A Critical Review of the Literature on UBI Experiments

By

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PLAGIARISM DECLARATION

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Abstract

A universally guaranteed basic income is said to be a disarmingly simple idea that could help solve the problems associated with economic insecurity. However, sceptics about this idea argue that it is too simple and that despite best intentions, could have adverse results. These possible adverse results include spreading the welfare revenue too widely and too thinly to have a meaningful effect on the poor who need it the most; demotivating people from working, thus expanding poverty and welfare traps and rendering the transfer unsustainable. While politicians and policymakers have been arguing about the merits of this idea, economists and other social scientists have run experiments to help them understand the mechanisms of responses to a guaranteed income and to be able to advise policymakers. While these experiments have been reviewed separately in the literature, they have not been collated and analysed jointly in a systematic manner.

These experiments have mostly been in the form of field experiments which have focused on testing the effects of the two main models of delivering a guaranteed income, that is a Negative Income Tax (NIT) and a Universal Basic Income (UBI). While guaranteed income experiments span over half a century and across the world, in both developed and developing countries, only a handful of these meet the criteria of being universal, unconditional, periodic, individual cash transfers. In this paper, we have sampled experiments that either fully met these criteria or fell at least one element short. We critically evaluate how they were designed, carried out, the inferences drawn from the observations they generated, and the validity of those inferences.

The experiments reviewed show that a UBI is efficient at reducing exclusion errors; improving welfare outcomes such as poverty, education, and health; and has a gender equalising effect both in households and in the labour market. However, the findings on the effects of a UBI on economic inequality still need further testing. The studies also show that contrary to expectations, a guaranteed income does not incentivise people to choose more leisure, instead they spend more time in education and training, take up care work or pursue entrepreneurial opportunities. We then recommend some aspects of a UBI that require further testing. These include funding mechanisms, the optimum amount and tenure of the guaranteed income, and the consumption behaviour of poor UBI recipients. Lastly, we recommend design imperatives and methodologies for future experimentation. While most of the experiments have been carried out in the field, we make a case for the use of lab experiments to better study the mechanisms of a guaranteed income.

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Abbreviations

AFDC	Aid to Families with Dependent Children
ALMP	Active Labour Market Policy
BIEN	Basic Income Earth Network
BIG	Basic Income Grant
ECLAC	Economic Commission for Latin America and the Caribbean
ESS	European Social Survey
GAI	Guaranteed Annual Income
GDP	Gross Domestic Product
Hh	Household(s)
MPUCT	Madhya Pradesh Unconditional Basic Income
NAMTAX	Namibian Tax Consortium
NIT	Negative Income Tax
NWRO	National Welfare Right Organisation
OBIP	Ontario Basic Income Pilot
ODSP	Ontario Disability Support Program
OECD	Organisation for Economic Co-operation and Development
OEO	Office of Economic Opportunity
PDS	Public Distribution System
REC	Real Economy Currency
RIME	Rural Income Maintenance Experiment
SEWA	Self-Employed Women's Association
SIME/DIME	Seattle-Denver Income Maintenance Experiment
SMI	Municipal Inclusion Support
TIPEEG	Targeted Intervention Programme for Employment and Economic Growth
UBI	Universal Basic Income
UCT	Unconditional Cash Transfer
UNDP	United Nations Development Programme

Introduction

Confucius once said, the easiest way out is through the door. While this idea sounds simple enough, Confucius later laments that very few people ever use this method. A universally guaranteed basic income has become one of the most debated policy ideas of our time. Supporters of a UBI come from all walks of life, cutting across religious, political, ideological, and geographical backgrounds. They include Archbishop Desmond Tutu and Pope Francis, Andrew Yang and Jeremy Corbyn, Thomas Piketty and Yanis Varoufakis, Johann Rupert and Elon Musk. While the idea of a guaranteed income started in ancient Greece, it was only in the second half of the 20th century that most countries have started giving it serous policy consideration. This interest in UBI is a product of the dissatisfaction with the targeted, heavily bureaucratic, paternalistic, and inadequate social security regimes around the world, the eagerness of finding more efficient alternatives, and the growing optimism about using direct cash transfers to tackle economic insecurity. Proponents of a UBI believe that the easiest way to fight the interacting cluster of poverty, inequality, unemployment, and the growing problem of income insecurity is to give cash to everyone, unconditionally (Standing, 2017; Widerquist, 2005; Van Parijs, Cohen & Rogers, 2001).

It can be said that the main goal of development economics is to improve people's livelihoods by raising their incomes, particularly the poor. As a result, development funding is often devoted to addressing poverty, nutrition, health, and education related problems (Banerjee, Niehaus & Suri, 2019). However, these issues, as important as they are, may not always be the most pressing issues that their intended recipients must deal with. This raises a paternalism problem, where policy makers and donors worry how the poor will use extended grants and charity. Additionally, giving people money may not always be the most efficient way to help them out of insecurity. Banerjee, Niehaus & Suri (2019) argue that poor people may be hindered by access to markets, insurance, and credit from escaping poverty.

Economists have resorted to experiments to collect empirical data on the behaviour of economic agents under controlled institutional environments. The experiments are used to test, modify theory, and to collect data and evaluate policies. Since the late 1960s economists have been using experiments to investigate the impact of a guaranteed income. These experiments act as test beds or economic simulations, allowing experimental subjects to make economic choices with real economic consequences. In this way, economists can investigate important aspects of a guaranteed income and its mechanisms.

There are two major models of a guaranteed income, Negative Income Tax (NIT) and Universal Basic Income (UBI). The former was first tested in the late 1960s in Northern America. These experiments focused more on the labour market effects of an NIT. These experiments died out in the late 1980s due to perceived adverse effects of the guaranteed income on the labour market participation and marriage dissolution, these effects are discussed in Chapter 3. UBI experiments started more recently in 2008. These experiments happened in both developing and developed countries. The experiments in the developing countries were mostly based on the UBI model, while those in the developed countries were mostly based on the NIT model.

This paper will (1) review economic, historical, philosophical literature on UBI, and (2) analyse field experimental designs to investigate behavioural responses to UBI mechanisms. The idea is to contribute to a growing discussion on the merits of a UBI. While most studies have focused on the results of UBI experiments and pilots, this paper takes a step back and surveys the design of the experiments and pilots to assess the reliability of their inferences. Chapter 2 summarises the theory of a universal unconditional basic income, its philosophical underpinnings, and the history of its experimentation. Chapter 3 summarises the first wave of basic income experimentation. Chapter 4 summarises partial experiments that were conducted after the first wave experiments but that do not qualify as universal unconditional income experiments. Chapter 5 summarises contemporary UBI experiments. Chapter 6 finishes off with a discussion and Chapter 7 closes with a conclusion.

Chapter 1: Historical Background of Guaranteed Income

The idea of a guaranteed income has a long history dating back as far as ancient Greece. Over the years this idea has evolved, adapting to the prevailing political environment. Below is a brief history of this evolution. This chapter highlights some major milestones in the development of a UBI as a political economy theory and as a public policy mechanism.

The First Wave

The concept of a guaranteed income dates to 461 BC in ancient Greece. According to Standing (2017:10) Pericles and Ephialtes introduced the idea of paying citizens for participating in jury services. However, while they viewed this form of civil participation as a moral duty, receipt of the citizens income was not conditional on participation. Centuries later, in 1516, Thomas More proposed the idea as a solution to the problem of food theft. More believed that giving poor people money for subsistence would be a more astute way of fighting theft as opposed to sentencing thieves to death (More, 1516 in Standing, 2017). Later, Johannes Ludovicus Vives (1526) developed More's idea into a detailed public policy proposal with a comprehensive argument based both on theological and pragmatic considerations (BIEN, 2020). Vives' proposal formalised the idea of publicly funded assistance to the poor rather than relying on discretionary charity from the church and the rich (Standing, 2017). Over the years this idea was developed by many others including Condorcet (1794) and Paine (1796) who advocated for an unconditional once-off grant, and later, Charlier (1848) and Mill (1849) who also proposed an unconditional basic income. After the First World War, Russell (2004) proposed a vagabond's wage, which was a small basic income sufficient for necessities but not luxuries. According to Russell (2004) a basic income is necessary for the creative mental activities that are the basis of art and science. A larger income would be payable to those who engaged in some work that the community recognised as useful.

The Second Wave

The primary aim of these experiments was to test whether a guaranteed basic income would make people work less. The basic income was based on Friedman's (1960) NIT model. While the experiments showed positive results for poverty alleviation, they were not scaled up due to their perceived work disincentive effect. The design and findings of these experiments will be discussed in Chapter 3, however, for now it is important to note that the criticism of how these experiments were conducted led to the use of field experiments in policy design. What came out of these experiments were debates not only on the findings alone but the way these findings were drawn. These experiments were the world's first major social science experiments and for this reason they have been called experiments in how to conduct experiments (Widerquist, 2005).

The Third Wave

In 1986, in response to the emergence of mass insecurity, rising inequality as well the threat of labour displacement by robotics, automation and artificial intelligence, a group of economists, philosophers and social scientists established the Basic Income European Network which was later renamed the Basic Income Earth Network (BIEN). This organisation stipulated the core elements of a UBI, i.e., an individual, unconditional, universal, periodic cash payment. This organisation was responsible for the renewed calls for guaranteed income by economists and political commentators in Europe and the North America. These peaked after the 2007/8 Global Financial Crisis, which acted as a catalyst to wide range support for a UBI (Standing, 2017). Soon after this, but not directly because of it, the first UBI experiment was carried out in Namibia. This experiment marked a second wave of guaranteed income experimentation. These experiments were based on the two models of paying out a guaranteed income, i.e., a NIT and a demogrant, with developed countries mostly testing the NIT model and developing countries testing the demogrant model.

The Rise of the Precariat

The twentieth century was a period of huge economic growth, as reflected in the rise in global incomes. However, according to Standing (2011:26) this was driven by a global transformation which was characterised by the commodification of all aspect of life. These aspects include family, education systems, labour institutions, social protection policy, unemployment, disability, occupational communities, and politics. Standing (2011) argues that this was a result of the dislocation of the economy from the society to create a global market economy based on competitiveness and individualism. This has resulted in a new social class, the precariat. According to Standing (2011), flexible labour market relations is the main cause of global precariousness. He argues that the rise of the precariat was based on the weakening of numerical, functional and wage security. Numerical insecurity resulted from the weakening of employment security, as governments competed to weaken labour protection to attract and retain foreign capital. Functional insecurity resulted from the strengthening of managerial prerogatives to shift workers between tasks, positions, and workplaces without incurring additional costs. Lastly, the restructuring of social income has resulted in the widening of wage gaps between regular and precarious workers. Since employer contributions such as medical insurance, paid holidays and subsidised transport and housing constitute a large share of labour costs, firms have resorted to offshoring, outsourcing, or converting their regular staff into non-permanent staff who do not qualify for such benefits. As a result, the precariat tend to have the following characteristics.

- They live with existential insecurity, occupying unstable jobs with no occupational narrative throughout their careers.
- They perform a lot of unremunerated and unrecognised work, which they feel compelled to do.
- They are overqualified and underpaid.
- They rely on money wages, without access to non-wage benefits like paid vacation and medical leave.
- They are always on the verge of falling into unsustainable debt.

- They have no political representation as a class and thus their civil rights are constantly eroded.

As a result of these issues, a growing number of people do not earn enough to escape poverty and income insecurity no matter how hard or long they work. Standing (2017) argues that a UBI can help minimise this burgeoning precarity trap.

The COVID-19 Pandemic as a Catalyst

While deliberations on the prospect of guaranteed, basic income have been in policy discussions since Nixon's Family Assistance Plan in the 1960s, the COVID-19 pandemic has presented perfect storm conditions for an expedited global discussion on the prospect of income security. Across the world, the idea of an emergency basic income has been advanced as a leading proposal to counter the most severe effects of the pandemic. For instance, ECLAC (2020) made a call to Latin American governments to guarantee temporal cash transfers to help households meet their basic needs and to also encourage the consumption that is required to recover from the economic impact of the pandemic. The UNDP (2020) also proposed a short-term basic income for poor and vulnerable people to provide shock-resistance in developing countries.

While there has been an overall increase in social protection and assistance interventions in response to the pandemic, most of these interventions have been concentrated in high-income countries. Between March and July 2020, the number of social protection and assistance programs increased from 103 to 1,055 across 200 countries and territories (Gentilini et al, 2020). A third of these programs are non-contributory cash transfers benefiting 15 per cent of the world's population. However, in this same period, the total expenditure by low and middle-income countries on these programs amounted to USD 77.9 billion, or 13.2 percent of the global total of USD 589.6 billion. In per capita terms, low to middle income countries are spending an average of USD 7 on social assistance or USD 9.5 if social insurance and labour market programs are included, compared to the averages of USD 121 or USD 123 recorded by high-income countries (Molina and Ortiz-Juarez, 2020).

Also, besides the financial constraints faced by low to middle income countries, Molina and Ortiz-Juarez (2020) suggest that one of the major challenges hindering developing countries in implementing a short-term basic income is lack of political will. This lack of political will is coupled with lack of strong empirical evidence on the impact of a basic income. The resulting the lacuna is filled with speculations on the impact of the basic income on the labour market, uncertainty about the appropriate funding mechanisms and misplaced paternalism about what poor people need and should be given.

Public Policy and Experimentation with Guaranteed Income

The interest in a guaranteed basic income did not start with the pandemic. In fact, according to Stanford Basic Income Lab (2020), in the past 60 years, 16 basic income schemes, pilots and experiments have been completed, and another 15 are still on-going. These programs and experiments started with the North American NIT inspired experiments in the late 1960's. The experiments were spurred by the recognition that despite the growing economy and job creation that came with it, the economic expansion, was not trickling down sufficiently to eradicate poverty. The US and Canadian governments conducted the pilots to test the impact of a guaranteed minimum income on people's willingness to work. These experiments ended in the early 1980's but their findings were never taken up by their respective governments, due in part to political interference with how the experiments were designed, analysed and understood (Munnell, 1986).

Despite the failure of the NIT experiments to provide sound and persuasive evidence, the seeds for an unconditional income were planted and would start sprouting in other areas of the world. In 1982, the state of Alaska started paying out an unconditional social dividend to all its citizens. The scheme was funded by the Alaska Permanent Fund, which is a state-owned investment fund financed by oil revenues (Goldsmith, 2002). The dividend, which is paid out once a year, fluctuates according to the profits generated by the fund, is paid out to all Alaskan citizens, without any means-testing or behavioural conditions. The only condition to qualify for the dividend is that the beneficiary must be domiciled in Alaska. This scheme has been viewed as a natural experiment for an unconditional income.

In 1996, in North Carolina, the Eastern Band of Cherokee Indians opened a casino in the Great Smoky Mountains region and promised to pay out a dividend of the casino profits to all 15000 members of the tribe (Lapowsky, 2017). This coincided with the Great Smoky Mountains study which was a study of child psychiatric epidemiology that began in 1992 which sought to study the need for mental health services in the rural American area. The coincidence of the study with the casino social dividend became an opportunity for another natural experiment to study the effect of the guaranteed income on child mental health.

In 2011, the Islamic Republic of Iran implemented a nationwide basic income scheme. Prior to the scheme, the government used proceeds from the oil sector to fund government expenditure and subsidies designed to reduce the prices of fuel related products. In 2008, President Mahmond Ahmedinejad's new government resolved to phase out the price subsidies, replacing them with a more direct cash transfer to Iranian households (Tabatabai, 2012). This *de facto* basic income scheme is the first and only nationwide universally guaranteed income scheme, benefiting 96% (72.5 million people) of the Iranian population (Tabatabai, 2012). The Iranian basic income is also a relatively generous scheme, accounting for about 15% of GNI in 2011.

Perhaps partly attributable to these natural or accidental experiments and the establishment of BIEN, the interest in basic income experiments surged in the late 2000's, this time mostly in developing countries. These experiments showed renewed interest in a UBI as a potential policy intervention to mitigate systemic poverty and the rising level of inequality (Standing, 2017), the challenges associated with climate change (Maslin and Lewis, 2019) and the threat of technological unemployment (Standing, 2017). Basic income has become a substantial part of discussions about social policy and welfare state reforms in several countries. This has resulted in an increasing number of basic income experiments planned and conducted in different parts of the world in recent years.

However, while a lot of ink has been spilled in arguing the merits of a basic income[1], very few basic income programs have been carried out[2] to test its impact. These programs

[1] According to Google scholar, 48 800 articles have been written on "basic income".
[2] According to the Stanford Basic Income Lab, 31 basic income programs have been conducted across the world. This includes UBI-related experiments, pilots, programs, and policies throughout the world since the 1970s NIT experiments.

include pilots, experiments, and government schemes, some of which targeted certain groups of people, carried conditions on how the money was earned, and were short term or once off transfers. In fact, the only governments that have implemented a universally guaranteed income are the state of Alaska in the US and the Islamic Republic of Iran. And only three experiments on the actual impact of universal basic income have been carried out. These are the Namibian BIG, Indian MPUCT and the Kenyan GiveDirectly Experiment(Banerjee, Niehaus & Suri, 2019).These experiments including the "first wave experiments" and similar partial UBI experiments need to be critically consolidated within a unified assessment frame and that is what this dissertation aims to do in critically reviewing the experimental literature on UBI.

Chapter 2: Theory of a Universal Basic Income

What is a Universal Basic Income?

