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Technequality

Micro- and Macro-Economic Effects of Unconditional Basic Income and Participation Income: A Systematic Review

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Abstract

This paper reviews 48 studies on the micro and macro-economic effects of Unconditional Basic Income (UBI, including NIT or Negative Income Tax) and Participation Income related programs (PI). Compared to previous review studies in the field, it advances on viewing the broader (un)intended) effects on income, (mental) health, subjective wellbeing and related outcomes (trust, social participation, substance abuse, crime etc.). Given the increasing number of studies on UBI-like programmes, the review focuses on RCT field experiments and simulation studies of UBI-PI while leaving out conditional cash transfer programs (CCT) and laboratory experiments. All programmes aim at providing a guaranteed minimum income either for the population at large or for specific groups (such as unemployed). We employ a systematic review with the main purpose of bringing together and learning from the evidence on these broader effects. Our focus is on studies of the effects of the older programmes in Canada and the United States during the 1970s and 1980s and of the more recent programs in the European context from the 1990s on. Recent US/Canada studies re-estimating the found negative labor supply effects (notably for married women with children) in the original studies of the experiments, came to much lower and even insignificant estimates. The effects on reducing poverty and inequality but also on health and subjective wellbeing were however more positive. The studies on the European programmes and experiments (Finland, Spain, Netherlands) show slightly more positive but still mostly insignificant labour supply effects. More positive and significant effects were however found on subjective wellbeing, (mental) health and trust which are however less pronounced than in Canada and the US. Some welfare state lessons that can be drawn from these results are briefly discussed.

Keywords: basic income, participation income, negative income tax, micro-economic effects, labour supply, poverty, income inequality, health, subjective wellbeing, trust, public policy.

1. Introduction

In recent years, the idea of a Guaranteed Minimum Income (GMI) through a Universal Basic Income (UBI), a Negative Income Tax (NIT) or a Participation Income (PI) has gained prominence in public and policy debates throughout the world. The UBI is a GMI that provides a periodic cash grant to all individual members of a community, without means test or work requirements, and at a level that is sufficient to ensure subsistence security. An NIT in combination with a UBI provides a periodic cash grant paid out through a cash allowance that is gradually taxed away when income increases (claw-back rate), e.g. through additional earnings up to a certain income threshold at which the benefit is fully withdrawn. A PI is a GMI to everyone whose income fails to make ends meet and that replaces or comes on top of the minimum social benefit and that is conditional on an income or (volunteer) work requirement.

UBI/NIT schemes used to be considered as rather utopian but not a realistic or viable option. However, many countries started in the last few decades with experimenting with different variants of UBI/NIT proposals including PI. This study aims to provide a systematic overview of the existing evidence on GMI related interventions and their effects on a range of outcomes. It entails a systematic review with the main purpose of bringing together and learning from the evidence on the broader effects. It focusses on the effects of the original programmes in Canada and the United States during the 1970s and 1980s and of the more recent programmes not only in the US and Canada, but also in the European context from the 1990s on. Compared to previous review studies in the field (Gibson, Hearty & Craig, 2020; de Paz-Báñaz et al., 2020), it advances on viewing the broader intended and unintended effects of GMI schemes including recently published studies in the European context. Due to the increasing number of studies on UBI-like programmes, the focus is on RCT field experiments and simulation studies of UBI-PI while leaving out conditional cash transfer programs (CCT) and laboratory experiments. All programmes aim at providing a GMI either for the population at large or for specific groups (such as unemployed). The paper discusses three main issues.

First, this paper discusses which variants in GMI support programmes exist. Second, it brings together the available evidence on intended and unintended micro- and macro-economic effects of these alternative income support programmes. These include labour market related outcomes such as labour supply, non-employment activities including schooling and volunteering activities, health related outcomes, as well as a range of social outcomes including health and subjective wellbeing, but also social participation, marital stability, self-efficacy, social and institutional trust and crime. Finally, this study discusses the existing gaps in knowledge and the need for further scrutiny in future research but also the policy lessons to be learned from the evidence presented. From the results lessons are also drawn on the way guaranteed minimum income programmes can potentially mitigate the negative impact of technological change on employment and social inequalities.

2. Conceptual framework

Below we briefly sketch our conceptual framework against the background of which the review study is conducted. The current debate on the consequences of technical change for welfare state policies constitutes the underlying reason for defining a workpackage on alternative policy designs or scenarios. Therefore, first, the debate on UBI-like reforms is briefly discussed. Each of the micro and macro-economic studies on UBI-like reforms reviewed here uses its own theoretical framework, that is different dependent on the discipline (economics, psychology or sociology), the empirical model (static or dynamic behavioural), the method (simulations, quasi-experiments, field experiments) and the studied outcomes (labour supply, poverty, health, wellbeing, crime). Therefore, the theoretical and methodological underpinnings of each study are briefly dealt with in each of the reviews and will not be part of this section. However, the theoretical frameworks in the current UBI-debate seem to have focussed on some specific and relatively new strands in the literature providing ideas on which some of the recent European experiments were build. These will therefore be briefly discussed here.

The current debate about these alternative systems stems from concerns about the income and employment prospects of people with insufficient or inadequate skills caused by the alleged impact of automation and technological progress on employment (Hogarth, 2017; Smith & Anderson, 2017). Many routine tasks can now be automated and performed by robots or digital and computer-aided technologies such as AI-systems (Brynjolfsson & McAfee, 2014; Frey & Osborne, 2017). Consequently, a considerable share of jobs and tasks within jobs are at risk of being fully or partly automated (Frey & Osborne, 2017; Nedelkoska & Quintini, 2018). At the same time, new jobs and new tasks requiring new skills are created for which people with strong skill-deficits are not equipped or for which they cannot always be adequately trained. In some of their sketched automation scenarios, Levels, Somers and Fregin (2019) propose that a GMI in the form of UBI, NIT or PI might offer at least partly a solution to mitigate the adverse income effects of automation-induced job losses or the downgrading to lower level jobs (Pohlig, 2021). The current debate on welfare state reform stems partly also from current challenges dealing with increasing income and wealth inequality, rising poverty, and the growing unbalance between time spent on work and on family and leisure (Bregman, 2017).

Advocates of UBI also posit that GMI programmes might provide an answer to the rise of in-work poverty caused by the alleged dualization and polarization of the labour market. A substantial share of notably low skilled workers in many European countries work in unstable contingent jobs for too short hours at rather low wages to be able to earn a living at subsistence level. A GMI would prevent them from falling in poverty. According to these proponents, UBI/NIT systems are also simpler to operate and require less bureaucracy notably when the guaranteed income is paid out in the form of a negative income tax (NIT). The benchmark are the current workfare systems which are rather strict with respect to income and job search requirements (i.e. Workfare). By operating this way, they put high levels of stress on people's lives and mindset. This creates unintended behavioural effects reducing people's self-regulation capacities

such as those associated with job search (cf. Mani et al. 2013; Mullainathan & Shafir, 2013). Reversely, the avoidance of stress by putting more trust in people's self-regulation capacities through more relaxed and tailored support regimes might generate feelings of positive reciprocity with positive spin-offs for job search and reintegration outcomes. Findings from experimental economics indeed shows that, in exchange for the conveyed trust, people are extra motivated and reward the trustor with extra effort and performance (Bohnet et al. 2001). A GMI implies the relaxation of job search requirements. A more relaxed support regime also allow beneficiaries more time to wait for the best job match and is therefore likely to reduce the odds of short-term employment but instead to increase those of long-term employment (Sinn, 1994). Moreover, most welfare and unemployment programmes in place have high benefit withdrawal rates for extra earnings which are close to 100% and therewith creating high marginal tax rates on working while also discouraging individuals to increase working hours. Evaluations of RCT experiments and simulation studies with alternative forms of a GMI show that earnings release schemes, tax allowances or work subsidies and work bonuses on top of people's benefits might create stronger, compared to basic income, work incentives and therewith also reduce the costs of existing social security systems. These GMI systems might also have the advantage to reduce stigmatization and non take-up issues associated with the strict eligibility requirements of current welfare state benefits. This in combination with selective hiring practices of employers might explain why many people have reduced access to jobs or why beneficiaries do not claim the social benefits to which they are entitled.

However, opponents of a GMI, especially in the form of a UBI system, argue that these systems tend to create strong work disincentives and increase welfare dependency because people stay longer on benefits. They also argue that a full UBI at subsistence level leads to high costs and income taxes for funding, making it unaffordable, while in the end it might also not reduce poverty notably for the poorest. A flat-rate UBI payment also raise fairness concerns because people with specific needs such as the disabled or the very sick would not be served, as the benefits would not be targeted to their needs.

The discussions between proponents and opponents about the desirability and feasibility of a GMI sometimes lack empirical basis. Results of (weakly) conditional and unconditional cash transfer programmes are already reviewed in quite a number of review studies although mostly in developing or low and middle income countries. For conditional cash transfers in developing countries we refer to Fiszbein and Schady (2009) and Cecchini and Madariaga (2011), and more recently for 30 low and middle income countries also to Bastagli et al. (2019). A recently published scoping review on public health, employment, education and social effects in the Lancet in 2019 (Gibson et al. 2020) reviewed the evidence in middle and high income countries and from laboratory experiments. All programmes reviewed here have in common that they aim to provide a GMI either for the population at large or for specific groups (such as unemployed).

The remainder of this paper is structured as follows. Section 2 describes the methodology of this systematic review. In Section 3, we describe the results. We start with a synthesis of the results from simulation studies (Section 3.1), and subsequently discuss the results from field- (Section 3.2.1) as well as

basic income dividend experiments (Section 3.2.2). Section 4 concludes and discusses the policy implications of our findings.

3. Method

For our systematic review, we define a set of inclusion criteria to narrow the extensive body of research down to a manageable set of studies for review. All programmes reviewed aim at providing a GMI either for the population at large or for specific groups (such as the unemployed or low-income groups). Following the definition of an Unconditional Basic Income of van Parijs (2004), the definition used by the Basic Income Earth Network (BIEN, 2016) reads as follows: 1) it is paid periodically (e.g. monthly), and not as a one-time payment, 2) it is paid in cash or in appropriate medium of exchange, allowing the recipient to decide what they spend it on. It is not, therefore, paid in kind (such as food or services) or in vouchers dedicated to a specific use, 3) it is paid to individuals and not, for example, to households, 4) it is universal, i.e. paid to all members of a community without means test, and 5) it is unconditional, i.e. it is paid without requirements to work or to demonstrate willingness to work. The definition refers to a full basic income but also to partial basic income schemes. As we will show in Section 3, there is a large variety in the type of basic income related experiments that have been conducted or are currently being implemented and studied. The experiments with a GMI differ along a number of dimensions, such as the amount of the income grant received and whether it sufficient to cover people's basic needs, the eligibility requirements with respect to income and work, the way it is funded by additional taxes or by reducing other benefits, whether it is conditional upon the recipients' (job search) behavior, whether it reduces other income transfers that might go along with it and at what (marginal effective tax rate) rate the benefit is withdrawn from extra earnings. The 'full basic income' is a fixed amount paid out at regular occasions and high enough to let people live in subsistence security and out of poverty and to permit every individual to take part in society. The amount that is associated with a 'partial basic income' can be set at any level but is generally too low for the average citizen to make ends meet. Nevertheless, there is no commonly accepted definition of what 'full' and 'partial' BI means with respect to its level.

There are no or only very few GMI programmes that meet all of the aforementioned criteria for a UBI. We therefore sought evidence from studies or interventions that meet some but not all of the criteria. We describe these as basic income-related interventions to clarify that they do not meet all the criteria, and to avoid the conceptual confusion that arises when effects of basic income are extrapolated from interventions which are not fully representative of a basic income scheme. The freedom to choose whether to engage in paid employment or not is arguably the key feature of an unconditional basic income. Therefore, intervention studies were included that study programmes providing regular (periodic), unconditional, cash payments to individuals or households without being necessarily universal. To gain insight into the potential effects of other features of UBI-like interventions, we included interventions that were in one way or another aimed at providing a guaranteed minimum or basic income, but which would allow people to cover their mimimum needs and to live more or less in subsistence security. We therefore

also included studies of GMI programmes providing conditional transfers on top of the regular cash benefits. Studies on interventions that provided one time-off lump-sum cash payments were excluded. Moreover, regular cash benefit transfers as part of the existing social protection systems which are strictly conditional on labour market participation, health service usage, educational attendance, or other behavioural requirements are excluded as they are strictly conditional and not aimed at providing a GMI.

With respect to the outcome measures, we view the broader (un)intended) micro- and macro-economic effects on employment (including flex work and entrepreneurship) and non-employment outcomes (including education and volunteering work), but also on income, (mental) health, subjective wellbeing and related social outcomes (trust, social participation, substance abuse, crime etc.).

The population of interest is not only the general population, but also low income groups, or unemployed individuals. Hence, social assistance programmes targeted at people on low incomes were included. Targeted interventions to very specific vulnerable groups, including children, elderly people, widows, orphans, disabled people, and people suffering from particular diseases were excluded. Mainly experiments and policy interventions that took place in high income countries were considered as were studies from low- and middle income countries if they dealt with experiments with all characteristics of a full basic income. Given that there are no high income countries that have experimented with a full universal basic income, the review provides only insights into the potential effects of some of the features of a full basic income which are also tested in high income countries.

Moreover, this study seeks to include the available causal evidence on the effects of basic income related experiments. Therefore, we include studies that applied (clustered) randomised controlled trials (RCTs) or quasi-experimental techniques, such as difference-in-differences, propensity score matching, synthetic control, instrumental variable, regression discontinuity designs, but also panel data analysis. We also reviewed descriptive analyses if no other types of analyses were available for a specific experiment. Moreover, we also include simulation studies to shed light on the micro -and macro-economic effects of a synthetic basic income policy reform.

Finally, we include articles from peer-reviewed journals, but also working papers and scientific reports. We include the latter two types of studies as a number of basic income experiments have recently been conducted - some which have not been completed yet - that provide potentially interesting insights in the effectiveness of these programmes.

We performed a computerized systematic search using a wide range of search terms or keywords, namely: 'Basic income', 'Citizens income', 'Negative income tax', 'Cash transfer NOT conditional', 'Participation income', 'Job guarantee program*', 'Guaranteed income', 'Income guarantee', 'Guaranteed minimum income', 'Guaranteed annual income', 'Universal income guarantee', 'Income maintenance program*', 'Universal allowance', 'Social assistance program*', 'Income support program*', 'Income support scheme', 'Individual placement support' or 'Employment support', combined with 'Experiment*', 'Impact', 'Evaluation', 'Treatment', 'Implement*', or 'Intervention' (see Appendix A for the exact

combination of search terms). The search was conducted in the following electronic databases: Web of Science and Econlit[1].

Figure 1 provides an overview of the selection process of relevant studies. Web of Science initially provided 552 studies that were published between 1 January 1988 and 21 of April 2021. Econlit provides us with 72 potential records that were published before 22 April 2021. Based on this first assessment, we kept 31 relevant articles. After reading the title, abstract, and if necessary full text, we kept 24 relevant records that meet our inclusion and exclusion criteria.

In addition to our systematic search using basic-income related search terms, we conducted additional searches in Econlit for every experiment or basic-income related policy we became aware of (see Appendix A for the exact search terms used). These additional searches resulted in 92 additional potentially relevant records. After screening these articles against our inclusion and exclusion criteria, we included 16 additional studies. The last step of our selection procedure was to screen the websites of recently conducted or ongoing experiments that we became aware of to search for evidence from studies that have not been peer-reviewed yet. This final search led to the inclusion of eight additional studies on six recently conducted basic income related experiments. In total, 48 studies are selected for our systematic review.

[Figure 1 around here]

The 48 papers that meet our inclusion criteria were coded along a variety of dimensions. First, we recorded information on the intervention: a short description of what the intervention entails, which of the atypical characteristics of a full-basic income the intervention has, the level of treatment (e.g. household or individual), the type of treatment, the eligibility requirements, the guarantee level, and whether the intervention was combined with other simultaneous treatments (e.g. job seeking support, training etc.). Furthermore, we recorded the location(s) of the interventions, the period in which the experiment took place, the period studied, the population studied, and the outcome variable(s) of interest. Finally, the data that were extracted also included the study design, the data source, the sample size, the year of publication, and the findings.

4. Results

Below, we present the results of the studies starting with simulation studies in Section 3.1 in twelve countries. In Section 3.2.1, field experiments in five high-income (Canada, US, Finland, Netherlands, Spain) and three (lowe) middle-income countries (India, Iran and Kenya) are discussed. Then in 3.2.2, we finish with three very special cases of a partial UBI dividend in the US and Iran.

[1] The search and selection of the various studies was mainly conducted by the first author whereas the reviews were done by all authors jointly. The first author also screened all papers and assessed whether they meet the jointly decided inclusion and exclusion criteria. When it was not immediately clear whether or not to include the study, the researchers decided jointly. The studies were first reviewed by either one of the three authors, which review was subsequently checked by one of the others.

4.1 Evidence from simulation studies

This section reviews 17 simulation studies on participation (PI) and conditional (CBI) and unconditional basic income (UBI) like interventions and programmes including negative income tax (NIT), analyzing the effects on a mixture of micro-economic and macro-economic outcome indicators in twelve countries. Micro-economic outcome indicators entail a mix of income, payroll tax, employment and non-employment indicators: net disposable income, poverty head count, income inequality, participation tax rate (when working), marginal effective tax rate on earnings, labour supply or working hours, employment participation and family or marital stability and crime rates by crime subcategory. In micro-macro-economic simulation studies, micro-economic indicators were examined jointly with some macro-economic indicators, such as total tax revenues, programme costs or budget expenditures, GDP or consumption. The seventeen studies cover 8 UBI, 4 NIT, 1 PI and 4 other conditional BI interventions and programmes or combinations of them. The characteristics of these studies are presented in Table 1.

[About here: Table 1 Characteristics of 17 simulation studies on UBI-NIT and PI-CBI schemes]

Ten out of these seventeen simulation studies used a static simulation model to view the effects of a particular UBI, NIT or PI scheme on poverty, income inequality, costs or crime. The estimation of labour supply effects can be rather complicated because it requires the modelling of labour market behaviour that is determined by varying marginal effective tax rates and budget constraints on hours worked at different levels of income notably in progressive tax systems. Six studies estimated the employment effects by implementing such a behavioral labour supply model. One study included labour supply behavioural effects by using pre-fixed income and wage elasticities. Only three of these seven studies view the effects on labour supply and employment jointly with the effects on poverty and inequality. The other four studies examined the sole labour supply or work incentives and employment effects of the simulated PI/UBI like interventions and programmes. In the rest of this section, we present the main findings in four tables representing the effects of each of the four intervention or reform types UBI, NIT, PI and CBI. First, we present the findings of the 8 UBI simulations in Table 2.

