name and surname) donate to helping sick children through the foundation Rafael dětem

_________________ CZK.

The size of your donation along with your name can be shared among the members of the research team. We will not share it with anyone outside the project, especially anyone in charge of the municipal housing strategy in Brno.

(a) Blue box first

*Answer sheet: game 4, page 2*

**Donate to the foundation Dobrý anděl**

The foundation Dobrý anděl helps families who have been in a difficult situation due to illness. It helps children with severe diseases such as cancer, cystic fibrosis, muscular dystrophy (Duchenne and worse types), butterfly wing disease, acute phase of chronic bowel disease or chronic organ failure.

Your donation to helping sick children through the foundation Dobrý anděl will be

**ANONYMOUS**

Your donation will be in a closed box, that will be mixed with other respondents' boxes after your decision. This means that only you and no one else will know the amount of your donation.

(b) Red box second

Figure 5.2: Experimental sheets

## 5.3 Data

Table 5.1 provides basic summary statistics of the questionnaire data. These data were collected from one person per participating household; these were the primary caregivers and were predominantly women. The K6 questions are answered on a scale from 1 to 5; a higher number indicates a lower frequency of negative feelings. The ESS question responses range from 0 to 10, with higher numbers representing more pro-social perception. The numbers are relatively low compared to the data from the 2016 ESS based on a representative sample of the Czech population.[10]

Table 5.1: Questionnaire data summary statistics

| | Baseline | | 6 months | | 12 months | |
|---|---|---|---|---|---|---|
| Statistic | N | Mean (SD) | N | Mean (SD) | N | Mean (SD) |
| K6 nervous | 148 | 2.34 (1.18) | 134 | 2.50 (1.31) | 137 | 2.54 (1.29) |
| K6 hopeless | 147 | 2.97 (1.41) | 133 | 3.24 (1.47) | 137 | 3.35 (1.39) |
| K6 restless | 143 | 3.21 (1.38) | 134 | 3.26 (1.61) | 137 | 3.27 (1.46) |
| K6 depressed | 144 | 3.87 (1.40) | 133 | 3.62 (1.51) | 137 | 3.82 (1.48) |
| K6 effort | 144 | 2.95 (1.42) | 134 | 2.90 (1.56) | 136 | 2.99 (1.58) |
| K6 worthless | 143 | 3.86 (1.39) | 134 | 3.84 (1.57) | 134 | 3.87 (1.52) |
| K6 index | 132 | 3.25 (1.06) | 133 | 3.23 (1.19) | 134 | 3.31 (1.17) |
| ESS life satisfaction | 147 | 4.41 (3.24) | 134 | 5.40 (3.04) | 137 | 5.10 (3.18) |
| ESS people trusted | 147 | 2.56 (2.55) | 134 | 2.84 (2.84) | 137 | 2.93 (2.70) |
| ESS people fair | 146 | 3.10 (2.59) | 133 | 3.13 (2.95) | 137 | 3.07 (2.96) |
| ESS people helpful | 144 | 3.05 (2.97) | 134 | 3.10 (3.31) | 136 | 3.00 (3.13) |

We collected the experimental data in individual interviews, for which all adult members of the participating households were eligible, in spring and summer 2018. In total, we completed 161 experimental sessions (55 in treatment households, 106 in control households).[11] We have at least one participant from 123 out of 150

[10] The life satisfaction score was 6.79, people trusted 5.05, people fair 5.38, and people helpful 4.77 (all weighted by the pspwght). All these values are highly significantly different from the baseline values obtained in our survey.

[11] One participant left the interview after the initial risk- and time-preference elicitation due

households (82%). The participation rate is similar in both the intervention (80%) and control groups (86%).

We measured participants' attention using the d2 test (Brickenkamp, 1962; Bates and Lemay, 2004): Participants receive two sheets of paper with a large number of letters p and d with 0, 1, or 2 vertical lines above and below the letter (see Appendix A.1.3 for full instructions and examples of the answer sheets). Their task is to find and mark all d characters with a total of two vertical lines around them (two above, none below; two below, one above; or one above and one below) within the time limit of 4 minutes and 40 seconds. Out of several possible overall performance measures, we use concentration performance (d2CP), which seems more resilient to test-taking strategies (Bates and Lemay, 2004). Concentration performance equals the total number of correctly marked letters minus the total number of incorrectly marked letters. This measure was also incentivized; participants were paid their d2CP score in CZK (unless they achieved a negative score, in which case they received 0 CZK).

We also observe their housing type before the start of the project. *Origin hostel* is a dummy variable equal to 1 if the family lived in a hostel. These are the families living in especially hard conditions. Finally, we use several variables from the post-experimental questionnaire.

- Days to pay: *How many days are left until you receive your next paycheck or benefits?* (number of days)
- Missing money: *Will you be unable to pay for something important over the next three days?* (yes or no)
- Sleep: *How many hours did you sleep last night?* (number of hours)
- Ill child: This variable is based on the analysis of the answers of these questions: *Do you have a seriously ill child?* (yes or no) *If yes, what is your child's illness?* If participants reported that one of their children was ill and their illness was similar to the diagnoses of the children helped by the selected charities, *Ill child* equals 1, otherwise it equals 0.

to health problems, and was not paid.

Table 5.2 presents summary statistics. The donations made in the observed condition are higher than the donations made anonymously. Figure 5.3 displays the distributions of donations in both treatments. There are border choices: approximately 9% of participants donated 0 CZK, and approximately 20% sent the full amount of 150 CZK. The control variable statistics are displayed below.

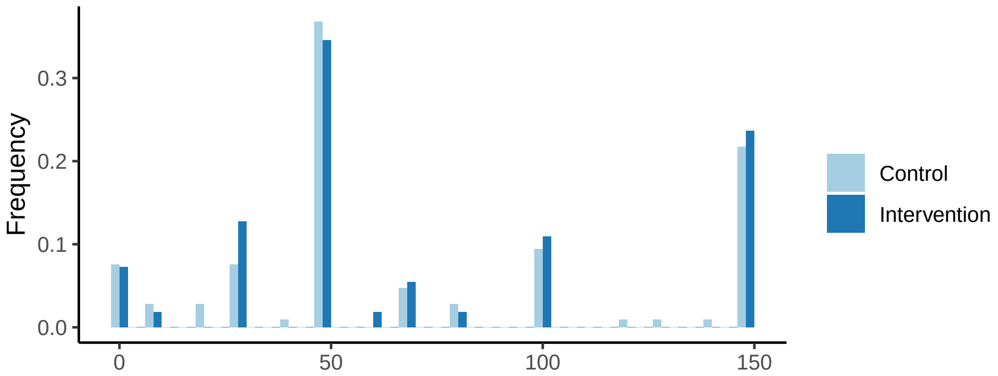
Table 5.2: Summary statistics

| Statistic (unit) | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| Observed donations (CZK) | 161 | 73.41 | 48.94 | 0 | 150 |
| Anonymous donations (CZK) | 161 | 59.25 | 48.18 | 0 | 150 |
| Male | 161 | 0.25 | 0.43 | 0 | 1 |
| Origin hostel | 161 | 0.39 | 0.49 | 0 | 1 |
| Missing money | 156 | 0.48 | 0.50 | 0 | 1 |
| Days to pay | 159 | 15.04 | 8.75 | 0 | 30 |
| d2CP | 153 | 43.18 | 25.22 | −34 | 104 |
| Sleep | 160 | 7.00 | 2.36 | 1.5 | 20 |
| Ill child | 161 | 0.19 | 0.39 | 0 | 1 |

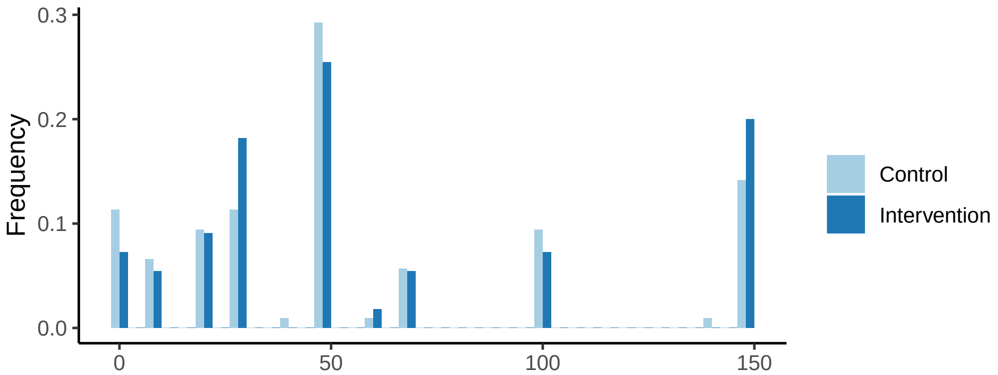
## 5.4 Results

Table 5.3 presents changes in life satisfaction and psychological distress (K6). It shows the average values in the control (C) and intervention (I) groups at three different times: once before the treatment (baseline), and twice after the treatment (6 months and 12 months).