A UBI is basically money that is given for free, to everyone, to guarantee at least basic subsistence. This paper adopts the definition used by the BIEN, according to which a UBI is a periodic cash payment unconditionally delivered to all on an individual basis, without means-testing or work requirements (BIEN, 2020). Implied in this definition is the idea that the payment is a guaranteed amount that does not fluctuate depending on private income or other factors. This is one of the distinguishing features between a UBI and its close relative the NIT and its more successfully implemented cousin, the social dividend. The NIT responds to changes in private income: as private income rises, the NIT drops. A social dividend is determined by profits: when profits are high, the government or administrator of the fund can pay out more and when the profits are low, the fund pays less. A UBI on the other hand does not have this elasticity to private income and profits. The elements of the UBI are discussed below in more detail.

A Cash Benefit

A UBI is a direct cash transfer paid to the beneficiary. It is not the same as in-kind benefits such as food stamps, vouchers or housing subsidies paid directly to landlords (BIEN, 2020). The idea behind using cash instead of in-kind benefits is to empower beneficiaries away from external paternalism, be it from the state, a partner, or social workers (Van Parijs

1997). Thus, beneficiaries can use the income as they see fit, buying groceries, paying their rent, financing further education, or even gambling it away. This sense of freedom is paramount to bring concerns for dignity and autonomy back to welfare. Friedman (1962) phrased the idea as “Nobody spends somebody else’s money as carefully as he spends his own. Nobody uses somebody else’s resources as carefully as he uses his own.” Thus, instead of the state buying poor people what it thinks they want, it should rather give them the money to use it as they see fit. To dispel the argument that if you give poor people cash, they will squander it on drugs and alcohol, Friedman (1962) explains that recipients of in-kind benefits can always sell or transfer their benefits to others if they want drugs and alcohol. So, while the state can spend its resources both human and financial, to get people what it believes they ought to get, it cannot make them to consume those resources. In this sense, a cash benefit avoids the unnecessary bureaucracy associated with buying people what they should need and allows them to make their own decisions about what they need. (Van Parijs & Vanderborght 2017).

An Individual Based Benefit

A UBI is conceived by supporters as an individual right, and as such should be paid individually to each man, woman and child (Standing, 2017). There are at least three reasons for paying UBI at the individual level. The first is to ensure that the amount is the same irrespective of the size of the beneficiary’s household or whether the beneficiary is in a spousal arrangement. When two people live together, their combined expenses are less than the sum of their individual expenses. However, paying people who live together less than what they would get if they were living apart sends the unintended message that companionship is punishable and that being single is praiseworthy. Secondly, it is difficult to ascertain that people are single or not, due to the wide range of spousal arrangements people can make, for instance, civil partnership or separated marriages or life partnerships. It is difficult to ascertain these without encroaching on people's integrity and privacy.

Thirdly, most household-based benefits are means-tested with eligibility based on household income. This has the unintended effect of excluding eligible dependents simply because they live in well-off households. For instance, an unemployed housewife or spouse who takes care of the house and the children and perhaps an elderly parent while their partner makes enough money such that their average household income is more than the ceiling amount for welfare will not qualify for the welfare support. While this is fine for some families, it is based on the false premise that all families are spaces of solidarity and mutual support and overlooks families where there is contention and domination. An individual-based guaranteed basic income gives spouses from such abusive homes the security to be independent and able to lead a dignified life should they wish to leave that spousal arrangement. (Cantillon and Mclean, 2016). In this way, an individual-based basic income can be construed as an instrument of freedom from spousal abuse. The shadow-priced welfare gain associated with this could be very large.

An Unconditional Benefit

A UBI is paid without any income, spending, or behavioural conditions (Standing, 2017). This element carries three aspects, the first being there is no means testing, i.e., poor people do not have to prove that they are poor, which is a degrading and inefficient way of delivering assistance to those who truly need it due to stigma avoidance. Standing (2017: 76) warns against the use of stigma to ration social benefits. Secondly, there are no conditions on how the money should be spent, an imposition which is again a demeaning and paternalistic. This aspect is very close to the notion of republican freedom[3] which is a key motivation for a basic income. Thirdly, a UBI comes with no behavioural conditions. While most welfare benefits require the beneficiary to prove that they are unfit to work or that they are doing their best to find employment, a UBI is paid without any work effort requirements. Work effort conditions are based on the reasonable apprehension that work is good for both the individual and their community. However, in some instances, these conditions are quite stringent and force people into employment traps and exploitation. According to Bidadanure (2019) in the United Kingdom, a beneficiary can lose their benefits if they have not applied for enough jobs per week, or if they missed an appointment, or arrived late for work, or refused a job offer.

Conditional benefit schemes seek to deter free riders and to discourage attempts to cheat the system. A UBI on the other hand carries no condition to demonstrate work effort. It is based on the premise that most people want to lead productive lives. Thus, it allows its beneficiaries to take up jobs that are socially desirable but financially unviable like care work, volunteering, and activism. One of its primary objectives is to divorce the right to an income from the obligation to work. As Van Parijs (2016) puts it; “the money that one

[3] This is defined by its leading contemporary theoretician, Pettit (2007), as freedom from domination by more powerful agents. As the relevant powerful agents in a market-based economy are most frequently employers and enforcers of civic and private rules, republican freedom generally needs to be guaranteed by a strong state that can level bargaining fields by protective legislation and regulation.

seeks, is the instrument of their slavery and the money that is in one's possession is the instrument of their freedom".

A Universal Benefit

All assistance or transfer programs are targeted programs, as they are designed to ensure that those in need can access assistance. The fact that a UBI applies equally to everyone, regardless of group status, gender, employment or need, is one of its most appealing features (Powell, 2008). Thus, while most assistance programs are explicit about targeting the poorest and using means tests to screen beneficiaries, a universal basic income is different in that it is given to everyone. The main reason for this is to avoid the problem of low take-up associated with means-tested assistance and to remove administrative costs associated with means testing and monitoring. Means-tested assistance usually has low take-up because of exclusion errors, stigmatization and discouraging bureaucracies (Stuber & Schlesinger 2006)[4]. Eligible individuals often do not apply for assistance because they do not want to be demonized as scroungers or free riders. Also, the administrative process of proving that one is indeed poor and in need of social assistance not only adds another layer of bureaucracy but is also demeaning and so a lot of eligible people either do not start trying to access assistance or they give up during the process because of the discouragement of having to prove that they are poor (Moffitt 1983). When the basic

[4] It is worth noting that studies on welfare stigma are mostly concentrated in developed countries which generally have low unemployment rates. It would be interesting to see whether this phenomenon is as prevalent in developing countries too, especially countries with high unemployment rates as it is not obvious how beneficiaries from developing countries with high unemployment and/or high inequality rates would respond based on their developing country counterparts.

income is universal, the payment is made to everyone, thus avoiding the stigma and discouragement of not only acknowledging one's own state of poverty but also having to prove it to others. While the idea of giving assistance to everyone including the rich might seem wasteful, in most UBI models the money given to the rich is taxed back, thus rendering them net payers and not net recipients. This has the effect of destigmatizing public assistance and reducing the state bureaucracy.

Another reason for universality is to avoid welfare traps. Transfer programs that only target the poor are often withdrawn when their beneficiaries get a job. According to Segal (2017), any program that gives a benefit and withdraws or taxes back the amount once the beneficiary earns private income does not incentivise work, particularly if the benefit amount is greater than or equal to or insignificantly less than the wage amount. Thus, while targeted programs function as safety nets that can trap the poor in welfare programs, a UBI functions as a floor that everyone can stand and build upon (Van Parijs, 1997). Any income that is earned is added on top of the basic income instead of substituting for it. This is particularly important in the current age of precarious work, where if benefits are withdrawn because of employment, when that job is lost a person can go for days, weeks or even months without any income until they are put back on welfare.

<u>A Regular Payment</u>

A UBI is a recurrent cash payment made at regular intervals, be it weekly, bimonthly, or monthly. What is most important is not the frequency but its consistency. Standing (2017) explains that since a crucial part of basic security is predictability, the payment must be both guaranteed and known in advance. This is different to Ackerman & Alstott's (2006) Basic Capital idea of giving each person a once off lump sum amount once they turn 21 years old. A basic capital grant is susceptible to the moral hazard problem as the political circumstances in which it could arise suggest that the society would be empathetic to beneficiaries who fall into destitution after having squandered or gambled their basic capital away. But this would disincentivise recipients from being prudent in investing the grant. The recurrence of the UBI avoids this moral hazard and protects recipients from this potential destitution. In this way a UBI is a more efficient floor as it ensures economic security throughout one's life (Birnbaum 2012).

Another element of a UBI is that the amount of the UBI must be known beforehand and kept constant (in real terms). As mentioned above, predictability is a crucial feature of income security. The beneficiary of a UBI must know beforehand how much they will get and when they will get the amount regardless of whether they earn an income and how much that income is. According to Widerquist (2018) a UBI is a regular constant amount paid throughout one's lifetime, although the amount may differ for children, working age and the elderly. This is different to an NIT where an individual gets a guaranteed basic income if they have no private income, but it decreases as their private income rises until private income reaches a certain exit point from which the individual stops getting the NIT and starts paying a positive income tax. UBI does not have this clawback feature.

Other Elements of a UBI

Source of the funds

Perhaps one of the most important questions in a typical policy maker's mind about a UBI is the source of the funding. This question is crucial for sustainability. The funding of the basic income must be economically viable, i.e., it should support economic growth. A UBI that is not pro-growth is inevitably self-defeating. Some possible sources of the funds include an income tax, a wealth tax, consumption tax, financial transaction tax, carbon tax or a combination of these taxes (Widerquist, 2017). According to Standing (2017) the wealth that has accumulated today is a result of accumulated efforts of previous generations. This view is shared by Simon (1991) who estimated that about 90 percent of the wealth present today was derived from social capital, which includes trust, shared values and behaviours that allow a society to function effectively. So, it is unjust for this wealth to be concentrated in the hands of an elite few at the cost of the rest of society on the basis that all have an equal right to it. Following this logic, a UBI is a redistributive instrument of social justice that reflects the intrinsically social and collective nature of societal wealth. It does this by redistributing the social dividends that were accrued from the wealth that was created in the past and is a return on the use of the commons and the natural resources that belong to all. The funds can also be derived from sovereign wealth funds or a social dividend, like the Alaskan Permanent Fund and the Great Smoky Mountains casino revenues respectively (Standing, 2017). The funds could also be raised by redirecting funding from other welfare transfers or subsidies like the Iranian national cash transfer program that was funded by redirecting funds from the energy subsidy (Salehi-Isfahani & Mostafavi-Dehzooei, 2018).

Size of UBI

Two crucial elements of a UBI that fall beyond the scope of this paper are the size of the basic income and source of its funding. The size of the basic income can vary from a partial basic income, which is just enough to sustain a minimum level of subsistence. On the other end of the spectrum, the basic income could be the most generous amount, consistent with Kaldor-Hicks efficiency[5]. The variance in the amount reflects the variance in the value judgements about what the poor or the needy are owed. One principle espoused by Van Parijs (2017) is that the amount must be the highest sustainable amount at the highest imaginable scale, i.e., the most expansive and most generous sustainable amount. In practice, the amount that seems most reasonable is one that allows beneficiaries to able to be free from desperate poverty. For this reason, in most instances, the amount of the UBI is pegged to the poverty line. The question of whether the UBI will substitute for other welfare benefits or supplement them is crucial in this discussion. A substituting UBI would have to be inclusive of all the other transfers while a supplementing amount would consider these transfers and compliment them.

One way to approach this issue is by looking at what the basic income seeks to remedy, or as Dorsey (2012) puts it, looking at what the poor are owed. He outlines the following needs that a UBI can remedy: providing a level of primary goods and resources, ensuring a state of human subsistence, social needs, basic human needs or maintaining certain central capabilities. He argues that primary goods needs are ultimately basic human needs and that human subsistence needs are ultimately capabilities needs and that ultimately all

[5] A Kaldor-Hicks efficient UBI would satisfy the Hicks-optimal outcome criterion, i.e., the total income for all citizens would be the most it could possibly be.

five needs are either welfare needs, or capabilities needs. Dorsey concedes that one of the appeals of the capability approach is its lack of paternalism, in that it would not force anyone to live a life they do not value. But according to Dorsey (2012), the welfare approach is also not necessarily paternalistic since its conception of the basic minimum state is one that fulfils the preferences and evaluations that are consistent with one's conception of the good. Thus, the conception of what is valuable is not determined by the state, social workers, or any other external party but by the beneficiaries themselves. Another feature that both approaches share is their focus on the achievement of a sufficient threshold of wellbeing. In this regard, UBI experiments are proving that any little bit helps.

UBI an Important Intervention for Social Justice

Many reasons have been offered to justify the need for a basic income. These include the threats posed by technological change and the rise of precarious work and growing inequality explained above. These problems are positive reasons for the UBI, but there are also normative reasons why a basic income is a powerful tool for social justice as it promotes real freedom for all

One of the reasons offered by Van Parijs (2001) for a UBI is that it will do away with the bureaucratic inefficiency of the current welfare system. Rothchild (2001) and Barry (2001) agree with him that a UBI makes a compelling argument for bureaucratic efficiency and general interest. However, Rothschild (2001) believes that a truly universal basic income would have to be global. She takes issue with the practicality of such an international UBI given the global political environment. Her scepticism emanates from the lack of a political structure and administrative capacity to implement a worldwide guaranteed basic income. Van Parijs and Vanderborght (2010) admits that transnational migration is the biggest threat to the sustainability of a local UBI (Van Parijs & Vanderborght, 2010). They suggest the establishment of a supra-national, geographically limited, basic income that will facilitate the free movement of capital, products, and labour. A country level basic income in such a global economy could redistribute income and wealth from richer urban areas to poorer rural areas and thus avoid some of the negative externalities of migration.

Barry (2001) on the other hand agrees that an unconditional basic income that caters for subsistence may be necessary, depending on how subsistence is defined. However, he believes that the level of the basic income should not be so large as undermine incentives to work. However, this logic overlooks the efficiency benefits of removing leisure-loving people from the labour market. In the absence of a UBI, leisure-loving individuals are incentivised to give false signals in the market, portraying themselves as income-loving. This forces truly income-loving individuals to expend their resources acquiring costly qualifications that the leisure-loving cannot afford so as to price them out of competition. Similarly, employers also have to incur high screening costs to separate those who truly want to do the work from those who just want an income. This goes to the heart of the assertion to that an unconditional basic income divorces the right to an income from the obligation to work. Also, it is worth noting that Barry's disagreement with Van Parijs is about the size of the guaranteed income and not whether there should be one at all.

According to Van Parijs, Cohen & Rogers (2001) justice requires the maximisation of the minimum level of real freedoms. Galston (2001) rejects this basis in favour of a view based on reciprocity, particularly the obligation of the abled to assist those who need assistance.

The reciprocity justification is the basis of most welfare programs. This version of the obligation inherently entails identifying the needy, their needs and how they can be assisted to meet those needs, i.e., means based targeted programs. For this reason, Galston (2001) argues that targeted programs are more efficient at remedying the problem of poverty and growing inequality compared to universal programs that benefit everyone regardless of need.

Anderson (2001) on the other hand is more concerned about the stigma and exclusionary impact of targeted programs. Her problem is not with the universality of a basic income but with its unconditionality. She argues that whilst a universal basic income would deal with the problem of the stigmatisation of needy people, an unconditional basic income would fuel a resentment problem directed at the "undeserving" poor people. This is referred to in the literature as the "Malibu surfer problem". To get around this problem Anderson (2001) suggests a different justification for UBI, one that is more direct. Her justification is based on the idea that the wealth of a society emanates from shared ownership of its resources both physical and intellectual that enable it to produce what it produces. This includes dividends on public assets[6], resource and energy taxes, and income and/or wealth taxes (Van Parijs, Cohen & Rogers, 2001). This justification for a universal basic income is akin to Paine's (1796) argument that there are some transfers that one is entitled to, where the entitlement does not emanate from one's contribution to the collective pool, nor their need, but by virtue of belonging to that community. Paine (1796) believed that this entitlement is fundamental because opportunities are unequally distributed in a morally

[6] Like the Smokey Mountains Social Dividend program

arbitrary manner, thus justifying the redistribution of public property through a UBI. (Van Parijs, Cohen & Rogers, 2001)

The Negative Income Tax

As mentioned, previously, the concept of a UBI has evolved over time. The first experimentation was inspired by Friedman's (1962) NIT proposal. This was a response to America's welfare crisis and ongoing war on poverty at the time. Then President Nixon described the problem as an urban crisis, a social crisis, and an indictment of the government's capacity (Schulman, 2001). Between 1960 and 1975, the number of people on social assistance in the US doubled from 7 million to 14 million. In 1974, 24 million Americans were receiving some form of means tested benefit. According to Schulman (2001), the increase in the number of welfare beneficiaries was a result of the efforts of the National Welfare Right Organisation (NWRO) who sought to destigmatize welfare system and encourage eligible people to take up social assistance. For Friedman the problem was more than just administrative. He believed that the bureaucracy could be improved by removing its demeaning and paternalistic rules (Friedman, 1962).

With the normal (positive) income tax system, an income earner is entitled to a certain amount of personal exemptions and deductions from their pay. Once these exemptions and deductions have been accounted for, the residual is liable for taxation. A NIT applies to people who either have no private earnings or are paid too little to be liable for positive income tax collection. According to Friedman (1962) the working poor are still entitled to some of the exemptions and deductions that they would have received if they earned a taxable income amount. Thus, a NIT is fraction of the unused exemptions and deductions that is paid to the individual by the receiver of revenue. In this way, even a person who earns no private income, is guaranteed at least a minimum income.

