Universal basic income
A scoping review of evidence on impacts and study characteristics

Marcia Gibson, Wendy Hearty, Peter Craig
What Works Scotland (WWS) aims to improve the way local areas in Scotland use evidence to make decisions about public service development and reform.

We are working with Community Planning Partnerships involved in the design and delivery of public services (Aberdeenshire, Fife, Glasgow and West Dunbartonshire) to:

- learn what is and what isn’t working in their local area
- encourage collaborative learning with a range of local authority, business, public sector and community partners
- better understand what effective policy interventions and effective services look like
- promote the use of evidence in planning and service delivery
- help organisations get the skills and knowledge they need to use and interpret evidence
- create case studies for wider sharing and sustainability

A further nine areas are working with us to enhance learning, comparison and sharing. We will also link with international partners to effectively compare how public services are delivered here in Scotland and elsewhere. During the programme, we will scale up and share more widely with all local authority areas across Scotland.

WWS brings together the Universities of Glasgow and Edinburgh, other academics across Scotland, with partners from a range of local authorities and:

- Glasgow Centre for Population Health
- Improvement Service
- Inspiring Scotland
- IRISS (Institution for Research and Innovation in Social Services)
- NHS Education for Scotland
- NHS Health Scotland
- NHS Health Improvement for Scotland
- Scottish Community Development Centre
- SCVO (Scottish Council for Voluntary Organisations)

This is one of a series of papers published by What Works Scotland to share evidence, learning and ideas about public service reform. This paper relates to the What Works Scotland Mobilising evidence for Public Service Reform by: understanding and getting evidence into action and Poverty and Inequality workstreams.

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Abbreviations

AFDC  Aid for Families with Dependent Children
AFDC-UP  Aid for Families with Dependent Children - Unemployed Parents
APDF  Alaska Permanent Dividend Fund
BI/G  basic income/guarantee
BLT  Bantuan Langsung Tunai (BLT) Direct Cash Transfer programme
CBA  controlled before and after study
CCT  conditional cash transfer
DiD  difference-in-difference
GSMS  Great Smoky Mountains Study
HIC  high-income country
IGRA  Indian Gaming Regulatory Act
ITS  interrupted time series
ITT  intention-to-treat
LIC  low income country
LMIC  lower middle income country
Mincome  Manitoba Basic Income Experiment
MPUCT  Madhya Pradesh Unconditional Cash Transfer Pilot
NIT  Negative Income Tax
RCT  randomised controlled trial
TOT  treatment-on-treated
UCT  unconditional cash transfer
UMIC  upper middle-income country
1. What is already known and what this report adds

What is already known

There is increasing political and academic interest in the potential effects of implementing a universal basic income scheme in which all individuals are provided unconditionally with a substantial, regular sum of money on a long-term basis.

While a universal basic income has never been implemented, there have been a number of interventions that involve the unconditional provision of a substantial amount of money to individuals or households. We conducted a scoping review to identify evidence on the design, evaluation and impacts of such interventions.

What this study adds

This is the first study to use scoping review methods to systematically identify, extract, and interpret evidence from relevant studies. Data from the included studies extends our understanding of the effects of these interventions and will be of use for planning of pilot interventions and evaluations.

We identified 28 studies of ten interventions conducted in a wide range of settings. Some of these provided unconditional cash transfers to large populations on a permanent basis. The intervention designs were diverse, and the interventions were conducted in a wide range of contexts. Where studies report similar effects for dissimilar interventions, confidence in the findings is strengthened.

Evaluations used a range of experimental, quasi-experimental and qualitative study designs. Confidence in the findings of a number of included studies was reduced by small samples, multiple intervention arms, or by poor standards of reporting. However, a number of studies used large samples and robust quasi-experimental methods.

We found evidence on labour market participation, health, education, and a range of social and economic outcomes. In studies conducted in the 1970s, there were small reductions in labour market participation for male household heads, and larger reductions for second earners and single parents. Recent studies found little impact on labour force participation overall, with inconsistent effects in studies reporting women’s participation, and increases for small business owners.

There were consistent positive effects on how long young people stayed in education. A number of studies reported modest to strong positive effects on a range of health outcomes, including low birthweight, adult and child mental health, service use, and diet. There were also some strong positive effects on social outcomes such as parenting quality and offending.
Some adverse impacts have been reported, such as small increases in accidental mortality following receipt of transfers, and increased substance abuse and reduced productive activity, primarily among young people. However, many of these interventions involved large lump sum payments, and much of this evidence is from small qualitative studies.

There was some evidence of spillover effects on outcomes such as mental health, consumption, labour market demand, and remaining in education. There was also evidence that some effects on child mental health and parenting quality grew stronger over time. All of these have the potential to generate cost savings in the longer term.