The first two rows show the average life satisfaction and K6 index scores, with higher scores indicating better outcomes. These show that while the treatment leads to significant improvements in life satisfaction and psychological well-being among the treated (by 0.8 std dev. in life satisfaction and 0.4 std dev. in K6 index), neither of these outcomes change among the untreated. The lower part of the table shows the results of the individual K6 answers, to demonstrate that the pattern is similar across the questions: While treated participants report a lower frequency of negative mental states, the outcomes do not significantly change in




(a) Observed donations (blue box)




(b) Anonymous donations (red box)

Figure 5.3: Histograms of anonymous and observed donations

the control group, with the exception of two questions: Relative to the baseline, the untreated participants felt depressed and overwhelmed (that everything was an effort) more often after 6 months, but this effect disappeared after 12 months.

Table 5.4 shows that the treatment does not have any effect on the perceived pro-sociality of the untreated. The only effect among the treated is in the first row: treated participants consider other people more trustworthy.

Next, we proceed to our measures of generosity based on the lab-in-the-field experiment. This experiment was conducted only once, approximately 12 months

Table 5.3: Life satisfaction and psychological distress

| | | Baseline | 6 months | 12 months |
|---|---|---|---|---|
| ESS life satisfaction | C: | 4.51 | 4.55 | 4.19 |
| | I: | 4.22 | 6.93*** | 6.79*** |
| K6 index | C: | 3.15 | 2.89 | 3.01 |
| | I: | 3.42 | 3.82** | 3.85* |
| K6 nervous | C: | 2.22 | 2.26 | 2.35 |
| | I: | 2.56 | 2.94* | 2.90 |
| K6 hopeless | C: | 2.90 | 2.81 | 3.12 |
| | I: | 3.10 | 4.00*** | 3.77** |
| K6 restless | C: | 3.08 | 2.90 | 3.00 |
| | I: | 3.46 | 3.92* | 3.77 |
| K6 depressed | C: | 3.76 | 3.36** | 3.47 |
| | I: | 4.08 | 4.06 | 4.46 |
| K6 effort | C: | 2.93 | 2.48* | 2.61 |
| | I: | 3.00 | 3.65* | 3.69* |
| K6 worthless | C: | 3.73 | 3.56 | 3.53 |
| | I: | 4.10 | 4.35 | 4.49 |

*Note:* The table reports average scores in the control (C) and intervention (I) groups. Higher values are better: they mean a higher level of satisfaction in the ESS question and a lower frequency of negative mental states in K6. Stars denote significant differences from the baseline, using paired t-tests. Significance: $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

after the treated families moved into their new housing. The effect on the untreated participants, therefore, cannot be identified by comparing our generosity measures at different moments in time. Instead, we directly compare the values from the intervention group with those from the control group participants. If we assume that the intervention did not reduce generosity among those treated, any difference between the two groups can be interpreted as the upper bound of the intervention's negative impact on the untreated.

Figure 5.4 reports the results of the experiment. On average, participants

Table 5.4: Perceptions about pro-sociality

| | | Baseline | 6 months | 12 months |
|---|---|---|---|---|
| ESS people trusted | C: | 2.71 | 2.33 | 2.63 |
| | I: | 2.26 | $3.75^{**}$ | $3.48^{**}$ |
| ESS people fair | C: | 3.12 | 2.67 | 2.79 |
| | I: | 3.06 | 3.95 | 3.58 |
| ESS people helpful | C: | 2.94 | 2.82 | 2.58 |
| | I: | 3.27 | 3.60 | 3.77 |

*Note:* The table reports average scores in the control (C) and intervention (I) groups. Higher values indicate higher pro-sociality. Stars denote significant differences from the baseline, using paired t-tests. Significance: $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

donated slightly less than half of the available 150 CZK to charity. While the differences between treatments is not significant (anonymous: Mann Whitney (MW) $p = 0.57$, $d = 0.11$; observed: MW $p = 0.87$; $d = 0.02$), the donations made in the observed variant of the game were higher than those made when the contribution was anonymous (MW $p = 0.003$). The latter result is consistent with previous studies that have found that people are more willing to volunteer or contribute to charity if their actions are observable by others (e.g., Ariely, Bracha, and Meier, 2009; Lacetera and Macis, 2010; Linardi and McConnell, 2011; Soetevent, 2011).

These findings are confirmed by the OLS regressions presented in the following tables.[12] Table 5.5 is based on the anonymized data collected from the boxes (see Appendix 5.2.2 for a detailed description of the experimental procedures). Here, we observe only the value of the donations, the participant's treatment group (intervention or control), the donation conditions (observed or anonymous), the order of the game (we control for the observed box being the first) and the participant's sex. Standard errors in these regressions can only be clustered at the level

[12]All our findings also hold if we account censoring in the data (see Section 5.3) using Tobit models (see Appendix B.2).

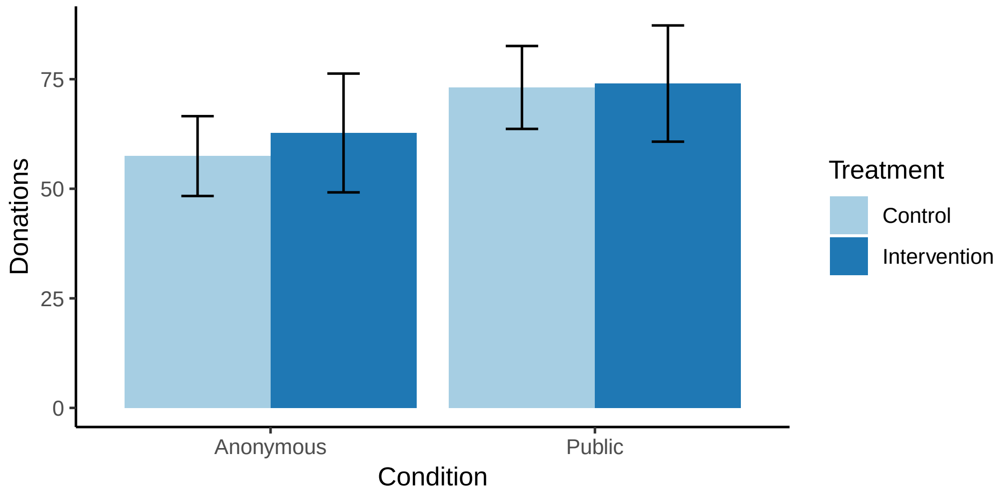



Figure 5.4: Anonymous and observed donations. The bars show confidence intervals.

of individuals, since our procedures designed to guarantee anonymity prevent us from matching observations from the same households. We are mainly interested in the *Intervention* variable. Model 1 controls for the donation conditions, model 2 adds controls for the order of the game, and model 3 for the participant's sex. We do not find that treatment has any significant effect in any of the models. The only variable that is consistently significant is *Observed*: the participants donated around 20 CZK more when their donation was observable.

Table 5.6 explains only observed donations. These values can be matched with the other experimental and survey data we collected from our subjects. Model 1 controls for the order of the game (*Observed first*). Model 2 additionally controls for liquidity constraints (*Days to pay* and *Missing money*), attention (*d2CP)*, whether a child in the family is severely ill (*Ill child*), how many hours the participant slept last night (*Sleep)*, and experimenter fixed effects. Model 3 adds controls for the participant's sex (*Male*) and Model 4 for whether the participants had originally stayed in a shared hostel or hostel (*Origin hostel*). Again, the treatment variable *Intervention* is not significantly different from zero in any of the specifications. This means that the upper bound of the potential

Table 5.5: OLS regressions – observed and anonymous donations

| | *Dependent variable:* Donations | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Constant | 57.453*** | 56.666*** | 59.236*** |
| | (4.607) | (5.565) | (6.037) |
| Intervention | 5.274 | 5.186 | 1.238 |
| | (8.173) | (8.212) | (9.364) |
| Observed | 15.660*** | 15.660*** | 15.660*** |
| | (3.746) | (3.752) | (3.764) |
| Observed × Intervention | −4.388 | −4.388 | −4.388 |
| | (6.900) | (6.911) | (6.933) |
| Observed first | | 1.604 | 2.085 |
| | | (7.009) | (7.031) |
| Male | | | −9.914 |
| | | | (9.613) |
| Intervention × Male | | | 16.053 |
| | | | (16.146) |
| Observations | 322 | 322 | 322 |
| $R^2$ | 0.022 | 0.023 | 0.029 |
| Adjusted $R^2$ | 0.013 | 0.010 | 0.010 |

*Note:* Standard errors in parentheses are clustered at the individual level. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

adverse effect on the untreated is zero.