Under the current system at the time, a person on welfare would have to relinquish their benefits once they got a job and started earning a private income. If the work was stable then that was fine, but if the work was short term or if the same person lost that job, shortly thereafter, they would have to reapply for welfare. This meant that social services would have to do another means test to make sure that the individual was eligible for assistance, a process that could days, weeks or even months, without the person receiving any income or assistance. This bureaucratic red tape according to Friedman (1962) discouraged people on welfare from getting jobs and punished those that did, when they lost those jobs.

The NIT would aid the poor based solely on their income and not some arbitrary characteristics. It would recognise people's agency by giving them cash instead of in-kind benefits such a food vouchers and housing subsidies. It would also simplify the cumbersome system of assistance programs. It would be more efficient than the existing system at the time as it would shed administrative costs and concentrate resources on the beneficiation of the poor. Friedman also believed that the NIT would not distort market prices in the same way that subsidies do.

Friedman believed that his NIT model would encourage people to seek employment even if they were already on welfare since they would still get a fraction of their welfare benefits even if they got a job. As opposed to withdrawing the entire welfare benefit once a person started earning an income, Friedman's NIT ensured that those who earn something would still be better off than those who rely solely on welfare. As the earnings rose, the negative income would become smaller and smaller until the private earnings were equal to the exemptions, a point which he called the break-even point. At this income level, the individual is not entitled to any negative income tax nor are they liable for any positive income tax. Friedman believed that this continuous threshold response would allow people to take advantage of job opportunities without worrying about having to wait long to get assistance if they lose their jobs.

Features of the NIT

A NIT has two essential features. The first is the guaranteed basic income level (G) which is the amount an individual would receive from the revenue collector if they do not earn any private income. The second feature is the marginal tax back rate (t) which is the rate at which the benefit is reduced as the recipient's private earnings rise. Intuitively, a guaranteed income carries an income effect since giving people a guaranteed income reduces their incentive to work. To offset this income effect, the NIT was designed to have a tax back rate that would encourage people to seek employment. At the time, the existing welfare programs removed benefits for every dollar of privately earned income at a ratio of 1:1. The NIT tax back rate would reduce the benefit at a fraction of the privately earned income, thus ensuring that those on welfare were always better off if they earned any private income. Thus, the individual's disposable income (Y_d) can be represented with the following equation.

$$Y_d = Y - tY + G \tag{2.1}$$

Where Y_d is the disposable income amount that the individual would receive after accounting for both the guaranteed income and the respective tax back rate, Y is the private income earned by the individual, t is the tax back rate or the rate that is deducted from the private income, and G is the guaranteed income. Thus, when private income (Y) is zero, the disposable income is equal to the guaranteed income (G). As private income rises, so does the amount taxed back from that income. This continues until the amount taxed back is equal to the guaranteed amount, which is the break-even point. When the recipient's private income rises beyond this point, the individual becomes a net-taxpayer (Levine *et al*, 2005).

The NIT v UBI

There are some similarities between Friedman's NIT and the contemporary UBI. They are both cash payments and not vouchers or payments in kind. They are both periodic, i.e., paid regularly and not once-off payments. And the payments are made per individual although they are often household-based in NIT experiments. Table 1 below shows the similarities and differences between NIT and UBI design features.

Table 1: A Comparison of NIT and UBI Design Features

Design Features	NIT	UBI
Periodic	✓	✓
Cash	✓	✓
Universal	X	✓
Individual	✓	✓
Unconditional	X	✓
Constant Amount	X	✓

Notes: While a NIT can be paid to anyone, it is technically means-tested, in that eligibility for it depends on an individual's private income. It sets an exit point beyond which, private earners do not qualify for the transfer. Although the NIT is not explicitly conditional, it is designed to incentivise people to earn private income. The guaranteed amount received under an NIT model is a function of how much private income the beneficiary earns; this amount decreases as their private income increases while it stays constant in a UBI model.

The differences between the two models results in the following administrative and philosophical differences.

1. To calculate the value or size of the top up for a NIT, some form of means testing is required. However, such means testing is not necessary for the guaranteed stable income of UBI.
2. Given that the NIT is a top up that is calculated once the taxpayer has declared or filed their earnings or lack thereof for the given tax period which is usually at the end of the tax year, the payment is made retrospectively and cannot be guaranteed

in advance. This makes the NIT more like a windfall gain than a steady source of basic income security (Widerquist, 2005).

3. Unemployed people or people with very low incomes generally do not file taxes, which makes the tax system a poor vehicle for delivering the relief to the people who need it the most. And while the NIT might incentivise some low-income earners to file so that they can benefit from the NIT, it still carries a moral hazard as it incentivises people to underreport how much they earn (Standing, 2017). A UBI on the other hand is paid to everyone, regardless of whether they work or not, and how much they get paid.
4. Standing (2017) concludes that while a NIT might be able to help alleviate poverty, it would not be able to advance republican freedom or assure income security, nor would it be a good vehicle for social justice.

Although the differences between the NIT and the contemporary UBI are important, the similarities between them allow for a more general analysis of guaranteed income mechanisms. Next, we review how the NIT experiments were designed, run, and analysed. While these experiments are important in identifying some direct effects of a guaranteed cash transfer, their framing and implementation affects the extent to which they reveal the true effects of a guaranteed universal and unconditional income.

Chapter 3: First Wave Experiments

Introduction

As discussed in the previous chapter, the concept of a basic income has a long history dating back to Ancient Greece. Having dealt with the moral and strategic case for UBI, we now turn to the history of experimentation on basic income. As mentioned above, the current burgeoning of interest in basic income appears to reflect both a fundamental shift in labour markets and the rise of precarious work in both developing and developed countries as well as a growing fatigue with the current social policy architecture. As these concerns are not entirely new, experimentation with the prospect of a guaranteed basic income as a possible solution to them started more than 50 years ago.

Heckman (2020) explains how the history of field experiments on a basic income can be illustrated as a bimodal distribution with the first wave starting in the late 1960s and ending in the early 1980s in North America (US and Canada). And the second wave which according to Heckman (2020) is currently peaking, started in 2008 in Namibia. However, there are differences between the two waves of experiments, in terms of the basic income model they tend to focus on, their geographic location, and experimental questions. The first wave experiments were NIT inspired experiments, conducted in North America, and primarily focused on labour market effects. The second wave experiments are mostly true (BIEN-style) UBI experiments[7]; conducted mostly in developing countries apart from

[7] Except for OBIP and B-Mincome, which were NIT experiments, the rest of the second wave experiments are UBI experiments.

Finnish and Ontarian pilots; focused on more direct effects of income security. In this chapter, we deal with the first wave experiments.

Historical Context

On the 20th of July 1969, the United States landed the first human on the moon. However, this giant leap for humanity was offset by growing poverty and failing anti-poverty programs. In a report commissioned by President Johnson titled *Poverty Amid Plenty: The American Paradox*, Heineman, *et al* (1969) explain the inability of jobs to lift Americans out of poverty. According to the report, about 25 million Americans were poor, and more than a third of those poor came from families where the head of the family worked throughout the year. Thus, despite having jobs, a lot of Americans were still poor.

Against the backdrop of the US *War on Poverty* the Heineman Report recommended that the patchwork of existing welfare schemes be replaced by a NIT (Forget, 2011b). However, instead of piloting the NIT for nation-wide rollout, the Office of Economic Opportunity (OEO) chose to experiment with the idea first (Levine et al., 2005). The main goal of the experiment was to measure the size of the labour market response. However, as the experiments progressed, other data were collected on the effect of the NIT on human capital accumulation, and family formation health and fertility (Forget, 2011). The first experiment was conducted on an urban population in New Jersey and Pennsylvania. The second experiment which focused on the impact of the NIT on single parent homes was conducted in Gary, Indiana. The third experiment was conducted in North Carolina and

Iowa to measure the labour market effect of the NIT on a rural population. The last and biggest experiment was conducted in Seattle and Denver.

In Canada, parallel but less dramatic developments were unfolding. After WWII, the government invested in a patchwork of national social assistance programs, including family allowances, the Canada Pension Plan and universal health insurance. However, income support remained in the jurisdiction of provincial governments (Forget, 2011). In 1970, the Social Security Review introduced the idea of a universal minimum income support. In the following year, this recommendation was also suggested by the Croll Committee Report and the Castonguay-Nepveu Commission. Based on these proposals, the Canadian government together with the Province of Manitoba, decided to conduct a guaranteed annual income experiment in the province, between 1974 and 1979. While the project was labelled an experiment, it was widely considered a pilot that would be scaled up just like the universal health insurance.

However, while the development of the experiments was less dramatic in Canada, compared to the US, its ending was quite dramatic. Oil price volatility and stagflation in the late 1970s led to changes in government priorities nationally and provincially. The government decided to cancel the experiments prematurely (Forget, 2011c). Due to the lack of government support, the project's staff did not complete the analysis of the outcomes, although analysis of labour market outcomes in the Winnipeg sample was conducted in the 1980s. Initially broad in scope, the project's research agenda was gradually narrowed to a measurement of work disincentives.

Table 2: NIT Experiment Design

	New Jersey	**Rural (RIME)**	**Seattle/Denver (SIME/DIME)**	**Gary**	**Manitoba (Mincome)**	**Manitoba Saturation site**
Location	Trenton, Patterson-Passaic, and Jersey City in NJ & Scranton in Pennsylvania	Dauphin county in North Carolina & Pocahontas and Calhoun counties in Iowa	Seattle and Denver	Gary, Indiana	Winnipeg	Dauphin
Eligibility	Intact households headed by able-bodied men aged between 18 to 58 years with at least one dependent and income less than	Same criteria as New Jersey experiment with the exception that female headed households and single parent homes were included. Hh	Families with at least one dependent and income below $11k (for Hh with only one employed head) and $13k (for Hh where both heads were	Black Hh, headed primarily female headed, aged between 18 and 58 years and with an income below 240% of poverty line.	Families with abled bodied heads under 58 years with an income below $13k for a family of 4	Everyone who lived in Dauphin between January 1974 and December 1978, excluding migrants who moved in during this period, including the

	150% of poverty line.	also had to have at least one dependent and income below 150% of poverty line	employed) per annum.			retired and the disabled.
Sample size	1,357 Hh; 725 in the treatment group and 632 in the control group.	1759 Hh: of which 809 (729)[8] Hh were in the treatment group and 950 were in the control group	4800 Hh; 2758 from Denver and 2043 from Seattle	1799 black Hh; 60% female headed	1300 Hh including families and singletons	706 Hh

[8] The experiment started with a sample of 809 households but finished with 729 households.

Sample characteristics	Low-income two parents Hh (Black, White & Hispanic) in declining urban areas.	Treatment group included 587 male-headed Hh, 108 female-headed Hh and 114 Hh were headed by both sexes.	Black, White, and Hispanic families.	Black Hh, primarily female headed	Urban and rural families	Rural farm families
Design features	8 combinations of t (0.3, 0.5 & 0,7) and G (0.5, 0.75, 1.0 &1.25)	8 combinations of t (0.3, 0.5, 0.7) and G (0.5, 0.75 & 1.0)	11 combinations of t (0.5, 0.7, 0.7-0.025y, 0.8-0.025y) and G (0.75, 1.26 & 1.48), training counselling and training subsidy (50% & 100%)	4 combinations of t (0.4 & 0.6) and G (0.75 & 1.0), social services counselling and day care subsidy (35% & 60%)	7 combinations in Winnipeg of t (0.35, 0.5 & 0.75) G ($3800, $4800 & $5800 for a family of 4)	Dauphin was a saturation site with t (0.5) and G ($3800)

Experiment duration	1968 – 1972 (3 years)	1970 – 1972 (3 years)	1970 – 1976 (7 years[9])	1971 – 1978 (3 years)	1975 – 1978 (3 years)	1974 - 1978

Notes: t is the tax back rate, y represents private income, and G is the guaranteed income level often given as a fraction of the poverty line in the US and in Canadian dollar amounts in Canada.

Sources: (Widerquist, 2005; Munnell, 1986).

[9] The experiment was originally planned to last six years, but later researchers received approval for a small group of subjects to continue until 1990(i.e., 20 years). However, this was later cancelled in 1980.

As mentioned above, the main aim of the experiments was to estimate the labour market effect of a NIT. The NIT-inspired experiments were the first large-scale social experiments to use the scientific method of randomly assigning experiment participants into treatment and control groups. Treating the basic income projects as legitimate social experiments and not typical policy "pilot projects", the researchers used matched controls and parameter variation to maximize the quality of collected data (Forget, 2011). In all the US experiments and in Winnipeg subjects were randomly selected and paired with matched controls from the same community. The subject families were isolated from one another. This allowed for the varying of the parameters of the NIT between households. By randomly assigning treatment and control subjects, the experiment sought to infer causality. Because the primary aim of the experiment was to measure the labour market effects of the NIT, institutionalised, disabled, and retired people were not included in these experiments (Hum & Simpson, 1993).

One important feature of the Canadian experiment was its investigation of the universal effects of a guaranteed minimum income. One of the Mincome modes of experimentation sought to measure the effect of a guaranteed income in a "saturation site[10]". Every family from Dauphin and its rural municipality (approx. 10000 people) was eligible to participate in the experiment, including retired and disabled people. Small rural communities were selected as the control group. Comparators were selected using hard-matching and propensity matching (Forget, 2011). Each treatment beneficiary was matched with three control comparators. The idea behind the saturation site was to simulate realistic

[10] A saturation site allows for the estimation of community-wide effects of a guaranteed minimum income.

administrative and community issues that would arise in a real universal program (Forget, 2011).

The Dauphin cohort received the same offer as the Winnipeg cohort. While the benefit did not make much of a difference to a family already receiving social assistance, it would make a significant difference to the elderly, the working poor, singletons, and employable males who did not benefit from the traditional welfare schemes. Particularly since Dauphin is a small farming town, the income guarantee made a major potential difference to the self-employed farmers in the event of a bad crop year or volatility in commodity prices.

Findings

While the main goal of a NIT is to reduce poverty and increase the well-being of low-income people, the main goal of the 1970s experiments was to estimate the labour market effects of guaranteed income. The reason for this decision was that it was generally accepted that the guaranteed income would improve well-being and alleviate poverty but the main concern of policy makers at the time was measuring the cost of this improvement in welfare. As a result, there was no systematic analysis of the effects of the income grant on poverty (Salkind & Haskins, 1982). However, as the data was collected, other effects were observed and studied. Amongst these was the effect of the guaranteed income on marital stability. Below is a summary of these findings.

Labour Market Effect

In the SIME/DIME experiments the impact of the guaranteed income on the labour market was estimated using the following models: a simple difference model between outcomes between treatment and control group, a structural model, and microeconomic simulation.

The simple difference model analysed difference between the average hours of work per week by the treatment group compared to the control group. The structural model decomposed the overall reduction in work effort into change due to the substitution effect[11] and/or changes due to income effect[12]. The microeconomic simulation generalised the results from the experiment to the national level by combining the income and substitution effect estimates with census data to predict the probable response of the low-income workers to alternative income maintenance plans (Burtless, 1986).

Overall, the NIT-inspired experiments found moderate labour market effects. On average treatment households showed a 13% decrease in work effort compared to their control counterparts. This included primary (mostly male heads), secondary (mostly wives) and tertiary (mostly adolescent males) earners. However, when looked at individually, primary earners showed no significant changes in work effort. Secondary earners used the guaranteed income to buy family responsibility time, as such they re-entered the labour market later after going on maternity or family responsibility leave compared to their control counterparts. On the other hand, tertiary earners in the treatment group tended to enter the labour later than their control counterparts, having spent more time at school developing their human capital.

The simple difference model showed a moderate reduction in work effort with SIME/DIME showing the largest difference due to its generous plans. The average reduction in work effort was 7% for men and 17% for women (Munnell, 1986: 3). These

[11] This is the idea that by reducing the value of the guaranteed amount by some fraction for every dollar earned, the NIT together with other income taxes will reduce the net value of the income and induce recipients to substitute leisure for work.
[12] This is the idea that by giving a family that has no other income source except for wages a guaranteed minimum income, the NIT could discourage family members from seeking paid work.

estimates were overstated to the extent that beneficiaries underreported their true private earnings in order to receive larger transfers and they were also understated to the extent that the limited duration of the experiment shows smaller response estimates than would be expected in a congruent but permanent program, especially in the more generous plans (Burtless, 1986).

On the other hand, the results of the structural model implied that the weighted average of the income and substitution elasticities were much smaller than the nonexperimental estimates had predicted. The total income elasticity represents the percentage change in work effort associated with a percentage point increase in income that is not accompanied by a change in the net wage rate (Burtless, 1986). The mean experimental income elasticities for men and women ranged between -0.07 and -0.18, thus on average, a 10% increase in unearned income reduced earned income by about 1% (Marinescu, 2017: 37). Although, the reduction in work effort was relatively small, the resultant losses in private earnings, particularly amongst breadwinners, constituted a larger fraction of the payments to low-income families (Burtless, 1986).

The microeconomic simulation used the elasticity estimates from the SIME/DIME experiment to estimate national level work effort responses and budgetary costs under alternative plans. The results highlighted the conflict in trying to provide work incentives to NIT beneficiaries and maintaining public interest. To encourage NIT recipients to work, policy makers must reduce the tax back rate. However, such lower tax back rates result in an increase in the number of eligible recipients and reduction in tax revenue, thus imposing a high fiscal burden while reducing aggregate work effort.