The studies provide useful insights into intervention and study design and implementation. Political opposition was a major issue for a number of the studies, as was managing public and media perceptions. In some cases, participants were subject to media intrusion or hostility from non-recipients.

Where interventions were universal or included large populations, it was possible to gain insights into spillover and community level effects. Simple interventions with large samples are needed to strengthen confidence in findings. Quasi-experimental methods can provide robust evidence in situations where RCTs are impracticable. Economic evaluation would advance understanding of the costs and benefits associated with wider social and economic effects.
2. Executive summary

Background

The idea of providing all individuals with a basic income (BI) which is unconditional, substantial, and regular is attracting increasing interest from policymakers and researchers worldwide. An unconditional BI is seen as a potential solution to decreasing job security and the predicted automation of many routine jobs. Reflecting this interest, pilots of basic income or similar interventions have been initiated in a range of locations. Since the effects of a universal, permanent BI would differ from those of a trial, it is difficult to extrapolate impacts from small or short-term studies. However, studies of interventions or policies unconditionally providing substantial, regular payments to individuals or families can provide evidence on the potential effects. Existing studies can also provide insights into intervention and study design, implementation, and the scope of what can be measured in such an evaluation.

Methods

To establish what can be learned about the potential impacts of a basic income, and about evaluation and implementation of relevant interventions, we conducted a scoping review of studies of interventions which unconditionally provided substantial cash transfers to individuals or families. We excluded interventions aimed only at particularly vulnerable groups or conducted in low income countries as they are less likely to be transferable to the relevant context. We included RCTs, quasi-experiments, qualitative studies, and before/after studies with a separate comparison group which reported the effects on any outcome of unconditional payments for low income people or the general population. We also searched for ongoing or planned pilot studies. We extracted data on intervention and study design, implementation, outcomes and data sources. We also extracted an overview of impacts, considering how far included studies met the definition of a basic income, and what could be learned about the potential effects of BI on this basis. Data were charted, thematically analysed, and reported in light of the research questions, in line with recognised scoping review approaches. As this is a scoping review, formal critical appraisal was not undertaken, but readily apparent methodological issues were noted and discussed.

Intervention design

We found 28 studies of ten interventions. The interventions were diverse and were implemented in a wide range of settings. Five negative income tax (NIT) interventions were evaluated in North America in the 1970s. There were five contemporary unconditional cash transfer (UCT) interventions; two fuel subsidy reform interventions and a basic income pilot were conducted in Asian low and middle income countries (LMICs) from 2005 to the present day, and studies of two dividend distribution interventions conducted in North America...
included data spanning from the early 1960s to 2015. The underlying purpose varied, but all unconditionally provided substantial payments to individuals or households. Five interventions and one study site were universal or available to everyone who met certain criteria, and the remainder included geographically dispersed samples. They ranged in duration from 9 months to 30 years, and three interventions were permanent or intended to be permanent. All the NIT studies guaranteed a subsistence level income, but were withdrawn at varying rates in response to other income. The remaining interventions provided varying proportions of household income below subsistence level, but were not affected by income from any other source. There may be issues of transferability of evidence from studies of somewhat different interventions in disparate settings. However, where similar effects are found across a number of dissimilar studies, confidence that the results would be replicated in other contexts is strengthened.

**Study design**

The majority of the interventions were evaluated using RCT and quasi-experimental study designs. There were several qualitative studies and one controlled before and after study. A number of the studies were limited by small sample sizes or multiple subgroups, but there were some innovative approaches to quasi-experimental evaluation of policy level interventions using routinely collected data. All the studies reported some measure of labour market participation, and all but one reported impacts on some aspect of health. Other outcomes reported by one or more of the studies included measures of education, marital dissolution, crime, and a range of economic outcomes. Qualitative studies investigated mechanisms linking transfers to health, impacts on community relations, and attitudes towards transfers.

**Methods issues**

Confidence in the findings of a number of the included studies was reduced by small samples and multiple intervention arms. In addition, evidence on community level and spillover effects was limited because many of the studies used scattered samples and targeted low income populations. However, there were studies of policy-level interventions and one “saturation” site in which anyone below the income threshold was eligible for payments. These used innovative quasi-experimental study designs to identify individual and community level effects where interventions were universal or targeted larger populations. Useful evidence on intervention mechanisms, community perceptions, and influence on community relations was generated by the qualitative studies. However, standards of reporting in some cases led to reduced confidence in the findings.