4.1.1 Eight UBI reform simulations

UBI reform in Columbia to mitigate adverse COVID-19 effects

The first simulation study by Cuesta and Pico (2020) is rather unique because it analyses the effects on *income inequality, poverty and unemployment* of a fiscal neutral Universal Basic Income programme in Colombia compared to a variety of recently launched policies to mitigate the adverse effects of the COVID-19 pandemic. The researchers use a static microsimulation model to calculate the effects on poverty and poverty reduction in scenarios with and without COVID-19, and with gradual or immediate economic recovery thereafter. A three months lockdown period is presumed in which people are expected to have

lost on average 50% to 100% of their income that is compensated by additional social transfers and/or additional recovery benefits. The UBI level is set equal to the average level of the extra social transfers and/or the economic recovery benefits people receive to mitigate the income loss due to COVID-19. Colombia is a relatively poor country with high poverty rates according to the national poverty line where the ÙBI level is set at a level of almost 62 dollar per month which is just below the national poverty threshold of 69 dollars per month. The effect of UBI assuming 50% income loss and gradual recovery from the pandemic crisis on the poverty rate is however surprisingly large with poverty being reduced from 26.5% in the baseline scenario to less than 23%, that is -3.57pp.

UBI reform in Germany

The second study by Schubert (2018) for Germany simulated the *labour supply effects* of two UBI scenarios, scenario 1 with 800 euros per month and a second scenario with 1000 euros per month including 200 euros for health care premiums using a micro-macro behavioural simulation model. The UBI was paid out in the form of a negative income tax (tax allowance). The level of the average flat-rate tax to finance the UBI programme appears to be 50% in scenario 1 and 80% in scenario 2. People with gross incomes above the respective tax allowance threshold face a marginal tax rate of 61.3% in scenario 1 and 34% in scenario 2. According to the general equilibrium model that takes account of the labour supply reactions to the policy measure, they find overall negative labour supply effects amounting to -0.95% in scenario 1 and -1.2% in scenario 2. For the low skilled the labour supply effects were somewhat higher negative (-2.44%) than for the high skilled (-1.02%).

The negative effects on the labour participation rates were higher with -2.8% in scenario 1 and -2.83% in scenario 2. Effects on GDP and national income turned out to be positive in scenario 1 (respectively 0.04% and 0.07%) and negative in scenario 2 (respectively -0.87% and -0.16%). Effects on consumption were positive in both scenarios (0.16%; 0.30%). The UBI causes an income effect which is positive for married men but (strongly) negative for married women, whereas the high marginal tax rates create negative substitution effects notably in scenario 2 and for people above the tax allowance threshold. Overall, they conclude that based on these findings there are few arguments to propose the implementation of a basic income scheme in Germany.

Full UBI in Romania

The third study of Stroe, Dragoin and Cambir (2018) simulates the effect on poverty of a full UBI in Romania that was actually implemented in Romania according to the minimum income Law in 2002. It also foresaw an additional allowance of 15% when one member of the family is working and receive earnings. In 2016 it was followed by the minimum inclusion income law that came only into force in 2019 that aimed at creating stronger work incentives by allowing people to retain 50% of their earnings up to a certain maximum amount per month. The authors used a static simulation model without estimation of labour supply effects and the Family Budget Survey data of 2014 as their data source Two UBI scenarios were

distinguished: a so-called pessimistic and optimistic scenario, with claw-back rates at 35% and 65% of additional earnings of the family beyond which threshold, earnings are withdrawn from the benefit. Effects are calculated on the poverty head count rate where poverty is defined according to the 60% median equivalent income threshold. Eurostat figures for 2014 shows the poverty rate to be 25.1% according to the 60% threshold. The found effects are modest with the poverty rate reduced with 1.0 pp in scenario 1 (35%) and 1.6 pp in scenario 2 (50%).

Full UBI in Turkey

The fourth study by Yakut-Cakar, Erus and Adaman (2012) uses a static simulation model as well to estimate the effects on poverty, inequality and government costs of a UBI in Turkey that in 2012 had no minimum income scheme. They used EU-SILC household data for Turkey in 2007 to run the simulations. The UBI guarantee is set at 40% of median equivalent household income (457 euros pro month). The effects on poverty rate reduction are calculated for five thresholds, three income thresholds set at 40%, 50% and 60% of median equivalent income and two poverty thresholds, the insecure poor (lack of social security rights) and the formally poor (green card for free health care holders). Corresponding poverty rates were 10.4%, 16.57% and 23.51% for the income thresholds and 15.5% and 9.5% respectively for the poverty thresholds. Effects are calculated for two variants, the 40% UBI-guarantee and the top-up variant where income is top-upped to the relevant poverty line threshold of 40%, 50% or 60%. The effects appear substantial in both variants for the five groups. Even with the cheapest UBI-guarantee set at the 40% level (457 euro), effects range from -10.4%, -10,1% to -8.61% for the three income groups and -8.36% and –5.76% for the two poverty groups. With the top-up variant to the 40% threshold, poverty is reduced with 10.4% (to 0%) in the 40% income group and with 11% and 9% in the other income groups. In the top-up variant to the 60% poverty threshold, poverty is reduced to 0% in all five income groups. This goes not without a price; the effects on government expenditures are substantial and range from +2.72% of GDP or 403 million Turkish lire (the lowest threshold 5), to +4.90% or 718 million Turkish lire (UBI at 40%) and +11.46% or 1,701 million Turkish lire (UBI at 60%). Inequality measured by Gini (41.06% in 2014) is reduced with only 0.71pp with UBI at 40% level but with a substantial -5.1pp with the most generous UBI at 60% level. Behavioural labour supply effects due to increases in marginal tax rates are not taking account of in these calculations.

Partial UBI and NIT in Finland

The fifth study by Kangas et al. (2016) concerns the 2016 ex ante static simulations of the effects of the proposed Finnish unconditional basic income experiment that was actually implemented between 2017 and 2019 among 2000 unemployed. The paper also estimates the effects when the same level of UBI (550 or 750 euros pro month) is paid out in the form of a negative income tax (NIT). Using survey and administrative data for 2016 their study focuses on the simulated effects on work incentives or tax rates (marginal tax rates on extra earnings and participation tax rates when working), child poverty, income

inequality and government expenditures or costs. The study was restricted to effects on work incentives through taxes, because of which no (behavioural) effects on actual labour supply or employment are estimated. Three variants were considered: a full UBI of 1,000 and 1,500 euros, a partial UBI of 550 or 750 euros per month without other social insurance benefits, and a partial UBI with other social insurance benefits. At different levels of earnings and UBI thresholds, they estimated the change in tax rates in these variants compared to the current situation. The UBI was supposed to be financed by a cost-neutral flat-rate tax at 60% or 79% level. With a full basic income of 1,000 euros, the effective marginal tax rates on labour supply (extra working hours) increase in both tax regimes with higher earnings meaning that work incentives are reduced. In the full UBI scenario (1,000 euros per month) participation tax rates (when starting working) move up strongly from 51.7% to 73.4% for extra earnings up to 500 euros and to 82.9% for extra earnings up to 1,000 euros. In the partial UBI scenario of 550 or 750 euros per month and extra earnings up to 500 euros, participation tax rates for a single worker are lower than in the current legislation, moving down from 80% to 50.2% and 63.9% respectively. With low levels of extra earnings, NIT schemes also result in lower participation tax rates and with higher levels of earnings (>1,000 euros) in higher marginal tax rates than the current rate. With higher thresholds and higher extra earnings both tax rates are larger in the UBI and NIT scenarios than under the current legislation. Child poverty (13.2%) increases with 0.8pp with UBI of 550 euros but decreases with -1.5pp with UBI of 750 euros. NIT reduces child poverty with -0.3pp already at lower levels of NIT (550 euros). Inequality (Gini=26.4%) would be reduced with -0.3pp in UBI 550 scenario and -2.2pp in UBI 750 scenario. Budgetary costs are substantial especially in more generous variants amounting to 23 and 26 billion for UBI 550 and 750 respectively.

UBI reforms and perpetual growth, a theoretical Dutch study

The sixth study by two Dutch authors Normaler and Verspagen (2020) forms a special case with the theoretical simulation of two main scenarios, a *x* % capital tax that is redistributed to wage earners in the form of an unconditional basic income (UBI) and a *y* % savings rate on wages that is fully reinvested in robot technology (robotisation, machine learning, artifical intelligence) and which may create perpetual growth but with low wage income shares. Perpetual growth is a state of advanced technology as embodied in capital goods in which there is growth of per capita income without technological progress. According to such a technology scenario, future levels of income inequality will, without social policy reforms, rise unprecedently according to the authors because labour is hardly needed for production anymore. The study therefore examines the effects of the two policy responses on inequality, measured by the wage income share, and on perpetual economic growth, which are subsequently compared to view their differences. The first is a UBI scenario funded by a capital tax and the second a wage-savings scheme combined with investments in robots.

For the calculation two methods were applied, the first is a static simulation approach assuming that the savings rate is exogenous and fixed, the second is a behavioural simulation model with labour supply where it is assumed that the tax rate on capital affect the savings rate. In the 'optimal taxation'

literature it is usually considered that the capital tax rate distorts the accumulation of capital or savings implying that the funding of an UBI by a capital tax distorts economic growth. For various levels of *x* and *y* the outcomes on the income shares of wage earners and on the perpetual economic growth rate are calculated. Focusing on a 40% tax rate on capital and a 10% savings rate on wages for robot investments the effects are presented in Table X2. The 40% capital tax exerts small effects on reducing inequality or raising the wage income share in national income because it reduces perpetual economic growth strongly from 3.7% with zero capital tax to 0.5% with 40% capital tax. The wage income share went up about equal to the tax rate (from 0% to 45%). In the 10% savings scenario the effect on economic growth is small moving up a little from 1.130% to 1.135% but the wage income share went up to 65%. With 60% of savings invested in robot capital, the wage income share goes up to 95%. The wage-savings robot-investment scenario produced better results than the UBI-capital tax scenario and the author argues that one might think of a voluntary but also a forced wage savings scenario comparable to the pension schemes in many countries.

Guaranteed BI in Canada

The seventh study by Koebel and Pohler (2019) combines a guaranteed basic income (GBI) scheme at social assistance level in Canada with a 30% earnings subsidy provided in the form of a negative income tax allowance scheme with a claw-back or phase-out rate of 50% when disposable income passes the poverty line threshold at the Market Basket Measure level. The GBI is conditional on working 9-16 hours a week for people between 18 and 65. The GBI is a cost-neutral scheme by maintaining social assistance and income transfer programmes and abolishing some tax credits. The study uses a static simulation model for estimating the effects on marginal tax rates, poverty and disposable income by deciles and family type. The average poverty rate in the pre-GBI case is 7.7% which is shown to be strongly reduced with -3.10pp. Notably singles (-6.10pp), couples (-3.10pp) and two-parent families (-2.3pp) see their poverty risks reduced whereas older singles and couples see their poverty increased or slightly decreased. With respect to income the findings show that overall income increases with 1.46% but that there are large differences across family types. Non-senior singles and couples profit most whereas senior singles, single parents and senior couples see their income decline from -4.5% (senior couple) to -6.33% (senior single). Especially people in the lowest 4 deciles appear to gain in income.

UBI dividend in Alaska

The last UBI study by Dorsett (2021) deals with the Alaska Permanent Fund Dividend that is paid out every year in the form of a UBI grant since 1982 to every non-prisoned citizen in Alaska. The fund is created from the oil revenues of the state and pays out 1,000 to 3,000 US dollar every year that amounts on average to 6.28% of annual income. The study uses a static microsimulation approach to estimate the effects on crime and sub-crime rates. Two methods are used, a synthetic control case-study and a difference-in-difference approach using Structural Bayesian Time-Series (SBTS) analyses, in which treated and synthetic

controls are compared while allowing for unobserved influences to vary over time (no behavioural effects modeled). Effects are shown by crime and sub-crime rates such as on property, burglary, larceny, vehicle theft, violent, murder, rape, assault and robbery. The study found little evidence of impact on crime, not for property crime and not for the other crime categories, except for some negative (significant) effects on murder. Bayesian time series results are not very much different showing a lack of impact on property crime except in 1984 and the last two years with positive impacts. Burglary and larceny crimes show as most reliable subcategories no effect. Next, a small significant decrease is found in property crime that is larger for Alaska than for the other states, providing some reassurance that a true effect has been captured.

[About here: Table 2 Simulated micro-economic effects of UBI-like programmes]

Conclusions on UBI-like simulation studies

UBI-like simulation studies provide a mixed picture on the effects of a full or partial UBI reform dependent on the outcome measure used, whether it is a partial or full UBI, and the way the payments are funded either through payroll taxes, dividends from capital income, or through replacing other social benefits. All studies dealt with the effects on income inequality and poverty, labour supply, employment or budgetary costs. None of these UBI simulations studied the effects on (mental) health or subjective wellbeing. The effects on poverty and inequality were generally estimated with a static simulation model not taking behavioural labour supply or savings effects into account which might lead to incorrect estimates of the true effects. The findings show that the reducing effect on poverty and inequality vary a lot but can be substantial especially in the case of a universal and full UBI paid out to every citizen. This goes however with a cost, notably a full UBI leads to high budgetary costs which can only be financed through raising income tax rates. High income tax rates combined with a high level of the UBI threshold creates high participation and high marginal tax rates on participation or employment and on labour supply or working hours with disincentive effects on labour supply. Only in behavioural microsimulation approaches can these effects be accounted for. Only two studies applied such a behavioural approach and only one dealt with micro labour supply effects. The study showed that the labour supply effects for Germany are overall negative caused by a negative income effect for married women with (young) children due to the guaranteed income and a negative substitution effect notably for men due to high marginal tax rates. Overall, the labour supply and employment effects turned out negative but are not large dependent on whether a partial or full UBI scenario is considered (-1% to -3%).

4.1.2 Negative Income Tax: four NIT simulation studies

The evidence from the NIT simulation studies is presented in Table 3 below.

A theoretical British study comparing a a flat-rate tax (FRT) with a NIT regime

The first NIT study by Thompson (2012) compares the simulated outcomes of two regimes, a revenue-neutral flat-rate tax (FRT) combined with a tax allowance and a GMI combined with a universal NIT

scheme. Their simulation study is based on two theoretical models, one with the pre-tax distribution as exogenous and a partial equilibrium labour supply model with the pre-tax distribution determined endogenously by taking the behavioural effects of labour supply to tax rates into account. They calculated head count poverty rates and minimum tax rates to minimize poverty which are needed to finance the tax allowance in the FRT and NIT regimes. They compared the outcomes for mean-based and median-based relative poverty lines. They also consider the inequality among the poor population and the inequality aversion of the population by using Foster' class of inequality measures index that combines the head-count poverty rate and the poverty gap for people below the poverty line weighted with the inequality aversion level. They used for the calculations the 50% poverty line and three levels of the inequality aversion parameter (1,2,3) to simulate the poverty effects. In the endogenous model with median-based poverty lines the minimum tax rate needed to minimise poverty and fund the scheme is 14.8% in the FRT regime and 42.86% in the universal NIT regime with almost similar rates in the exogenous model. Effects are qualitatively the same under both tax systems and mean or median poverty lines in the exogenous and endogenous model. With median-based poverty lines, poverty monotonically decreases with increasing allowance in a FRT or a NIT programme. In the endogenous model due to the increase in the critical wage rate for people below the poverty threshold due to paying zero tax, labour supply increases that will decrease poverty in the endogenous model more than in the exogenous model.

UBI combined with NIT in Spain

The second NIT simulation study by Perez and Fernandez (2019) estimates the macro and micro-economic effects of a revenue-neutral full universal basic income in Spain for the year 2014 where the tax allowance is equal to the tax rate times the income exempt (5,155 euros for adults and 1,547 euros for minors in 2013). The tax allowance for people with incomes below the threshold replaces the non-contributory benefits in Spain. Three different tax rates are assumed, a flat-rate tax of 50% or a progressive tax scheme with either two tax-rates (48% and 56% for incomes >12,800) or three tax-rates (47%, 50% and 60% for incomes >21,000). The simulations are based on a static micro-macro simulation model while using data from the living conditions survey of 2014. With respect to the micro-economic effects, the study views the effects on income inequality (using Gini before and after-tax) and the redistributive effect of the programme by using the Reynolds-Smolensky's index that calculates the difference in the Gini index of income before and after tax. The inequality reducing effects are substantially reducing Gini from 33.6% in the current tax system to 24.9% in BI-model 1 (-5.7pp) and 24.1% in BI-model 3 (-6.5pp). The redistributive effects of the BI-models are more than twice as large as in the current tax system: a -16.9pp reduction of Gini after tax in model 1 compared to -8.1pp in the current tax system. With respect to poverty, the 40% and 60% of median after-tax income thresholds are used to define relative poverty. By using the FGT inequality index, three ratios are combined in one index: the head count ratio (FGT0), the poverty gap or the distance of the income to the poverty line ratio (FGT1) and the inequality among the poor ratio (FGT2). By comparing the three poverty ratios of pre-tax and post-tax income in the current tax system compared to the three BI

models one gets a picture of the reduction in poverty under the different tax regimes. Using the 60% poverty threshold it is shown that according to all three ratios, poverty is reduced more strongly comparing pre-tax and post-tax income in the BI models than in the current tax system, moving from -22% reduction in the current system to -57/8% according to the FGT0 or head count ratio, to -90/3% post-tax according to the FGT2 ratio. It means that not only poverty is reduced more in the BI-models than in the current tax system but also the inequality among the poor. It is important to note that these calculations do not take behavioural responses into account. Hence, the effects might be biased either downwards or upwards.

NIT compared to UBI and Workfare in Italy

The third NIT simulation study by Aaberge, Colombino and Strom (2004) simulates the effects of three revenue-neutral hypothetical income guarantee regimes for Italy. The three regimes are an NIT, a UBI scheme with a flat-tax and a Workfare scheme (i.e. a basic income scheme that is conditional on working a specific number of hours) compared to the 1993 tax system as the status quo. The simulations were based on survey income and wealth data on 2,160 married couples of 18-54 years old, rescaled to the entire population, of the Bank of Italy of 1993. They used a behavioural micro-simulation model while taking behavioural labour supply effects into account based on a pre-estimated household labour supply model. They also estimated the social welfare gains or losses in each income decile by estimating utility functions (functions of equivalent income, taxes, and male and female labour supply) and social welfare functions to be able to compare the welfare gains of households in the different regimes. The social welfare function is a weighted function of equivalent income compared to a reference household (median equivalent income household). The welfare function represented by the cumulative equivalent income distribution by decile is then shaped by the inequality measure (Gini) and the inequality aversion parameter b. The authors argue that because budget-neutral taxes are assumed (i.e. higher marginal tax rates but also showing lower average tax rates), effects of higher marginal tax rates on labour supply are therefore not necessarily negative while it depends on simultaneous labour supply choices and working hours constraints notably for females. Likely for that reason, female labour supply shows a small reduction for NIT, a small increase for Workfare and a slightly larger increase for the flat-rate tax compared to the 1993 tax regime. However, in the two lowest deciles female labour supply was positive in all three policy reforms, showing no disincentive or poverty trap effect. The authors suggest that positive females' cross elasticities for labour supply for men's wages (0.82) in couples with working husband might be held responsible. They conclude that labour supply responses are strongest in the lower and average deciles and not in the highest deciles. The social welfare gains seem to be positive compared to the 1993 tax rules and there are always more winners than losers. The mean social welfare gain is largest under a flat-tax, 2.1%, second largest under Workfare, 1.1% and smallest under NIT, 0.8%. Taking the social welfare gain by deciles into account too, for Gini, with the inequality aversion parameter b=2 (based on Bonferroni welfare function), the welfare gains are then largest under Workfare and NIT (1.6%-1.5%) and smallest under flat-tax (0.9%).