Independence Effect

The experiment also showed apparent marital dissolution attributed to the supplementary incomes. According to Groeneveld, Tuma & Hannan (1980: 664) the SIME/DIME experiments showed 76%, 46% and 26% higher marriage dissolution rates amongst Whites, Blacks and Hispanics respectively compared to their control counterparts. The dissolution rate was higher amongst beneficiaries of the most generous plans. This indicated that the guaranteed income had a non-linear independence effect that is a function of income and the change in income because of the NIT (Groeneveld, Tuma & Hannan, 1980). However, following Groeneveld, Tuma & Hannan (1980) "even-history analysis", Cain (1986) after having made some minor adjustments on the study sample including removing childless couples[13], separating beneficiaries into those who received the NIT only and those that were in training programs and received the NIT, and analysing the results over the whole five-year period of the experiment instead of Groeneveld, Tuma & Hannan (1980) first three years analysis, found small and inconsistent effects on marital stability. Cain (1986) found that with White and Hispanic couples, the guaranteed income had no significant effect on marriage dissolution, whereas among Black couples, only those who benefited from both the guaranteed income and the training programs showed a significant and destabilising effect. In the pure NIT group, none of the couples showed any significant destabilising effect. As a result, Cain concluded that the evidence for a destabilising effect was not decisive or even conclusive (Forget, 2011). It is also worth noting that marriage

[13] Since the counterfactual to the NIT beneficiaries regarding family stability would have been AFDC beneficiaries, couples without children who would not have been eligible for the AFDC were removed from the comparison group.

disruption is not necessarily a negative outcome. Indeed, this is a good outcome for women who were financially trapped in unhealthy relationships.

Community-wide Effects

The Dauphin saturation site ran into difficulty as soon as it got off the ground. Originally, the project had sought to measure four focal areas: work incentives, sociological effects including family orientation, community cohesion and social attitudes), and administrative issues. Due to budgetary cuts, these areas were gradually cut out of the project, eventually leaving a focus only on administrative issues. Furthermore, there was a shift from collecting and analysing the data to just collecting and archiving it (Forget, 2011).

Criticisms

Due to the nascency of social field experiments at the time of the NIT-inspired experiments, these experiments were riddled with flaws based on selection biases, high attrition, and misreporting (Ashenfelter & Plant, 1990). Attrition is a normal feature of longitudinal studies. However, large attrition values like the ones observed in the NIT-inspired experiments can lead to attrition bias. For instance, families in treatment groups whose private earnings had fallen had an incentive to stay on the experiment (benefiting from the guaranteed income), while their counterparts in the control group who had lost their jobs and thus had no income had no such incentive not to drop out. Similarly, in marital stability analysis, a decrease in income because of a reduction in labour supply results in a continuum of small or large decreases in income. However, for a woman in a dissolved marriage, the loss in income is very large. Thus, the women in dissolved marriages in the treatment group had a choice to make between dropping out of the experiment and going back to the Aid to Families with Dependent Children (AFDC) or continuing with the experiment and benefiting from the NIT. Women in the control group did not have this option, and thus had no incentive to continue with the experiment when their marriage was dissolved (Munnell, 1986). Munnell (1986) notes how this incentive manifested within the treatment plans in the experimental group. Apparently, wives in the more generous plans show lower attrition that those in the less generous plans. These biases resulted in the overrepresentation of participants who had reduced the labour supply and dissolved their marriages in the treatment than in the control group.

There were also sampling problems attributed to the fact that besides Dauphin, the experiments were not truly random samples as they only enrolled low-income households. This concentration on low-income earners, meant that most of the respondents in the treatment group did not have well-paying jobs that would incentivise them to continue working (Widerquist, 2005). The samples were also not randomly assigned to control and treatment groups. Since a higher work disincentive for higher pre-tax income earners would increase the cost of the grant, the designers of the Gary and the SIME/DIME experiments adopted a cost minimisation strategy. Higher income earners were assigned to more generous programs. This strategy resulted in biases when it came to the response estimation of these parameters (Widerquist, 2005).

Cogan (1983) reanalysed the New Jersey-Pennsylvania NIT-inspired experiment and found that there was a large, statistically significant labour supply withdrawal of White male heads from the labour market. These estimates were markedly higher than in previous analyses for the same group and suggest that the estimated costs of the guaranteed income may have been seriously underestimated (Cogan, 1983). Cogan's model differentiated between participating eligible families and nonparticipating eligible families and explicitly controlled for state welfare programs for intact households. However, even his experimental labour supply response does not fully indicate the size of response to an economy-wide program. The estimates may be unique to the New Jersey-Pennsylvania accounting system, and the response in a permanent program may be quite different from the three-year experiment. Even so, the responses initially estimated could be quite different to the true effects and Cogan's analysis indicates that the true costs might be much higher.

Conclusion

While the analysis above suggests that the lessons derived from the NIT-inspired experiments should be taken with a pinch of salt, the NIT arguably had a much larger influence on successive social experiments than in the examination of the policy they were designed to test. And while a lot of ink has been spilled debating the internal as well as external validity of these experiments, UBI theory and experiments have taken a conceptually different approach.

According to Rainwater (1986) one of the major shortcomings of the NIT-inspired experiments was that they showed some effects of a basic income but failed to explain the mechanisms behind the effects. Rainwater (1986) described the experiments as a black box, where experimenters knew what went into the box, and knew what came out but did not know what went on in between. Thus, the findings were hard but brittle. The first wave experiments focused on the labour market effects of the guaranteed income without necessarily developing the methodological apparatus that could adequately measure these effects. The second wave experiments have tried to avoid this problem by focusing on best-practice general methodology over answering specific questions.

As Heckman (2020), argues contemporary experiments are more focused on methodological rigor than in trying to answer big and important questions. The experiments focus on what can be tested and not just what should ideally be tested. Another feature of how contemporary UBI theory and experiments have gone around the methodological problems of the earlier NIT inspired experiments is in broadening the concept of work effort. Newer experiments recognize various forms of non-labour market activities like care work, activism, and entrepreneurship as work efforts.

Chapter 4: Partial Field Experiments

Introduction

After the wave of NIT-inspired experiments, experimentation with a basic income almost died out. As mentioned above, a few guaranteed income schemes that can be construed as natural experiments were undertaken in North Carolina, Alaska, and Iran. These natural experiments offer key insights into possible funding mechanisms for a guaranteed income program which will be discussed later in this chapter. However, for more than 25 years there were no basic income experiments. As far as public policy was concerned, an unconditional, universal basic income was not generally regarded as feasible or desirable. Instead, governments focused exclusively on targeted conditional schemes.

However, after the establishment of BIEN and the second wave of experiments in the developing world discussed below in Chapter 5, basic income experiments started getting traction again in developed countries. Table 3 below shows the elements of a universal basic income that each of these experiments encompassed. But since none of these experiments covered all the elements, we refer to them as partial UBI experiments as they only measured some of the characteristics and effects that a full UBI would have. This chapter reviews these experiments, the inferences drawn from them and the soundness of those inferences.

Table 3: Partial UBI Experiments Features

Features	Finland 2017-8	Ontario 2017-8	Barcelona 2017-9
Cash	✓	✓	✓
Individual	✓	✓	X
Unconditional	X	X	✓ + X
Universal	X	X	X
Regular	✓	✓	✓
Constant	✓	X	✓ + X

Source: (Author's own using official reports and academic sources). Notes: The B-Mincome had one unconditional treatment arm, and within that arm, beneficiaries were randomised into an unlimited and a limited transfer group. The unlimited group received a constant amount regardless of their private income. The OBIP used a tax credit model, so the guaranteed income amount decreases as private income increases.

Finland

Context

Finland has one of the most innovative economies amongst the OECD countries, with a highly skilled workforce and world class education system. However, a 1990s economic recession had a devastating effect on the Finnish social security system. According to Kautto (2003) the economy suffered severe changes resulting in an economic crisis, an employment crisis, and a crisis in public finances. In response to these changes, the government sought to reform the social security system. The idea was to make the system more responsive to problems in the labour market; to incentivise work; and to simplify the current bureaucratic system (Kangas *et al*, 2019).

To explore alternative systems, the Finnish government initiated a randomised field experiment on a partial basic income model. Finnish unemployment beneficiaries are obliged to participate in Active Labour Market Policies (AMLPs). These include job search assistance, training programs, private sector incentives like subsidised work and public works projects. Adherence to these requirements is monitored by KELA, the Finnish social security agency. Failure to comply with this requirement can result in the reduction of the unemployment benefit at a rate of 4.65% for the following 65 days of payment (KELA, 2018). This sanction continues until the beneficiary adheres to the activation measures. The reason for this provision is to incentivise work effort. So social security benefits are contingent on participation in ALMPs.

The basic income experiment sought to examine a mechanism that divorced social security from ALMP participation. To do this, the beneficiaries enrolled in the experiment would be exempt from ALMP sanctions, thus, making the basic income unconditional. Secondly, in the current programme, the income benefits are means tested, and private income earned is deducted from the benefit. The beneficiaries in the experiment were also exempt from this condition, thus any income they made outside of the basic income would be added to rather than substituted for their basic income. Thirdly, because the recipients of the basic income did not have to report their private income or their participation in active measures, this reduced the bureaucracy they encountered and the insecurity of fluctuating benefits (Hilamo, 2020).

However, in 2018, the Unemployment Benefit Reform was introduced during the experiment[14]. The reform impacted both treatment and control but asymmetrically. According to the new rules, benefits were reduced by 4.65% for the following 3 months if an unemployment beneficiary or applicant had not participated in an ALMP for 5 days or worked for at least 18 hours during a 3-month period (Hämäläinen, Verho & Kanninen, 2021). Only those in the treatment group who sought to apply for supplementary unemployment benefits in excess of the basic income were also subjected to this reform while all those in the control were affected by it.

Number of Participants, Selection Criteria and Justification

The pilot targeted 2000 social security beneficiaries aged between 25 and 58 years, living anywhere in Finland without any geographic specification. These people were randomly selected from the population of basic daily allowance or labour market support beneficiaries who were registered in November 2016 with KELA[15]. To avoid self-selection bias, participation in the pilot was made mandatory for all selected beneficiaries (Kangas *et al*, 2019). The control group was comprised of a random selection of 5,000 persons among the 173,222 beneficiaries who also received unemployment allowance or labour market subsidy from KELA. There is no explicit mention of participation being mandatory for the control group in the literature, in which case selection bias was in fact not fully controlled.

[14] The reform was designed to tighten eligibility for unemployment benefits and income related schemes.
[15] The Social Insurance Institution of Finland

Selection into the treatment group was done through matched random sampling. The idea was to match treatment and control group participants to such an extent that the basic income would be the only significant difference between the two groups. The table below shows how this matching was done and the extent of the differences between the two groups. From this there was no significant difference between the two groups with respect to the response variables. However, there was a slight difference with regards to background variables. With respect to the benefit category the groups differed at the 5% significance level. However, overall, the two groups were matched.

It was decided that the experiment should spread across Finland as far as possible with some constraints on its outreach. The first constraint pertained to the experimental benefit amount, which according to the Finnish Constitution could not improve or weaken the economic situation of the treatment group in an unreasonable manner (KELA, 2016). This provision would cap the benefit amount around the existing KELA benefit amounts and exclude pensioners. The second constraint was on the sample selection. Due to their large population size, youth aged below 25 years were excluded from the study. The idea was that including a representative sample of this group would exhaust the study budget. It was also reasoned that given the available funds for the experiment; it would be inefficient to include people with high incomes whose responses to the basic income would be inelastic. Thus, the study population was limited to low-income individuals whose earnings fell below the annual median income of EUR23,301.

Table 4: Finnish Pilot Sample Statistics

	Treatment group	**Control group**	**Difference**
Response variables			
Number of days in employment	23.83	24.00	-0.17
Earnings and income from self-employment (EUR)	1,864	1,896	-31
Unemployment benefit from KELA	8,063	8,068	-6
Background variables (%)			
Benefit category			
Labour market subsidy (EUR)	87.15	84.63	2.52
Basic unemployment allowance	12.85	15.37	-2.52
Sex			
Female	47.75	47.48	0.27
Male	52.25	52.52	-0.27
Age			
25 – 34	33.50	35.12	-1.62
35 – 44	27.45	27.14	0.31
45 – 59	39.05	37.74	1.31
Number of observations	**2000**	**173,222**	

Source: (Kangas *et al*, 2019;15)

Experimental Question

The pilot was designed to test efficiency of a basic income scheme compared to the current Finnish workfare policies which require beneficiaries to participate in active labour market programmes such as training and subsidised work. Under these workfare policies, beneficiaries who refused to participate in the active labour market programmes could forfeit their benefits for up two months or even have them removed entirely. The premise behind these policies is that unemployed people need a stick to help them get a job. The basic income on the other hand is based on the idea that unemployed people know what is best for themselves and that they do not need a stick to help them navigate the labour market but instead should be given a carrot in the form of a basic income to help them stay on their feet (Hiilamo, 2020). To compare these two models, the Finnish basic income model was different from the existing workfare programmes in the following three ways.

1. They removed the sanctions that came with the refusal to participate in active labour market programmes.
2. They removed means-testing, which allowed beneficiaries to keep their private income without forfeiting any of the basic income.
3. To streamline the unemployment office bureaucracy and to reduce the uncertainty of fluctuating benefits because of work status, they applied a flat rate of unemployment benefit.

The primary aim of the experiment was to study the effect of the basic income on labour supply and incomes. According to Kangas *et al*, (2019), these effects would be measured using register data collected from official registers on employment, taxable income, participation in employment-promoting activities and KELA benefit pay-outs. The experiment also tested the effect of the basic income on the beneficiary's social and financial wellbeing, subjective health, employment, and job seeking activity as well as attitudes towards the basic income. In January 2018, an active labour market measure was introduced to the study to incentivise work effort. The study was adjusted to also measure the effect of these active measures.

Experiment Duration

In 2002 the Finnish government ran its first survey on support for a basic income (KELA, 2016). This survey was meant to measure the general appetite of Finnish citizens for a basic income without much specificity. A second survey was conducted in 2015, during the planning stage on the basic income experiment and found that 69 percent of Finns supported the idea of basic income. Having considered four models; an unconditional full basic income, a partial basic income and a negative income tax, the government decided to go with the partial basic income. The experiment would run for 2 years, starting in January 2017, and ending in December 2018. The benefit amount decided on was EUR560 per month, an amount which corresponded to the monthly net amount of the basic unemployment allowance and the labour market subsidy provided by KELA (Kangas *et al*, 2019).

Data Analysis

As mentioned above, the aim of the study was to measure the effect of the basic income on employment, income, and well-being. To measure these effects, the experiment analysed register and survey data of the beneficiaries in comparison with a matched control group. The employment and income effects were measured using the register data gathered in official registers on employment, taxable income, participation in employment-promoting activities as well as KELA benefits pay-outs. These registers included data from the 2000 beneficiaries of the basic income and the 173000 control group participants.

The effects of the basic income on well-being were measured with a survey that was conducted near the end of the pilot in October to December 2018. The survey was conducted on the 2000 beneficiaries and a sample of 5000 from the 173000 control group participants. The questions asked in the survey included questions from standardised population surveys like the European Social Survey (ESS 2016 and 2018), the International Social Survey Program and the European Union Survey on Income and Living Conditions, to compare with other Finnish and European citizens (Kangas *et al*, 2019). While the response rate for the survey was rather low at 31% and 21% for the treatment and control groups respectively, the results were later combined with the register data. Prior to this survey, phone surveys were carried out at the beginning of 2017, targeting unemployed job seekers who were KELA beneficiaries.

Findings

The results of the experiment would be released in two stages. The preliminary results for the first year of the project were released in 2019. These results include preliminary register based statistical analysis of the employment effects of the basic income experiment. It is worth noting that the introduction of the Unemployment Benefit Reform in the middle of the experiment had an asymmetric but unquantifiable effect on the labour market activities of both treatment and control group participants. Hämäläinen *et al* (2021: 11) reckon that the reform most likely increased job search and ALMP participation amongst control group members owing to the threat of benefit losses while its effect on the treatment group members would have been minimal due to the income security provided by the basic income.

1. Employment Effects

The preliminary results showed no significant difference in the number of days in employment between treatment and the control groups in the first year of the experiment. According to Kangas *et al* (2019), the average number of days in employment for the treatment group was 0.39 days more than the control group (49.6 and 49.3 days respectively). After the active measures were introduced in 2018, the final report showed that the number of days in employment increased to an average of six days more for the treatment group compared to the control group (78 and 73 days respectively) from November 2017 to October 2018. Kangas *et al* (2020) concede that the employment effects for the second year of the experiment were a joint effect of the basic income treatment and the active model.

2. Welfare effects

The survey results showed significant differences between the treatment and the control groups on key indicators that measure aspects of subjective well-being. On average, respondents from the treatment group assessed their well-being more positively than their control counterparts. They reported higher generalised and institutional trust as well as confidence in the future and self-confidence. They also showed fewer stress and depression symptoms and higher cognitive functioning than the control group. The results also showed better financial well-being for the treatment group compared to the control group. The respondents also reported as having experienced less bureaucracy than their control counterparts.

Lessons for Policy Designers

An unconditional basic income is an income one receives regularly that does not come with conditions or prescriptions on how to earn it and use it. A crucial element of this type of income is the autonomy it gives to its beneficiaries to spend the money as they please without fear or worry of it being taken away. Thus, experiments that have biases towards work effort invariably violate this crucial aspect of the guaranteed income. With the introduction of the active measures in the second year of the experiment the Finnish experiment violated this element of a UBI.