## 5.5 Conclusion

This chapter addresses the concern that random assignment into treatment groups might negatively impact the untreated, especially if participants are aware of their treatment status and the treatments differ in their desirability. This concern is motivated by evidence from cash-transfer experiments (Baird, De Hoop, and Özler, 2013; Haushofer, Reisinger, and Shapiro, 2015) and by survey and

Table 5.6: OLS regressions – observed donations

| | *Dependent variable:* Observed donations | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Constant | 71.043$^{***}$ | 86.498$^{***}$ | 86.180$^{***}$ | 85.357$^{**}$ |
| | (9.531) | (31.992) | (32.895) | (34.897) |
| Intervention | 0.914 | 7.010 | 1.627 | 4.452 |
| | (11.408) | (11.958) | (13.014) | (17.548) |
| Male | | | −9.746 | −9.644 |
| | | | (15.381) | (15.421) |
| Intervention × Male | | | 24.190 | 24.726 |
| | | | (33.113) | (34.146) |
| Origin hostel | | | | −0.566 |
| | | | | (15.247) |
| Intervention × Origin hostel | | | | −7.168 |
| | | | | (27.103) |
| Observed first | 14.324 | 5.536 | 6.001 | 6.347 |
| | (11.016) | (12.081) | (12.060) | (12.220) |
| Days to pay | | 0.018 | 0.018 | 0.007 |
| | | (0.742) | (0.743) | (0.768) |
| Missing money | | −24.980$^{**}$ | −24.179$^{**}$ | −24.250$^{**}$ |
| | | (11.797) | (11.794) | (11.975) |
| d2CP | | 0.603$^{**}$ | 0.609$^{**}$ | 0.615$^{**}$ |
| | | (0.269) | (0.267) | (0.269) |
| Ill child | | 6.786 | 7.212 | 7.534 |
| | | (16.268) | (16.532) | (17.235) |
| Sleep | | −4.426$^{*}$ | −4.205 | −4.127 |
| | | (2.568) | (2.567) | (2.660) |
| Experimenter FE | No | Yes | Yes | Yes |
| Observations | 161 | 147 | 147 | 147 |
| $R^2$ | 0.008 | 0.130 | 0.139 | 0.139 |
| Adjusted $R^2$ | −0.005 | 0.052 | 0.047 | 0.033 |

*Note:* Standard errors in parentheses are clustered at the household level. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

experimental evidence suggesting that people often prefer non-random systems of allocation for scarce goods or services (Erez, 1985; Haushofer, Riis-Vestergaard, and Shapiro, 2019).

We use a rehousing RCT to test the potential negative impacts of assignment into the less preferred treatment arm (the usual care or control group). To complement previous findings, we look at life satisfaction and psychological distress. We compare the values before and after the outcomes of the treatment lottery were announced and find no significant adverse effects on the untreated. Furthermore, we investigate potential negative spill-over effects by measuring participants' perceptions of pro-sociality (perceived trustworthiness, fairness, and helpfulness of other people) and their generosity using incentivized dictator games with charitable organizations as recipients. We do not find any difference in perceived pro-sociality after the treatment assignment was announced, nor do we find any differences in the charitable donations made by treated and untreated participants. Assuming that the treatment has no adverse effect on the generosity of the treated, we may therefore conclude that the generosity of the untreated was unaffected.

Our research design is limited by the scope (size) of the re-housing experiment we study. The number of participants is not large enough for us to detect small effects with confidence ($d < 0.5$). Nevertheless, we can conclude that our project did not generate any large or medium-sized adverse effects. Previous evidence provided by Baird, De Hoop, and Özler (2013) and Haushofer, Reisinger, and Shapiro (2015) suggested that randomly allocated cash transfers negatively affect the life satisfaction and mental well-being of the untreated. This raises a question about the external validity of our findings. Apart from the nature of the intervention (cash vs. housing), the main difference in our setup is that in the cash transfers project, the untreated were in close contact with the treated as they were neighbors (Haushofer, Reisinger, and Shapiro, 2015) or school mates (Baird, De Hoop, and Özler, 2013). Our treated families moved to municipal apartments scattered throughout the city and typically did not know the untreated families, so contact between the two groups was limited. Our findings might be relevant for all such cases in which participants in different treatment do not interact closely with one another.

Undoubtedly, more evidence is needed. Whenever reasonable, (high-powered) RCTs with baseline and after-treatment surveys might consider reporting the impact of being untreated on measures of interest such as life satisfaction and mental well-being. If no adverse impacts are found, this is good news for RCTs. Even if this were the case, however, and confirmed our results, the question remains as to what should be done with evidence that negative effects do exist, such as the evidence presented by Baird, De Hoop, and Özler (2013) and Haushofer, Reisinger, and Shapiro (2015). There are at least two reasons why any such evidence would need to be handled with caution.

First, even if some RCTs are found to have adverse impacts on the untreated, this does not necessarily mean that the lottery is an inferior mechanism for the allocation of scarce resources. The potential negative outcomes of random allocation need to be compared with the potential negative outcomes of alternative allocation mechanisms: are the impacts of randomization any worse than the impacts of, say, allocation based on need or merit. Second, random allocation enables to draw causal inference from the differences between the treatment groups. In many instances, this might provide a crucial argument in favor of randomization, even if the participants themselves clearly prefer an alternative allocation mechanism.

# Chapter 6

# Summary and discussion

We begin this final chapter with a concise summary of the main results of the thesis, before proceeding to discuss two issues that we have as yet only partially touched upon: First, we address the issue of external validity and shortly discuss other limitations of our research; Second, we provide a short discussion of the policy implications of our results.

## 6.1 Summary of results

In this thesis, we have studied the impact of improved housing conditions on poor people's well-being and their economic decisions. We have also reflected on how a history of living in substandard housing conditions may affects how one is perceived by other people. To draw conclusions about causal relationships, we took advantage of the random assignment of better housing conditions to participants of the Housing First RCT that started in Brno, Czech Republic, in 2016. We measured societal perceptions in laboratory experiments with student participants, which we conducted at the Masaryk University Experimental Economics Laboratory (MUEEL) in Brno. Our results therefore originate from observed choices made by (urban poor) people living in a developed country. The thesis consists of three studies looking at different aspects of the topic.

The first study, presented in Chapter 3, contributes to the discussion on causal relationships between poverty and preferences (Guiso and Paiella, 2008; Tanaka, Camerer, and Nguyen, 2010; Carvalho, Meier, and Wang, 2016) and between poverty and cognitive function (Shah, Mullainathan, and Shafir, 2012; Mani et al., 2013; Carvalho, Meier, and Wang, 2016). We show that the improvement in housing quality provided to the Housing First intervention families had a large effect on their life satisfaction; the intervention therefore substantially improved the families' living standards. We use incentivized multiple price lists adapted from Sutter et al. (2013) to elicit risk and time preferences and the d2 test to measure sustained attention (Bates and Lemay, 2004). We establish the causal effect by comparing the outcomes between the intervention and control families. We find similar risk attitudes, time preferences and d2 scores in both the treatment and control families. Therefore, we do not find that the improvement in housing conditions had any effect on our measures of preferences and cognitive abilities. These findings contradict the results of several previous studies (Tanaka, Camerer, and Nguyen, 2010; Spears, 2011; Shah, Mullainathan, and Shafir, 2012; Mani et al., 2013), which either induced scarcity experimentally, or were based on data from developing countries. On the other hand, our findings are in line with those of Carvalho, Meier, and Wang (2016), who based their study on data collected from the poor population of another developed country, the United States.