NIT reform in Switzerland

The fourth and last NIT simulation study by Abul Naga and Kolodziejczyk (2008) for Switzerland simulates the effects of seven different policy reforms distinguished according to whether there is a means-test involved and/or a work requirement. These are a universal unconditional and revenue-neutral NIT scheme at 50% or 100% level (NIT50, NIT100) financed through a flat-rate tax, a guaranteed minimum at subsistence expenditure level with an integrated (cantonal plus federal) or simpler federal flat-tax system of 62% for incomes above the subsistence expenditure threshold, a guaranteed participation income at 50% level (PI50) conditional on meeting the work hours requirement of each individual in the household, a means-tested (100% marginal tax rate) income support or social assistance scheme top-upped to 100% and a means-tested conditional on working hours earned income tax credit topping up to 100%. They used a tax-benefit simulation model based on administrative tax-benefit data and a behavioural labour supply simulation model based on the Swiss Expenditure and Income survey of 1998 consisting of 5,434 households. The study simulated the effects on employment, labour supply, disposable income, social welfare, income inequality and poverty. For inequality the Gini index was used plus Atkinson's and Foster's inequality index plus the ordinal inequality rankings of the policy reforms based on analyzing overlapping Lorentz curves across the reforms. For poverty, likewise, these Lorentz curves-based ordinal ranking approach was used viewing the so-called poverty deficit curves for the various reforms plus a poverty head count ratio where the poverty line is set at the subsistence expenditure level.

Results on employment show that due to the required high marginal tax rates to sustain the revenue-neutral reforms, participation rates and labour supply tend to decline. For NIT50 participation and labour supply reduces with 3.5% and -1.9 hours overall but with -5.7% and -3.1 hours for married females compared to -1.2% and -0.6 hours for married males. For fulltime working married females the reduction is -9.4 hours. For PI50 the figures are more positive with participation increasing with 3.5% and labour supply with a small 0.2 hours, but still reducing labour supply with -3.2 hours for fulltime working married-females. For income, the simulations show that NIT100 and NIT50 appear to reduce income with -9.3% and -5.8%, respectively, whereas PI50 showed a small positive effect (+0.6%). On social welfare improvement only PI50 outperforms the base scenario. On poverty, the NIT100 and income support scenarios yield the best results, reducing poverty (basically from 3.3% in the baseline to 0%). The third best performance is obtained by NIT50 (1.1%) and fourth best by PI50 (1.7%). However, the authors voice a warning that these poverty figures notably for income support mask the poverty trap issue for incomes just above the threshold raising poverty from 0% to 5% because people cease working due to the 100% marginal tax rate below the threshold. For inequality, the Gini measure shows that NIT100 performs best in reducing inequality with -36% and NIT50 second best with -24%. Third best performs PI50 with reducing inequality with -10%. For the inequality indices such as Atkinson's measure, inequality drops with -61.8% for NIT100 and with -43.7% for NIT50. PI50 is again third best with a reduction of -14.8%. The ordinal Lorentz-curves based rankings yield basically similar results for poverty and inequality.

[About here: Table 3 Simulated micro-economic effects of NIT-like programmes]

Conclusions on the NIT simulation studies

The NIT simulations results are very similar to the results found for the UBI simulations with respect to finding positive effects for poverty and inequality reduction and small negative effects on employment and labour supply for men but more substantial negative effects for married women. But again, the evidence is mixed with regard to labour supply even though now three of the four studies used a behavioural labour supply model. Two of these three studies show similar results confirming the evidence found with the UBI studies that in the case of a revenue-neutral way of financing the NIT-reforms, high marginal tax rates on hours work and earnings are needed ranging from 50% with NIT50 to more than 60% with NIT100, creating strong disincentive effects on labour supply which are strongest in the latter case. However, in one study the authors found positive labour supply effects notably for females in the lowest deciles for which they also gave an explanation. They refer to using a household instead of an individual labour supply model in which the labour supply decisions of the spouse and partner are simultaneously modelled and cross-elasticities of males and females are considered affecting each other labour supply decisions. In former approaches these cross-wage elasticities are not taken into account. In their view budget-neutral financing indeed will lead to higher marginal tax rates with NIT reforms but also to lower average tax rates, implying that effects on labour supply are therefore not necessarily negative while it depends on simultaneous labour supply choices in the household (spouse and partner) and working hours constraints notably for females. Their results on social welfare gains are also different compared to the other two studies because they find for NIT as well as for Workfare and flat-tax reforms positive welfare gains whereas the other two studies found negative effects on income and social welfare notably for NIT at 100% and NIT at 50% level. Again, the former study used different specifications of the social welfare function in which not only equivalent income by income decile and inequality aversion but also the labour supply decisions of both partners at different levels of the income distribution are included whereas in the other two studies only equivalent income and inequality aversion are the main parameters of the welfare function.

The effects on poverty and inequality reduction are however much more similar showing that the more generous the minimum income guarantee is (NIT50; NIT100), the more poverty and inequality is reduced. Generous income support or PI at 50% are next best in reducing poverty and inequality. Taking the endogenous labour supply effect on board, the first study shows that it might even further reduce poverty and inequality because of the lower marginal tax rates in the lower income deciles with notably NIT reforms. The lower marginal effective tax rates (METR) at low incomes result in higher wages and hence increased employment and labour supply and therewith increasing income and reducing poverty. Workfare or conditional income support programmes on the other hand seem to reduce work incentives because of the 100% marginal tax rate below the poverty threshold. Various NIT studies compared the findings with the findings of other reforms such as basic income and participation income. The PI reform providing an income guarantee at 50% level performs worse with regard to poverty and inequality reduction

compared to NIT100 or NIT50 but better in terms of social welfare improvement (income) and labour supply, showing lower negative effects. Below, we discuss the found simulation studies on PI and basic income.

4.1.3 Five participatin income (PI) and conditional basic income (BI) simulation studies

In two of the three NIT studies a comparison was made with a Participation Income scheme, which is a GMI but conditional on meeting income or work requirements. We found only one PI study but four other conditional basic income simulation studies.

PI reform in the UK compared to two other policy scenarios

The first study by Atkinson et al. (2017) simulates the effects of three budget-neutral policy reform scenarios of which *Scenario 1* is a progressive tax (with three rates: 25%, 35-55% and 65%) combined with universal child benefits (18% of median equivalent household income or 0.3 equivalent factor for each child) and more generous social insurance benefits (raising unemployment with 50% and pension benefits with 25%). *Scenario 2* is a combination of a PI scheme with a progressive-tax and universal child allowances. Finally, *Scenario 3* simulated a rise in the minimum wage up to the Living Wage level in 2014 for the UK (48% of the median minimum wage of fulltime employees). This third scenario is taken alone or together with the two reform packages. The baseline is the current social security system in which over time means tested minimum benefits increasingly have displaced social insurance schemes providing a replacement income notably in the UK for older people. The policy reforms are aimed at providing an alternative to current policies. The authors used the static simulation model of EUROMOD to simulate the effects on incomes by deciles and on inequality and poverty. They simulated equivalent household incomes for 2014 using data from the Family Resources Survey 2009/10 containing 25,200 households and 57,380 persons. For poverty, the 60% of mean equivalent household income threshold is applied to calculate the poverty head count and the poverty gap. For inequality, measures as Gini, Atkinson's inequality measure (inequality aversion at 0.5), the mean log deviation (MLD) and Theil's entropy measure were used. The findings show for both scenarios and according to all measures, that inequality and poverty was substantially reduced, but more in the second PI than in the first SI scenario. Also, the poverty gap was reduced with -20% and -60% respectively. The children's poverty gap was even reduced with -80% in the PI scenario. With respect to income gains and losses by decile and according to both PI and SI reforms, deciles 1-7 gain, decile 8 shows stability and deciles 9 and 10 loose. The poorest decile gains with 16% in the SI and with 50% in the PI scenario. More than three quarters of the oldest households over 65 gain in the SI, but only one in ten in the PI scenario. The impact of the third minimum wage reform on inequality and poverty appeared to be modest, it affects 22% of the households who experience some income gain while it reduces Gini and the poverty headcount with less than 0.5pp compared to the baseline. The reason is that low wages are spread over a large part of the household income distribution and that part of the income gain due to raising the minimum wage is withdrawn from the means-tested minimum benefits. Because women are more likely to

be in low pay, the reduction is even less for them. The effects of the SI and PI reforms on taxes and therewith on work incentives are complicated, they seem to raise the marginal effective tax rates and therewith reducing incentives notably in the PI reform, whereas the participation tax rates in the PI reform are less affected. By comparing the SI and PI reforms, some insight is obtained on the different effects of the reforms on inequality and poverty but not on the effects of each of the different components (child benefits, SI benefit increase, PI and tax increase), of the reforms separately.

Conditional universal basic income schemes in Australia

In a second study by Spies-Butcher, Philips and Henderson (2020), the fiscal and distributional effects of two scenario-options of an individual, conditional, affluence tested universal basic income scheme have been simulated using a static macro-economic simulation model for Australia. Because both schemes are conditional on the level of earnings or market income and total income, they are similar to a PI-model. The difference between the two options is only its level and its effect on the tax rate (current 2017-2018, 24.7%; +5.5% versus +6.7% of GDP) to fiscally-neutral fund the two simulated universal basic income schemes of 14,647 and 18,253 AUSD a year.

The BI schemes cover the entire adult population up to pension age. The first BI scheme, is based on the level of the main unemployment benefit in 2018, called NewStart, the second on Newstart plus 75 AUSD per week. Both BI-schemes implies a reduction of the benefit withdrawal rate notably at lower incomes. With market incomes up to 10,000 AUSD, 30% of market incomes are withdrawn from the benefit, whereas benefits are withdrawn to zero approximately at an income of 180,000 AUSD (the top marginal tax threshold with a 45% tax rate). The authors make us of of a tax-benefit microsimulation model of ANU-CSRM (the so-called PolicyMod tax-transfer model of 2019) using data from the Survey of Income and Housing for 2015-2016 on 27,000 people in 14,000 households. The opted model shows similarities with NIT because it is income-tested and its level is reduced with income. The findings show that viewing the fiscal-neutral funded BI-schemes, equivalent incomes in the first quintile profit most with 30% in BI-scheme 1 and 42% in BI-scheme 2. In BI scheme one, only the fifth quintile show negative effects on equivalent income of -8.9% whereas in BI scheme two, the fourth quintile lost a little -0.6% whereas the fifth quintile lost is -11.6%. The poverty head count is reduced from 11% to 8.7% and 9.1% in BI-scheme 1 and 2 respectively whereas Gini inequality is reduced from 33.9% to 28.9% (-5.0pp) and 27.4 (-6.5pp) in both BI-schemes.

Conditional basic income in Canada (Quebec)

The third study by Clavet, Duclos and Lacroix (2013) simulates the effects of a conditional BI-scheme aimed at the prevention of poverty and exclusion in the state of Quebec in Canada for 2004. In Quebec, already in 2002, a unique law to combat poverty and social exclusion was enacted, showing that poverty and social exclusion was top priority of Canadian social policies. In 2009, a special committee on the combat against poverty and social exclusion (CCLP), recommended to set the universal benefit at 80% of the

Market Basket Measure (the income equivalent of a minimum basket of consumption goods) for municipalities with less than 30,0000 residents and to 80-100% of the MBM threshold for workers working less than or at least 16 hours a week. The study simulated the effects of the 80%-100% baseline and of 3 variants: a less generous variant than 100% MBM by changing the cut-off from 16 to 30 hours, a more generous variant raising the financial support from 80% to 100% of the MBM without a work requirement and a third variant providing a work bonus of three Canadian dollar subsidy per hour for finding a job and working at least 30 hours a week. The study uses the so-called Canada Social Policy Simulation Dataset 2004 (SPSD) based on some administrative data and two survey data sources, the SLID-survey of labour and income dynamics- panel data, and the survey of household spending. Asset data (housing) come from the survey of financial security and from census data. They applied a behavioural micro-simulation model to study the income and ex-ante labour supply effects of the three BI-models based on estimating a structural labour supply model with budget constraints. For calculating taxes and transfers, they used the Canadian Tax and Credit Simulator (CTaCS) developed by Milligan (2008). The 2004 survey data consisted of 3031 singles 18-65 years of age, while couples were omitted from the analysis. However, only 1.2% of couples would qualify for the BI-schemes against 12-14% for singles. The simulations show substantial costs of the MBM measures (the baseline and variant 1 and 2) due to lower income taxes received and more transfers to be paid out. Total costs for the baseline measure are estimated as 480 million dollars when no behavioural effects are taken into account and more than 2 billion dollars (1,8 billion for Quebec) or 2,870 (2,429 for Quebec) Canadian dollars per capita (Quebec plus Federal State) when behavioural labour supply effects are taken account of. When the GMI would be 100% of MBM, the costs increase to 3.7 billion Canadian dollars or 14,377 dollars per recipient. The three-dollar wage subsidy on the other hand show the lowest net costs (3,844 dollar per recipient) because it increases federal tax revenues and lowers transfer payments. Also, the labour supply effects calculated from a structural labour supply model with behavioural effects turn out to be substantially negative. For the CCLP recommendation of 100% MBM for people working at least 16 hours, the results for single men and single women show that non-participation increases with 13.8pp and 12.6pp respectively, while labour supply reduces with -12.4pp and -10.7pp in the 36-44 hours bracket. The reduction in supply is stronger at lower wage-levels and mostly caused by stopping working (extensive margin) instead of by reducing hours (intensive margin). The CCLP variant implies that the labour supply at the intensive margin (working hours) is reduced with -8% and -10% for single males and females in the first quartile, but with only -0.36% and -0.73% in the fourth quartile. The effects at the extensive margin (employment participation) are much stronger with -27% and -30% for males and females in the lowest quartile and -7% and -10% in the highest quartile. The less generous 80-100% with 30 hours cut-off variant has similar outcomes: for single men and single women non-participation increases with 14.2pp and 13.0pp, whereas labour supply decreases with -11.9pp for men, and -10.4pp for women (36-44hrs). Finally, the third three-dollar wage subsidy variant, shows much more positive results, decreasing non-participation with -3.3pp for single men and -4.0pp for single women and single mothers. On the other hand, labour supply remains the same for both, single men and women (0 pp change) in the 20-28 hours

bracket while it increases with 2.4pp and 3.12pp respectively in the 36-44 hours bracket. The authors therefore argue in favour of this wage subsidy variant because it is much cheaper and increases labour supply instead of reducing it substantially as with the universal guaranteed basic income.

A conditional basic income scheme (RMIG) in Portugal

Switching to European conditional basic income simulation studies, the first one is a simulation study on the effects of a GMI policy implemented in 1997 in Portugal on income inequality and poverty. The effects are however calculated with and without inclusion of labour supply effects. The wage elasticities are not estimated from empirical data sources but pre-fixed according to five scenarios. The baseline scenario entails a guaranteed minimum income but without labour supply effects. In scenarios with higher wage and income elasticities, participation rates and transfer costs are higher. Poverty is slightly reduced in the baseline with the head count declining from 10.5% to 9.8% (-6.7%) at 50% median income level while Foster's index with inequality aversion set at 2 (poverty intensity) was reduced with 51%. In the labour supply scenarios the poverty head count declines only slightly from 10.6% to 10.5% in scenario A and to 9.9% in scenario E while this Foster 2 index declines again much more with 38% in scenario A and 48% in scenario E. Hence, it is especially the poverty severity and intensity (inequality among the poor), and not so much the incidence that is reduced with the minimum income guarantee policy. The overall decline in inequality is more modest. In the baseline GMI scenario inequality is reduced with -1.2% according to Gini and -7.4% according to Atkinson's index (inequality aversion set at 2). In the labour supply scenarios, income inequality according to Gini is reduced with -0.7% in scenario A and -1% in scenario E whereas according to Atkinson's index 2 inequality is reduced with -5.5% in scenario A and -7.0% in scenario E.

Simulating two conditional basic income regimes in Spain

The fifth but second European study by Labeaga, Oliver and Spadaro (2011) simulated the effects for Spain for 1999 of two variants of the conditional minimum income guarantee on labour supply and individual and social welfare (utility). Following two tax reforms in 1989 (split tax liabilities for married couples) and 1999 (Personal Income Tax allowances were introduced) two variants of minimum income schemes are simulated, a BI lump-sum transfer (13,997 euro) and a Vital Minimum tax allowance per equivalent adult in the household (4,632 euros), both funded with a flat-rate tax (VMFT). Tax rates are set at different levels: 25%, 30%, 38% and 46%. The flat-rate tax proposals replace the 1999 personal income tax reform but leave the social security contributions scheme unchanged. The simulation study is based on a tax-benefit simulation model (GLADHISPANIA), a social welfare function (direct measurement of utility assigned to income based on Blundell 2000) and a behavioural labour supply equation (based on Van Soest's discrete choice model of family labour supply 1995). Survey income date from the ECHP 1995 were used which were updated to 1998-1999 for Spain. The ECHP sample size was 6,522 households representing 12 million households in Spain (self-employed and retired were excluded). Efficiency effects or labour supply effects are simulated from working hours transition matrices at individual (singles) and family level (couples) taking

account of household substitution effects between head and partner. The labour supply or efficiency effects appeared negligible in all scenarios and for each household type. Labour supply was reduced from 3% (with 38% tax rate) to 4.3% (with 46% tax rate) with higher taxes. The social welfare effects (utility winners and losers) of the Vital Minimum reform were clear with more winners than losers and winners notably in higher income deciles (4th to 8th decile). The BI reforms show more winners in the lower deciles and more losers in the higher deciles. Using a different approach similar to optimal taxation, shows that the social welfare gains vary somewhat with the level of inequality aversion. The welfare gains compared to the reference scenario (1999) are larger when the inequality aversion (lambda) is higher and the government is primarily interested in the welfare of the poorest (Rawls' approach; lambda=-2) than when the inequality aversion is lowest or 1 (utilitarian). The social welfare improvement is, however, 50 to 60 times higher with BI than with the Vital Minimum. Moreover, an increase in the tax rate from 25% to 46% (irrespective of the level of the inequality aversion) shows that the BI flat tax option provides the best tradeoff between equity and efficiency. The authors therefore argue that a BI with a flat-rate tax of 46% is optimal whatever the inequality aversion is. The social welfare gains of a higher BI compensate the efficiency losses of a higher tax rate. The authors conclude that the BI with 46% flat-tax variant has potential to reduce inequality, that is to increase individual and social welfare, without creating efficiency losses.