**Implementation**

Many implementation issues related to framing and the way in which politicians, the press, and the public viewed the intervention. The belief that unconditional cash transfers
promote laziness and dependency was widespread and often led to negative publicity and political hostility. Media interest was sometimes intrusive and resulted in loss of anonymity. Where payments were only available to certain groups, there could be jealousy towards recipients and conflicts around eligibility. Lack of cooperation between research teams and all relevant levels of government was a source of considerable operational issues for the early NIT experiments.

Findings of the studies

Completed studies

All of the studies reported impacts on labour market participation. The NIT studies reported effects on male household heads, second earners, female household heads, and young people. The remaining studies reported employment for men and women, young people, small business owners, or for the population as a whole. There were small reductions in hours worked for male heads in the NIT studies, which primarily involved longer spells between jobs. Absolute reductions for second earners and female heads were small, but larger as a percentage of lower baseline hours. There was evidence to suggest that second earners and single parents spent more time in the home, and that the presence of pre-school children in the home was a stronger predictor of hours worked for male heads than NIT receipt.

In the contemporary studies, there was either no effect or a small positive effect on male employment. Female employment impacts were less consistent, with one study showing no effect, one a small positive effect, and one a small negative effect, suggesting that female labour market participation is more influenced by contextual factors. There was some evidence from both groups of studies that small business owners and farmers increased the hours worked in their own businesses. The NIT studies reported reductions in adolescent employment, and child labour decreased in two LMIC studies which reported it. There was consistent evidence of children and young people spending more time in education. Evidence on educational attainment was less consistent, but some studies reported positive effects. The evidence suggests that where people reduced labour market activity, the time gained was channelled into other productive activities.

A number of studies reported modest to strong positive effects on a range of health outcomes, including low birthweight, adult and child mental health, service use, and diet. Improved parenting quality and reduced financial strain were among the suggested mechanisms underlying some of these improvements. These effects were less consistent than those for labour market outcomes and educational participation, possibly because the outcome measures or the samples included in the analyses differed. Several studies reported no effects on marital dissolution, and one study reported strong positive effects on parenting quality, family relationships, and child personality traits. Studies that collected data on criminal behaviour reported reductions. Effects on health and social outcomes were
often stronger in the most disadvantaged or at risk groups. A number of educational and health impacts were stronger than typical effects of interventions directly targeting these outcomes.

There were some adverse impacts, with two studies reporting small increases in accidental mortality following receipt of transfers, in part linked to substance abuse. However, it seems that this is a recognised pattern following receipt of any large payment, including salaries and standard social security payments. Some small qualitative studies also reported increased substance abuse and reduced productive activity, primarily among young people, although these interventions also included a large lump sum for young adults on reaching 18 years. In some cases the methods used were unclear. Social conflict also arose in some cases where there was resentment around eligibility criteria for targeted payments.

Where interventions were universal or affected large populations, there was some evidence of spillover and community level effects. A universal intervention which found no reduction in labour market participation reported that potential reductions were offset by higher consumption, which led to increased demand for labour. One study reported a large reduction in hospital admissions for the whole community in the saturation site, although only 30% of residents received supplements. In the same intervention, there was a larger reduction in labour market participation in the saturated site, apparently due to reduced stigma associated with transfer receipt. Several studies reported increased business activity. There was also evidence that improvements in some social and health outcomes grew larger over time, suggesting that a permanent intervention could have cumulative effects. There was, however, no evidence that permanent interventions had stronger impacts on labour market participation. We did not identify any studies which attempted to evaluate the economic impacts of changes in outcomes such as service use, educational participation, or business activity.

Pilot studies

We also identified five ongoing or planned pilots, and one which began in 2017, but was subsequently cancelled following a change of government. Three ongoing studies are in high income EU countries, and one is in a lower middle income African country. One US study is still in development, and one Canadian study has been ended early. The interventions evaluated include two fixed value UCTs, two means-tested UCTs, a negative income tax, and a basic income. All but one intervention targets people who are on low incomes or unemployed at the time of enrolment. Two interventions are being evaluated using an RCT design, and one is a cluster RCT randomising at village level. The remaining three studies appear to be RCTs, but we could not locate information on the allocation method. Excepting the cluster RCT, all the pilots have adopted a dispersed sample approach. All studies are collecting data on labour market outcomes, and on health and well-being, in addition to a wide range of other outcomes.
In common with the completed studies, several of the pilots are limited by small samples or complicated interventions with multiple study arms. Some of the studies make use of administrative data, which will save on research costs and reduce the research burden for respondents. All the ongoing studies have succeeded in developing payment arrangements and working alongside existing tax and benefit systems. Pre-pilots have been used by some to refine recruitment and implementation approaches. Some studies have encountered difficulties with recruiting respondents, and one opted for mandatory participation, which raises ethical concerns. In one case, interactions with the media led to misleading reports of purported results, potentially altering respondents’ responses to the trial. Political resistance has been a major issue for some pilots, hampering implementation and leading to the cessation of the cancelled pilot. Since most of the pilots target dispersed samples of low income or unemployed individuals, evidence on the effects of a universal, permanent basic income will be limited.