The second study (see Chapter 4) looks at how substandard housing influences whether people are perceived as trustworthy and able to focus. The perceptions are elicited in the laboratory using our sample of students. Our design enables us to separate the effect of housing quality from the effect of the bad signal provided by a history of inadequate housing. We find that *bad housing conditions* reduce perceived trustworthiness, but leave expected concentration performance unaffected. Families with three children are seen as less trustworthy than families with two children. This suggests that our measure of trustworthiness corresponds to a perceived need for money, i.e. people in bad housing conditions seem to be more in need of money than people living in municipal flats. Conversely, *bad housing history*, i.e. living or having lived in substandard housing, reduces perceived concentration performance but does not affect trustworthiness. The student subjects perceive that people with histories of sub-standard housing differ in their abilities from people without such histories. This suggests that the interpretation of homelessness in the domain of cognitive abilities differs from that in the domain of trustworthiness: the students seem to believe that cognitive abilities impact living conditions but not that housing conditions affect cognitive abilities.

The third study, presented in Chapter 5, investigates whether random assignment into treatment groups negatively affects the untreated (as found by Baird, De Hoop, and Özler (2013) and Haushofer, Reisinger, and Shapiro (2015)), especially whether participants are aware of their treatment status and the treatments differ in their desirability. In line with evidence from cash-transfer experiments (Baird, De Hoop, and Özler, 2013; Haushofer, Reisinger, and Shapiro, 2015), we look at the impact of being untreated on life satisfaction and psychological distress. In the case of Housing First in Brno, we find no significant differences between the values before and after the outcomes of the treatment lottery were announced. We also investigate potential negative impacts on participants' perceptions of pro-sociality (perceived trustworthiness, fairness, and helpfulness of other people) and their generosity, using incentivized dictator games with charitable organizations as recipients. We do not find any difference in perceived pro-sociality after the treatment assignment was announced, nor do we find any differences in the charitable donations made by treated and untreated participants.

## 6.2 Limitations

Our results are subject to several limitations. The main limitation of the studies based on the Housing First data is their sample size. As we have explained, for example in Section 3.3, our sample size provides us with sufficient power to detect large and medium-sized effects (Cohen's $d \geq 0.5$) but not small effects. This has different implications in each of our studies. In the first study (Chapter 3), we are not able to rule out that improved housing conditions lead to a small effect on preferences and sustained attention. In the third study (Chapter 5), we cannot exclude the possibility that the Housing-First RCT had a small negative effect on the untreated.

Another important problem of randomized experiments is their external validity (Banerjee and Duflo, 2009). One question, left undiscussed so far, is whether the results we have obtained from the urban poor in a developed country can tell us anything about the impacts of extreme poverty. A comparison of the lives of the (extremely) poor (Banerjee and Duflo, 2007) and the lives of the urban poor in our sample reveals many similarities between the two groups, such as life in crowded low-quality housing and limited financial resources. However, the differences are substantial. Most importantly, poor people in developed countries usually benefit from the welfare state. They typically do not face food insecurity and have easier access to necessary calories; they enjoy better health and usually have better access to health care, etc. Furthermore, the results of our first study, which differ from previous findings based on data from developing countries (Tanaka, Camerer, and Nguyen, 2010; Mani et al., 2013), yet coincide with findings from the US (Carvalho, Meier, and Wang, 2016), suggest that there might be important differences. I believe that we should take extra care when generalizing our results for the poor or extremely poor of the developed world. Our results are relevant mainly within the context of poorly housed urban poor in the developed world.

## 6.3 Policy recommendations

The results our first and second studies may inform public policy at two levels: First, by studying how poverty impacts choices and perceptions in general, we may be able to improve the design of development strategies. Second, the outcomes of these studies are important for evaluating public housing policies.

In the first study (Chapter 3), we do not observe that improved housing conditions have any effects on preferences and cognitive abilities. We therefore do not provide support for the existence of behavioral poverty traps. We find that a substantial and costly improvement in housing quality may not translate into better economic choices through more patience and lower risk aversion, or through improved focus. Similarly, our results do not provide any further boost to the Housing First movement. However, it must be emphasized that the goal of these projects is to provide stable housing to those in need, not to help them out of poverty by changing their preferences and cognitive abilities.

Our second study (Chapter 4) suggests that people living in inadequate housing conditions face adverse expectations from the rest of society. Here, our results may inform development policy and provide an additional reason for funding rehousing projects. People who moved out of inadequate housing are perceived as more trustworthy than people still living in poor conditions, even when there are no other differences between these groups. This may be especially important in randomized experiments in which participants' histories of bad housing are known to the project team or the partners of the project, and where trustworthiness might be a key determinant of success in the intervention. Regarding the impacts of the treatment on concentration performance, we find that our student sample expects higher performance in the d2 task only from participants without any history of substandard housing. Here, the good news is that those making hiring decisions on the job market are typically uninformed about applicants' housing history.

Finally, our third study (Chapter 5) provides good news in support of efforts to use randomized experiments to evaluate public policy. Contrary to the evidence provided by cash-transfer experiments in which treated and untreated participants belonged to the same social groups (Baird, De Hoop, and Özler, 2013; Haushofer, Reisinger, and Shapiro, 2015), providing scattered housing to a randomly selected group of participants does not seem to affect the life satisfaction, mental health and pro-sociality of the untreated participants. This provides support for the idea that a successful strategy to reduce the impacts of random assignment on the lives of the untreated might be to randomize at the level of locations and justify the random allocation of superior treatment by budget constraints or administrative capacity (Banerjee and Duflo, 2009).

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# Appendices

# Appendix A

# Experimental instructions

## A.1 Lab-in-the-field experiment

Questionnaire version BT [1]

### Introduction

Our meeting will have five parts. We will call them Games 1 through 5. After the last game, a short questionnaire and a cash prize will follow. Your earnings will depend on how you make decisions in each game. Therefore, please follow the explanation of the rules carefully. I will always explain the rules of each game before it begins. In some games, your result will also depend on chance. Most of the money will be paid today in cash, some of it will be paid during the meeting and some of it will be paid at the end of today's meeting. Earnings from one game will be delayed. I will explain all the details later.

Is everything clear? Do you want to ask any questions? (***Show them money we have ready for the payoffs. Prepare contracts.***)

Since we are meeting at your home and we are going to pay you based on a contract containing your personal information, your decisions in this experiment

[1] Version of the questionnaire with blue first in Game 4, as opposed to a game in which subjects start with the anonymous choice in red box, and Game 5 (some of the questionnaires did not include Game 5).

are not anonymous. However, we will not ask you for any sensitive data in this session. You can also be sure that no one outside the project will be able to match your responses to your personal information. In particular, you can be sure that whatever you do here will not affect whether [Control: you get housing] [Intervention: you keep your housing]. That decision is taken by the city of Brno and we will not be providing them with any of your answers containing your personal information.

Is everything clear? Do you want to ask any questions? (***Show them money we have ready for the payoffs. Prepare contracts.***)

Now we need to sign a contract in order to be able to pay you money. Take your time to read it. In the contract we need to fill in your details and the date of the experiment. Next, we need you to sign the other side of both copies of the contract. In the annex to the contract, we will fill in the total payment of the experiment at the end of today's session. So you will sign this part at the end of today's session (***let them sign the contract***). Any questions? (***If not, continue to play 1***)

### A.1.1 Game 1

First we will explain the rules of Game 1. From now on, please listen carefully. If anything is not clear to you, interrupt me and ask.

In this game, you decide whether you want to get a certain amount of money with certainty or whether you want to take a risk and have a chance of getting 220 CZK. The chance will be created by drawing tokens from a bag. There are tokens of two different colors in the bag: 10 green tokens and 10 blue tokens (Show them the tokens in the bag). There are numbers on the tokens. These are not important yet. The draw from the bag will work as follows: You first choose one of the colors. Let's say you choose green. Then you randomly draw one token. If it is green, you will get 220 CZK from us. If you draw a blue token, you get nothing.

Is everything clear? Do you want to ask a question?

(***Show them the answer sheet***).