[About here: Table 4 Simulated micro-economic effects of PI and CBI-like programmes]

Conclusions on conditional BI or PI simulation studies

The five studies on a conditional BI scheme or PI-scheme presented here confirm the results obtained for UBI and NIT-like options. The conditionality of the PI schemes either pertains to a work requirement or to (market) income or earnings requirements. The equity effects on reducing poverty and inequality are substantial whereas the disincentive or labour supply effects show mixed evidence dependent on the specification of the labour supply and social welfare function. In the Canadian study the efficiency losses caused by the disincentive or adverse labour supply effects of the higher marginal tax rates needed to fund the PI scheme are large whereas in the Spanish study using a different specification of the labour supply (family labour supply) and welfare function (direct utility assigned to income) the adverse labour supply effects turn out to be negligible. In the latter study, a conditional BI scheme with a flat-tax provided the best trade-off between efficiency and equity independently of the level of inequality aversion in society. In two studies, the PI-scheme was compared with a wage subsidy for each extra working hour with a reduced withdrawal rate of earnings and a substantial increase in the minimum-wage respectively. In both of the schemes, the extra income received from market income or earnings partly reduce the costs of income transfers. The simulated effects show lower disincentive effects on labour supply and lower costs due to the lower transfers and higher taxes received from the extra income compared to a conditional but universal BI or PI scheme.

4.2 Evidence from RCT field experiments studies

Here, we switch from simulation studies to RCT field experiments of BI (like), NIT and PI interventions which are considered to be better equipped to study the causal effect of particular reforms on employment, health, wellbeing, income, and other societal outcomes. In Section 3.2.1, we first discuss the field experiment of a full BI, after which we discuss in sections 3.2.2, 3.2.3 and 3.2.4. field studies on a partial BI, NIT, and PI studies. In section 3.3 we eventually discuss the evidence from policies on basic income dividends.

[About here: Table 5 Characteristics of 31 (quasi-) experimental studies on basic-income related schemes]

4.2.1 *Field experiment* studies *on* full UBI

Full UBI field experiments: UBI experiment in Madhya Pradesh

In 2011-2012, a UBI experiment was held in the Indore district of the Madhya Pradesh state in India. This universal, unconditional RCT experiment was conducted by an Indian trade union that represents poor women in self-employment called Self Employed Women's Association, and was funded by UNICEF. Eight villages were randomly selected as treatment group and 12 villages served as control group. In the treatment group, all inhabitants of the village received a monthly basic income for a period of 12 months. Adults received 200 Indian rupees per month and children below the age of 18 received 100 Indian rupees per month. The money for the children was provided to the mother. Even though the money amount was not enough to provide full subsistence security (Davala et al., 2014), it covers basic needs and is likely to improve the living standard of the village people.

The study of Beck et al. (2015) analyses health effects of the BI received as part of this experiment in India. Household surveys were carried out before[2], during (eight months after the start) and after the completion of the experiment, including some individual questions. Based on the surveys, three dichotomous health outcomes are distinguished: 1) any illness or injury that needed treatment (outside the hospital) and lasted at least 24 hours in the preceding three months, 2) any illness or injury that needed treatment in the hospital in the preceding year, 3) whether all children in the household aged between 6 months and 5 years had received all vaccination doses. A rich set of control variables was constructed from the surveys as well. Some significant composition differences between the control and treatment groups were observed, but they are taken into account in the propensity score matching (PSM) analyses. People from the treatment group, receiving the basic income, significantly less often experienced any illness or injury that needed treatment (outside the hospital) and lasted at least 24 hours in the preceding three

[2] However Beck et al. (2015) argue that the quality of this survey is relatively low. Therefore, they use most the data from the other surveys.

months. The odds are 46 percent lower in the intervention than in the control group. For illnesses/injuries requiring hospital treatments and vaccination, no significant effects are found.

UBI RCT experiment in Kenya

The UBI experiment in Keyna is the largest and longest experiment to date and funded by GiveDirectly (a global NGO specialised in delivering digital cash transfers). The experiment started in January 2018 in two of the poorest regions within the country. The randomised control UBI experiment comprised of four treatment conditions: a control group, short-term transfers, long-term transfers, and one-off lump-sum transfers. The treatments were randomly assigned at the village level. The control villages received no resource transfers. In the short-term and long-term UBI treatments, each adult over the age of 18 received US $0.75 per day. The short-term treatment lasts 2 years, while the long-term treatment lasts 12 years. The amount is calculated to be sufficient to cover the most basic needs. In the lump-sum treatment, each adult over the age of 18 received a one- payment of about US $500. This amount was calculated as the equivalent in net present value terms of the short-term transfers.

Banerjee et al. (2020) assess the short-term impacts of the UBI experiment during the covid-19 pandemic. The impacts of the UBI were assessed on a range of outcomes: labour market (including wage employment, self-employment, and earnings from these occupations), health (falling ill, hospital utilization) and well-being (including food security and depression) related outcomes. The authors estimate the intent-to-treat (ITT) effects by regressing the range of outcome measures on three dummy variables indicating whether or not households were assigned to the long-term, short-term or lump-sum treatment. The three treatment groups were compared to households in the control condition. Most outcomes are either measured in the covid-19 survey (conducted between late April and late June 2020), which is also the focus of their study. For some of the outcome measures, the authors also make use of data from the standard endline survey of households conducted prior to the pandemic (between August and December 2019) to assess changes over time.

We first summarize the findings with respect to wage and self-employment. Prior to the start of the pandemic, households in all treatments were significantly more likely (4.6/4.9 pp) to be self-employed while operating a non-agricultural enterprise. There is suggestive evidence that some of these enterprises shifted labour away from wage employment, although the effect is only significant for the long-term treatment. There is no evidence that the increase in non-agricultural enterprise employment reduced employment in agricultural enterprises. In fact, the number of agricultural enterprises even seems to have slightly increased in both, the short-term and lump-sum treatments. The covid-19 pandemic was only associated with small changes in newly created enterprises. The treatments effects during the covid-19 survey are fairly similar to those in the endline survey conducted before the onset of the pandemic.

In terms of earnings, a similar pattern can be observed. Before the pandemic, non-agricultural enterprise profits are higher in all treatments (only significant for the long-term and lump-sum treatments) compared to the control group. There are no significant effects on income from wage employment, and

only a significant increase in agricultural earnings for the short-term group. In contrast to the results for occupational changes, enterprises experience significant reductions in earnings. The large increases in earnings of non-agricultural enterprises, are almost completely reversed (although not negative compared to the control group) when the pandemic hits. Hence, new enterprises were not insensitive to the shock caused by the pandemic. The negative change in non-agricultural enterprise profits is particularly prominent for the long-term treatment, arguably due to greater investment in the enterprise. Overall, the results suggest that a basic income guarantee encourages risk-taking and entrepreneurial activity.

With respect to health, households in the experimental treatments indicated less frequently (-3.6/-5.6 pp) that any of the household members was ill (e.g. fever, nausea, not feeling "their usual self" physically or mentally) during the last 30 days than the control group (44% fell ill).[3] Illnesses most likely entail to non-covid-19 illnesses as the number of reported covid cases in Kenya yielded only 0.01% when the phone survey was completed. The authors also find significant reductions (-2.8/-4.6 pp) in hospital consultations. Banerjee et al. (2020) point out that this finding contrasts results in the literature and argue that the negative impact might reflect mechanical consequences of lower illness rates in treated households. In fact, when the treatment effect on hospital consultation is conditioned on illness, the effects largely disappear.[4]

Moving on to well-being measures and starting with food security, roughly two-thirds of the control group experienced hunger in the past 30 days. All three treatment groups experienced significantly less hunger and the effect size is roughly twice as large in the long-term treatment(-10.8 pp) than in the short-term treatment(-4.9pp) and lump-sum treatment(-6.4 pp). Nevertheless, the effect size in the long-term treatmentis economically modest, with the rate of hunger reducing from 68% to 57%.[5] With respect to depression, 43.7% of the control group were considered "depressed" according to the Center for Epidemiological Studies Depression Scale (Radloff, 1977). The depression scores fell in all three treatment arms, although the effects were only significant for the short- and long-term arms. Already before the pandemic, all treatments are scored significantly lower on the depression scale, but no significant change is observed between the two surveys.

[About here: Table 6 Micro-economic effects of Full UBI field experiments]

Conclusion on full UBI experiments studies

The two RCTs studies indicate that a full UBI generates a range of positive effects. With respect to employment outcomes, the Kenyan experiment shows some evidence that wage employment is reduced in favour of self-employment. Indeed, the results suggest that a basic income guarantee shifts labour from

[3] Also the number of ill households members fell with 4.7 pp in all treatment arms (although not significant in the long-term arm).

[4] Banerjee et al. (2020) also assess how interpersonal and commercial (physical) interactions (which help to transmit the virus) differed across treatment groups. The results are not discussed here.

[5] Banerjee et al. (2020) also report effects on the intensity of hunger (i.e. share of meals per day and whether households ate meat/fish), conditional on experiencing it. The findings are not extensively discussed here. In general, the findings suggest that transfers modestly reduce the extremity as well as the incidence of hunger.

wage employment to self-employment: it encourages risk-taking and increases entrepreneurial activity. Most of the health measures improved in both experiments and the experiment in Kenya also demonstrated that a UBI intervention positively affects well-being. Treated individuals faced less hunger and experienced on average lower levels of depression.

4.2.2 Field experiment studies on partial UBI

A partial UBI means that the income transfer amount is set below the poverty line. Therefore, the amount is inadequate to meet a person's basic needs or to resolve poverty. Several countries already have established unconditional cash transfer schemes for individuals or households on a permanent basis. The Alaska Permanent fund and the Iranian income transfer programme are examples of a permanent and universal but partial UBI scheme. These programmes will be discussed in section 3.2.2. In this section, we discuss the evidence on the effectiveness of three non-universal partial BI experiments that were conducted recently, namely, the Finnish BI experiment, Barcelona B-MINCOME, and the Stockton Guaranteed Income Experiment. These are non-universal basic income schemes because they are targeted at low-income individuals.

Finnish partial BI Experiment

The study by Verho, Hämäläinen and Kanninen (2021) deals with the basic income experiment in Finland that is held in the years 2017 and 2018 among 2,000 unemployed people randomly selected out of the unemployment population. The sample is limited to people of 25-58 years with low social security incomes, with either a type 2-benefit, i.e. a flat-rate unemployment benefit, or a type 3 benefit, that is similar to a social assistance benefit. The experiment provides an unconditional partial basic income with waiving of any job search requirements of 560 euro per month plus on top the existing child and housing benefits to which unemployed people are entitled. People may keep their additional earnings on top of their BI without claw-back rates on their earnings and without the phasing out of the benefit. It means that the implicit tax on participation in work is on average reduced from 66% to 43% (-23%) which provides extra work incentives. The study examines the employment effects on the number of days worked. As from 1 January 2018 on there was a reform in social security policies, the researchers used data from 1 November 2017 to 31 October 2018 to estimate the employment effects. The study uses register data (unemployment records at Kela, data of local labour offices, tax registers, population and pension registers). To calculate these employment effects, they applied OLS regression with a treatment dummy to compare the effects for the treatments with the control group. The regression model contains covariates as age, gender, education, ethnicity, family situation, municipality and type of benefit. In year 1 they found no significant effects. The average treatment effect estimated was +1.5 days on an annual basis. In year 2 they found stronger and significant employment effects (+6.6 days or +8.6pp), but these were confounded with the effects of the benefit reform in 2018 making entitlement conditions stricter. Pure treatment effect could only be calculated for year 1 and turned out to be rather small.

Recently, a new study on the Finnish experiment by Kangas et al. (2021) appeared in which the authors examine the employment and non-employment effects of the basic income RCT experiment but now using the survey data instead of the register data (information on 586 treated out of 2000 and 1,047 controls out of 5000). The survey data stem from telephone interviews at the end of the experiment after two years. The data were reweighted for taking account of survey non-response and attrition. Weights are defined with (logit) regressions including covariates like gender, age, family situation, ethnicity, residence region and unemployment benefit. The OLS estimates of the employment effects at the end of the experiment appeared marginally significant for the treated (<0.11) compared to the control group. The survey data showed that 35% of the treated were employed against 28% according to the register data; two-thirds worked full-time and one-third part-time. The perceived ability to work appeared strongly associated with finding work and health barriers to work. Overall, re-employment probabilities were higher for the treated compared to controls. Treated also had more opportunities to do meaningful work or to improve their material living standards than controls. More people in the treatment group (58.5%) rated their health as good or very good compared to the control group (51.4%). Also, a larger number of people in the control group reported having a disease, disability or mental disorder that hinders daily life. For mental health (MHI-5 index), significantly more people felt down or sad in the control group whereas significantly more people reported their ability to cheer up in the treatment group than in the control group. No differences were found for the other 3 items. Subjective wellbeing scores were significantly better in the treatment group than in the control group (average 7.3 for the treated and 6.8 for the controls). Eventually, the treatment groups responded significantly more positive on each of the three capability questions on memory functioning, learning new things and ability to concentrate than the control group.

Barcelona B-MINCOME Pilot

The B-MINCOME experiment consists of a variety of experimental treatments and some also have characteristics of a negative income tax. The scheme entails strong work incentives because the majority of treated individuals (66.2%) are assigned to the unlimited earnings release condition (i.e. any additional income the household receives is reduced from the benenefits received). Between 2017 and 2019, ten neighbourhoods making up the Eix Besòs, one of the most deprived urban areas in Barcelona, partook in a GMI experiment. Households were eligible to receive Municipal Inclusion Support (SMI) if the sum of basic needs and housing needs fell below the SMI threshold. The level of SMI was designed to meet basic needs, including housing, and the amount received depended on the number of household members.[6] Depending on the type of treatment, the SMI was either conditional or unconditional on participation in the active support group and the benefit amount was either proportionally increased (decreased) or only partially reduced with increased earnings. Beneficiary households were randomly assigned to one of the ten

[6] The SMI complemented the income of people in the household and guaranteed a basic income. The amount of basic needs (excluding housing and including basic energy and water supplies) equalled €402.6 per month for the first adult, and €148 per month for each additional member. Amount of housing needs: €260 per month for the first adult, €110 per month for the second member, €40 per month for each additional member.

treatment groups that combine the following conditions: 1) conditional: receiving the SMI was conditional on mandatory participation in the assigned active support group (training and employment, social entrepreneurship, room rental aid, community participation), 2) non-conditional: receiving the SMI was not conditional on mandatory participation in the assigned active support group, 3) limited earnings release: any variation in the household's computable income entailed a proportional variation, positive or negative, of the initially estimated SMI, 4) unlimited earnings release: the change in the household's income only entailed a partial change in the SMI, it was reduced by 25% for the first 250 euros net monthly income earned above the initial SMI and by 35% for income above 250 euros.

Riutort et al. (2021) report on the effects of the different treatments on labour market outcomes, non-employment activities (including schooling), health, subjective wellbeing, economic wellbeing and societal and other individual outcomes.[7,8] With respect to labour market outcomes, individuals in nearly all treatments reduced their labour supply. No significant effect was found for treated individuals who received the SMI conditional on mandatory participation in the assigned active support group (participants assigned to the training and employment were not included in the final analysis). The likelihood to work full-time (with an indefinite contract) also reduced for most treatment groups (except for the conditional and limited earnings release groups). With respect to non-employment activities, the SMI had little impact on schooling activities and none of the treatment groups increased participation in training. School drop-out rates were only reduced for the SMI group, and repetition of school years increased in the conditional group.

In terms of health, poor health for minors (<16 years of age) was only reduced in the unlimited earnings release group. Moreover, the likelihood to develop mental illness and scores on the social support and stress scale were unaffected. Having quality sleep improved in a number of treatment groups. In contrast, measures related to well-being were all affected in the expected direction. Experiencing high levels of well-being significantly increased for all treatment groups, while the likelihood of going to bed with hunger and severe material depreciation declined for all treatment groups (the latter did not decline for SMI and active support). Also participants' economic well-being improved on a range of outcomes. First, satisfaction with the economic situation significantly improved in all treatment groups. This finding most likely reflects an improvement in the recipients' financial situation. The probability of having unpaid bills, having leaks/damps, needing to borrow money from family and/or friends, and having an outstanding debt reduced in at least half of the treatment groups. The need to use social services only reduced in the SMI group.

Only minor effects are observed for the range of societal outcomes. Surprisingly, the incidence of social volunteering decreased in the SMI group and social participation was only increased in the conditional group. The probability to receive more social support from others increased for the conditional group.

[7] Riutort et al. (2021) do not report effect sizes. Therefore, we only document treatment effects in qualitative terms.

[8] Laín (2019) report the preliminary results of the B-MINCOME project for similar outcome measures for the period 2017-2018. Here we only document the final results over the period 2017-2019.

Finally, at the individual level, only participants in the conditional group were better able to enjoy social leisure activities.

To conclude, B-MINCOME positively contributed to the reduction of severe material deprivation and food insecurity among treated participants. Some positive effects are also observed in terms of housing security. Participants subjective and economic well-being improved significantly. Apart from improved sleep quality, the programme did not lead to substantial improvements in participants' health. Finally, in most treatments, negative employment effects were found since income recipients reduced their participation in the labour market except in the conditional SMI group with active support and the limited earnings release group where no significant effects were found.

Stockton Guaranteed Income Experiment

The Stockton Economic Empowerment Demonstration (SEED), a city in California, was launched in 2019. To be considered for SEED, recipients had to be at least 18 years old and live in a Stockton' neighbourhood with a median income at or below $46,033, the city's median household income. Someone's own income was irrelevant. 4,200 Inhabitants were randomly selected to participate in the experiment. From the households that showed their interest in taking part, 125 residents were randomly selected as treatment group and received $500 per month for 24 months. 200 Individuals were randomly assigned to the control group. In a preliminary report by West et al. (2021), independent researchers report that SEED resulted in more full-time employment, less income volatility, and better ability to spend money on unexpected expenses. Moreover, it is mentioned that mental health and health in general is much higher in the treatment group than in the control group. However, estimates are not shown in their report.

[About here: Table 7 Micro-economic effects of partial BI field experiments]

Conclusions on partial BI field experiments

The findings of the partial BI experiments show mixed results with respect to employment. The B-MINCOME experiment in Barcelona suggests that income recipients tend to reduce their labour supply, whereas the Finnish experiment does not find any evidence for a reduction in labour supply. However, the small Stockton experiment in the US (125 participants) reported positive effects on full-time employment but without presenting the empirical evidence. Survey data collected after the Finnish experiment show that the (re-) employment probabilities were somewhat higher for the treatment group compared to the control group after controlling for attrition. The survey also showed that the perceived ability to work appears strongly related to finding work and experienced health barriers to work. Moreover, the Finnish experiment demonstrates that income recipients experienced more opportunities to do meaningful work. The findings with respect to health are somewhat mixed, although no negative effects are found. Recipients subjective well-being improved in the experiment. The same holds for their economic or financial well-being: the material living standards improved significantly.