Also, while the reasoning behind the targeting of the experiment is valid given its budgetary constraints, selecting only low-income individuals rendered the experiment inherently means-tested, violating another element of a UBI, universality. As a result, at best this experiment tells us about the impact of a basic income given to low-income individuals on provision that they demonstrate work effort through participation in an active measure. But even these results lack external validity because the active measure was introduced during the experiment. The findings of the experiment regarding employment are inconclusive and invalid. A crucial argument of a guaranteed income is that people want to work, and that they do not need to be forced through conditional income schemes to work. This experiment was originally designed to test this argument between the carrot (guaranteed basic income) and a stick (conditional minimum income), but by changing the carrot for a stick amid the experiment, this shattered the validity of the entire experiment. The key problem with conditionality is that conditions distort incentives and interfere with some outcome variables of most interest like the incentive to work or behavioural changes attributable to enhanced autonomy like quitting work to further one's studies.

Ontario

Context

In 2017 the Ontario government decided to revisit the idea of guaranteed basic income with an experiment. However, after a year of implementation, a newly elected government decided to terminate the experiment and to cease the evaluation of participant activities, as the experiment was alleged to be failing (McDowell & Ferdosi, 2020). Fortunately, some preliminary data had already been collected through the use of online surveys, more data were collected from the experiment participants (Ferdosi et al., 2020a). While these events fatally compromised the quality of this experiment, some insights are worth considering.

Number of Participants

The Ontario Basic Income Pilot (OBIP) targeted low-income citizens in urban, urban-rural mix and rural locations in Ontario. While the Segal report (Segal, 2017) recommended a much more expansive and expensive test site, the Ontario government opted for a smaller and less expensive program, selecting three locations that were considered representative of the overall population of Ontario. Targeting was performed at 2 levels; firstly, at the geographical level where low-income regions were identified and selected. The selected sites were Hamilton (where 2748 benefited out a population of about 700 000), Thunder Bay (where 1908 benefited out of a population of about 120 000) and Lindsay (where 1844 benefited out of a population of about 20 000), representing Ontario's urban, rural and mixed urban/rural areas. 2000 people were assigned to the control group (McDowell & Ferdosi, 2020).

The second level targeting was conducted at the individual level, where low-income earners were targeted. The pilot selected 4000 low-income applicants who were beneficiaries of the Ontario Works and Ontario Disability Support Program. To qualify, the pilot targeted singles who earned less than CAN$34,000 per annum and couples who earned less than CAN$48,000 per annum (McDowell & Ferdosi, 2020). These beneficiaries had to withdraw from the above programs to benefit from the OBIP. However, they could retain child, disability, and old age security benefits. Income from other schemes such as the Canada Pension Plan and Employment Insurance decreased the basic income they received by a ratio of 1:1.

Experimental Question

The main purpose of the Ontario Basic Income Pilot was to test a guaranteed basic income model that would replace the Ontario Works and the Ontario Disability Support Program (ODSP) (Ferdosi et al., 2020). The problem with these policies was that while they helped people living with poverty, they were riddled with work disincentives and welfare traps. They also required broad policing, control, and monitoring. Key experimental questions that the basic income pilot purported to answer included whether the guaranteed basic income model would reduce poverty more effectively, encourage work, reduce stigmatization, and produce better health and life outcomes for its recipients. Using administrative records, interviews with key informants, and data collected from participants, the pilot evaluation team was tasked to investigate the changes in participants' food security, stress and anxiety, mental health, health and healthcare usage, housing stability, education, and training, as well as employment and labour market participation (Ferdosi et al., 2020).

Experiment Duration

In March 2016, the Ontario government announced its intention to run a basic income pilot to reduce poverty in a sustainable manner. The pilot was designed to run for 3 years, but was cancelled after its first year, upon the election of a new government which declared the pilot to be ineffective (Ferdosi et al., 2020). Besides prematurely ceasing the basic income payment to beneficiaries, the new government also put a stop to the evaluation activities in July 2018. For this reason, the findings of the pilot were severely compromised, and the pilot is generally regarded as a missed opportunity.

Data Analysis

As mentioned above, not only did the new government of Ontario cancel the basic income payments but they also stopped the evaluation activities. By so doing, the government risked losing all the valuable insights from the pilot. This meant that the access to the administrative data was terminated, and the pilot evaluators had to use online surveys and qualitative interviews to evaluate the pilot (McDowell & Ferdosi, 2020).

Findings

The primary question of the pilot was to test whether the basic income would make positive impact on people's lives. The preliminary evidence shows that the pilot did indeed improve the lives of the participants in a meaningful way. While the experiment was terminated prematurely, preliminary results show an improvement in physical and mental health, food and housing security, and social behaviour. Respondents also used the emergency healthcare less frequently than their control group counterparts while reporting increased access to health care services (Ferdosi et al., 2020).

Lessons for Policy Makers

Self-evidently, the biggest error from this pilot pertains to the issue of its premature cancellation. As explained above, this rendered the entire project a missed opportunity. While the pilot did show some positive preliminary results, these were compromised by the untimely cancellation. But unfortunately, this was not the only problem with the pilot.

Unlike Dauphin, where every household was eligible for the Mincome, in Lindsay, only households whose income met the income threshold during enrolment were eligible to take part in the experiment. The idea behind a saturation site is to be able to measure community wide and spill over effects. This universality allows for some mechanisms of a basic income to be calibrated, for instance a person who has a well-paid job can afford to resign or go on sabbatical leave, take study leave to enrol for further education or start up their own business if they wish. Labelling Lindsay as a saturation site, despite this shortcoming, increased the chance of the experiment committing type 2 errors or false negatives (Mendelson, 2019). That is, the experiment reduced the evidence of community-wide effects that would have been observed in a true saturation site.

The third criticism of the pilot pertains to its enrolment process and its implications on the randomization of the treatment group. According to Mason (2018), the experiment had a significantly low take-up rate. Out of the 37000 invitations that were sent out to areas where household incomes were likely to be low enough such that most of the households would qualify for the basic income, only 150 participants were enrolled. Compared to the expected 2000 participants, this was a far cry from the target. According to Mendelson (2019) this low take-up rate can be attributed to the prevalence of scam offers at the time that were difficult to authenticate, excessive legal jargon contained in the offers, complexity in filling out the form, and the requirement to give access to tax information. To address this problem, the government revised its enrolment strategy by setting up group meetings with households that were likely to be eligible. This method of recruitment compromised the randomization of sampling and created self-selection bias.

Barcelona

Context

Barcelona is one of the richest cities in Spain, in 2019, the average annual income for Catalan households was almost 14% more than the average household income in Spain (Idescat, 2020). The average household annual net income in Barcelona is 10% higher than the average Catalan annual household income. This growth is driven by the city's strong services sector, thriving SME industries and an employment rate that is higher than National or Catalan average (Colini, 2017). However, Barcelona also suffers from an 'urban social policies' dilemma. Since the 2008 financial crisis, poverty and social inequality have increased in Barcelona. While income per capita has increased by 11% in rich districts, it has fallen by 27% in the poorest districts. As a result, over the past 5 years, the income gap has more than doubled (Urban Innovative Actions, 2020).

Urban social policies sought to reduce inequality and poverty, while avoiding the undesirable effects of well-intended policies. One of the ills the policy sought to remedy was gentrification, which was an unintended result of neighbourhood infrastructure improvement policies which ended up crowding out poor households by raising housing prices. The second unintended consequence was economic and geographic polarization which was a result of human capital improvement policies which resulted in brain drain in vulnerable communities as the most skilled individuals migrated to areas with more opportunities. One proposed response to this problem was to provide Municipal Inclusion Support (SMI[16]) to poor families. The B-Mincome experiment was conducted to estimate the effect of such a program.

Number of Participants

In the original design of the experiment, 1000 households were selected through a random and stratified draw from 1527 eligible applicants. Of these eligible applicants, 383 households were assigned to the control group and 144 households were kept as substitutes for households that dropped out. The participants came from vulnerable households from ten neighbourhoods making up the Eix Besòs region, which is one of the poorest areas in the city of Barcelona.

To qualify for the experiment participants had to meet the following requirements.

1. The households' members must be registered in Barcelona since 30 June 2015, live in one of the neighbourhoods in the Eix Besòs area and commit to do so until the end of the project.

[16] SMI is an economic benefit that was designed to complement the income of needy households.

2. At least one member had to be aged between 25 and 60 years old on 31 July 2017 to take part in the active policies (particularly in the training and employment provision).

3. All households' members had to be living in effective co-existence[17], as legal or natural demographic changes (deaths, divorce, adoption, etc.) would change the amount of the SMI.

4. To sign the document accepting the terms and conditions for the provision of the benefit in the modality awarded and to accept payment of 25% of the SMI with the new citizen currency (REC).

5. To sign a commitment agreement to take part in processes for directly obtaining information and consent for follow-up to be conducted through public administration databases.

6. Households whose assets exceeded the value of four years' worth of estimated SMI benefit, excluding debts, the main house, and a parking space, were not included.

Due to administrative reasons, changes in employment, residential issues, household composition or motivation, some of the households in the original treatment group (1000 eligible households) were removed from the list and replaced with substitutes. In the end, 950 households remained in the treatment group (Laín, 2019).

[17] A household or unit of co-existence is made up people within the same residence with a matrimonial tie or other officially recognised stable connection and with blood ties up to the fourth degree or the second degree if the tie is by affinity, including the children of each of the parents living together as a result of reconstituted families. They must share (not divide) household expenses.

Experiment Question

The main objective of the pilot was to test the efficiency and efficacy of the SMI when coupled with an active policy on social inclusion. To examine this impact, the effect of the SMI and Active policies was tested on the following areas: changes in labour market participation, food security, housing security, energy access, economic situation, education participation and attainment, community networks and participation, and health, happiness, and well-being. The pilot was also meant to examine whether the SMI reduces the administrative and bureaucratic responsibilities of social workers. The active policies covered the following areas, training and employment, entrepreneurship in social solidarity and cooperative economy, housing reform and community participation.

Experiment Duration

The pilot lasted for 3 years, from November 2016 to October 2017 with a pilot design followed by 2 years of intervention and evaluation from November 2017 to October 2019.

Data Analysis

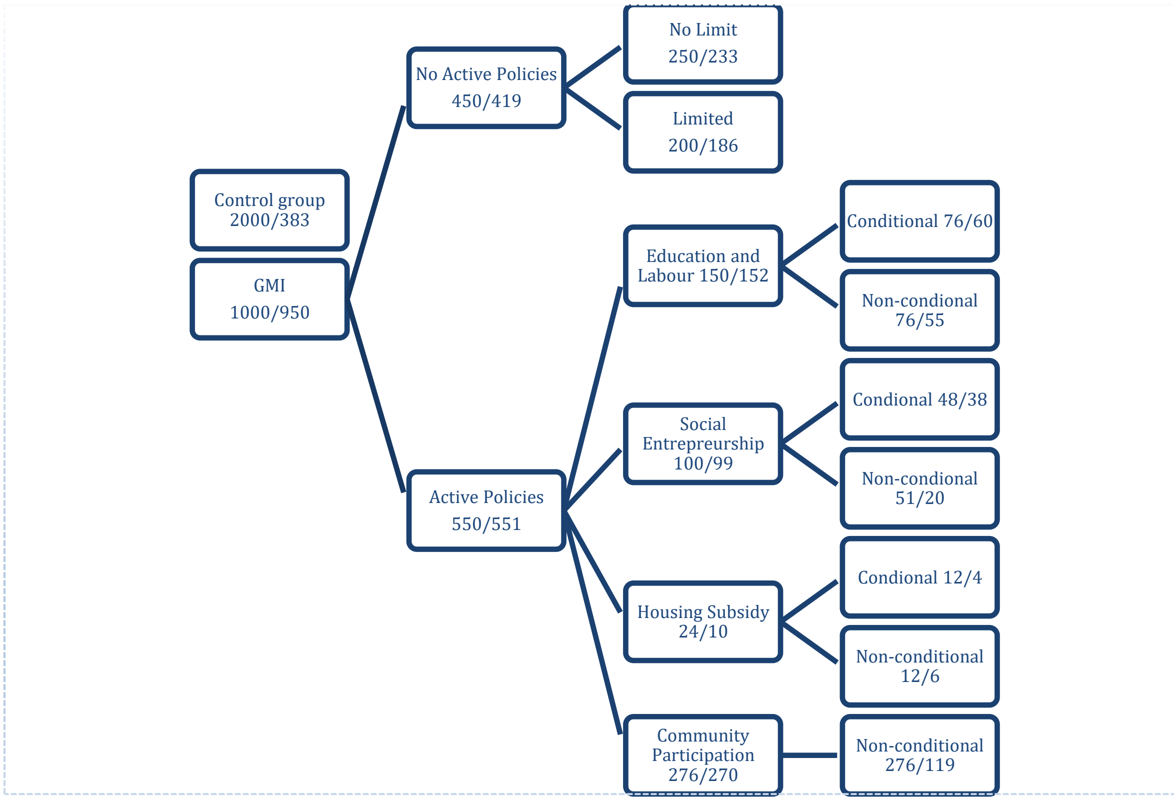
As mentioned above, the sample data were compiled through stratified randomisation. The stratification was based on the following three criteria, creating seven strata.

1) Whether the household was eligible for housing grants policy for renting rooms out, i.e., if the property had an available room measuring at least 10 sq.m. The other two criteria were applied to the remaining households:

2) The value of SMI transfer divided into three groups: EUR 1,100 or more, between EUR 600 and EUR 1,100, and less than EUR 600.
3) Employability, i.e., whether the household had at least one member, or any member that could be employed to take part in the active training and employment policy.

The treatment group was further divided into four modalities of participation: conditional and non-conditional, and limited and unlimited. This resulted in ten treatment groups with 419 households receiving the SMI only without any requirement to take part in any active policy. Of these 186 households were in the limited mode, which meant that as their private income rose, the SMI that they received was decreased. The other 233 were in the unlimited mode, meaning that the amount of SMI they received was not affected by how much private income they made. The other 531 households in the treatment group received the SMI and were also involved in active policies. Of these 152 were in the training and employment policy, 99 were in the social entrepreneurship policy, 10 were in the housing subsidy policy and 270 were in the community participation policy. The tenth group was the control group which was made up of 383 households. These modalities of the experiment are illustrated in Figure 1 below.

Figure 1: B-Mincome Treatment and Control Arms



Source: (Colini, 2019a; 15)

The first numbers in the first, second and third columns indicate the original design estimates, the second numbers indicate the actual sample collected. In the fourth column the first number indicates the number of people assigned to each active policy, the second number indicates the actual number of people taking part in the active policy. It is worth noting that for 233 households the program worked much like a UBI, i.e., the participants in this group received a guaranteed monthly income unconditionally. The only differences to a contemporary UBI were that the B-Mincome was a household and not an individual based payment and the payment was not universal. Thus, the study could not measure the individual and community wide effects. To analyse the data, the researchers conducted an initial and a follow-up survey, run in September 2017 and November 2018, respectively.

Findings

The preliminary results of the experiment show that overall, the SMI and Active policies had a positive impact on the major indicators and dimensions of interest. For instance, material deprivation and food insecurity decreased while well-being increased. Active policy groups tended to do better than the unconditional and control groups. One possible reason suggested for this was that the social relations in the active programs cultivated more prosocial interactions (Colini, 2019a). Under financial inclusion, the experiment showed that the SMI did not have a significant impact on household savings. This result was attributed to the fact that the SMI was not designed to incentivise savings. However, the experiment showed that the SMI did reduce financial uncertainty. Participants in the non-conditional and unlimited modes showed significant decreases in the need to obtain non-wage income (Colini, 2019).

The experiment also found the impact of the SMI on employment was minimal and mostly insignificant (Laín, 2019). In the conditional groups, the experiment found that finding work was negatively impacted by the lock-in-effect of work insertion programmes. For the health effects, the researchers did not have access to administrative data and so relied on self-reported data. These results showed that there was no significant difference between the self-reported health of treatment and control groups. However, respondents in the treatment group did report a decrease in mental illness and improvement in their quality of sleep.

Under community dimension, there was increased social volunteering in the active policy groups, with higher female involvement in the control group. However, the SMI showed no statistically significant impact on social participation in any of the modalities or groups. This was attributed to the initial degree of vulnerability, exclusion, and social isolation on the beneficiaries (Laín, 2019). But the respondents in the treatment group did express an increase in life satisfaction (27% from the initial score of 5 to 6.45). Again, the respondents in the active policies scored 9.5% higher than the general average of the other groups. The experiment also found that life satisfaction is proportional to income, fulltime employment, financial stability, and negatively related to the respondent's age, poor health, and introverted personality.

However, there were sampling issues with the study. Generally, studies with many treatment arms require bigger sample sizes to conserve their inferential power (Glennerster & Takavarasha, 2013). The B-Mincome pilot had nine treatment arms, six of these arms had samples less than 100 observations, and three of those six had less than 30 observations. As a result, the inferential power of this study was highly compromised.

Lessons for Policy Makers

These findings show that the B-Mincome had a positive impact on the well-being and life-satisfaction of its beneficiaries. However, due to flaws in its implementation, many beneficiaries complained of improper or insufficient calculations of their benefits, fluctuations in the amount received and delays in receiving the correct amount (Hill-Dixon et al, 2020). These flaws sustained or exacerbated the precarity, insecurity and distrust in public administration of many intended beneficiaries. According to Hill-Dixon *et al* (2020) in most cases the reason for the complaints was that the amounts due to the beneficiaries were not communicated in advance or even at all, thus not allowing them to plan and prepare. This is one of the reasons why a flat basic amount is better than a means-tested amount.