**Game 1**

| | Draw from the bag: equal chances of winning 220 or 0 CZK | | or | | The amount you receive with certainty |
|---|---|---|---|---|---|
| [1] | draw from a bag | O | or | O | 11 CZK with certainty |
| [2] | draw from a bag | O | or | O | 22 CZK with certainty |
| [3] | draw from a bag | O | or | O | 33 CZK with certainty |
| [4] | draw from a bag | O | or | O | 44 CZK with certainty |
| [5] | draw from a bag | O | or | O | 55 CZK with certainty |
| [6] | draw from a bag | O | or | O | 66 CZK with certainty |
| [7] | draw from a bag | O | or | O | 77 CZK with certainty |
| [8] | draw from a bag | O | or | O | 88 CZK with certainty |
| [9] | draw from a bag | O | or | O | 99 CZK with certainty |
| [10] | draw from a bag | O | or | O | 110 CZK with certainty |
| [11] | draw from a bag | O | or | O | 121 CZK with certainty |
| [12] | draw from a bag | O | or | O | 132 CZK with certainty |
| [13] | draw from a bag | O | or | O | 143 CZK with certainty |
| [14] | draw from a bag | O | or | O | 154 CZK with certainty |
| [15] | draw from a bag | O | or | O | 165 CZK with certainty |
| [16] | draw from a bag | O | or | O | 176 CZK with certainty |
| [17] | draw from a bag | O | or | O | 187 CZK with certainty |
| [18] | draw from a bag | O | or | O | 198 CZK with certainty |
| [19] | draw from a bag | O | or | O | 209 CZK with certainty |
| [20] | draw from a bag | O | or | O | 220 CZK with certainty |

Figure A.1: Risk-preference sheet

The choices are clearly shown in the answer sheet. E.g. in the first line you can choose either to draw from the bag or to receive 11 CZK for sure. If you choose to draw from the bag, you have equal chances of winning 220 CZK (if you draw the right color) or winning nothing (if you draw the wrong color). If you choose 11 CZK, you will surely get 11 CZK at the end of today's session. Note that the only thing that changes is the amount you can receive for sure. This amount increases as you move down in the lines. We will gradually fill in 20 lines with 20 choices.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again***.)

Now I will explain how you will earn money in this experiment. You will fill in 20 lines with 20 choices. We will pay you according to your answer in one of these lines. The tokens have numbers from 1 to 20 on them (***show them that there are numbers from 1 to 20 on the tokens***). At the end of today's session, you will draw a number which will correspond to the line number and we will pay you according to your answer in that line. If you check the left box (pull from the bag) on this line, you choose the color and you will draw again. If you draw the chosen color, you will get 220 CZK. If you draw the second color, you will get 0 CZK. If you select the right field on this line, you will be given the amount stated on the line with certainty. Since the selection of all 20 lines is equally probable, you need to fill in each line of the questionnaire together as if it were the one that would be selected for payment.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

If there are no other questions, we can fill in the questionnaire together.

### A.1.2 Game 2

Now let's explain the rules of game. 2. If anything is not clear, just ask. There is money at stake in this game as well.

You will decide whether you want to receive a certain amount of money at an earlier date or another higher amount at a later date. For example, we will ask you to decide between a lower amount of money you will receive on the next business day and a higher amount of money that you will receive a week from the next business day. We will insert your payoff in an envelope and leave it at the post office of your choice. If you choose an earlier option, you will have the money from the post on the next business day. If you choose a later option, the money will be ready in a week from the next business day. We will explain all the details going forward.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

The questionnaire consists of 4 sheets. Each sheet shows the times of earlier and later payments and different amounts on 20 lines. Individual sheets differ only in times when you get the money from us. Earlier dates will be either the next business day, or a week after the next business day. For a later payment, you will have to wait additional 2 weeks or 10 weeks after the earlier date. Let's look at the first sheet of the questionnaire you will fill out (***show them the first sheet of the questionnaire*** [**see Figure A.2**]). In this case, the earlier date is ... (***tell them which date it is***) and the later pay date is ... (***tell them which date it is***). You can see that the amount on the left is 98 CZK in all 20 lines. The amount on the right starts at 118 CZK and increases in steps of 20 crowns to 498 CZK on the last line. On each line, you decide whether you want to get 98 CZK on ... (***tell them the specific date***) or the amount in the right column (***tell them the specific date***). Note that the amount in the right column increases in each row. We will explain the choices in each answer sheet in detail before we start filling in answers.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

As I mentioned earlier, we will send you the money to the post office of your choice in a nontransparent envelope. The coins will be covered in hard paper so that it will not be possible to tell that there is money in the envelope. On the envelope we will write the date of payment, your name, poste restante[2] and postal code. At the same time, we will leave you a note with all the necessary information such as the name on the envelope, the amount you will receive from us, the date of payment and the postal code (postal address).

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

[2]If the address contains poste restante", the letter stays at the post office for a determined time period (14 days).

**Game 2 (sheet 1), Type Z2**

| | amount **the following business day Date:** | | or | | amount **in 2 weeks after the following business day Date:** |
|---|---|---|---|---|---|
| [1] | 98 CZK | **O** | **or** | **O** | 118 CZK |
| [2] | 98 CZK | **O** | **or** | **O** | 138 CZK |
| [3] | 98 CZK | **O** | **or** | **O** | 158 CZK |
| [4] | 98 CZK | **O** | **or** | **O** | 178 CZK |
| [5] | 98 CZK | **O** | **or** | **O** | 198 CZK |
| [6] | 98 CZK | **O** | **or** | **O** | 218 CZK |
| [7] | 98 CZK | **O** | **or** | **O** | 238 CZK |
| [8] | 98 CZK | **O** | **or** | **O** | 258 CZK |
| [9] | 98 CZK | **O** | **or** | **O** | 278 CZK |
| [10] | 98 CZK | **O** | **or** | **O** | 298 CZK |
| [11] | 98 CZK | **O** | **or** | **O** | 318 CZK |
| [12] | 98 CZK | **O** | **or** | **O** | 338 CZK |
| [13] | 98 CZK | **O** | **or** | **O** | 358 CZK |
| [14] | 98 CZK | **O** | **or** | **O** | 378 CZK |
| [15] | 98 CZK | **O** | **or** | **O** | 398 CZK |
| [16] | 98 CZK | **O** | **or** | **O** | 418 CZK |
| [17] | 98 CZK | **O** | **or** | **O** | 438 CZK |
| [18] | 98 CZK | **O** | **or** | **O** | 458 CZK |
| [19] | 98 CZK | **O** | **or** | **O** | 478 CZK |
| [20] | 98 CZK | **O** | **or** | **O** | 498 CZK |

Figure A.2: Time-preference sheet

Now I will explain how you will earn money in this experiment. You will fill in the 4 answer sheets. Each of them contains 20 lines with 20 questions. Altogether it makes 80 decisions on 80 lines. We will pay you according to your answer in one of these lines. The number of the line will be drawn from a bag containing numbers from 1 to 80 (***show them that there are numbers from 1 to 80 in the bag***). If the left field was checked on the line, we will send an envelope with

98 crowns to the post office of your choice at the earlier date. If the right field was checked on that line, we will send an envelope with a higher amount at the later date. Since the selection of all 80 lines is equally probable, you need to fill in each line of the questionnaire as if it were the one that would be selected for payment.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

If there are no other questions, we can fill in the questionnaire together.

### A.1.3 Game 3

I will explain this game to you through a practice test (***Give them a practice test, see Figure A.5***). In the test, you get two sheets of paper with a large number of letters p and d with 0, 1 or 2 vertical lines above and below the letter. Your task will be to find and circle all d characters with a total of two vertical lines around the letter. You will have a total of 4 minutes and 40 seconds for this task, so you have 20 seconds for each of the 14 lines.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

Now I will give you 40 seconds to try to mark all the search characters on the first lines of the test (***take out the stopwatch, start the time, and when they fill the test, go through the template test with them, show them where they missed the characters and made mistakes. Also, count the correct answers***).

(***Calculate the number correctly, point out the wrongly circled answers and explain how much they would earn.***)

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

If you do not have questions, we can proceed to the test. You get two test paper sheets from us. Start with Sheet 1. You can proceed to Sheet 2 at any time during testing. Now you have 4 minutes and 40 seconds to complete it. We will evaluate the test and pay you the money at the end of today's session.