4.2.3 Field experiments studies on NIT

Just like a full basic income, NIT schemes provide a GMI but it is paid out in the form of a tax allowance. If individuals' income falls below the guaranteed minimum level, the income loss is compensated by means of a tax allowance. Earnings are withdrawn from the social security benefits at an implicit tax rate that is generally below 100% to provide incentives to work. In this way, it combines a social security scheme with a tax allowance scheme. It is not universal because the benefit is fully taxed away at a certain income level beyond which no benefits are received anymore. The income level at which benefits are reduced to zero is however higher than the poverty threshold and depends on the level of the implicit tax rate. The NIT replaces the existing welfare schemes, and it provides an alternative to the full UBI. This section reviews a set of NIT experiments from the US and Canada. All (except one) field experiments were held during the 1960s and 1970s. More recently, the government of Ontario launched the Ontario Basic Income Pilot (although it was prematurely cancelled). The outcomes of the former experiments in Canada and the US from the 1960s and 1970s are already frequently discussed in the literature notably in various review studies, but the outcomes of the Ontario Basic Income Pilot only became available recently.

Gary-Indiana NIT experiment

Burtless and Hausman (1978) deal with the Gary-Indiana NIT RCT-experiment held between 1971 and 1974 with 380 participants (247 in experimental treatments) providing income support to families in the capital city. The programme contains 4 NIT plans with two flat tax rates (40% or 60%) and different levels of income support (two received slightly more than poverty level and two received one-quarter less). The support is dependent on family size and tax rate of 40% or 60% up to a certain hours of work threshold after which earnings are taxed at the usual tax rate. They use a structural non-linear labour supply model with non-convexities in the budget line while estimating indirect utility functions for different after-tax wage levels to derive labour supply estimates for each of the different segments of the kinked budget line. They found very low wage elasticities (0 to 1.8pp) and low income elasticities (-3.3 to -4.8pp) because of which the overall effects of the NIT programme on labour supply are very modest and mostly determined by the income support levels. For the same reason the effect on reducing poverty is positive (lowering the head count) because of zero wage elasticities and low income elasticities bringing people with NIT on poverty line level of income.

The study by Greenberg, Moffitt and Friedman (1980) also focuses on the Gary-Indiana income maintenance experiment in the 1970s but entails only one of the four NIT income transfer experiments in which 1,800 only black families participated (1,000 in experimental groups). They used the 4-monthly survey data as well as employment register data containing earnings information and quarterly employment records (8 per individual) to re-examine the effects on work effort or earnings. The study tries to correct for the underreporting of income and earnings that appeared substantial in all groups in the survey data, being at least 25%, and for some groups even more than 50%. Treatment effects were calculated with Tobit and Probit and error-decomposing GLS models (controlling for autocorrelation over time). Treatment effects

were slightly less negative in the GLS estimates but still strongly negative notably for female heads. Effects of underreporting bias are all positive but not very precise (five out of 9 significant at the 15% level). Underreporting was higher with greater pre-enrolment earnings of female heads and lower earnings of husbands. The findings suggest that the negative labour supply effects in the survey data must be partly attributed to the underreporting bias and may be considerably less adverse in reality. However, the underreporting raises the costs of the programme because families collect more transfers than they are entitled to, given their real earnings.

Seattle-Denver NIT experiment

In Seattle-Denver, during the 1970s, four maintenance experiments were funded by the federal government. They were all conducted in a similar manner, by means of a stratified allocation of families into treatment groups. Four assignment variables were used: site, family type, race-ethnicity and pretransfer family income. The Seattle-Denver income maintenance experiments (SIME/DIME) is one of them. Within combinations of these four assignment variables, there was random assignment into the treatment or control group. About 4,800 families were enrolled in the experiments of which 60% were assigned to one of the 11 different financial treatments for either a three- or five-year period. Families of four in the treatment group received a combination of the following financial treatments: support equal to either $3,800, $4,800 or $5,600 per year[9], initial tax rates of either 0.5, 0.7 or 0.8, and a decline in the average tax rate of either 0.0 or 0.025 per $1,000 of income per year.

Brandon Tuma and Robins (1980) analyse the effect of this NIT experiment on changes in employment status. The expectations are that NIT programmes decrease net wage rates and increase net nonwage income, causing lower incentives to work. These lower incentives could affect the extensive and/or intensive margin. Brandon Tuma and Robins (1980) develop a continuous-time model of changes in employment status. Whereas this model has some advantages over a simple Probit model, it does not take into account the stratification on pretransfer income, nor does it take into account the problems that arise due to the requirements that participants of the experiment had to meet. Keeping these shortcomings in mind, and making use of information retrieved from interviews, the authors find that indeed, the NIT programme acts as a work disincentive.

The probability of being employed at the end of the experiment is significantly lower for wives and unmarried female heads in the treatment groups. For husbands, the NIT programme increased the rate of leaving employment significantly. Men on the 5-year NIT programme left employment at a rate of 25 percent faster than men not on the NIT. Entering employment happens at a much slower rate for husbands (22 percent), wives (40 percent) and unmarried female heads (35 percent) when participating in the NIT programme. In general, the findings of the NIT programme lasting three years is much smaller and sometimes even in the opposite direction.

[9] Adjustments are made for other family sizes.

Stephens (2007) conducts a re-estimation of the labour supply effects of the Seattle-Denver (SIME/DIME) NIT experiment (4,800 participants) taking account of the difference between the five-year and three-year experiment, the duration differences in job training and whether the treatment was a single or combination treatment. About 60% were in the NIT experiment but also in the job training programme. The authors argue that time allocation interactions of people joining both experiments (due to substitution between work hours, hours in training and leisure time) cause specification bias for which needs to be controlled. After including job training duration indicators in the model, the difference in effects of the three- and five-year programme becomes 50% smaller and insignificant in the second and third year. Controlling additionally for enrolment in the five-year programme reduces differences between both programmes further while becoming insignificant. Next, controlling for a single or combination treatment (NIT + job training) shows that single NIT treatment provides for significant differences in effects for the three- and five-year programme again in the first two years. For women the re-analysis shows largely similar results as the original estimates. Also, for women taking account of training duration reduces the differences of the three- and five-year effects (-66%) while becoming insignificant. Adding the five-year programme enrolment variable in the model creates no further difference in the found effects for women. The change in labour supply at the end of the experiment is the same across NIT households in the three- and five-year programme and the work effort of treated and controls is similar in the post-experimental period. The authors conclude that labour supply responses are mostly driven by intertemporal substitution supporting the life cycle model.

The study by Aschenfelter and Plant (1990) uses a non-parametric approach to re-estimate the labour supply effects and costs of the Seattle Denver experiment in the 1970s. The study is methodological while it deals with the endogeneity of labour supply caused by the non-random assignment to treatments. Families with higher pre-experimental earnings were assigned to more generous transfer programmes who were therefore more likely to decrease their earnings (and hence labour supply) to become eligible for the programme. Data are reweighted to apply non-parametric estimation. The effects are composed of an income (extra lump-sum) and substitution effect (wage rates decrease). Some labour supply effects were found with positive net payments for people in the experimental groups compared to the controls in 32 of 33 programme categories (11 programmes for each of the three years) of which 10 were significant. However, compared to the three year treatment, the five-year results show for more income categories negative results. Attrition families are excluded here. If assignment appears non-random due to selective attrition, non-parametric estimation is impossible. Attrition appears to be related to the net gain of the transfer programme. Zero-payments are therefore underrepresented. Including attrition, the study still found smaller but negative labour supply effects in most income categories, raising the transfer costs of the programme.

In contrast to all earlier studies, Greenberg and Halsey (1983) also reestimate the labour supply effects of the Seattle-Denver experiment in the 1970s. They do not solely make use of the data retrieved from the interviews, but also make use of data reported by the employers of the SIME/DIME participants

to the Washington and Colorado State Departments of Employment Security. Individuals' full names and social security numbers were used for matching. Greenberg and Halsey (1983) estimate a mean effects model in which the regular controls are included. Some problems arise due to data restrictions. For example, employer information is not available for all individuals, so that there is no measure available that can check whether there are self-reported biases. Therefore, the authors have imputed values to the corrected work effort variable, with the disadvantage of biased standard errors. It turns out that, except for female family heads, interview-based estimates overstate true reductions in work effort. For example, the mean treatment effect for husbands based on interview data is -0.071pp, whereas the same treatment effect based on employer data is estimated to be -0.048pp. The main lesson to learn is that in large-scale social science experiments, the validation of data should be treated very seriously.

The Canadian Mincome experiment

The Manitoba Basic Annual Income Experiment (Mincome) was conducted between 1974 and 1979. Mincome was the Canadian version of the NIT experiments conducted in the US. The experiment was targeted at low income households and had two experimental sites in the province of Manitoba: Winnipeg and the rural community of Dauphin. A number of small rural communities were also selected to serve as controls for the Dauphin subjects. Both Winnipeg and Dauphin residents with no income from any source were eligible to receive an annual payment set at 60% of Statistics Canada's low-income cut-off (LICO), which varied by family size. Winnipeg (city of 450,000 at the time) hosted a classical experiment. In Winnipeg, participants were randomly assigned to one of eight guaranteed annual income treatments. These treatments had combinations of high, medium, or low guarantee levels, and high (75%), medium (50%), or low (35%) tax-back rates, excluding the high guarantee/low-tax back rate plan) or a control group. For a negative tax rate of 75%, every additional euro earned from any other source, including wage income, MINCOME payments were reduced by $0,75 dollar. The guarantee level varied by family size.[10] Dauphin (with a population of approximately 10,000) was selected as the "saturation" site. That is, all families in Dauphin were eligible to participate in the experiment, and received income subsidies if they met the income test. The saturation site was chosen to provide insight into administrative issues associated with the policy in a less artificial environment. In Dauphin, the negative tax rate was set at 50%.

The study by Hum and Simpson (2001) aims at comparing the labour supply effects of Mincome with the former NIT RCT experiments in the US which were held between 1968 and 1974. The researchers applied a non-structural (ANOVA) difference-in-difference analysis with a treatment dummy and a structural labour supply model to estimate the incentive effects (substitution and income effects) for singles and families. For families, no cross-wage elasticities were considered. The labour supply effects were much

[10] In addition to one control group, the eight treatment programs for a four-person family in 1974 dollars were the following: (1) guarantee = $3,800, NIT = 35%; (2) guarantee = $3,800, NIT = 50%; (3) guarantee = $3,800, NIT = 75%; (4) guarantee = $4,800, NIT = 35%; (5) guarantee = $4,800, NIT = 50%; (6) guarantee = $4,800, NIT = 75%; (7) guarantee = $5,800, NIT = 50%; (8) guarantee = $5,800, NIT = 75%.

more modestly negative for Mincome in Canada than the original estimates found for the four US experiments. In the non-structural approach, the US effects range between -6% to -19%, whereas for Canada, labour supply effects range between -1% for male husbands, -3% for wives and -5% for unmarried women. The estimates were insignificant when time effects are properly included. In the structural labour supply approach, the Canadian results were negative and again much smaller in magnitude with non-significant wage elasticities found for husbands, wives and single women of -0.07, -0.08 and -0.17. and income elasticities of -0.03, 0.07 and -0.01. The presence of pre-school children determines according to the authors the labour supply response of married men (positive) and women roughly to the same amount (negatively). Effects were dependent on the generosity of the plans (three generosity levels).

Calnitsky and Latner (2017) also investigate the impact of Mincome on labour market participation in Dauphin. This study attempts to disentangle the treatment effect from "social interaction" of "community context" effects. The latter can also be considered the result of a macro experiment. The authors distinguish four social mechanisms that might remain unobserved in a micro experimental setting: diminished stigmatization, labour demand effects, reductions of overemployment, and changes in power relations. First, the stigma of leaving work could be diminished when an entire community is faced with this option (cf. Calnitsky, 2016). Consequently, the labour supply might decrease more than would be expected in a setting where choices are made in isolation in the context of traditional choices and values. Second, micro level experiments do typically not capture the potential change in labour demand that would occur if basic income were to be introduced at a larger scale. On the one hand, if people reduce their labour supply, wages might be increased as the labour market tightens (i.e. the substitution effect). On the other hand, as wages rise, the same level of welfare can be achieved with less work (i.e. the income effect). Third, overemployed workers may reduce labour hours, making it easier for underemployed and unemployed workers to find work. This mechanism results in a redistribution of work, without necessarily increasing or decreasing labour supply. Finally, a guaranteed income or meaningful 'exit option' enables workers to follow other interests such as engaging in entrepreneurial activities or enrolling in training or education. To assess the micro level impact of Mincome, Calnitsky and Latner (2017) compare changes in the share of labour market participants over the course of the experiment between households in Dauphin and Manitoba control households. To estimate the community effect, the change in labour market participation in Dauphin (i.e. the saturation site) is compared to the change in Manitoba where the experiment was not fully implemented. The individual level average treatment effect (ATE) is a reduction in labour market participation of 11.3 pp. The "social interaction" treatment effect is calculated as being 3.1pp. This implies that relative to the control group, over 70% of the ATE effect of the 11.3pp reduction can be ascribed to individual level mechanisms, and the remainder (30%) is due to community effects.

Whether Mincome was indeed considered as an 'exit option' is being tested by Calnitsky, Latner and Forget (2019). Calnitsky et al. (2019) examine the impact of the income guarantee on non-employment activities. In this study, the authors focus on the Winnipeg site where participants were randomly assigned to one of the treatments or a control group. The non-employment activities are divided into individually

and socially valuable activities. Based on Mincome's baseline survey and a set of follow-up surveys, 10 categories were constructed indicating the reasons for participants to withdraw from the labour market: 1) "laid off", 2) "did not want to work", 3) "education", 4) "family", 5) "ill/disabled", 6) "job/work conditions", 7) "retirement", 8) "self-employed", 9) unpaid vacation, and 10) other/unknown. For each of these categories, the authors apply a difference-in-differences analysis to compare the baseline study period change in the frequency of selecting particular answer categories within the treatment group and the baseline study period change within the control group. Positive effects are found in the categories "family" (3.9pp), "job/work conditions" (5.9pp), "did not want to work" (4.0pp). The most important family related reason for leaving the labour market was that participants wanted to take care of their family. The category "job/work conditions" can also be further decomposed into more specific categories and the largest effect is found for "no jobs available". This does not mean that literally no jobs were available, but it also points to the possibility of leaving a job if it is not considered good enough (i.e. low wages). Positive effects were also found in the categories "education" and "self-employed" (2.7pp). Moreover, a negative effect was solely found for the category "ill or disabled". The sample is also split up by gender, age groups, and amount level of the annual guarantee. The results show that Mincome mostly affected women for whom the largest positive reasons for not working include "any reason" (11.7pp), "job/work conditions" (11.3pp), "education" (9pp), and "family" (6.8pp). For men, all treatments effects are close to zero. When looking at different age groups, the main reason for younger participants (age $\leq$ 25) to not work is "education", while for middle aged participants (age 26-49) the most important reason is "family". The oldest group (age $\geq$ 50) shows the largest positive treatment effect for "any reason" (16.7pp). When the sample is split by the size of the guaranteed income ($3,800, $4,800, $5,800), only a few differences remained.

Calnitsky (2000) examines how firms responded to the introduction of Mincome. The aim of the authors was to test two competing theoretical perspectives to explain the impact of a guaranteed income on employers. The first view considers the guaranteed income as a subsidy to employers, a policy facilitating the efforts of large, labour intensive companies to preserve poverty level wages (Levi, 1970; Howell, 1997; Young & Mulvale, 2009). The second view posits that a guaranteed income facilitates workers to exit the labour market, reducing competition at the lower end of the labour market, improving the bargaining power of labour and pulling up wages (Block & Manza, 1997; Widerquist, 2005; Wiederpan et al., 2015). Calnitsky (2020) looks at the impact of Mincome on wage rates, applications, hiring, and work hours in the past four months in local businesses in Dauphin during the period 1974-1975. Using a difference-in-differences design, changes in outcome variables in Dauphin are compared to changes in those variables in seven control towns. Compared to the control firms, the median hourly wage on all advertised job openings in the past four weeks (prior to the survey) increased by 66 cents more in Dauphin (this effect corresponds to an increase of 21.5%). Also the hourly wages on all new hires in the past four months (prior to the survey) increased faster in Dauphin than in control towns, namely by 17 cents (or 6.8%). The effect however, is insignificant. Although the latter reflects wages that are actually negotiated, the hourly wage on all advertised job openings might be a better reflection of the most recent position of firms on the labour

market. The second survey was conducted nine months after Mincome was introduced and workers and firms might have needed time to fully adjust to the programme. With respect to applications, the experiment shows an insignificant decline of 12.2 pp which provides some evidence of a tightening labour market in Dauphin. These fewer applications did not translate into fewer hires. Instead, Mincome significantly increased the number of applications by 18.6pp. Finally, the analyses show an insignificant reduction of 0.07 hours in the vacancies and a significant reduction in hours worked of 2.76 hours for new hires. The insignificance of some of the coefficients could be explained by the small sample sizes. To summarize, this study shows that Mincome forced wages up and reduced applications to Dauphin firms. These findings suggest that the programme did not favour the position of local employers on the labour market.

Finally, two studies look at the effects of Mincome on health care utilization (Forget, 2011, 2013). As both studies run similar analyses on the same dataset, we discuss them simultaneously. A guaranteed income can positively affect health in a number of ways. The relation between poverty and poor health is well established (e.g. Evans & Stoddart, 1994; Marmot et al., 2010). A guaranteed income can also improve health outcomes through the reduction of risks. The existence of a guaranteed income will affect the decision making of individuals receiving stipends, but also for those whose incomes are close to the eligibility line for those stipends. Typically, decisions are made without full certainty of whether one qualifies for payments. A guaranteed income can thus be considered as a form of insurance policy that reduces the likelihood of mental health issues. Lastly, health can be affected through behavioural 'spill-over' effects in a community. For example, the decision to vaccinate a child or not has an impact on others through herd immunity. This study focuses on the saturation site Dauphin. The health effects include hospital separation rates and physician claims which can both be further decomposed into "accidents and injuries" and "mental health diagnoses". As all Dauphin residents with no income from other sources were eligible to receive the guaranteed income, a control group was constructed through a combination of hard-matching and propensity score matching. For every treated subject, three other Manitoba residents were selected as a comparison group. Using a segmented time series model, Forget (2011, 2013) shows that overall hospital separation rates, hospital separation rates due to accidents and injuries, and hospital separation rates due to non-congenital mental health problems were all significantly higher for treated Dauphin participants than in the control group closely before the introduction of Mincome. Between 1973-1978, when Mincome was in place, the hospitalisation rate began to drop in Dauphin relative to the control group. Similarly, the gap in hospital separation due to "accidents and injuries" or "mental health" narrowed between Dauphin and controls. By the end of the Mincome experiment, there was no significant difference anymore between the treatment and control group. With respect to physician claims, changes in the rate of "mental health" displays a similar pattern as for hospitalization rates. No significant changes were observed for "accidents and injuries". As hospitalization rates are considered a better measure for health utilization (patients tend to have less control over the decision to be admitted to hospital than they do over whether to consult a physician), we conclude that Mincome had a positive impact on a range of health outcomes.