Another lesson for policy makers is that as mentioned above, household-based transfers punish couples by paying them less per person than single people, thus discouraging communal living. While the budgetary reasons for using household instead of individual based pay-outs is understandable, researchers and policy makers should bear in mind that the results obtained from this experiment do not show the full effects of an individual based guaranteed income. Also, the results pertaining to the interaction between active policies and work insertion programmes needs further investigation. Lastly, while self-reported data can be useful, it is equally important to have access to objective administrative data.

Conclusion

In conclusion there are obvious overlaps between the 1970s NIT experiments and the partial UBI experiments discussed above. The type of basic income paid in both streams was a form of NIT and not a demogrant. As a result, in all these experiments the basic income was targeted at low wage earners or people looking for work. This invariably placed a big emphasis on the labour market effects of the basic income. The problem with this emphasis on labour market effects is twofold; for one, labour market effects of a basic income cannot be fully tested in an experiment, especially localised short-term experiments like the partial ones explained above. Secondly, while negative work effort effects of a basic income might be a real threat to the affordability and sustainability of a basic income in developed countries, the experiments in developing countries that will be evaluated in the next chapter show that a guaranteed income does not necessarily have negative work effort effects. Another criticism of the focus on labour market effects is that there are more direct effects that can be measured with basic income experiments. We now turn to these experiments.

Chapter 5: Full Field Experiments

Introduction

While the guaranteed income experiments discussed thus far have mainly been carried out in in developed countries, and to a large extent to address precarious work and the working poor phenomenon, full UBI experiments conducted in the second wave experiments have mostly been conducted in the developing world. These experiments were mostly designed to measure other effects of an income guarantee besides labour market participation. They cover a range of issues from food security to women's empowerment. Only three experiments qualify as universal, unconditional, basic income experiments. These were conducted in Namibia, India, and Kenya.

Table 5: Elements of Full UBI Experiments

	Namibia	**India**	**Kenya**
Cash	✓	✓	✓
Individual	✓	✓	✓
Unconditional	✓	✓	✓
Universal	✓	✓	✓
Regular	✓	✓	✓

Namibia

Context

With average per capita income of USD 4100 p.a. in 2008, the World Bank classifies Namibia as a middle-income country. And while the country has made considerable strides in reducing its income and wealth inequality, since independence, according to the World Bank (2021) its Gini coefficient was still amongst the highest in the world in 2008 at 59. In 2002, the Namibian Tax Consortium (NAMTAX) proposed a universal grant to help put Namibia on a positive economic path. The consortium argued that the grant would help avoid poverty traps by giving people access to the economy (Haarmann & Haarmann, 2012). Building on the NAMTAX proposal, the BIG coalition developed a proposal for detailing the administration, scale, and scope of the grant. To fast-track policy discussion the Coalition decided to conduct a pilot program that would provide empirical evidence to bolster their proposal. In January 2008, the first basic income pilot in a developing country was conducted in the small Namibian village of Otjivero-Omitara (Standing, 2017). In the pilot, every resident living in the village was given N$100 each month until December 2009. The pilot was organised by the Namibian Basic Income Coalition and funded through foundations and individual donations. The aim of the pilot was to test the practicality of the recommendations of the NAMTAX for a nationwide policy (Haarmann & Haarmann, 2012).

Number of Participants

The pilot included 970 individuals registered as Otjivero-Omitara residents since July 2007, aged below 60 years. People 60 years and older qualify for civil pension of N$450, and so were excluded from benefiting from the grant[18]. The entire village community volunteered to participate. The village was chosen because of its small manageable population size, its level of accessibility and its level of poverty. While residents had access to free water due to the proximity of a large dam, the area was impoverished, prone to disease and struggling to subsist as a viable community. Thus, there was no reason to believe that Otjivero was in any way advantaged compared to neighbouring villages. The pilot did not have a control group, due to statistical and ethical considerations based on the size of the location (Frankman, 2010).

Experimental Question

The pilot originated from the Namibian Government's Tax Commission (NAMTAX) proposal of a citizenship right. The proposed Basic Income Grant (BIG) would be funded through a progressive expenditure tax on the affluent (Haarmann *et al*, 2009). The pilot was implemented to move the policy discussion forward and to produce empirical evidence on the benefits of a universal basic income. As such, the researchers collected data on the effects of the BIG on nutrition, health, education, economic activity, and gender dynamics.

[18] While the number of residents living in the village increased after the pilot started, beneficiary registration was done on 31 July 2007, anyone who moved to the village after registration was not eligible for the BIG.

Data Analysis

To assess the impact of the grant, the experiment used a fourfold research methodology. Researchers conducted a baseline survey in November 2007. The survey collected retrospective and current socioeconomic data on health, nutrition average income etc. Then during the experiment, they conducted two interim panel surveys in July 2008 and November 2008 on the same participants from the baseline survey. The researchers also collected information from key informants from and around the study area such as the local nurse, the police chief, local leaders, and shop keepers. And lastly, they used individual case studies. The impact of the grant was assessed using the pre-post impact evaluation methodology which tracks average changes in the outcomes of interest before and after the program.

Experimental Findings

The experiment tested whether the BIG would disincentivise beneficiaries from seeking work or help finance their job search or fund their entrepreneurship. According to Haarmann & Haarmann (2012) the introduction of the BIG resulted in a decrease in the unemployment rate in the treatment village by 15 percentage points from 60% to 45% between 2007 and 2008. This trend was not observed nationally, as the average rural unemployment rate in 2008 was 64%. On average, the monthly incomes (excluding BIG) increased by N$ 34 from N$ 118 to N$152. The BIG facilitated economic activity, helping beneficiaries start their own businesses. Below is a summary of the findings[19].

Table 6: Namibia Treatment Effect: Pre-Post Analysis

	Baseline	**With BIG**
Impact on Poverty		
Citizens Living below National Poverty Line (%)	86	68
Citizens Living below Food Poverty Line (%)	76	37
Impact on Education		
Number of Children not Attending Due to Financial Reasons	12	7
Drop-out Rate (%)	30	0
Impact on Economic Activity		
Unemployment Rate (%)	60	45
Average Monthly Income per capita (N$)	118	152
Total Number of Reported Economic Crimes	85	54
Stock Theft	16	9
Other Theft	21	17
Illegal Hunting & Trespassing	20	1
Other Crimes	28	27

Source: Own calculations using BIG Pilot Project Assessment Report.

[19] It is worth noting that the standard errors for these descriptive statistics were not reported, which limits the extent to which the statistical differences between baseline and endline can be assessed.

The experiment also found positive educational effects. Before the project, 49% of Otjivero-Omitara families with children of school going age reported that their children were not attending school regularly. The reasons cited for this included lack of school fees and inability to afford school uniforms. After the introduction of the BIG, 90% of primary school fees were paid. The number of children not attending school due to financial reasons dropped by 42% (Haarmann & Haarmann, 2012).

The BIG also reduced poverty. Before the BIG, 86% of Otjivero-Omitara citizens were extremely poor, living below the lower bound national poverty level of N$220 pc/pm. The introduction of the BIG led to a decrease in food poverty (excluding migrants) of 16 percentage points, and a decrease in severe poverty by 43% percentage points. From the original survey families (excluding migrants), child malnutrition dropped from 42% to zero in the first 18 months of the project. The BIG also led to better health care, as more HIV positive people could afford to fetch their medication from the clinic (Haarmann & Haarmann, 2012).

One of the anticipated effects of a guaranteed income is an increase in the consumption of temptation or ‘bad’ goods such as alcohol, drugs, and gambling. However, the BIG found no evidence of an increase in the consumption of alcohol associated with the BIG. In fact, contrary to expectations, the number of shebeens in the village after the introduction of the BIG decreased from 8 to 7 suggesting that there was no increase in the sale of alcohol (Haarmann & Haarmann, 2012). Economic crimes such as illegal hunting, stock theft and fraud dropped from 85 reported cases in the previous year to 54 reported cases in the period of assessment (Haarmann & Haarmann, 2012).

Extent of Peer Review

Empirical data on cash transfer programs is often stored on an open access online database. However, the Namibian BIG researchers refused to submit the data from this project, citing the need to protect the confidentiality of the project participants (Haarmann & Haarmann, 2012). This limited the extent to which the experiment could be peer reviewed. For instance, Osterkamp (2013) raises a few concerns about how the data were collected, citing the fact that the project report did not include the survey questionnaire. As a result, Osterkamp (2013) questions how technical economic jargon like income and unemployment were translated or explained to the respondents. Secondly, Osterkamp (2013) raises questions on the plausibility and rationality of some of the figures inferred by the study. For instance, Osterkamp (2013) questions the accuracy of the increase in per capita income, questioning its sustainability given that it was only within the first year the project had been implemented.

It is common practise for social policy initiatives and particularly cash transfer programs to create databases of empirical data about the specific project approaches and the reactions of the target groups. This data are contained in online databases which permit open access. Researchers also normally publish a detailed project methodology. However, in this respect, the BIG pilot project in Namibia was different. The researchers (Haarmann & Haarmann, 2012) explicitly refused to provide open access to their database. They argue that the Otjivero residents were promised confidentiality of their personal information. This decision affected both the internal and external validity of the experiment and as explained above, is contrary to common practices.

Lessons for Policy Makers

The Namibian Basic Income Grant pilot was a pioneering experiment in at least two ways; it was the first universal basic income experiment and the first basic income pilot to be conducted in the developing world. Local government buy-in, investment and involvement in the project is crucial for the upscaling of the project. The Namibian BIG was supposed to be a pilot project that would show the government how a basic income grant could be implemented in the country. However, the project was run by Desk for Social Development (an NGO staffed and funded by the German United Evangelical Mission), a Namibian labour economics research institute called LARRI and an advisory group of mostly foreign academics. Thus, besides, the LARRI, there were no government institutions participating in the pilot. As a result, despite the favourable results, the Namibian government refused to upscale the BIG or even consider conducting another pilot elsewhere. Once the pilot was completed, the government denied that the BIG would improve the lives of the poor and instead adopted alternative development strategies. The first was increasing pensions and then implementing the Targeted Intervention Programme for Employment and Economic Growth (TIPEEG), which later proved to be inefficient at fighting poverty, as it is capital intensive and created very few, low paying jobs (Haarmann, Haarmann & Nattrass, 2019). Eventually, the Ministry of Poverty Eradication adopted a food bank program that mostly benefited the urban poor but not the rural communities (Haarmann, Haarmann & Nattrass, 2019). However, perhaps the greatest flaw of this experiment was in the researchers' refusal to share their data and methodology. This refusal seriously affected what could be learned from this experiment and rendered it worthless.

India

Context

The second universal basic income experiment was conducted in Madhya Pradesh, which is one India's most rural regions. The experiment was inspired by the two relatively smaller pilots, the Namibian BIG discussed above, and the Delhi Choice Pilot. Building on the lessons from these experiments, the Madhya Pradesh Unconditional Cash Transfer (MPUCT) was also an innovative experiment in that the experiment sought to test the joint impact of income and voice mechanism. So far most basic income experiments have focused on income security. The MPUCT was the first UBI experiment to test the impact of a basic income mechanism when paired with a voice mechanism.

According to Standing (2017) income security is a necessary but insufficient condition for republican freedom. The second "meta-security" that is necessary for republican freedom is a voice of agency organisation. This organisation functions as a mechanism that gives individuals access to collective representation, allows them to guard against unaccountable domination and limits the power of dominant classes. In this way, a voice organisation functions as an instrument of democratisation by providing individuals institutional representation.

The MPUCT pilot worked with the Self-Employed Women's Association (SEWA), an Indian NGO that functioned as a voice organisation. SEWA is a union organisation that seeks to improve income and work security, social security, and self-reliance. SEWA also advises government on policies, mobilises women and helps agencies implement policies. The designers of the MPUCT wanted to test whether there would be a significant difference between areas where there was a combination of SEWA and BI compared to those that had either SEWA, BI or neither.

Number of Participants, Selection Criteria and Justification

For studying the double effect of a UBI coupled with a voice mechanism, Indore district in Madhya Pradesh was chosen as the test site. This decision was primarily informed by SEWA's presence in the region (Davala et al., 2015). From this district, 50 villages were identified, of which, 20 were SEWA villages[20] and 30 were non-SEWA villages. Sample villages were selected randomly from a list of villages with similar basic infrastructure and amenities (Bharat and SEWA, 2014). The sample was drawn by IDF, which is an independent research organisation. In the treatment villages, everyone was given the basic income, while in the control villages no one received it. The impact of the basic income would be measured by comparing outcomes between individuals, families, and villages in the treatment group versus matched participants in the other partial treatment arms and the pure control villages.

To justify the randomisation at the village level, SEWA pointed out that an individual or household level randomisation would create inter-family resentment. Such randomisation would also be self-defeating since it is most likely that there would be sharing between treatment and control neighbours. Also, the village-wide sampling would allow for the estimation of community-wide effects. The decision to use a sample of villages with an

[20] A SEWA village was defined as a village where SEWA had been operational for at least 5 years before the pilot started.

average village size of 100 households across the treatment and the control group was based on two reasons. The first was that this village size was the most efficient given the budget of the project. Secondly, 100 households is the average size of most Indian villages (Bharat and SEWA, 2014).

Another criterion that the MPUCT used in selecting participants was that the villages selected for the pilot (both treatment and control) were non-tribal villages. The reason for using this criterion was that tribal villages tend to be more homogeneous in terms of demographics, economic activities, and social hierarchies. Non-tribal villages on the other hand tend to be heterogenous in these regards. As such, the villages selected for the pilot were at least 50 percent non-tribal (i.e., at least half the village population was made up of non-tribal members).

From the original surveyed participants who took part in the baseline survey, 4874 individuals were eligible to receive the unconditional cash transfer. However, once the pilot was launched, 673 individuals came forth, claiming they had been erroneously excluded from the eligibility list of participants. This increased the number of beneficiaries to 5547. A total of 11231 people participated in the experiment, of which 5547 were in the treatment group (Davala et al., 2015; Bharat & UNICEF, 2014).

Experiment duration

The experiment was originally designed to last 12 months, starting in June 2011 and ending in May 2012. A baseline survey was conducted in May 2011. However, before the experiment ended, UNICEF volunteered to add additional funding for another 6 months. Thus, the experiment ended in November 2012.

Experimental Question

The experiment sought to test the double effect of a guaranteed basic income coupled with a voice mechanism. As mentioned above, Standing (2017) believes that these two are the complementary components that foster republican freedom. Thus, the sample was stratified into two groups: SEWA and non-SEWA villages. Impact measurement indicators included financial inclusion, housing and sanitation, nutrition and diet, health, education, income and assets, and women's empowerment (Bharat & UNICEF, 2014).

Data Analysis

Having decided on the study site and the eligibility criteria, 50 villages were filtered as eligible for the MPUCT. These villages were then assigned to treatment and control groups through matched randomisation. The matching was based on the similarity of each pair of villages' socio-economic structures, their distance from urban centres and their proximity to a public primary school. These villages were then stratified into SEWA and non-SEWA villages. There were 20 SEWA villages and 30 non-SEWA villages. For statistical power, there had to be at least 10 villages for each stratum, so the team chose 12 SEWA and 15 non-SEWA villages. This sample was then clustered and matched across treatment and control groups based on the following objective variables.

- The proximity of the village to secondary/middle school
- The proximity to a registered doctor
- The distance and access to proper roads
- Connectivity to a bus or public transport
- Presence of Public Distribution System (PDS) shops

A 70 percent similarity coefficient was decided as the minimum similarity rate between clustered villages. 10 villages were shortlisted from each cluster. Finally, 4 villages were randomly selected from each set of 10 villages as treatment villages. This resulted in a list of 8 villages in the basic income group and 12 villages in the control group (Bharat & UNICEF, 2014).

To measure the impact of the cash transfer, the researchers conducted four individual surveys and two community surveys. These included a baseline survey before the pilot started; an interim survey halfway through the pilot; a final evaluation survey upon completion of the pilot; a post final evaluation several months after the last payment was made to measure legacy effects; and two community surveys at the beginning and end of the pilot to measure community wide effects. These surveys were supplemented with hundreds of family case studies and feedback from key informants.

Findings

The experiment found that the unconditional cash transfer had a positive impact on the measured variables. Households in the treatment villages had significant improvements in their living conditions, upgrading their sanitation, cooking, and lighting energy and getting mosquito protection. The basic income also had positive food security and nutritional effects. More respondents in the treatment group shifted towards market acquired food as opposed to acquiring their food from the food security system. There was also a shift to a more vegetable and fruit-based diet. With respect to the health effects, the treatment group reported lower incidents of illness and spending on medical care than the control group (Bharat & UNICEF, 2014).

The experiment also showed that the basic income had positive effects on schooling. Households from the treatment group increased their spending on schooling needs (including uniforms, fees and stationery). School registration figures were also higher for the treatment villages compared to the control villages, with teenage girls showing the biggest increase. School attendance and performance was also better in the treatment group compared to the control group (Bharat & UNICEF, 2014).