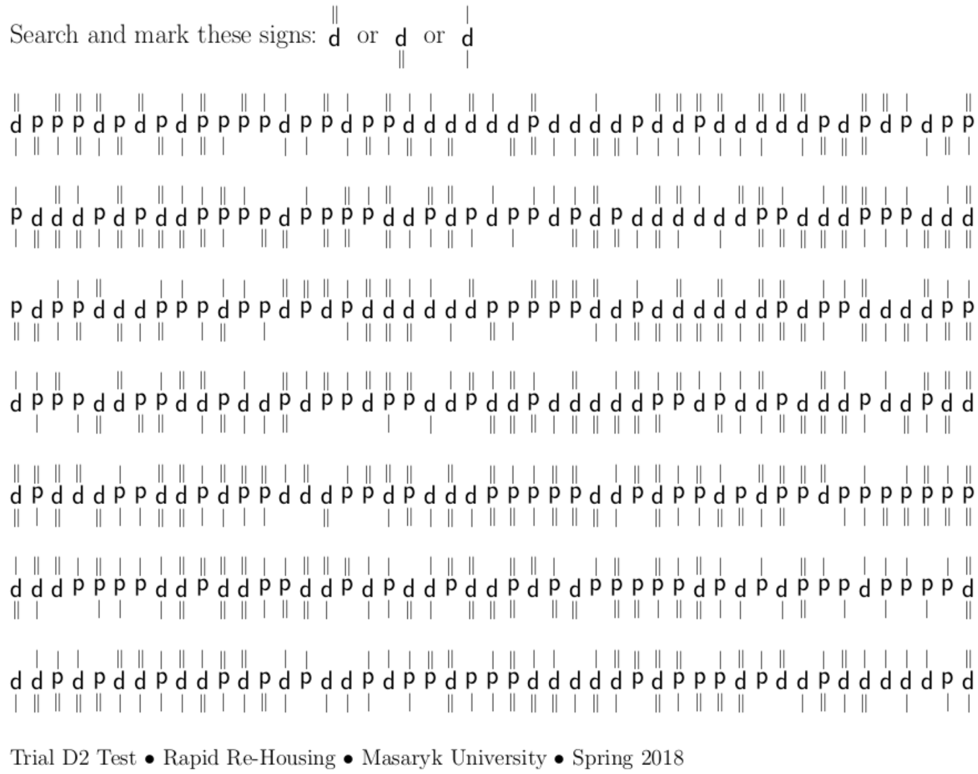


Figure A.3: d2 test – trial answer sheet

Can we start the test?

### A.1.4 Game 4

Now we proceed to the penultimate game. In this game we will give you two boxes. In each of them is 150 CZK in such coin denominations that allow you to select any amount from 0 to 150 CZK in multiples of ten i.e. 0, 10, 20, 30, 40, 50 to 150 CZK.

I will open the blue box now. You can check what is in it. Note that there is no sign or mark on the box except for I or K, indicating whether you have been given an apartment or not (***show them the blue box***).

The contents of this box are yours - we will write the entire 150 CZK from this part of the game into your contract as your payoff, so you can keep all the money. However, we will give you the opportunity to send an amount between 0 and 150 CZK to the children of the foundation "Rafael dětem" that helps severely ill children. The activities of this foundation will be explained in the following text.

(***Give them page 1 of the answer sheet. Read the first paragraph aloud to them. Ask them if everything is clear.*** [**see Figure 5.2a**])

Your task is to decide what amount between 0 and 150 CZK you want to donate to the foundation Rafael dětem. I will read the detailed instructions now. Take all the money from the blue box and put them on your hand. Put the amount you want to donate to the foundation back into the box. You can put the rest of your money in your wallet or pocket. You will also write your name and surname on the line and the amount you want to contribute on the next line of the answer sheet. Then I will check whether the amount in the box matches what you wrote in the sheet. Then close the box and cover it with adhesive tape. We will send the money to Rafael for the children. We do not pay any additional fees, so the foundation will receive the whole amount you donate.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

The size of your donation along with your name may be shared among the members of the research team. We will not however share it with anyone outside the project, especially anyone in charge of the municipal housing strategy in Brno.

You will now proceed as follows:

1. Open the blue box and remove the money.
2. Decide what amount you would like to donate from 0 to 150 CZK to help the seriously ill children. Keep the rest of your money in your pocket or wallet.
3. Put the amount you want to donate back into the box.

Now let us proceed to the second part of this game with the red box. I will open it now. You can check its contents. Note that there is no sign or mark on the box (Show them the red box). In addition to 150 CZK, this box also contains several metal circles. These are here to ensure the anonymity of your decision. We will explain everything now.

The contents of this box are yours - we will write the entire 150 CZK from this part of the game into your contract as your payoff, so you can keep all the money. But we will give you the opportunity to send an amount between 0 and 150 CZK to the children in the foundation Dobrý anděl that helps severely ill children. The activities of this foundation will be explained in the following text.

(***Give them page 2 of the answer sheet. Read the first paragraph aloud to them. Ask them if everything is clear.*** **[see Figure 5.2b]**)

(***The following text explains the instruction***) Your task is to decide what amount between 0 and 150 CZK you want to donate to the foundation Dobrý anděl. Now I will read the detailed instructions on how to do this. Put the entire contents of the red box on your palm, i.e. all the money and metal circles. I will turn around [or leave the room – if possible] and not look. Put the amount between 0 and 150 CZK that you want to donate to the foundation Dobrý anděl and all the metal circles back into the box. Put the rest of your money in your wallet or pocket so that I cannot see how much money you have left. Close the box and tape it. We will not control the contents of the box. Finally, just stick this box to the blue box using a tape. On the blue box, the letter K or I is written to indicate whether or not you have received an apartment from the city apartment project. Otherwise, the boxes do not reveal any information about you. At the end of the game, we put the boxes taped together in a bag containing other boxes (***Carefully remove several packages of boxes from the bag. Show the packages to them, but do not let them touch them so they cannot weigh them and estimate how much others have donated.***). Mix the boxes in the bag so that we are not able to tell which package belongs to you. We will send money to the foundation Dobrý anděl. We do not pay any additional fees, so the foundation will receive the whole amount you donate.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

Your donation will be in a closed box that will be mixed with other respondents' boxes after your decision. This means that only you and no one else will know the amount of your donation.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

You will now proceed as follows:

- Open the red box and empty the contents of it into your hand.
- Decide what amount between 0 and 150 CZK you would like to donate to help the seriously ill children. Keep the rest of the money in your pocket or wallet so that it is not visible.
- Put the amount you want to donate and all the metal circles into the box at the same time.
- Tape the closed box.

### A.1.5 Game 5

This is the last game. It will only be followed by a short questionnaire. Now I will explain the rules. If something is not clear, please ask directly.

This game is played by you and one student of Masaryk University. This student was randomly assigned to you. We won't tell you who the student is nor will you know whether he was a man or a woman or what (s)he studies. This student played the following game with us at the beginning of March. (S)he received CZK 150 from us. (S)he could keep this money. (S)he could also decide to send you one of the 30, 60, 90, 120, or 150 CZK amounts to you. The student did not know who (s)he was playing with. (S)he only received some information about the average family in the housing project.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear what should be explained again.***)

Now comes the important part. The student knew that you will receive three times the amount (s)he sent. (S)he knew that if (s)he sacrificed 30 CZK from his earnings, you would get 90 CZK, when (s)he sacrifices 60 CZK, you get 180 CZK, when (s)he sends 90 CZK, you get 270 CZK, when (s)he sends 120 CZK, you get 360 CZK, and when (s)he sends 150 CZK, you get 450 CZK. (S)he also knew that you will have an opportunity to send him back part (or all) of this triple amount. This game simulates a very profitable but insecure investment: profitable because the money invested in you triples, uncertain because the student does not know how much of this tripled amount (s)he will get back.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear what should be explained again.***)

Now we can proceed to the game itself. Here is an answer sheet with the amount sent to you and the amount you receive from us. Now let us look at the answer sheet (***turn over the answer sheet and show them how much the student has sent and how much they received***). Your task will be to write on the paper the amount you want to send back to the student. When you decide, we will immediately pay you the rest of the amount you received. The student you were paired with will be paid depending on how much you send back. If you want, we will calculate the student's payoff and your payout for any amount you are considering to send back.

Is everything clear so far? (***Wait until they agree. If they hesitate, ask what is not clear and what should be explained again.***)

Answer sheet of game 5

The student sent you 60 CZK.

We will give you 180 CZK.

How much do you want to send back to the student? _______ CZK

The students payoff in this game is: _________ CZK

Your payoff in this game is: ________ CZK

Figure A.4: Trust game – example of an experimental sheet

### A.1.6 Post-experimental questionnaire

This section shows the English translation of the post-experimental questionnaire. The questions reflect data used in the analysis with the exception of the question 4 where the coding was reversed, so that higher numbers capture higher intensity of the feeling.