Ontario Basic Income Pilot

In 2017, the Government of Ontario initiated a basic income pilot that was supposed to last three years in three counties: 1) Hamilton and Brantford, and Brant County, 2) Thunder Bay, along with the Municipality of Oliver Paipoonge, Township of Shuniah, Municipality of Neebing, Township of Conmee, Township of O'Connor, Township of Gillies, and 3) Lindsay. In 2018, a new government announced that the pilot would be cancelled, just more than a year after the pilot was launched. The pilot ran from April 2018 until March 2019. The payments were targeted at individuals living on a low income.[11] The payments aimed to ensure a minimum level of income, equal to 75% of the Low Income Measure. The payments, in combination with other broadly available tax credits and benefits, would provide an income that meets household costs and average health related spending. The pilot followed a tax credit model ensuring a payment of $16,989 per year for a single person, less 50% of any earned income, and $24,027 per year for a couple, less 50% of any earned income.[12] Hence, the basic income amount will decrease by $0.50 for every dollar earned through work. It means that the basic income scheme provides positive incentives to paid work compared to welfare benefits with much higher implicit tax rates on earnings.

Despite the fact that the pilot was prematurely cancelled, researchers of the McMaster University conducted an online survey which was held between January and August 2019 (Ferdosi et al., 2020) and available to recipients from Hamilton, Brantford and Brant County. The survey included 70 questions to assess how the basic income affected a wide range of areas. Approximately 1,000 low-income households from Hamilton, Brantford and Brant County were enrolled in the experiment. A total of 217 former recipients participated in the survey. The findings of the survey should be cautiously considered as former participants might have selected into the survey non-randomly. The representativeness of the survey participants is not discussed in the report.

With respect to labour market outcomes, the survey illustrates that 23.9% of the recipients who were employed 6 months before receiving the basic income stopped working during the pilot. Of the participants who made the transition from employment to unemployment during the pilot, 40.6% enrolled in fulltime education with the intention of re-entering the labour market later with higher qualifications. Out of the respondent who were previously unemployed, 18.2% found employment during the pilot. Some anecdotal evidence suggests that a basic income can also enable individuals to become employable by removing an important source of stress. In fact, 61.5% of all respondents indicated that it became "somewhat or much easier" to search for a job. A basic income also seems to help workers in precarious

[11] The income threshold for receiving a basic income was under $34,000 for singles and $48,000 for couples.
[12] People with a disability will also receive up to $500 per month on top. Participants currently receiving child benefits will continue to be eligible to receive them during the pilot. Participants receiving Employment Insurance (EI) or Canada Pension Plan (CPP) payments will have their monthly basic income payment reduced dollar for dollar. People receiving support through social assistance needed to withdraw from Ontario Works or the Ontario Disability Support Programme (ODSP) to participate in the pilot and receive Basic Income. People who leave Ontario Works to participate in the pilot continue to receive the Ontario Drug Benefit. People who leave ODSP to participate in the pilot continue to receive the Ontario Drug Benefit and dental benefits.

employment (i.e. casual, permanent part-time, temporary contract of less than 1 year, and fixed-term contract more than 1 year) or less desirable permanent jobs to take risks and move into self-employment. Out of the workers who were in precarious forms of employment 6 months before the pilot, 16.19% moved into self-employment. For workers in full-time positions, 26.32% entered self-employment during the pilot. Moreover, the basic income seems to have improved the hourly rate of pay, the working conditions, and the job security of workers who were already employed before the pilot. A substantial share of previously employed respondents indicated that these aspects of work were "somewhat or much better", while a much smaller share indicated that these aspects became "somewhat or much worse". In line with these findings, 78.9% of the respondents indicated to be "somewhat or much more motivated" to find a better paying job.

Besides improvements in individuals' labour market outcomes, basic income recipients also increased their participation in education or training, volunteering, extracurricular activities, socializing (e.g. with community members), and time devoted to unpaid personal interests.

With respect to health, the majority of respondents indicated that their (children's) general as well as mental health was somewhat or much better compared to before receiving the basic income. The pilot also affected health care utilization: emergency room visits and doctor visits on average declined among participants. Moreover, participants on average experienced less stress and anxiety, pain, depression, anger, and felt less tired compared to before the pilot. Participants also demonstrate healthier behaviour: tobacco use and alcohol consumption on average reduced, while physical activity increased for the majority of income recipients. In terms of well-being, 80.8% of the respondents indicated that self-confidence was 'somewhat or much better'. For the majority of recipients, their diet and consumption of nutritious food improved, meals were skipped less often, and food bank usage decreased substantially. Perhaps most importantly, 86.2% of the respondents reported that their outlook on life improved compared to before the pilot.

Related to the positive health effects, the majority of respondents was better able to pay for medication, dental care and psychotherapy, compared to before the pilot. An improvement in economic security is also reflected in respondents' improved ability to repay debt, finance household as well as clothing purchases. On average, recipients also appear to be able to afford better housing options as they moved to higher rent categories during the pilot. This is supported by the observation that the quality of living accommodations on average improved according to the respondents. Furthermore, participants had to rely less on family and friends for financial assistance and payday loan usage. Finally, respondents felt more prepared for financial emergencies.

In addition to the survey conducted by McMaster University, the Basic Income Canada Network (2019) and the Ontario Basic Income Network (OBIN) ran the so-called Recipient Survey among participants from all three participating counties between December 2018 and mid-January 2019. When the pilot was prematurely cancelled, BICN and OBIN ran a petition to save the pilot. Over 1,500 basic income recipients signed the pilot out of which 424 participated in the survey. The survey respondents arguably have a positive attitude towards the basic income scheme. The characteristics of the respondents in the

Recipient's Survey are not fully representative for the characteristics of the respondents in the baseline survey which was part of the pilot. Hence, the interpretations of the findings from the Recipient Survey deserve some caution. The survey questions mainly concern how basic income receipt change respondents' situation in a variety of domains. Many of the items assessed in the Recipient Survey are highly similar to the items included in the McMaster University survey. Hence, we restrict the discussion to items that are not covered in the McMaster University survey and to findings that deviate (qualitatively) from the ones reported above.

With respect to labour market outcomes, most of the findings of the McMaster University survey are confirmed by the Recipient Survey. In addition, respondents indicated that basic income enabled them to afford child care (5.6%) and transportation to work (20.15%). In terms of health, 17% of the respondents indicated that they were able to reduce medication. Recipients also found it easier to buy glasses and they felt better able to save as well as manage their money. Moreover, recipients felt less marginalized and felt more dignity. Finally, the majority (63.18%) reported to have more money for recreation, and a substantial share participated more often in community events (40.38%).

[About here: Table 8 Micro-economic effects of NIT field experiments]

Conclusion on NIT field experiments studies

The field studies on the NIT experiments from the 1970s and 1980s in the US and Canada confirm the results obtained before from the NIT simulation studies. The evidence on the labour supply effects is again mixed with larger negative effects in non-structural models or difference-in-differences analysis (ANOVA, OLS with a treatment dummy) and smaller negative and sometimes insignificant results in structural labour supply models. Smaller effects were also found at least for men when the duration of the experiment is controlled for or when the outcome measures are corrected for the combination of treatments (the fifth study on three- and five-year duration and the combination of NIT and job search training). Eventually, smaller negative labour supply effects are found when the data are corrected for the underreporting of income or earnings in the survey evidence on which most of the US estimates were based. The latter therefore uses register employment data to correct for this underreporting bias and shows considerably smaller negative labour supply effects. The labour supply effects differ strongly by age, gender and ethnicity but notably also by the methods used. If a structural labour supply approach is combined with estimating indirect utility or social welfare functions, the wage and income elasticities turned out to be much smaller and become often insignificant. In terms of utility, the disutility of negative labour supply effects for women with young children are then weighted with the utility of extra time for caring and leisure. The more recent experiment in Ontario suggests that employed individuals tend to reduce their labour supply. On the other hand, unemployed individuals experience less barriers to find employment. Additional evidence from Mincome and the Ontario experiment also show that an NIT makes it easier for individuals to quit employment when they work in unfavourable job conditions or to engage in entrepreneurial behaviour.

Other reasons for quitting work include family related reasons or starting a training or education. Finally, with respect to labour market outcomes, Mincome also showed that it forced wages up and did not act as a wage subsidy to employers. Finally, NIT seems to be related to better health outcomes and improved economic situation.

4.2.4 Participation income field experiments

The term participation income stems from a paper by Atkinson (1996) in which he proposes a universal but conditional GMI scheme based on the idea that in return for active participation in paid work, volunteer work, caring or societal activities, people have access to a GMI that is complementary or topped up on the means tested welfare or income replacement benefits people currently receive. It differs from a conditional cash transfer programme such as welfare or public pension because it is universal, it is not means tested and it guarantees a particular minimum income (citizen's income) level that guarantees a minimum of subsistence security and that can be set at any, and hence, also a higher level than the current minimum benefit level. As long as people's income is below the threshold, people can keep their earnings without any benefit reduction. Only for earnings beyond the threshold level, social benefits can be withhold. It therefore creates stronger incentives to work than UBI or the current benefit schemes. Any scheme that provides a GMI and that is conditional on participation can be framed as a conditional basic income or participation income scheme. The idea has never been implemented nor piloted in a field experiment in the UK, but a simulation study by Atkinson et al. (2017) simulated the income and employment effects (see Section 3.1). A Dutch experiment in eight cities that we discuss below might be considered a particular form of a participation income that is, however, not universal but restricted to welfare recipients.

Dutch participation income experiments

The study by Muffels et al. (2021) concerns an RCT field experiment in eight Dutch municipalities with a guaranteed minimum or participation income for unemployed people of 18 to 65 years on welfare during the period 1 October 2017 to 31 December 2019. Six experiments (in the cities of Deventer, Groningen, Nijmegen, Utrecht, Tilburg and Wageningen) were formally acknowledged as an experiment in the framework of the experimentation article 83 in the Participation Act (welfare). Two cities, Apeldoorn and Oss, used the existing room in the Participation Act to design the experiment[13]. Three experimental treatments were designed: a self management group with relaxed work requirements, a tailored extra support group and an earnings release or extra work bonus group for participants finding work or working extra hours. The benefit reduction rate in the last group was reduced from 75-100% to 50% with the same maximum amount of earnings release of €200 per month as in the current legislation. The work bonus is therefore very modest because the implicit marginal tax rate on extra earnings is still nearly 100% for people

[13] A ninth city Amsterdam was not acknowledged formally but started a 3-year long unofficial experiment in June 2018. Because results for Amsterdam were yet not available, this study did not include the experiment in Amsterdam.

working more than 10 hours a week (minimum wage is about 10 euros per hour). The unofficial experiments were formally not allowed to set up the work bonus treatment. The eight experiments were different in design also because some municipalities implemented combination treatments notably of the self management and the extra support group with the earnings release group. They further differ in the target group, content and implementation of the experimental treatments. More than 4,420 people participated in the experiment of which 1,108 people were in the control group and who got the regular Workfare-oriented treatment. The survey consisted of three surveys (based on oral interviews or online interviews), one at the start, one after one year and one at the end after two years. The Dutch questionnaires were shared with the Finnish researchers who hold telephone interviews at the end of their experiment because of which the measures used to assess the non-employment effects were rather similar. The average response rate was 60-70%, with much higher response rates for the first survey (80-90%). For the analysis of the employment effects, the experimental data were linked to population register data and OLS/Logit regressions were applied plus Two-Stage Least Squares regressions with the assignment to treatment used as instrumental variable. For the non employment effects, a similar analysis approach was adopted but with the outcomes at the start for each group as controls in the regression models. Overall, few significant positive effects were found on labour supply. Register data showed positive significant ($p<0.10$) effects on parttime and fulltime employment (working more than 8 hours a week), notably in Utrecht (control group=12.1%) for extra support (+6.7pp) and earnings release or work bonus (+7.6pp). In Apeldoorn, positive significant effects were found for the intensive support treatment (+10pp compared to 12% in the control group). Negative effects were found in Nijmegen and Groningen for exemption and extra support and in Tilburg for extra support without work bonus. Insignificant results were observed in all other cases. Selection effects in Nijmegen and possible experiment effects (so-called "John Henry" effects showing a different behaviour in the control group) in Groningen and Tilburg are assumed to have affected the latter results. No or small effects were found on physical and mental health. Most health effects were positive but insignificant in all treatments. In some cities, negative health effects were found in one of the treatments (Groningen, Utrecht, Wageningen and Oss) but only the effect for the extra support group in Oss was significant. Subjective wellbeing scores were not significantly different from those in the control group in the various treatments in most cities. Only in Groningen and Oss, a negative significant effect was found for one of the groups: the exemption and the extra support group, respectively. Furthermore, positive significant effects were reported on self-efficacy and on social trust and trust in the caseworker (being part of institutional trust). Overall, positive effects were found on self-efficacy except for Oss in the extra support group. For Utrecht, the effects were positive and significant in the exemption and extra support group. Also for social trust, positive effects were found notably for the exemption group in Utrecht, Groningen, Deventer, Tilburg, Oss and Apeldoorn, but in Utrecht also for the extra support group. They were mostly insignificant except for the extra support and work bonus group in Groningen. For trust in the caseworker, in Tilburg and Oss out of the four cites who have provided information, positive and significant effects were found. The study concludes that overall modest effects are found notably on

employment but also on the 'soft' subjective measures of health and wellbeing. The researchers argued that due to small sample sizes positive effects in some cities became insignificant. They also reasoned that the small effect sizes might be caused by the fact that small interventions cannot lead to strong effects or that the effects of exemption, tailored support and earnings release cancel each other out within each of the three treatment groups because for methodological reasons they are not tailored to the specific needs of each individual. For the modest effects on health and wellbeing it is mentioned in the study that changes in these measures needs a longer observation window or time horizon to capture these effects which tend to change only over a longer time frame. The study also mentions the alleged existence of selection and experiment effects which might have affected the outcomes in Groningen, Nijmegen and Tilburg. Following the comments in their study, these issues need further scrutiny in follow-up research.

[About here: Table 9 Micro-economic effects of PI field experiments]

Conclusion on participation income field experiments

The guaranteed minimum or participation income field experiment in the Netherlands is unique because it shares similarity with a basic income in the so-called self-management condition whereas in the extra support and earnings release condition it looks very much to the active support and limited earnings release condition in the B-MINCOME experiment in Spain but also the participation income regime for the UK. The simulation study on the participation income scenario in the UK showed strongly reducing income inequality and poverty figures but also strong disincentives caused by the increase of the marginal tax rates. The Dutch study reported on the employment and health and wellbeing effects but not on the inequality and poverty effects. Overall the Dutch employment effects confirmed the found insignificant effects of the Spanish programme for the active support and limited earnings release group. The Dutch findings show that most labour supply effects were insignificant except in one city (Utrecht) with positive (just insignificant) effects for the extra support group and positive significant effects for part-time work in the earnings release condition. The results for the Dutch self-management group providing more or less an unconditional minimum income were overall insignificant compared to the negative employment effects found in Spain for the unconditional group. In both studies no behavioural (family) labour supply model was estimated nor a social welfare function because of which the labour supply effects have to be considered with caution.

4.2.5 Basic income dividend experiments

Three very special cases of an unconditional and universal basic income (UBI) guarantee in the form of (annual) dividends are discussed here. First, we discuss the Alaska Permanent Fund dividend that from the late 1970s on provides a permanent annual dividend from the wealth fund that is created out of the oil and gas revenues of the State Alaska. Second, we turn to the Indian Tribe dividend that is since 1989 paid out (not necessarily in cash) to the population of the reservations and which money came from the tribal gaming

revenues. Finally, we discuss evidence from Iran's cash transfer programme, that was launched in 2011 and since then continued to distribute oil revenues more equally and to compensate for the sudden removal of energy subsidies.

Alaska Permanent Fund Dividends

Since 1976, the state of Alaska managed a sovereign wealth fund which is financed by license and fee revenues that are generated from the oil and gas extraction industry in the state. Since 1982, the state distributes so-called "permanent fund dividends (PFDs)". The amount varies every year and depends most directly on the APF's net income and expenses.[14] The funds are universal and therefore available to all Alaskan residents. The PFDs were not presented as a (guaranteed) basic income and the annual transfers are too small to fully rely on these funds. Nevertheless, for a moderate-to-large family, the funds can replace a large share of poverty-level income.[15] Receipt of the dividends also does not preclude a family from receiving other safety net benefits such as food stamps or unemployment compensation.

First, we discuss the labour market effects of the PFD. Jones and Marinescu (2018) estimate the extensive and intensive employment effects of the universal unconditional cash transfer in Alaska. By making use of data from Current Population Survey, they estimate a difference-in-differences model based on the synthetic control method. In this way, a weighted average of control states is chosen to best match Alaska for the variable of interest and other observable characteristics before the dividend payments begin. With respect to the employment to population ratio (extensive margin), they do not find a significant effect. However, with respect to the population share of people working parttime (intensive margin), they find a positive increase of 1.8pp. In other words, due to the PFD, there is an increase of 17% in the population share of working parttime. Jones and Marinescu (2018) suggest that the absence of a negative effect on the extensive margin, can be due to an increase in consumption, and in turn labour demand, which subsequently could cancel out potential negative employment effects. Heterogeneity analyses provide some findings in line with this explanation: whereas significant negative employment effects are found in the tradable sector, they seem absent in the non-tradable sector. The authors also estimate heterogeneous effects with respect to gender and marital status, as well as age groups. They find that the increase in parttime employment is entirely driven by married women. Their treatment effect is 3.5pp, whereas for men they find an insignificant treatment effect of 0.8pp. They do not find significant differences across age groups.

Feinberg and Kuehn (2018) analyse the effects of the Alaska PFD on the nonlabour income elasticity of labour supply. To control for unobserved time varying factors, they use a difference-in-differences framework, next to their baseline model. In a first specification, they compare eligible Alaskans to ineligible Alaskans (i.e. Alaskans who do not meet the 1-year residency requirement). In a second specification, they compare eligible Alaskans to residents from other states. A triple difference model is estimated using both comparison groups together. The evidence on the effects of the PFD are relatively

[14] Between 2000-2016, real PDF amounts (in 2016 dollars) varied between $923 and $3,758 per household member.
[15] For example, in 2000 a family of six would have received a PFD payment worth more than 40% of the federal poverty threshold.

stable across the different specifications and across the different measures of labour supply. Therefore, we report only the effects from their baseline models in which they use the number of hours worked (including those not working) as their variable of interest. By this, the average treatment effect they estimate includes both the extensive and intensive margin. The findings confirm the theoretical prediction that the Alaskan PFD has disincentive labour supply effects. Results do turn out to be different across gender and marital status. In the baseline model, single women respond least to the PFD payment, with an elasticity of -0.141. Men follow with an elasticity of -0.153. Married women respond most. They have an elasticity of -0.183. In the difference-in-differences and triple difference models, the elasticities of married women are still most strong, whereas the difference between men and single women is less consistent.