The unconditional cash transfer also had positive economic and equity effects. The changes in earned income (excluding the transfer) were higher for the treatment group compared to the control group. This was a result of many people, mostly women expanding or starting their own businesses to supplement their wage income. There was also a higher increase in productive assets in the treatment group compared to the control group. The emancipatory value of the cash transfer was found to be higher than its monetary value. The reason for this is that in cash-scarce areas the value of money as a commodity increases driving up the cost of loans, debt and prospective costs of shocks and hazards (Davala et al., 2015). This phenomenon is called the Lauderdale Paradox. In such places, the basic income gives recipients the vital liquidity needed to weather the shocks, hazards and uncertainties of life

(Bharat & SEWA, 2014;191). This emancipatory value is driven by both the financial freedom the basic income gives each beneficiary and the increase in the supply of money which is a scarce resource in poor communities. This results in the reduction in the price of money as a commodity, thus, combating debt traps by reducing high short-term borrowing interest rates which can be as high as 40 percent a year (Davala et al., 2015). The MPUCT found that the basic income enabled some households to reduce their debt, some to avoid getting into debt and some to borrow enough to improve their living conditions or to invest in income earning activities.

Lessons for Policy Makers

One of the unique features of the MPUCT was its testing of the impact of voice. Indeed, the evidence suggested that basic income recipients benefited more from the advice and organisation of collective agency, particularly women, who benefited both relatively and in absolute terms. Women in the SEWA villages became financial citizens, participating more in social and economic decision making compared to before the pilot and in comparison, to their counterparts in the control villages.

Collective agency also played a role in helping with financial inclusion. More than 90 percent of the accounts that were opened during the period of observation were assisted by SEWA or IDF. These accounts stayed open after the cash transfers ended and were used for other transactions besides receiving the transfers. The pilot also indicated that for a UBI to work optimally, financial inclusion is necessary, and that there is a positive loop, wherein the UBI can induce universal financial inclusion.

Kenya

Context

Kenya is one of the three biggest economies in Sub Saharan Africa. Although Kenya's economy is the largest and most developed in eastern and central Africa, according to the Kenyan Institute of Public Policy and Research (2012) 20.1% of the Kenyan population lived below the international poverty line of less than USD 1.90 per person per day in 2011 PPP terms. This poverty is associated with economic inequality, government corruption and health problems (Mwega, 2009). While the country is not as aid dependent as its SSA neighbours, ODA still plays a significant role in its economy. Between 1980 and 2006, 1.2% of all ODA transferred to developing countries went to Kenya. Among SSA countries, this amounted to 3.3%, in the same period (Mwega, 2009).

In 2011, Give Directly[21] launched an unconditional cash transfer (UCT) targeting households living below USD 1 per capita per day (Haushofer & Shapiro, 2013). The pilot was designed to examine the efficiency of unconditional cash transfers in delivering assistance to the needy. This was the 3rd basic income experiment to be conducted in the developing world. The main aim of the project was to provide evidence of cash-based aid as opposed to other forms of aid. The pilot started in April 2011 with the identification of the study area. A baseline survey was conducted in May 2011. The UCT payments were carried out between June 2011 and January 2013. After the last payments were made, an end line survey was conducted in February 2013.

[21] Give Directly is an international NGO that pays unconditional cash transfers to poor households in developing countries.

Number of participants

As mentioned above, the UCT project targeted poor households. Selection into the study sample was carried out at two levels: the village and household level. Rarieda, Kenya was selected as the study district based on national census data which showed that the district had high poverty rates and sufficient M-Pesa penetration to make the payment of the transfers feasible (Haushofer & Shapiro, 2013). Within the district, 100 villages were chosen based on the prevalence of eligible households in the village. The eligibility of households was based on whether the home was made up of mud, grass, and other non-solid material. Once the villages with the highest number of poor households were identified, the researchers used objective criteria to enrol the poorest people from each village. This was done by doing door-to-door sign-ups to verify the macro data and check for fraud.

This resulted in a sample of 1500 households in 100 villages. Randomisation was then carried out on two levels:

1. Across villages where 50 villages were randomly split into exclusively treatment and control groups.

2. Within the treatment villages, 50% of the households were randomly selected to receive the UCT while the other half would be tested for spill over effects.

This resulted in 500 households for each the treatment, spillover and pure control groups. With a sample size of at least 1000 households from all three groups the researcher's power calculation showed that the sample is sufficient to detect an effect size of 0.2 standard deviations with 89% power (Haushofer & Shapiro, 2013). The treatment effect within the treatment groups (gender, frequency, and size) could be compared with 60% power.

Experiment question

The researchers aimed to study the economic and psychological impact of an unconditional cash transfer. The experiment sought to measure the economic effects on consumption (including consumption of "bads" like alcohol, cigarettes etc.), income, enterprise and asset holdings, broader welfare effects with respect to health, food security, education, women's empowerment and intra-household bargaining and domestic violence. The impact of the UBI on the psychological well-being would be tested using stress analysis through cortisol level measurements. Secondly, the experiment also tested whether the UCT had a gender effect by testing whether there were differences in outcomes between households where the recipient of the UCT was female versus male recipients. Thirdly, the researchers wanted to test whether returns to the transfer are increasing or decreasing. The project also sought to examine the efficiency of mobile banking solutions such as M-Pesa as delivery methods. To examine the question of how the UCT should be structured, the researchers looked at the outcome variance given the following design parameters:

- Gender randomisation determined whether the transfer was paid to the male or female head of the household.
- Payment structure determined whether the transfer was paid as a lumpsum or in nine monthly instalments.

- Transfer size measured the impact of the magnitude of the transfer. In small transfers, the beneficiaries were paid USD 404 PPP per month, while large transfer beneficiaries received a USD 1,121 PPP per month top-up from the small transfer for 7 months.

Data Analysis

The experiment used a two-stage cluster-randomized controlled trial (Haushofer & Shapiro, 2016). The first stage randomizations led to treatment and control villages, and the second stage randomization carried out at the household level resulted in treatment and spillover households within the treatment villages. The spillover effects were to be examined by comparing outcomes in spillover households with the pure control group households. Spillover effects were to be identified by using GPS data on village location to identify cross-village spillover effects.

Within the treatment villages, the gender of the recipient[22] (primary male or primary female), frequency and period of transfer[23] (monthly, lump-sum) and the magnitude of the transfer[24] (small v large) were randomized. This randomisation allowed researchers to control for gender, payment structure and size of the transfer. These controls provided for the assessment of the impact of the UCT on the following sub-treatment arms:

[22] The male v female head of the household randomisation only applied to cohabiting couples.
[23] Lump sum v monthly instalment randomisation only applied to small transfer group.
[24] Large transfer only applied to treatment households.

1. Gender randomization allowed researchers to identify differential welfare effects between households with both male and female primary heads.
2. Magnitude randomization allowed the researchers to test whether returns to transfers were increasing or decreasing.
3. The frequency randomization allowed for an analysis of the existence of savings and credit constraints.

The data were collected through surveys (baseline, midline, and end-line), anthropometrics and biomarkers. The surveys were in two parts, a household, and a personal survey. The household survey recorded household assets, consumption patterns, food security, health, and education data. The personal survey collected data on psychological wellbeing, intra-household dynamics and economic preferences. Anthropometric data collected were on height, weight, and upper-arm circumference measurements of children less than 5 years old. And finally, saliva samples were collected to measure cortisol levels (Haushofer & Shapiro, 2016).

<u>Findings</u>

Table 7 shows that overall, the UCT had a significant and economically meaningful impact on the outcomes of interest, i.e., asset holdings, household consumption, food security, self-employment revenue, psychological well-being, and female empowerment (Haushofer & Shapiro, 2013). With respect to recipient gender, the researchers found no evidence of a gender differential in economic outcomes. However, the trends in the point estimates suggest that homes with male household recipients tended to have larger impacts on standard measures of economic welfare. On the other hand, homes with female household recipients tended to have larger impacts on outcomes that benefit children and they scored higher on the women empowerment index (Haushofer & Shapiro, 2013). The researchers suggested that larger samples with higher power might detect significant differences in these outcomes. With respect to returns to transfer, the researchers found large and significant differences between outcomes for small compared to large transfer beneficiaries, with large transfer beneficiaries showing better outcomes. These outcomes include asset holdings, household consumption, psychological well-being, and female empowerment (Haushofer & Shapiro, 2013). And lastly, with respect to the frequency of the transfer, the researchers found that this had a significant effect on outcomes. Monthly transfer beneficiaries showed improved food security while lump-sum beneficiaries showed higher levels of asset holding (Haushofer & Shapiro, 2013).

Among economic effects, the UCT had a positive impact on consumption across a range of goods including food, educational and medicinal expenses. However, contrary to expectations, there was no significant increase in the consumption of “bads” (tobacco and/or alcohol). There was also an increase in the investment in livestock, furniture, and roofing improvements for thatched roofs households compared to the control group (Haushofer & Shapiro, 2016). Most of the investment came from lump-sum recipients as opposed to the monthly transfer group. This implies that the monthly recipients were savings constrained. There was also no significant difference in the economic outcomes of spill over households compared to the pure control village households. This meant that there was no significant impact on the neighbours of treatment villages (Haushofer & Shapiro, 2016).

Table 7: Treatment Effects: Index Variables

	Control mean (std. dev.)	Treatment effect	Female recipient	Monthly transfer	Large transfer	N
Value of nonland assets (US$)	494,80	301,51***	-79,46	-91,85**	279,18***	940
	(-415,32)	(-27,25)	(50,38)	(45,92)	(49,09)	
		[0,00]***	[0,52]	[0,28]	[0,00]***	
Nondurable expenditure (US$)	157,61	35,66***	-2,00	-4,20	21,25**	940
	(82,18)	(5,85)	(10,28)	(10,71)	(10,49)	
		[0,00]***	[0,92]	[0,99]	[0,22]	
Total revenue monthly (US$)	48,98	16,15***	5,41	16,33	-2,44	940
	(90,52)	(5,88)	(10,61)	(11,07)	(8,87)	
		[0,02]**	[0,92]	[0,59]	[0,84]	
Food security index	0,00	0,26***	0,06	0,26**	0,18*	940
	(1,00)	(0,06)	(0,09)	(0,11)	(0,10)	
		[0,00]***	[0,92]	[0,13]	[0,25]	
Health index	0,00	-0,03	0,10	0,01	-0,09	940
	(1,00)	(0,06)	(0,09)	(0,10)	(0,09)	
		[0,82]	[0,72]	[0,99]	[0,72]	
Education index	0,00	0,08	0,06	-0,05	0,05	823
	(1,00)	(0,06)	(0,09)	(0,10)	(0,09)	
		[0,43]	[0,92]	[0,99]	[0,84]	
Psychological well-being index	0,00	0,26***	0,14*	0,01	0,26***	1474
	(1,00)	(0,05)	(0,08)	(0,08)	(0,08)	
		[0,00]***	[0,43]	[0,99]	[0,00]***	
Female empowerment index	0,00	-0,01	0,17*	0,05	0,22**	698
	(1,00)	(0,07)	(0,10)	(0,12)	(0,11)	
		[0,88]	[0,51]	[0,99]	[0,22]	
Joint test (p-value)		0,00***	0,11	0,04**	0,00***	

Source: Haushofer & Shapiro, 2016: 1999

Among psychological effects, the researchers found that treatment households reported an increase in well-being, with large transfer recipients recording higher improvements compared to the small transfer group. The improvement was attributed to a decrease in depression and stress and an increase in life satisfaction and happiness. While there was no significant difference in self-reported indicators between the lump-sum and the monthly transfer recipients, the cortisol levels for the monthly recipients were significantly higher compared to the lump-sum recipients. One suggested explanation for this discrepancy is that the monthly recipients stressed about their inability to save and/or invest their income.

The UCT also had a positive impact on women's empowerment with households that had female primary recipients measuring 0.16 standard deviations lower cortisol levels compared to their male primary recipient counterparts. These estimates were attributed to increased female decision-making power and reductions in domestic violence. The UCT also had a positive impact on psychological well-being, particularly amongst female primary recipients and large transfer recipients.

Lessons for Policy Makers

The size of the transfer had a significant impact on recipient's psychological well-being and their ability to save and make economic investments. While the amount of the UCT seems very small (US$ 0.75 per person per day), many rural Kenyans live on less than US$ 1.00 per day and so the transfer made a big difference. This experiment shows that with a small amount, some of the benefits of a UBI to the poorest people in the world can be realised. Indeed, the size of the UBI is an important consideration for policy makers; and as such this aspect of the UBI requires further investigation.

Chapter 6: Discussion

What the Experiments Show

The main reason for the proposal of a UBI is that it is a potentially powerful tool for social justice as it promotes real freedom for all (Van Parijs, 1997). While targeted assistance programs are effective in relieving poverty, they do face the following limitations: large coverage gaps due to non-take-up of poor households, resource leakages to the rich at the cost of the most needy, and susceptibility to poverty traps as discussed above (Francese & Prady, 2018). A universal unconditional basic income is argued to be instrumental for combating poverty, inequality, and the effects of precariousness (Standing, 2017). However, as reviewed above in Chapter 2, opponents of a UBI argue that it is an inefficient way of solving these problems and that instead it adds to the problem by rewarding slacking (Van Parijs, Cohen & Rogers, 2001).

Besides providing counter arguments to these objections, the UBI experiments reviewed above can provide insights into some of these objections. Perhaps the best way to respond to the efficiency objection above is to run an efficiency analysis comparing a UBI with a targeted program. While there have been several studies that have tried to simulate such a study *ex post* (Banerjee, Niehaus & Suri, 2019; Ortiz et al., 2018; Hanna & Olken, 2018), no experiment so far has been conducted for this specific purpose. This chapter summarises the findings of the field experiments reviewed above and draws on some useful insights from natural experiments that can help answer some of these questions. We also build upon some lab experiments that have been conducted to shed insights into generalisable UBI mechanisms. Lastly, we offer some recommendations on how experimental methodologies could be adjusted to support more reliable and policy relevant insights. First, we look at how the empirical data measures up against the objections levelled above.

The Inefficiency Objections

Opponents of a UBI argue that giving everyone a basic income is inefficient and wasteful as it spreads the welfare amount too thin to have a real impact on those who truly need assistance and is superfluous to those who do not need it. They argue that targeted welfare programmes are better suited to address poverty as they identify the poor through means testing and provide the support that they need. While this argument can be refuted by making the claim that a UBI that is set at or above the poverty line will by definition eradicate poverty (Widerquist, 2018), the experiments above are consistent with the hypothesis that a UBI avoids exclusion errors and is quite effective in alleviating poverty, improving health outcomes and nutrition, combating intergenerational poverty by improving educational outcome and helping people out of welfare traps (Haushofer & Shapiro, 2016; Davala et al., 2015; Bharat & UNICEF, 2014; Haushofer & Shapiro, 2013; Haarmann & Haarmann, 2012; Munnell, 1986).

Another inefficiency argument against UBI is that giving money to everyone reduces the amount that could be given to the needy and thus exacerbates inequality. However, if the same amount of UBI is given to both poor and rich individuals, it will represent a larger share of total income for the poorer individuals than it will for richer individuals, thus reducing relative inequality (Atkinson, 2013). Also, depending on the funding mechanism, a UBI that is funded through a gap-reducing mechanism such as a progressive tax (be it an income or wealth tax or both) will reduce inequality. While empirical data on this mechanism is scarce, in Alaska, the Permanent Fund Dividend did increase income inequality (Goldsmith, 2002). The reason suggested for this is that while the poor use the dividend for basic consumption the rich reinvest it thus making them richer. This phenomenon has implications for both the funding mechanism of the UBI and the consumption bundles of the poor which will be discussed later. Both these aspects still need careful investigation.

The third inefficiency objection against UBI is that it makes people work less and rely on the guaranteed income. According to Phelps (2001) by guaranteeing income security a UBI overlooks the satisfaction of self-support, development, participation, and contribution. Thus, instead of remedying the effects of precariousness particularly associated with low-wage work, Phelps (2001) argues that a UBI would make low-wage work extinct. The persuasiveness of Phelps' argument about the importance of low-wage work goes beyond the scope of this paper. Of course, requiring higher incentives to draw people into dull, uncomfortable, and dangerous work is plausibly a good thing. Nonetheless, the experiments reviewed above did test the hypothesis that a guaranteed income makes people want to work less and enjoy more leisure time. The 1970s NIT-inspired experiments focused on this aspect. It is worth noting that these experiments tested the effects of a NIT and not a UBI. It is also worth noting that the distinction between the two models plays a significant role in work incentive analysis. An NIT has a tax back mechanism which while designed to make sure that those who earn a private income are better off than those who rely solely on the guaranteed income still falls short to a UBI which does not have this clawback mechanism. Nonetheless, the 1970s experiments failed to indicate the existence of a significant work disincentive (Munnell, 1986). Instead, more recent UBI experiments in India, Kenya and Namibia have shown that income guarantee makes people work more (Bharat & UNICEF, 2014; Haushofer & Shapiro, 2013; Haarmann & Haarmann, 2012). These conflicting results warrant further investigation and possible research methodologies will be provided below.

<u>Other UBI Effects</u>

1. Economic Effects

A UBI can be used on consumption, or it could be used to accumulate capital. The latter could be done by funding education or investing in physical and financial assets. While means tested programmes place a limit on how much one can earn in order to qualify for a transfer, a UBI functions as an income floor, placing no ceiling on how much beneficiaries can save and invest. In Kenya for instance, the UCT led to significant increase in savings (Haushofer & Shapiro, 2016).These savings can allow individuals to afford assets that will contribute to their long-term income (Banerjee, Niehaus & Suri, 2019). For instance, in Namibia, the increase in savings was reflected in the increase in large and small livestock and poultry (Haarmann & Haarmann, 2012). They also act as safety net allowing individuals to invest in human capital development reflected in the increase in educational outcomes for both boys and girls (Banerjee, Niehaus & Suri, 2019; Davala et al., 2015; Haushofer & Shapiro, 2013; Haarmann & Haarmann, 2012). Also, the income guarantee allows beneficiaries to start their own businesses, for instance, in India, the UCT led to an increase in entrepreneurial risk taking (Bharat & UNICEF, 2014). UBI also increases money supply which puts downward pressure on exploitative interest rates, particularly in rural areas where the financial sector is characterised by inefficiently high interest rates in the informal sector and cheap but inaccessible formal loans (Banerjee, Niehaus & Suri, 2019; Bharat & UNICEF, 2014). In this way, a UBI facilitates financial inclusion and equitable economic growth.