## A.2 Trustworthiness and conc.: experiment 1

*This task was part of a larger experiment with several parts (called experiments), which as played in two sessions on different days with two weeks-time in between. This task was the second experiment in the first session. Before starting the task subjects did not receive any information about the payoff from experiment 1.*

Experiment 2 will now follow. Selected adult members of families living in Brno, which will be described in the experimental environment, participated in the attention test. As part of the test, they received a sheet of paper with a large

# Final questionnaire

1. How many hours did you sleep last night?

| | |
|---|---|
| Number of hours: | |
| (Don't know) | 98 |
| (Refused) | 99 |

2. Is this number of hours of sleep rather exceptional?

| | |
|---|---|
| Yes | 1 |
| No …………………… RA: MOVE TO QUESTION NO. 4 | 2 |
| (Don't know) | 98 |
| (Refused) | 99 |

3. If exceptional, how many hours do you sleep normally?

| | |
|---|---|
| Number of hours: | |
| (Don't know) | 98 |
| (Refused) | 99 |

4. How do you feel today?

| Completely true 1 | Rather true 2 | Halfway 3 | Rather untrue 4 | Completely untrue 5 | (Don't know) 8 | (Refused) 9 |
|---|---|---|---|---|---|---|

| | | |
|---|---|---|
| a) I have nerves / I am nervous | 1 2 3 4 5 8 9 | |
| b) restless or unfocused | 1 2 3 4 5 8 9 | |
| c) in such a depression that nothing pleases me | 1 2 3 4 5 8 9 | |

5. How many days do you have until you receive your paycheck or benefits?

| | |
|---|---|
| Number of days: | |
| (Don't know) | 98 |
| (Refused) | 99 |

6. Will you be missing money for something important over the next three days?

| | |
|---|---|
| Yes | 1 |
| Ne …………………… RA: MOVE TO QUESTION NO. 8 | 2 |
| (Don't know) | 98 |
| (Refused) | 99 |

7. What will you miss the money for?

| | |
|---|---|
| | |
| (Don't know) | 98 |
| (Refused) | 99 |

8. Do you have a seriously ill child?

| | |
|---|---|
| Yes | 1 |
| Ne …………………… RA: MOVE TO QUESTION NO. 10 | 2 |
| (Don't know) | 98 |
| (Refused) | 99 |

9. What is your child's illness?

| | |
|---|---|
| | |
| (Don't know) | 98 |
| (Refused) | 99 |

10. Do you dress in better (outdoor) clothes when you go out of the house and stay around the house (eg walking your dog or shopping a in nearby shop)?

| | |
|---|---|
| Yes | 1 |
| Ne | 2 |
| (Don't know) | 98 |
| (Refused) | 99 |

11. Are the following statements true?

| Completely true 1 | Rather true 2 | Halfway 3 | Rather untrue 4 | Completely untrue 5 | (Don't know) 8 | (Refused) 9 | |
|---|---|---|---|---|---|---|---|
| a) I care about what my neighbors think of me. | | | | | | 1 2 3 4 5 8 9 | |
| b) I care about what people I meet on the street or in the store think about me. | | | | | | 1 2 3 4 5 8 9 | |

number of letters *p* and *d* with 0, 1 or 2 dashes above and/or below the letter (see the practice test sheet in front of you [*see Figure* A.5]). Their task was to find and mark all *d*s with a total of two commas around the letter. They could test the test in 40 seconds first. We then corrected and scored the practice test. Then we proceeded to the scored test (a total of two sheets), for which they had a total of 280 seconds.

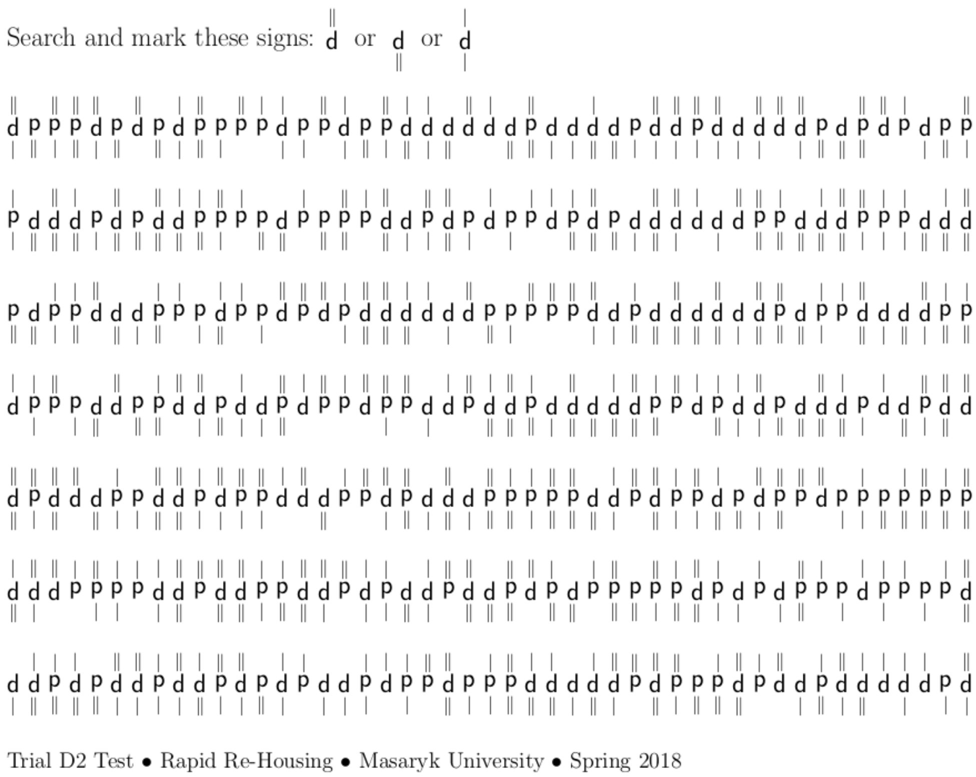


Figure A.5: d2 test – trial answer sheet

The score of this test was calculated as follows. Participants received one point for each correctly marked symbol. For each incorrectly marked character, on the contrary, one point was deducted. The total number of points was equal to the number of correctly marked symbols minus the number of incorrectly marked symbols. Participants in the experiment received a payoff equal to the total number of points in crowns.

Your task in this experiment will be to estimate the average score of the members of a particular family type from this test. But first, we will give you the opportunity to try the test yourself to get an estimate of the difficulty of the test. Test administration will be similar to that of the experiment participants. First we will give you 40 seconds for the practice test. Then you will be able to evaluate the test yourself. If there are any questions about the evaluation, raise your hand and we will come to you and answer your questions.

Subsequently, you will receive from us page 1 of the same scoring test filled out by the participants of the field experiment. You will get the page with the blank side up. You can turn it when instructed. To complete the test we will give you 70 seconds, which is  of the 280 seconds participants of the field experiment had to complete the test. You will fill in the test with a pen, after you pass the test, you will hand in the pen, write your identification code on this page with a pencil. Then you will be given a few minutes to correct the test yourself. The goal will be to find out the total number of points, which equals number of correct minus the number of wrong cancellations. You enter your total points in the experimental environment. We will check your test in the next 14 days. After the next session you will receive the number of points in CZK for completing the test. If your test score is correct, i.e. the number of points entered into the experimental environment will correspond to the number of correctly and badly cancelled symbols in the test, you will receive an extra bonus of 10 crowns.

After completing both tests, we proceed to the 4 screens of the experimental environment. The screens will contain a short description of the families that participated in the testing. This information will vary from screen to screen. Please read this information carefully. On these screens, you will fill in the estimated average scores for these families. Please note that the adult members of these families had 4 times more time than you to complete the test. At the end of this experiment, one of your decisions will be randomly selected. If you are 5 points or less from your actual score, you will receive a second bonus of 50 crowns, which will be paid in 14 days too. We will also let you know if you guessed correctly at the end of the session in 14 days.

## A.3 Trustworthiness and conc.: experiment 2

*This task was part of a larger experiment with several parts (called experiments). This task was the first experiment.*

Now we will explain the rules of experiment 1. From now on, please listen carefully. If you have a question, please raise your hand. One of the organizers will come to you and answer your question.

In this game you will make a total of 8 decisions. One of these decisions will be randomly selected for payment. These decisions are independent, so none of your decisions will affect the payout of other decisions. Your payout depends on the decision of other people to play this game in the next two to three months. Therefore, the payout of Experiment 1 will take two to three months. We will pay you the money by bank transfer. We'll ask you for your account number in the next few days via the email in your tip. If, for some reason, you are unable to provide us with an account number, we will send you an email within two to three months with information on when and where you can collect your money in person at the ESF.