Feiberg and Kuehn (2020) look at the impact of the Alaska PFD on entrepreneurial activity, measured by small firm births (between one to four employees) and self reported self employment. One of the motivations put forward for introducing a guaranteed basic income is to encourage risktaking by offering a safety net. Hence, one could expect the introduction of a basic income to promote entrepreneurial activity. To assess the impact of PFD on small-firm births, Feinberg & Kuehn (2020) conduct two sets of state-level analyses for the period 1977-2014: 1) a before-and-after regression analysis within Alaska, and 2) a full panel analysis for all US states. The before-and-after analysis shows that the implementation of the PFD increased small-firm entry by 26% over the subsequent 32 years. Allowing the effect of the PFD programme to vary over time, the results show that the upsurge in small-firm births was concentrated in the first years after the introduction of the programme. Small-firm entry increased by 21% in 1983, but the growth rate declines to zero after three years. The authors argue that the spike in small-firm entry shortly after the introduction of the programme could potentially be explained by the size of the transfer of the first payment. The level of the first PFD payment was relatively generous and amounted to $1,000 in nominal dollars, a level which was not reached again until 1996. The authors find no evidence of a significant impact of the amount of the PFDs on small firm entry. This finding is not surprising if entrepreneurial activity is affected by the longer-term implications of the programme. In the full panel analysis, the authors only find a significant effect when allowing for changing effects over time. The impact of PFDs yields a 30% increase in the number of small firms in Alaska, although the effect approaches zero within 7 years. These findings are qualitatively similar as the findings obtained from the before-and-after analysis in Alaska. To measure the effect of the programme on self-employment, the authors regress self-employment on a dummy variable indicating whether individuals were eligible Alaskans, and a continuous variable indicating the PFDs real value per person. Consistent with the findings for small firm entry, the introduction of the PFD increased the self employment rate with 0.7pp (i.e. a 9% increase compared to the average Alaskan self-employment rate over the sample period), while the continuous variable shows no significant effect. Therefore, the introduction of the PFD programme increased entrepreneurship, while year-to-year variations in the payment amount had little impact.

We now continue with studies focusing on poverty effects of the PFD as well as income inequality. The first paper looks at income inequality and is from Kozminski and Baek (2017). They analyse the effect

of PFD on income inequality in the Kuznets curve framework (Kuznets, 1955). The idea is that by paying an equal amount to everyone, a decrease in the rich to poor income ratio can be achieved (Humphreys et al., 2007). Kozminski and Baek (2017) use three measures of income inequality: (1) Gini coefficient, (2) relative mean deviation, and (3) Theil entrophy index. For all measures, a higher value means more income inequality, so that positive coefficients reflect a worsening situation. The authors estimate short- and long run estimates via an autoregressive distributed lag approach to cointegration. Results show support for the Kuznets' hypothesis: after a certain point, income growth indeed seems to reduce income inequality. PFD payments seem to worsen income inequality as the coefficients of the PFD payments are positive and significant. This holds both for the long and short run and for all three measures of income inequality. The coefficients on PFD payments in the short- (long-)run in the model explaining the Gini coefficient are 0.09 (0.21). The model explaining the relative mean deviation yields coefficients of 0.06 and 0.13 for the short- and long-run respectively. For the Theil entrophy index, the following coefficients are found: 0.27 (short-run) and 0.36 (long-run). The model also shows significant structural breaks, implying that the Gini index and the relative mean deviation significantly increased since 1985. All in all, Kozminski and Baek (2017) conclude that the PFD payments worsen the income inequality in Alaska in both the short- and long-run. This could be explained by differences in spending behaviour of low- and high-income inhabitants of Alaska; whereas low-income inhabitants seem to use the PFD payments for disposable goods, high income inhabitants typically use this money to invest (Widerquist & Howard, 2012), though evidence for this has not been found (Humphreys et al., 2007).

The second paper deals with poverty effects of the PFD and is from Berman (2018). He follows three steps and starts with the decision to use the US Census Bureau definition of poverty and poverty thresholds. In a second step, he uses 1990-2000 data from the Alaska Long-Form Survey PUMS, and 2005-2015 data from the ACS annual PUMS data. In a third step, Berman (2018) makes use of (suggestive) information about the amount of PFD income reported for each individual, to calculate income with and without PFD income for everyone separately and for families. Berman (2018) shows that the number of people living below the poverty threshold would have been much higher without PFD payments. On average, over the years 2005-2015, income without the PFD would have resulted in over 28% of the people living in Alaska to have an income below the poverty threshold. With the PFD, this percentage is estimated to be 22%. The estimated effect of the PFD is somewhat smaller in more recent years. Whereas in 2000, the PFD resulted in a 46% reduction in the population in poverty, in 2011-2015, this reduction was 22%. Berman (2018) also finds heterogeneous results with respect to household types and age. PFD has caused a larger decline in poverty rates among Alaska seniors and children than what would have been expected to happen without PFD, though PFD is less effective in reducing poverty among children between 2011-2015. The PFD reduces poverty of the working-age population less than among the elderly and children, as their income is less dependent on PFD.

There is one study focusing on health effects of the PFD. Evans and Moore (2011) examine the impact of the PFDs on short-term mortality. Although higher income groups typically show better health

and lower mortality rates (references), the within-month mortality cycle shows that mortality steadily decreases towards the end of the calendar month and is highest during the first days of the month (references). The relatively high mortality at the beginning of the month coincides with the cash transfers that a large share of the population receives during the same period (e.g. Kitigawa & Hauser, 1973; Backlund et al., 1999). Likewise, mortality tends to increase during economic booms and decline during recessions (Ruhm, 2000). The authors argue that the increase in mortality rate could potentially be explained by increases in consumption and economic activity shortly after income receipt (Mastrobuoni & Weinberg, 2009; Shapiro, 2005; Stephens, 2003; Stephens 2006). According to the authors, increased consumption might go hand in hand with an increase in substance abuse, while economic activity might increase the likelihood of traffic fatalities or heart attacks. To assess the impact of the PFDs on short-term mortality, Evans and Moore (2011) apply a difference-in-differences design to compare the change of deaths during the first week after dividend payments are made in the state of Alaska with the change of death in all other states. The estimates are obtained from data for the period 2000-2006 when a large share of recipients received payment by direct deposit (initially, payments were made by paper checks). Compared to the rest of the US, Alaska witnesses an increase of 9.1% for the week in which the funds are received. The increase in deaths in the first week is offset by a decrease in deaths in the subsequent three weeks. This suggests that the increased number of deaths shortly after the income receipt can be explained by mortality displacement. Results for urban areas of Alaska, in which a much larger fraction of the direct deposit recipients live, show a 13.3% increase in mortality during the week in which the direct deposits are made. In contrast to findings for the full sample, there appears to be a net increase in mortality in the four weeks after the deposit in urban areas. Hence, it is not fully clear whether the income receipts cause an increase in net mortality or whether it only results in mortality displacement.The authors also observe whether the cause of death is due to substance abuse. In Urban areas, 8% of the deaths among Alaskan can be attributed to substance abuse, leaving the other causes of death unexplained.

Last, we discuss a paper analysing crime effects of the PFD. Watson, Giettabi and Reimer (2020) use a lifecycle consumption framework to analyze the effects of the dividend payment and its timing on daily accounts of policing events (violence, substance abuse, property crime, requested medical assistance) in the capital city. The data come from daily police records. The evidence on the effects of the dividend payment on crime is mixed with a significant increase in daily substance-abuse (+10%) and police assistance (+9%), but a decrease in property crime incidents (-8%) in the four weeks after payment. They also found an average daily increase in medical assistance calls in these four weeks (+9%). On average, no change was observed in the frequency of violence crimes. On annual level substance abuse increased less (+1.05%) and property crime declined less (-0.61%). Substance abuse elasticities appear four times less large than those of the food stamp program. Daily police activity decreased in the two weeks post-payment which is not offset in the two weeks thereafter resulting in a net decline in property crime. The negative income effect on property crime dominates the positive 'loot' (utility of crime increases because of availability of more cash) effect. The authors also calculated the net social costs of the effects on crime and conclude that these

costs are very small. In terms of welfare gains and losses they estimated the net social costs between -0.17% (social savings) and +1.78% (social expenditures) of the total amount of PFD payments. Overall, the net effects on substance abuse and property crime appear rather modest as are the social costs or welfare losses involved.

Indian Tribal Dividends in the US

Since the introduction of the 1988 Indian Gaming Regulatory Act (IGRA), tribal gaming revenues increased tremendously for American Indian tribes after the passage of the law. This law provided a standardised method for American Indians to establish casino operations on their federally recognized tribal lands. The purpose of the IGRA was to provide tribal governments a source of revenues in an era of declining federal support. The establishment of tribal gaming enterprises created novel economic opportunity for tribal governments. The IGRA stipulates that, unlike privately owned casinos, tribally owned casinos must reinvest net revenues into the tribe to promote economic development. Net revenues can, among other things, be distributed as direct transfers to tribal members in the form of dividends. However, there is large variation in the net revenues gained between tribes and many tribal governments do not provide a cash transfer from casino revenues.

Anderson (2013) examines the impact of the opening gaming facilities on per capita income, family poverty rates, and child poverty rates. Using reservation-level data, Anderson (2013) applies a difference-in-differences framework to compare the outcome measures between 1989 (shortly after passage of the law) and 1998. Changes in the outcome measures of American Indian reservations that started slot-like gaming operations are compared to changes in the outcomes of reservations that did not open a gaming facility during this period. During this period, 64% of the federally recognized reservations in the sample opened casinos. Per capita income increased with 7.4% for American Indians in reservations that opened gaming operations between 1989-1998. Poverty rates dropped with 4.9pp, while child poverty rates reduced by 4.6pp. Anderson (2013) correctly points out that casinos can affect per capita income and poverty rates through changes in wage and non-wage income. On the one hand, per capita income can increase through employment and wage benefits (i.e. at the gaming operation or auxiliary industries). On the other hand, casinos can increase non-wage income through the distribution of cash transfers. As the treatment captures whether a reservation opened gaming operations during the sample period, this study cannot isolate the precise contribution of cash transfers to increases in per capita income and reduction in poverty rates.

A second study on Indian tribal dividends confirms that the presence of casino operations positively affects the economic outcomes of American Indians. In contrast to Anderson (2013), this study attempts to separate out the effect of casino operations from the effect of cash transfers. Simeonova, Akee and Jones (2021) observe the incomes of both American Indians and non-American Indians. Although both groups might experience employment and wage benefits from gaming operations, only tribally enrolled American Indians are eligible for receiving cash transfers. Using panel data for individual families, incomes are ranked within the birth cohort of each tax filer for every year between 1989-2017. The income

rank is regressed on a casino variable which measures whether a casino is operating in reservation *r* in year *t*, and on a cash transfer variable indicating whether a tribe uses casino revenues to distribute cash transfers among tribe members. The regressions also control for a set of time varying characteristics and include year and state county fixed effects. Residing on a reservation with an operating casino increases a family's income rank on average by 0.44 points on a scale of 0 to 100 (almost half a pp), which is considered economically meaningful. In contrast, cash transfers do not appear to significantly affect a family's income rank. Interactions between the reservation resident's race and the casino and cash transfer variables display that the total effect of casino operations on American Indian's family rank (-0.37) is not statistically significant. However, the total effect of cash transfers on American Indian's family rank is significant and equals 1.67. Therefore, we can conclude that American Indian families are economically definitely better off with the cash transfers.

Iran's Cash Transfer Programme

In 2011, Iran started to offer monthly cash transfers to all households. The payments were a compensation for the sudden removal of energy subsidies. Despite the fact that energy subsidies disproportionately benefit wealthier individuals (who tend to consume more energy), they also constitute a larger share of poor people's budget. Hence, the absence of a compensation scheme could lead the poor to oppose the removal of subsidies. Iran's programme was considered an attempt to fulfil a long-standing promise to distribute oil revenues more equally. In December 2010, the government increased prices for bread and energy products and simultaneously released the cash it had deposited in dedicated household bank accounts. Irrespective of income, all households could register for the transfers and open a bank account. Over 95% of the population applied and received the governments transfers which amount to 28% of the median per capital household income (worth about US $90 monthly per person). The monthly transfers boosted the incomes of nearly all households, especially the poor. The poor were more than compensated for higher bread and energy prices. However, the programme amounted to 6.5% of the GDP and also caused deficit spending and inflation.

Salehi-Isfahani and Mostafavi-Dehzoeei (2018) assessed how the introduction of the Iranian cash transfers affected labour supply in the year after the implementation. The authors exploit exogenous variation in the timing of the transfer. All households were eligible to receive the cash transfer if they registered for the transfers and opened a bank account. About 30 percent of the population did not submit their paperwork in time to open the bank accounts before the window for doing so closed. These households were able to register when registration re-opened three months later. Early participants form the control group, while late participants form the treatment group. In a difference-in-differences design, changes in hours worked and labour force participation before and after the introduction of the policy change are compared for the treatment and control group. The treatment effects are estimated separately for men and women. For men, no significant effects were found on weekly hours worked or on labour force participation. In contrast, the cash transfers positively affected the weekly hours worked and labour

force participation of women. Salehi-Isfahani and Mostafavi-Dehzoeei (2018) also estimate a fixed effects model to look at the impact of the intensity of treatment which is their preferred specification. The intensity of treatment is measured as the ratio of cash transfers (minus increased expenditures on energy and bread) to per capita expenditures. Now, the estimated programme impact on hours worked for men is positive and significant, but insignificant and close to zero for women. On the other hand, difference-in-differences estimates demonstrate that the intensity of the treatment had no significant effect on labour force participation. Although Iran's cash transfer programme has been criticized as it might disincentive work, this study finds little support for negative labour supply effects. The estimates are either positive or not significant when negative. One drawback of the study is that the authors do not discuss to what extent the timing of the transfers is truly exogenous, and thus, whether treatment and control group are compabable. Therefore, the results should be interpreted somewhat cautiously.

The impact of Iran's cash transfer programme on household resources, poverty, and inequality during the first three months of the programme is assessed by Salehi-Isfahani and Deutschmann (2015). To examine the impact on household expenditures, the authors descriptively compare expenditures on water and energy with the size of the cash transfers by income decile. The balance was most favourable for households in the lower income brackets. Those in the lowest decile received nearly 13 times more from the cash transfers than they spent on subsidized products, compared to 1.8 times for the richest decile. The effects of the cash transfer on poverty (inequality) are evaluated by descriptively comparing the poverty (inequality) rates before and after the transfer programme was launched. The incidence of poverty decreased from 11.1% in 2009 to 9.7% in 2010. With respect to inequality, the Gini coefficient dropped from 0.46 in 2009 to 0.38 in 2010, suggesting a decrease in equality. In addition, the authors simulate the level of poverty and inequality in a situation without cash transfers and a situation with universal coverage. The poverty (inequality) rate would yield 14.4% (0.47) without cash transfers and 8.2% (0.33) with universal coverage. This study suggests that Iran's cash transfer programme was able to redistribute oil revenues more equally. At least during the first three months of the programme, significant decreases in poverty and inequality were achieved as a result of the cash transfers. Nevertheless, the findings of this study need to be interpreted with some caution due its descriptive nature.

[About here: Table 10 Micro-economic effects of BI dividend experiments]

Conclusion on basic income dividend experiments

The Alaska Permanent Fund Dividend (PFD) evoked a series of studies to view the effects on a wide range of outcomes, such as on fulltime and parttime employment, on poverty and income inequality, on small firm births, on mortality and on crime. The employment effects seem to be providing mixed evidence with positive effects found notably on female parttime employment but not on men's employment and negative effects for men and notably for married women. Positive and substantial negative effects were found for poverty but instead positive effects were found for inequality, meaning a worsening of the level of income

inequality according to various measures and for the short and longer-run. Studies on the impact of PFD on self-employment and small firms births showed interestingly that especially in the first years after the introduction of the PFD programme, the number of firms increased but that the effect fades out in the 7 years after. The effects on crime appeared to be small but positive (crime increasing) for substance abuse and negative for violence crimes. The net social costs in terms of welfare gains or losses appeared also to be rather small. The Indian Tribal Dividend turned out to be a success for the reservations population at large given the rise in cash income and the reduction in poverty rates. Nevertheless, there is substantial variation in income gain across the various rerservations. The total income effect on Indian's family ranking in the US income distribution appeared insignificant. Also Iran's universal cash transfer programme (90 USD dollar a month) raised the living standards of notably the poor Iranian population and reduced poverty rates significantly but at the cost of rising inflation and budget deficits. Overall one might conclude that UBI-like dividend payments lower poverty rates, worsen inequality somewhat, have negative effects on married women's labour participation, increase self-employment in the first years, has small net effects on violence crime reduction and on social costs but might mean a burden for the economy dependent on how it is funded.

5. Conclusion and discussion

A guaranteed minimum income (GMI) either in the form of a basic income (BI), negative income tax (NIT) or participation income (PI) has gained prominence in the public debate on the welfare state in a number of countries worldwide. The current debate about basic income inspired ideas for reforming the welfare state partly stems from concerns about the income and employment prospects of people with insufficient or inadequate skills caused by the alleged impact of automation and technological progress on the labour market. If people with skill deficiencies in the future labour market cannot be adequately educated and their skills upgraded to match the rising skill demands they run the risk of becoming long term unemployed or employed in low-paid insecure jobs also leading to rising in-work poverty rates.

In this last section, we attempt to discuss the main results found in the 48 studies on UBI and PI which we reviewed in this paper, to draw some conclusions and to formulate some welfare state policy lessons. The first idea underlying the review was to compare the outcomes of previous studies in the 1970s and 1980s in Canada and the US which are well-known and already reviewed by others with more relatively unknown recent studies in Europe which to date have not yet been published in the main journals. A second idea was to get a broader picture of the intended as well as the unintended micro- and macro-economic effects of the various programmes and proposals by not viewing only the income (inequality and poverty) and employment effects but notably also the effects on (mental) health, subjective wellbeing, financial stress, self-employment and risk taking, trust, social participation and crime. The third idea was to review also studies dealing with UBI- and PI-like proposals, experiments and programmes which not fulfil all the criteria which are generally formulated for the 'holy grail' of unconditional and universal basic income schemes. We decided to exclude conditional cash transfer programmes which are part of the existing social

protection systems, but also laboratory experiments of which results partly associated with the design and methodology have very limited generalisation capacity or external validity (cf. Paz-Báñez et al. 2020). The scoping reviews by de Paz-Báñez et al. (2020) and by Gibson et al. (2020) also examined the evidence for middle and high income Western countries including Europe. The former review study focused on the labour supply and employment effects whereas the latter reviews the public health effects alongside the employment effects and social outcomes. The findings of the first review covering studies on 50 cases worldwide, in the US, Canada and Europe as well as studies in other continents, was that the labour supply effects of a wide range of UBI, NIT, CCT, UCT and laboratory experiments tend to be positive instead of negative as the original UBI/NIT studies in the US suggested (de Paz-Báñez et al. 2020). The researchers only found some insignificant negative labour supply effects for special groups of people like children, the elderly, the sick and disabled, women with young children and youngsters continuing education. The second recent review study also dealt with employment effects and concluded that employment effects were inconsistent, although mostly small for men and larger for women with young children (Gibson et al. 2020).

Below, we will first discuss the effects on employment and labour supply in Section 5.1. In Section 5.2. we switch to the effects on income inequality and poverty and in Section 5.3 we discuss the health, subjective wellbeing, and other social outcomes. Eventually in Section 5.4 we draw some lessons for social policy.