2. Gender Effects

Spousal economic dependence is a major source of gender inequality (Baxter & Kane, 1995). Dependent spouses often do not take part in household decision making, are limited in terms of exercising their exit options out of the spousal agreement and in extreme cases are subjected to domestic abuse (Baxter & Kane, 1995). As explained above, household level assessment of welfare eligibility overlooks financially dependent spouses. UBI experiments have shown that individual transfers can have a levelling effect in intra-household power dynamics and can help facilitate exit options for spouses who wish to exit the spousal agreement.

In Namibia, women who were UBI recipients reported more control over their sexuality and less pressure to engage in transactional sex (Haarmann & Haarmann, 2012). In Kenya, UBI improved women's status in the female empowerment index (Haushofer & Shapiro, 2013). In India, female beneficiaries in SEWA villages reported that UBI empowered their household financial decision making (Davala et al., 2015). It also facilitated their financial inclusion, allowing then to open bank accounts (Bharat &

UNICEF, 2014). Women in SEWA villages reported more equitable distribution of household income, resulting in increased girl child nutrition and secondary school enrolment (Bharat & UNICEF, 2014).

UBI also affects gendered labour market inequalities. For instance, UBI can limit the financial risks of care work, offer those who choose to engage in care work the financial flexibility to do so, and remove the disadvantages of unpaid care work. For instance, in Dauphin, the GAI allowed women to take longer maternity leave, reduce work hours to take care of their children and take longer work breaks (Forget, 2011). A UBI also attenuates the feminization of informal work which further propagates the gender pay gap. For instance, in India, the UCT resulted in an increase in participation by women in formal economic activity (Bharat & UNICEF, 2014).

What Main Things Should be Tested

Funding Mechanisms

Should a UBI be considered an economic stimulus that is implemented for a defined period or is it a basic right that all citizens should enjoy, both current and future? On the benefit side, GiveDirectly is currently running the longest UBI experiment in Kenya, designed to last 12 years. It is hoped that this experiment will reveal the long-term effects of a guaranteed basic income. On the supply side insights can be drawn from the three natural experiments in the Smoky Mountains study, the Alaskan Permanent Fund, and the Iranian *de facto* Basic Income. These schemes offer insights into three funding mechanisms.

The Smoky Mountains social dividend is financed through investment in mostly casino and other gaming corporate ventures. The scheme started in 1997 and benefits about 15000 Eastern Band Cherokee Indians (Lewis, 2017). Over the years the dividend has grown from USD 4000 per capita per annum in 1997 to USD 3000 bi-annual dividends in 2017 (Lewis, 2017). Similarly in Alaska, the Permanent Fund was established in 1976 when oil production began in the Prudhoe Bay field which is the largest oil field in North America. The fund which is funded through a 25 percent share of royalties from the Prudhoe Bay field as well as production, property and income taxes from the energy sector was designed to save a share of the public revenues generated from non-sustainable resources (Goldsmith, 2002). These revenues are invested into the fund which in turn invests only in income generating investments. The earnings from the fund are averaged out over 5 years to mitigate expenditure driven inflation and paid out to roughly 700000 Alaskan citizens (Goldsmith, 2002). The Iranian *de fact*o basic income is by far the largest in terms of scale. The program started in 2011 and has nationwide coverage of around 72.5 million Iranians, paying out roughly USD 500 per capita per annum (Tabatabai, 2012). The basic income was funded by phasing out high fuel price subsidies and allocating these funds directly to households. While the Iranian model is like the Alaskan model in terms of its reliance on the oil sector, one of the differences between the two models is that the former is funded through current fuel consumption tax revenue, the latter is funded with reserves from accumulated revenues from past oil tax revenues that are invested into income generating investments (Tabatabai, 2012).

These funding mechanisms require further investigation as their generalisability is susceptible to the following flaws. The Cherokee Indian model works very well for a small tribe, but one can anticipate some difficulties in scaling up the scheme to the national level. The basis of this model's success is the region's large and long-term successful gaming enterprises. But this strategy cannot be copied in all regions. The Alaskan model of using a permanent fund is also attractive but not all countries have such large reserves of natural resource revenues. The Iranian model could work for many developing countries with high government subsidy budgets but if those subsidies are based on natural resources, then the sustainability of such schemes needs to be assessed. Most of the empirical data on UBI focuses on the benefits or lack thereof; not much empirical investigation is devoted to experimenting with different funding mechanisms. Yet this is an important aspect of the sustainability of the policy. These are just some mechanisms that could be tested and further developed.

Optimum Amount

The 1970s NIT-inspired experiments were designed to estimate the work disincentive associated with several guaranteed income plans. The plans were designed in such a way that policy makers could be able to choose from them the one that had a positive impact on poverty alleviation while carrying the least work disincentive. For the reasons explained above, these experiments, their inferences and the validity of those inferences have been contested both in terms of their internal and external validity. The former in terms of how the experiments were designed, and how the data were collected and analysed (Ashenfelter & Plant, 1990; Munnell, 1986; Cogan, 1983; Robins & West, 1980; Moffitt, 1979); the latter in terms of what inferences could be drawn from them and the generalisability of those findings (Widerquist, 2005). The recent experiments in Barcelona, Finland and Ontario have taken the work disincentive premise for granted. However, due to the reasons given above, their findings on this effect were inconclusive. Besides being terminated prematurely, OBIP had selection biases from the start (Hiilamo, 2020). The B-MINCOME and the Finnish pilot tried to test the labour market effects of a guaranteed minimum income program when coupled with an active labour participation program, but this complicated the analysis of the pure effects of a guaranteed income (Neuwinger, 2021; Hämäläinen, Verho & Kanninen, 2021; Mason, 2018).

The recent UBI experiments in the developing world, however, warrant a step back from the generally held assumption that a guaranteed income carries a work disincentive. Contrary to expectations of an income effect, the basic income experiments in Namibia, Kenya and India resulted in greater work effort from beneficiaries. This was so despite the lack of employment opportunities in the rural test sites of the experiments. Beneficiaries engaged in informal labour, craft and various entrepreneurial activity instead of enjoying more leisure. This seems to support Karelis' (2007) theory of increasing marginal utility of basic goods.

Karelis (2007) postulates that goods can be categorised into three groups: pleasers, relievers and composites of pleasers and relievers. Pleasers are the normal goods that cause positive experiences. These goods exhibit diminishing marginal benefits from consumption. They include ice cream and holiday vacations. Relievers are goods that relieve negative experiences. These goods exhibit increasing marginal benefit from consumption, with utility growing faster than consumption. These are pain, unhappiness and misery reducing goods. The third group of goods functions as relievers in lower levels of consumption and as pleasers at higher levels of consumption. They include basic goods such as food, shelter, clothing, transportation, and leisure. These goods have three levels of consumption: insufficient levels where shortfalls cause unhappiness and consumption yields increasing marginal benefit, sufficient level of consumption where consumption neither result in misery or happiness, and above-sufficient level where the consumer derives utility from consumption but at a diminishing rate.

Based on the developing country experiments, a UBI functions much like a basic good. At lower levels of consumption beneficiaries seem to experience increasing marginal benefits from consumption, i.e., as the guaranteed income rises, the poor can relieve more and more of the deprivations that they face. According to this theory, there should be a point where these basic needs are sufficiently met. So far, basic income experiments pegged to a poverty line[25], calibrated in such a way that they can relieve some basic needs like food poverty but not enough to pull beneficiaries out of poverty. While these experiments have provided key insights on how the poor spend the various guaranteed income amounts, none of the experiments thus far have focused on investigating the optimum guaranteed income amount sufficient to alleviate the depravations faced by the poor.

Consumption Behaviour

The main purpose of a UBI is to reduce insecurity; this contributes to other outcomes regarded as improvements, in particular: alleviating poverty, reducing inequality and improving general welfare. Thus, as opposed to just giving poor people money and

[25] Apart from the Kenyan experiment which provided a basic and a generous amount.

ignoring whether it serves the purpose it purports to serve, designers and policy makers of a UBI are supposed to make sure that its intended beneficiaries are indeed capable of escaping poverty because of it. Thus, a UBI must be designed in way that both allows people to decide how they want to escape poverty and capacitates them to follow through on their chosen path. The consumption bundles, behaviour and patterns associated with a UBI are an important consideration in this regard.

To this end, two aspects are worth consideration: the consumption bundles of the poor and avoidance of conspicuous consumption. By its nature, a UBI is an economic stimulus as the money is spent, it boosts economic growth. This is particularly so when the expenditure is on local goods where it carries a multiplier effect which further increases growth. In this sense a UBI can be a catalyst for a virtuous cycle. This was reflected in the increase in entrepreneurial activity in the basic income sites in Namibia, Kenya, and India.

Secondly, the consumption patterns of the intended beneficiaries have a normative aspect. Designers and policy makers want to ensure that the UBI is used to purchase necessities and not to fund conspicuous consumption, to acquire assets and/or non-durables and not be spent on temptation goods. While such paternalistic concerns are in tension with the goal of promoting real freedom and dignity, the field experiments conducted in developing countries show that the UBI was spent locally and not spent on temptation goods and conspicuous consumption. However, these experiments targeted rural locations which are relatively disconnected from global markets. In a nationwide scheme, where beneficiaries live in both rural and urban settings, different results could be observed. It is important for UBI designers to calibrate a UBI with the most beneficial economic stimulus. This is another aspect of a UBI that requires careful experimentation.

Intergenerational Traps

Given that a UBI is a mechanism of direct redistribution and assuming that it is financed appropriately (e.g., through a progressive tax) it should have a levelling effect on economic inequality and might even mitigate an intergenerational poverty trap. It is worth noting that poverty traps manifest differently in developed countries compared to developing countries. The manifestation of poverty traps in developed countries has been discussed above under conditionality and means testing. In these countries, the poor sometimes choose not to seek work, especially low paying jobs since, doing so would disqualify them from welfare support programs. Thus, in developed countries, a poverty trap is synonymous with a welfare trap. To this end, many basic income experiments in developed countries are trying to calibrate the mechanism of a basic income with active policies. As explained above, this aspect still needs further investigation.

However, in developing countries poverty traps are associated with multiple deprivations, inadequate access to basic services, poor health, low incomes, low education levels, poor housing conditions and financial exclusion (Araujo et al, 2018). These households are not able to invest in their children's education even though the returns on this investment are very high. This results in ghettoization of poverty and intergenerational poverty. This issue is intertwined with the optimum size and consumption bundles of the poor. For instance, in Kenya where the transfer recipient was randomised between male and female heads, the researchers found that when the money went to the male head, there was a larger impact on economic welfare (assets and consumption) whereas when the recipient was the female head the money was mostly spent on children's health, education and food security

(Haushofer & Shapiro, 2013). This brings more structure to the study of the optimum size of the UBI and the typical consumption bundles of the poor.

Design Imperatives for Future Experiments

Experimentation on UBI is driven by two forces which in the past have unfortunately been conflated: UBI as a government policy solution and UBI as an academic pursuit. The former has specific questions that are of interest to politicians while the latter has a different set of questions that appeal to academics. While these are not necessarily mutually exclusive, they are not always perfectly aligned. As a result, due to financial demands of conducting field experiments, particularly of the UBI nature, researchers have often relied on governments and state agencies to fund the experiments. The results of this have been detrimental to both parties (Noguera & De Wispelaere, 2006). For instance, both Canadian experiments were terminated due to political issues (Ferdosi et al., 2020b; Forget, 2012). In the US, the experiments faced political opposition due to false marital dissolution effects (Forget, 2011a; Hausman, 1973). And when the experiments were not terminated, they were altered in mid-course, compromising the validity of the studies (Hämäläinen, Verho & Kanninen, 2021; Hiilamo, 2020; Kangas et al., 2019). And while these studies all suggested that a basic income guarantee can alleviates poverty, none of the governments have implemented UBI as a national policy. On the academic side, political interference has led to inconclusive experiments, poor design and missed opportunities (Neuwinger, 2021).

It is important for future research on the topic to reduce government and political interference from academic studies of UBI. Governments can run UBI pilots in order to test their ability to implement such a policy, but these pilots should not be conflated with academic research on the matter. Due to the financial constraints of running long large-scale UBI field experiments, researchers should look for alternative ways to fund and study UBI mechanisms and effects. For instance, they could incorporate insights from both natural and field experiments, building on these findings to design lab experiments, which often cheaper and attract less attention from third parties.

Future Experimental Methodologies

So far UBI experiments have been mostly in the form of field experiments. While these experiments have in most cases been quite insightful, there is a slight exaggeration about their inferential power. While field experiments can elucidate key features of a UBI, what can be learned from them is limited due to the complex economic, social, political, and cultural facets of such a policy, which cannot be studied simultaneously in one or a few field experiments. According to Widerquist (2018) the ideal UBI experiment is a global experiment lasting 50 to 100 years, with randomisation into treatment and control group conducted at the national level. He explains how an experiment of this level would ensure statistical significance and allow researchers to examine long-term national-level community effects. However, Widerquist (2018) also concedes that such a mammoth experiment is not feasible.

Despite this seeming impracticality, large-scale field experiments are not the only or always best way to collect empirical data. For one, due to their high project costs, these experiments tend to be funded by governments, state agencies, or multinational NGOs. This renders them susceptible to political manipulation which may manifest as interference with the research process, or its outcomes as explained above (Noguera & De Wispelaere, 2006). Field experiments also face design constraints that affect both their scope and validity. For the former, the range of questions that can be studied in an experiment are limited by the size of the experiment. The more the number of questions the experiment seeks to answer, the bigger its sample size must be. If this is not well calibrated, the validity and robustness of the experimental findings will be compromised (Noguera & De Wispelaere, 2006). An example of this was the B-MINCOME which had 9 treatment arms from only 950 households, with the housing treatment arms having only 6 and 4 households for the non-conditional and conditional control arms respectively (Colini, 2019).

Noguera and Wispelaere (2006) argue that lab experiments are better suited for some types of UBI experimentation for the following three reasons. Firstly, because lab experiments are conducted in relatively closed environments, they require less funding and are thus less susceptible to political interference. Secondly, lab experiments have a greater capacity to uncover behavioural phenomena like judgement and choice behaviour. Lastly, while field experiments can only be run once, lab experiments can be reiterated in a series of experiments. This makes them cheaper and more flexible (Noguera & De Wispelaere, 2006).

Field experiments are a good form of testing ground to understand how basic income guarantee would function in real life situations. They also better support estimations of structural models. But they do not tell us much about basic income guarantee mechanisms. Lab experiments are better suited to study these mechanisms as they allow researchers to test a wide range of variables under controlled conditions, allowing researchers to identify and isolate causal relationships pertaining to behavioural phenomena.

Another troublesome feature of a guaranteed basic income is the idea that the unearned income carries an income effect that disincentivises people to work. This conception of work motivation is based on the premise that all work activities generate disutility and that in order to motivate agents to put in work effort, principals or employers must pay wages high enough to offset the work disutility. However, Deci (1972) showed that work activities can be categorised in at least two groups depending on the source of the motivation to do them. Deci (1972) postulated that some activities have high intrinsic motivation and others have low or no intrinsic motivation. Depending on the type of work and the motivation an agent has for doing it, external reinforcements like financial compensation can have a positive or negative effect on work motivation and effort (Deci, 1972). To understand this mechanism better, Pech (2010) suggests the use of motivation crowding theory in lab experiments to examine labour market effects of BIG on "good versus bad" jobs, where the former are jobs considered to have high intrinsic value and the latter with low or no intrinsic value.

Chapter 7: Conclusion

The evidence above does not show that a UBI is a panacea, however, this claim was never made nor tested. Social welfare should be construed as a dynamic phenomenon, responding, and adapting to the context of time and place. The simplicity of the idea of a UBI should not be mistaken for simplicity of its effects and implementation. A guaranteed income is a complex tool for social justice that encompasses many mechanisms both known a priori and/or posteriori. The experimentation with UBI helps with the understanding of the latter.

These experiments allow researchers and policy makers to test and modify theories, collect data, and evaluate policies. There are three main types of experiments: lab, field, and natural experiments (Harrison & List, 2004). Lab experiments have the highest inferential power but the most abstraction from reality whereas natural experiments are the most real but have the least generalisability. For this reason, field experiments are considered by many researchers the sweet spot between the types of experiments which has probably led to over-reliance on them. However, the distinction between these types of experiments should not be used isolate them but to use them as complements.

With respect to empirical data on UBI thus far, the labour market benefits of a UBI far outweigh its costs across all measured metrices. However, there is still a lot of room for the investigation of other mechanisms and effects of a UBI that are imperative for social justice. These include: the gender, mental, and psychological effects, and funding mechanisms. In this pursuit it is important to remember that "economics is not an exact science but a combination of art and elements of science… that we are not converging towards exactitude, rather we are improving our databases and our ways of reasoning about them" (Samuelson, 1915).

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