This experiment consists of 8 decisions. In each of these, your payout will depend on the decision of an adult member of one family who is randomly selected from adult members from a particular family group. We will call this person the recipient. This will most likely vary in each of the following 8 decisions. We will not tell you the details of the recipient's family. We will reveal selected characteristics of a typical family from a given family group.

**Decisions 1–4**

At the beginning of each of the decisions 1–4 you get from us **150 CZK** (the recipient in this game will not receive any money). You must decide how much money you send to the recipient. You can send any multiple of 30, that is, 0, 30, 60, 90, 120 or 150 CZK. Any crown you do not send will be part of your payout from this decision. Each crown you send to the recipient is multiplied by **three**. The recipient then decides how much money he will return. The recipient can keep every crown he will not send you.

Your earnings from this decision are equal to the amount you keep, plus the amount that the recipient will refund. The beneficiary's earnings are equal to the amount he retains.

The following four decisions differ only in the description of the group of families from which your recipient will be randomly selected.[3]

**Decision 5-8**

In Decisions 5-8, we ask you to try to estimate the beneficiary's decision. If your estimate is accurate, your payout from this decision will be 200 CZK. Now we will explain the details.

One of the experiment participants sent 60 CZK to the recipient. As the amount sent triples, the recipient receives CZK 180. It can therefore return an amount between 0 and 180 CZK. Your task is to guess how much the recipient will return. The recipient may send back any integer amount of CZK. If your estimate deviates from your refund by 10 crowns or less, your payout in this decision will be 200 CZK.

The following four decisions differ only in the description of the group of families from which your recipient will be randomly selected.

[3]For a detailed description of the decision screens, see Section 4.2.1.

# Appendix B

# Tobit regressions

## B.1 Preferences and cognition

Tables B.1 and B.2 present the Tobit estimates using the same regression equations as in Tables 3.4 and Tables 3.5, respectively. These tables confirm all the findings of the OLS models presented in the chapter.

Table B.1: Risk preferences – Tobit regression results

| *Dependent variable:* | Risk seeking | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Constant | $9.022^{***}$ | $8.283^{***}$ | $8.524^{***}$ | $8.017^{***}$ |
| | (0.631) | (0.722) | (0.888) | (1.851) |
| Intervention | 0.002 | −0.143 | 0.196 | −0.274 |
| | (1.022) | (1.132) | (1.514) | (1.806) |
| Male | | $2.665^{**}$ | $2.710^{**}$ | 2.115 |
| | | (1.164) | (1.169) | (1.353) |
| Intervention × Male | | 2.195 | 2.192 | 1.905 |
| | | (2.221) | (2.170) | (2.946) |
| Origin hostel | | | −0.657 | −0.658 |
| | | | (1.248) | (1.515) |
| Intervention × Origin hostel | | | −0.897 | −0.344 |
| | | | (2.029) | (2.250) |
| d2CP | | | | 0.008 |
| | | | | (0.024) |
| Days to pay | | | | −0.043 |
| | | | | (0.070) |
| Missing money | | | | 0.901 |
| | | | | (1.081) |
| Experimenter FE | No | No | No | Yes |
| Observations | 161 | 161 | 161 | 147 |
| Left-censored | 18 | 18 | 18 | 18 |
| Right-censored | 1 | 1 | 1 | 1 |

*Note:* Tobit regression with standard errors clustered at the household level. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

## B.2 The impact on the untreated

In this section we use Tobit model to reflect the censoring in the data. Table B.3 is based on anonymized data and provides the same regression equations as Table 5.5. Table B.4 only uses the data from the observed donation variant and corresponds to Table 5.6.

Table B.2: Time preferences – Tobit regression results

| *Dependent variable:* | Time preferences | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Constant | −2.091 | −2.649 | −2.672 | −1.625 |
| | (1.483) | (1.639) | (1.819) | (4.001) |
| Intervention | 0.344 | 1.732 | −0.470 | 1.492 |
| | (2.246) | (2.349) | (2.921) | (2.817) |
| Delay | $-1.245^{*}$ | $-1.239^{*}$ | $-1.227^{*}$ | $-1.337^{*}$ |
| | (0.643) | (0.640) | (0.631) | (0.683) |
| Intervention × Delay | 0.705 | 0.680 | 0.755 | −0.314 |
| | (1.273) | (1.260) | (1.282) | (1.067) |
| Long period | $6.257^{***}$ | $6.240^{***}$ | $6.210^{***}$ | $6.635^{***}$ |
| | (0.955) | (0.955) | (0.941) | (0.962) |
| Male | | 2.091 | 2.055 | 2.415 |
| | | (2.217) | (2.183) | (2.353) |
| Intervention × Male | | −6.716 | −6.781 | −5.418 |
| | | (4.836) | (4.891) | (5.398) |
| Origin hostel | | | 0.267 | 2.041 |
| | | | (2.451) | (2.667) |
| Intervention × Origin hostel | | | 5.307 | 0.855 |
| | | | (4.225) | (4.425) |
| LetterReturn | | | | −3.263 |
| | | | | (2.397) |
| Days to pay | | | | −0.035 |
| | | | | (0.119) |
| Missing money | | | | $3.782^{*}$ |
| | | | | (1.959) |
| Risk | | | | 0.121 |
| | | | | (0.193) |
| d2CP | | | | 0.041 |
| | | | | (0.048) |
| Tomorrow | | | | −1.258 |
| | | | | (2.306) |
| Experimenter FE | No | No | No | Yes |
| Observations | 644 | 644 | 644 | 592 |
| Left-censored | 329 | 329 | 329 | 298 |
| Right-censored | 42 | 42 | 42 | 40 |

*Note:* Tobit regression with standard errors clustered at the household level. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table B.3: Tobit regressions – observed and anonymous donations

| | *Dependent variable:* Donations | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Constant | 57.523*** | 56.013*** | 58.969*** |
| | (7.336) | (9.125) | (9.953) |
| Observed | 20.257*** | 20.261*** | 20.272*** |
| | (5.761) | (5.768) | (5.778) |
| Observed × Intervention | −7.131 | −7.129 | −7.136 |
| | (9.649) | (9.634) | (9.613) |
| Intervention | 8.833 | 8.653 | 4.116 |
| | (11.121) | (11.090) | (12.002) |
| Observed first | | 3.103 | 3.757 |
| | | (9.553) | (9.582) |
| Male | | | −11.539 |
| | | | (12.952) |
| Intervention × Male | | | 18.087 |
| | | | (28.138) |
| Observations | 322 | 322 | 322 |
| Left-censored | 28 | 28 | 28 |
| Right-censored | 62 | 62 | 62 |

*Note:* Standard errors in parentheses are clustered at the individual level. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table B.4: Tobit regressions – observed donations

| | *Dependent variable:* Observed donations | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Constant | $68.857^{***}$ | $84.799^{***}$ | $85.064^{***}$ | $84.175^{***}$ |
| | (5.721) | (20.514) | (20.753) | (21.034) |
| Intervention | 0.411 | 4.265 | −0.728 | 1.386 |
| | (8.163) | (8.476) | (9.895) | (11.798) |
| Male | | | −10.158 | −10.198 |
| | | | (11.350) | (11.593) |
| Intervention × Male | | | 22.187 | 22.651 |
| | | | (19.307) | (19.513) |
| Origin hostel | | | | 0.719 |
| | | | | (10.747) |
| Treatment × Origin hostel | | | | −5.423 |
| | | | | (18.749) |
| Observed first | 8.675 | 2.261 | 2.647 | 2.923 |
| | (7.725) | (7.729) | (7.805) | (7.933) |
| Days to pay | | 0.041 | 0.037 | 0.019 |
| | | (0.463) | (0.464) | (0.471) |
| Missing money | | $-17.140^{**}$ | $-16.381^{*}$ | $-16.358^{*}$ |
| | | (8.233) | (8.372) | (8.431) |
| d2CP | | $0.407^{**}$ | $0.410^{**}$ | $0.413^{**}$ |
| | | (0.168) | (0.172) | (0.174) |
| Ill child | | 2.384 | 2.703 | 3.041 |
| | | (10.582) | (10.464) | (10.426) |
| Sleep | | $-3.416^{**}$ | $-3.207^{*}$ | $-3.135^{*}$ |
| | | (1.682) | (1.750) | (1.771) |
| Experimenter FE | No | Yes | Yes | Yes |
| Observations | 161 | 161 | 161 | |
| Left-censored | 16 | 16 | 16 | |
| Right-censored | 26 | 26 | 26 | |

*Note:* Standard errors in parentheses are clustered at the household level. Significance: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.