5.1 Employment and labour supply

UBI and NIT studies

The UBI simulations and UBI-NIT field experiments studies show largely the same results for the effects of UBI-like programmes on labour market participation or employment and labour supply (working hours). Apart from some small UBI experiments like the small local Stockton UBI experiment (125 people) and the Ontario UBI pilot (1,000 people) reporting positive effects on employment, the effects on labour supply are mixed or inconsistent. Overall, the labour supply effects are negative, but rather small or even insignificant for men but larger negative for married women with young children. There is a negative substitution effect for men caused by the high marginal tax rates on earnings and a stronger negative income effect notably for married women caused by the GMI income allowing these women to spend more time on caring. The older UBI/NIT studies in the 1970s and 19080s show substantial negative effects notably in the US and less so in Canada, but recent studies re-estimating the original effects show that a number of methodological issues should be held responsible for the large effect sizes. Partly, these issues are *technical* and have to do with the sampling, underreporting and the design of the RCT experiments. The negative effects become much smaller or even insignificant when the results are corrected for selection bias (stratified random sampling by pre-transfer income), correcting for underreporting of earnings in survey questionnaires by using register employment data and controlling for the duration in the experiment (three and five-year durations) and the combination of treatments (UBI/NIT combined with job training) by estimating effects separately for different durations and for single and combined treatments. Also non-

parametric re-estimates turn out to lead to smaller negative labour supply effects than the original parametric calculations.

In the case of a universal budget-neutral NIT-reforms funded with a flat-rate tax, the resulting high marginal tax rates (notably, when the NIT is 100% of the poverty line level) might evoke strong labour supply responses. A diffence-in-differences approach does not take account of these labour supply responses whereas a structural labour supply model does. The marginal tax rate is dependent on the benefit withdrawal rate of earnings and varies along the income distribution (kinked budget lines) because of which labour supply effects differ at different levels of income. Because marginal tax rates are lower with NIT than with an UBI (=100% below the poverty threshold), the labour supply effects are ceteris paribus smaller and negative. Taking account of behavioural labour supply effects will therefore lead to smaller negative employment effects in the case of NIT than in the case of UBI.

But also the chosen theoretical framework and empirical model specification matters. The choice for using an individual or family labour supply function and for using an indirect utility or social welfare function whilst accounting for cross-wage elasticities between the partners matter for the estimated labour supply effects. One study shows that using a family labour supply model and a social welfare function with cross-wage elasticities yields positive labour supply effects for women in the lower income deciles instead of the strong negative effects found in the original studies. In the view of the researchers, labour supply is not necessarily negative because the higher marginal tax rates go along with lower average tax rates because of the tax receipts from the extra earnings of low income people. The net effect in the end depends on the simultaneous labour supply choices of the partners and working hours constraints notably for females. The study also showed that an NIT reform might yield net welfare gains when the negative income and welfare effect of the female partner will be compensated by the positive income and welfare effect for the spouse who increase working hours to compensate the reduced working hours of the female partner. It suggests that a family labour supply model instead of an individual labour supply model might provide a more realistic picture of the net effects. In addition, in terms of utility, the disutility of negative labour supply effects for women, notably with young children, should be weighted with the utility of extra time for caring and leisure. In other words, the extra time available for caring might lead to utility or welfare gains for the person and society at large which compensate the welfare losses due to notably female partners with young children working less.

Partial UBI-NIT experiments in Europe

The employment results for the BI RCT experiments in Europe, such as the partial UBI experiments in Finland and Spain (so-called B-MINCOME) appear rather different from the original US UBI-NIT experiments. Either insignificant or very small positive labour supply effects are found in Finland in the first year (the results for the second year cannot only be attributed to the treatment). In Spain, small negative labour participation effects are found overall, except for two conditional treatments, the active support and the earnings release group, for which the results are insignificant. These studies use a difference-in-

differences approach without testing a structural labour supply model for which reason the effects need to be taken with caution.

Basic Income dividend

Studies on three very special experiments with a so-called partial basic income dividend showed more or less similar employment results but with smaller sized effects because the dividend amount - funded by the revenues of natural resources (Alaska's Permanent Fund), Casino's revenues (Indian Tribe Dividend) or just taxes (Iran's income transfer) - is much smaller than in the case of a full basic income. Again the employment effects are mixed, one Alaska study found a strong increase in part-time work of women and insignificant effects for men whereas another Alaska study found negative effects on the labour supply of men and notably of married women. Also the Iranian cash transfer programme studies using a difference-in-differences approach provide mixed results but no conclusive evidence on negative labour supply effects. In all studies, a difference-in-differences analysis was conducted without accounting for labour supply effects in response to increased marginal tax rates. Since the costs for funding the programmes are low and covered by special funds or revenues, the impact on tax rates will be low as will therefore be the impact on labour supply.

Participation and Conditional Basic Income

The simulation and experiment studies on participation or conditional basic income (CBI) provide the same mixed picture as for the UBI/NIT studies. The conditionality pertains to a work or to (market) income or earnings requirements. Again, mixed evidence was found in both the simulation and field experiment studies associated with the specification of the labour supply and social welfare function. A simulation study in the UK based on a static simulation model (EUROMOD) comparing a PI, a more generous social insurance (SI) and a minimum wage increase reform, noticed that a PI reform might lead to higher marginal taxes and reduced labour supply without providing estimates. In a Canadian simulation study with an individual labour supply function and no social welfare function, the disincentive effects of the higher marginal tax rates are high whereas in a Spanish simulation study using a family labour supply and a direct utility or social welfare function the labour supply effects are nearly absent. A flat-rate tax of 46% combined with a conditional BI provided the best trade-off between efficiency and equity independent of the level of inequality aversion in a country. The social welfare gains of a higher BI compensates for the efficiency losses of a higher tax rate. We only found one recent PI field experiment in eight cities in the Netherlands showing overall insignificant labour supply effects in most cities for most treatments, except for two cities with positive significant employment effects (working 8 hours or more) with earnings release and intensive support in the city of Utrecht and intensive support in Apeldoorn. Also in the B-MINCOME experiment in Spain we found these two groups providing better (or less worse) employment results than the unconditional groups.

Impact on self employment

Some experimental studies, specifically the BI pilot in Ontario, the BI experiment in Kenya and the permanent BI dividend in Alaska mentioned the impact of these programmes on self employment, firm births and entrepreneurial activity and risk taking. The findings from the Ontario pilot study are based on the survey data (without correction for attrition) and showed that more people in insecure labour positions moved into self employment but also more people who got employed in fulltime jobs during the experiment moved into self employment. But also sound studies on the BI experiment in Kenya (before and during Covid) and the dividend in Alaska showed that more people started a non-agricultural business (without reducing employment in the agricultural sector) and that more people took up small scale entrepreneurial activities by starting a firm but only in the first years after the introduction of the dividend.

Impact on the macro-economic context, regional labour market, wages and labour demand

Micro-economic studies estimating the employment effects of these BI programmes generally neglect the effects on the labour market and macro-economic context even when they take the labour supply behavioural effects into account. Some of our simulation studies used a micro-macro empirical model in which they consider the micro and macro-economic effects simultaneously. Most studies only look at the impact on tax receipts and the budgetary costs of these programmes. Only a few studies also look at the impact on economic growth, and therewith on employment, or on wages and labour demand. Only one UBI-like simulation study examined the contextual effects whilst accounting for the impact of behavioural labour supply responses and technical change on economic growth and employment. The finding of this simulation study is interesting as it shows that spending money on UBI, funded by a 40% capital tax rate (affecting savings and therewith growth), is less efficient and growth jeopardising than spending the same amount of money on savings from wages which are next invested in robots or technical progress to stimulate growth. In this study, a wage-savings investments of 10% of the earned wage is optimal to maintain growth as well as to increase the wage share and reducing wage inequality. Some other experimental studies point to the macro contextual effects of the inrroduction of a BI on a large scale such as reduced stigmatisation of the unemployed, reduced overemployment, increased wages due to reduced labour supply and engagement in self-employment or enrolment in education, which are often neglected in micro-economic research. For the Mincome experiment in Canada, one study calculated the context or community effect to be 30% of the total negative labour supply effect but we found little evidence from other studies.

Final remarks

Our review confirms generally the findings of the earlier mentioned review studies showing mixed and inconclusive evidence on the alleged negative labour supply effects of UBI-NIT schemes. Our review in addition also shows that the results are very much affected by the design of the study, the data used (survey or register data) and the methodology used (family labour supply, social welfare unctions). Eventually, we

find that NIT and PI schemes perform best with respect to labour supply outcomes compared to full or partial UBI schemes. The reason is that the NIT-PI schemes yield lower costs and therewith lower marginal tax rates and hence, stronger work incentives for low income people, which are also associated and dependent on the benefit withdrawal rate or the provided work incentives and rewards. Studies on the field experiments showed that wage subsidies, earnings release and extra support treatments create the best employment outcomes. However, more research is needed into the tradeoffs between efficiency and equity with a view to the access of people with health and/or educational impairments to parttime and fulltime work. At the same time, more research is needed into the contextual effects of BI and PI related reforms on wages, labour demand, job matches and self-employment also with a view to the consequences of technical change for job displacement and job creation.

5.2 Income inequality and poverty

The findings on income inequality and poverty are much clearer and more consistent than the employment findings and also less challenged for methodological reasons. The results of all types of studies, simulations or field experiments point in the same direction whatever measures are used. The studies utilise a wide range of income inequality indicators varying from Gini, the log mean deviation, to Atkinsons's and Foster's inequality index in which the level of inequality aversion is incorporated. For poverty, in most cases, a relative poverty threshold is used based on a certain fraction of the mean or median of equivalent household income in a country. In other cases, also absolute poverty thresholds are used such as the Market Basket Measure in Canada. The effects on reducing inequality and poverty can be substantial especially in the case of a full UBI, but also in the case of an NIT. In most cases, the estimates on income inequality and poverty are based on a static micro-simulation model without taking account of behavioural labour supply effects. The effects can therefore be biased and overstated when labour supply effects would be negative and more people would loose their job or would reduce working time and therewith see their incomes decline or enter poverty. The more generous the minimum income guarantee is, the more poverty and inequality is reduced but this comes at a cost. The higher the benefit is, the higher the budgetary costs and the higher the tax rate needed to fund the scheme leading to reduced labour supply and employment. These labour supply effects are however less negative in the case of an NIT compared to a UBI scheme because of the extra work incentives and extra tax receipts of low income people working more. A few studies taking the endogenous labour supply effects on board showed that poverty and inequality are even more reduced because of the lower marginal tax rates for low incomes which result in higher net wages and more work and hence more income and less poverty. A study comparing the inequality and poverty effects of UBI, NIT and PI reforms, shows that the NIT and conditional BI or PI programmes at 50% level of the poverty threshold perform best in balancing efficiency (employment) and equity (inequality and poverty) concerns. Programmes at 100% level reduce poverty and inequality more but also labour supply.

The studies on the full UBI RCT experiments in India and Kenya also confirm that a BI programme cannot only promote wage and self employment but especially also exert positive effects on reducing

poverty. The studies on partial basic income field experiments such as the one in Finland showed that the material living standards of the people involved went up significantly because of which poverty is likely to be reduced. The PI simulation study in the UK showed that poverty and inequality is substantially reduced but without taking the endogenous labour supply effects into account. The study on the only PI field experiment in the Netherlands, provided no information on the effects on poverty or inequality. The treatments, except for the earnings release treatment, had little effect on people's income because the work bonus was not very generous. The BI dividend programmes all had positive effects on people's incomes, thereby reducing the likelihood of poverty. However, one study for Alaska's dividend showed positive effects on inequality, i.e. worsening of inequality due to different spending behaviour of high and low incomes (investments versus consumption).

Final remarks

These findings show that more research is needed in which the effects of BI and PI programmes on income inequality and poverty are integrated and balanced with the effects on labour supply by also estimating labour supply and social welfare functions to arrive at a total picture of the net effects. Micro-macro simulation studies might provide better insights into these equity effects taking also account of the consequences of technical change for the inequality on the labour market in terms of access to employment, wages and careers.

5.3 Health, wellbeing and other social outcomes

Eventually, we discuss the findings on health, subjective wellbeing and other social outcomes. These effects have not been covered in the simulation studies but are dealt with in some of the full or partial UBI-NIT field experiments, the Dutch PI field experiment and in one of the BI dividend studies. The studies on the UBI RCT field experiments in India and Kenya showed some significant positive effects on health and wellbeing. The India experiment in 20 villages (8 treatment) showed substantial lower odds of any illnesses in the last 3 months that needed treatment in the experimental group of villages than in the control group. The RCT UBI experiment in two of the poorest regions of Kenya showed that people in the experimental groups with short-term or long-term transfers had a significant lower chance to be ill or having any hospital consultations than people in the control group. Also objective wellbeing (hunger) and depression scores were lower in the two experimental groups compared to the control group. In the partial UBI RCT experiment in Finland some positive significant effects were found on health and wellbeing in the survey data, showing that more people in the experimental group rated their health as good or very good and less people reported having a disease, disability or mental disorder or felt down or sad while more people perceived the ability to cheer up than in the control group. Also subjective wellbeing scores were significantly better in the treatment group. In the Dutch participation income experiment, no significant effects were found on health and subjective wellbeing overall. In the Spanish partial basic income (B-MINCOME) experiment no significant health effects were found but all (subjective) wellbeing scores were

significantly higher in nearly all treatment groups whereas material deprivation scores were significantly lower except in the unconditional SMI group and the active support group. Also in the small Stockton UBI experiment the mental health scores were significantly better in the treatment groups. Positive health effects were also found in a study re-examining the survey data of the Canadian Mincome experiment viewing significant positive health effects (hospital separation rates due to accidents and injuries or to mental health issues) in Dauphin compared to a carefully matched control group of residents in Manitoba. Before the experiment, the health conditions were significantly worse in the treatment group in Dauphin but at the end of the experiment, no significant health differences were found anymore with the control group in Manitoba. Also in the Ontario Basic Income Pilot study, the survey data analyses (raw data without controlling for attrition) show that participants reported better scores on their and children's general and mental health conditions at the end of the experiment than before. Treated people also show healthier behaviour with respect to tobacco and alcohol use, physical exercising and nutritious food consumption. People's perceived self-confidence and their outlook on life improved as well for the majority of the treated. Finally, one study on the Alaska dividend dealt with the effect on health and mortality rates and showed that mortality rates increased shortly after the income transfers are received and people spend the money on consumption or increase their economic activities. Increased consumption might go hand in hand with substance abuse and economic activity with traffic accidents or heart attacks. The increase in deaths in the first week after income receipt is however partly offset by decreases in the three weeks after.

Overall, the evidence on subjective wellbeing is mixed with positive effects in some European experiments but no effects in others. For health, positive and meaningful effects are found notably in field experiments in low and middle income countries. Whereas positive (mental) health effects are also found in some high income countries, small or no (significant) health effects were found in some European field experiments. A recent review on 27 studies worldwide (RCT and quasi- experimental data) came to a similar conclusion: "Evidence on health effects was mixed, with strong positive effects on some outcomes, such as birthweight and mental health, but no effect on others (Gibson et al. 2020, p. 165).

Eventually, the effects on other social outcomes, such as volunteering and social participation, social and institutional trust and crime, have been dealt with in some of the field RCT experiments and in some of the BI dividend studies. Survey data analyses in the Finnish UBI experiment (controlled for attrition) showed that the (former unemployed) participants got more trust in the municipality and the case-worker. Similar results on increased trust levels in the case-worker in two of the four cities for which data were available were found in the Dutch PI experiment with unemployed welfare recipients. In that study, also positive but insignificant effects on social trust were found notably for the exemption group in Utrecht, Groningen, Deventer, Tilburg, Oss and Apeldoorn, but in Utrecht also for the extra support group. Only in Groningen the effect was significant for the extra support and work bonus group. In the Spanish B-MINCOME experiment, only small effects were found on social outcomes such as on social participation and receiving social support but only in the conditional active support group, while social volunteering decreased in the unconditional BI treatment. Effects on crime were studied for the Alaska Permanent Fund

programme and reducing net effects were found with only small (but significant) positive effects on substance abuse and police assistance but larger negative effects on property crime due to the higher income receipts.

Final remarks

The effects of GMI programmes on health and wellbeing and other social outcomes is much less studied in the literature than the effects on employment and income. The evidence stems mostly from survey data analysis in RCT UBI-NIT field experiments. The survey data suffer in most cases from quite high levels of attrition that might be selective if the attrition correlates with the outcomes. Controlling for attrition is partly possible with the information contained in the survey. However, controlling for selection at or before the start of the experiment is not feasible because of which the found effects are not necessarily measuring the true causal effects. Information from register data before the start of the experiment might resolve this. Apart from studies viewing the broader effects on physical and mental health, also studies looking at the heterogeneity of labour market effects by investigating the interaction effects of relevant employment outcomes with (mental) health conditions are needed.

5.4 Lessons for social policy

What can be learned from this review study for welfare state policy? Below, the first lessons we have drawn from these findings are summarized in five bullet points:

- Experimenting with different policy reforms in RCT-field experiments is a challenging and very valuable way to test the effects of alternative policies. The design, data collection and methodology of these experiments need careful consideration to assure that true causal effects are measured. Attrition, underreporting, selection and experiment effects impact the estimated effects and need scrutiny in future experimental research.
- For reducing inequality and poverty, full or partial UBI-NIT or PI reforms seem to be very effective. The more generous the minimum income guarantee is, the more poverty and inequality is reduced but this comes at a cost, because the higher the benefit is, the higher the budgetary costs and the higher the tax rate needed to fund the scheme leading to reduced labour supply and employment. These labour supply effects are however less negative in the case of a NIT and PI compared to an UBI scheme because of the extra work incentives and extra tax receipts of low income people working more due to the reduced marginal tax rates for low incomes. The NIT and conditional BI or PI programmes at 50% level of the poverty threshold seem to perform best in balancing efficiency (employment) and equity (inequality and poverty) concerns.
- We find mixed and inconclusive evidence on the labour supply effects of UBI-NIT reforms, but the findings suggest that NIT and PI schemes perform best with respect to labour supply outcomes compared to full or partial UBI schemes. The reason is that the NIT-PI schemes yield lower costs and therewith lower marginal tax rates and hence, stronger work incentives for low income people,

which are also associated and dependent on the benefit withdrawal or claw-back rate or the provided work incentives and work bonus. Studies on the field experiments also showed that wage subsidies, earnings release and extra support treatments create the best employment outcomes.

- The effects of guaranteed minimum income experiments and reforms on health and wellbeing and other social outcomes is much less studied in the literature than the effects on employment and income. Overall, positive health effects were found in various recent studies, on physical health (illnesses, health care use, hospitalisation), and notably also on mental health (financial stress, depression). Especially in low and middle income countries, a UBI grant or dividend even at rather low levels seem to improve the wellbeing and health of the people involved. In high income countries the effects on reducing (mental) health are ambiguous and deserve attention and further scrutiny.
- More research is needed into the tradeoffs between efficiency and equity with a view to the access of people with health and/or educational impairments to parttime and fulltime work. At the same time more research is needed into the contextual effects of BI and PI related reforms on wages, labour demand, job matches and self employment also with a view to the consequences of technical change (robotization, machine learning, AI) for job displacement and job creation.

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