**Conclusions**

The evidence from diverse interventions and settings suggests that impacts on labour market participation are small for male heads, and for men and women in contemporary studies. In groups where reductions in labour market activity occurred, time seems to have been channelled into other productive activities. There is also consistent evidence that children and young people spend longer in education. Although less consistent, there is evidence of positive impacts on some health and social outcomes. Some studies reported spillover or wider economic effects such as reductions in health service use and increases in business activity.

A number of studies used innovative quasi-experimental methods to provide robust evidence in situations where RCTs were impracticable. Future studies should aim to include large samples and test a simple intervention. Using economic evaluation to assess any effects on service use and wider economic impacts would provide data on the net costs and benefits of basic income.
### 3. Summary of impacts table

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<th>Outcome/Intervention</th>
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4. Introduction

Background

There is growing interest in the concept of providing all individuals with an unconditional and substantial basic income in many countries, including Scotland. A ‘full’ or ‘true’ basic income is generally defined as universal, permanent, unconditional, and not affected by other income (Basic Income Earth Network, undated). Some definitions also require that it should cover basic subsistence (Standing 2002). Because a basic income that meets these criteria has never been implemented, evidence on the potential effects is limited. However, there is relevant evidence from a number of studies of interventions that are similar to basic income. In the context of discussions about piloting a basic income in Scotland, this scoping review identifies and synthesises the available evidence on intervention and evaluation design, implementation, and intervention effects.

The rapid growth in political interest has been facilitated by an effective and vocal international movement that has been researching and campaigning for basic income for decades. A number of civil society organisations and think tanks have recently added their support to calls for a basic income, including the World Economic Forum, the Royal Society of Arts, and the Adam Smith Institute. However, there is a great deal of controversy around the idea of a basic income, with strong arguments both for and against. We provide a brief overview of some of the key arguments below.

Arguments for basic income

Proponents argue that basic income would promote social justice and equality, because every citizen would be entitled to the same payments (Standing 2002). It would reduce poverty and income inequality by redistributing resources more equitably (Beck 2005). Basic income would also increase liberty because people would not be forced to sell their labour in order to survive (Zwolinski 2015). Work disincentives inherent in current benefit systems would be removed, and the cumbersome bureaucracy associated with targeting and means testing could be drastically slimmed down (Painter and Thoung 2015). It would be possible for people to devote time to caring, education, community work, and creative or business projects, and it could improve health by reducing stress and stress-related health behaviours. There could be many wider effects such as reduced property crime, higher educational attainment, and increased entrepreneurship. All of these could lead to cost savings through reduced spending on services, or to increased economic growth. Crucially, in the current context, it is argued that basic income could help to deal with increasing job insecurity, in-work poverty, and the threat of mass job losses due to automation.

Arguments against basic income

One of the key arguments against basic income is that it would encourage people to give up work, and thus promote economic dependency. It is also argued that it is unaffordable, and would require taxes to be raised to an untenable level (JRF 2018). Opposition on the left
argue that basic income would not reduce poverty, because it would divert funds currently targeted at those in most need (CBPP 2016). They also argue that it would provide a justification for the abolition of all other social programmes (Open Democracy 2017), which is indeed envisaged in some right wing proposals (Murray 2016). People with higher needs would not be served by a flat level payment, and it does not address the issue of highly variable housing costs (JRF 2018).

The current policy context

From a public health perspective, the growing acceptance of the importance of structural and economic determinants of health and health inequalities means that there is a great deal of interest in the idea of simply giving people money. At a UK policy level, the argument that welfare payments disincentivise work and lead to so-called dependency on benefits has had a strong influence on the direction of policy. This is reflected in the design of Universal Credit, which extends in-work conditionality to a large proportion of the working-age population. In the Scottish context, attitudes to social security reform are quite different and the use of sanctions to enforce conditionality is not seen as a positive development (Scottish Government 2016). Within a policy framework which seeks to promote dignity in the social security system, and to evaluate the impact of different approaches, many people view experimentation with basic income positively.
5. Research questions

In the Scottish context of feasibility work on evaluating a basic income, this review provides an overview of the impacts of interventions similar to basic income, and what can be learned from them about the potential effects of a universal, permanent basic income. This context informs the nature of the research questions, which are focused on what can be learned from the existing evidence about a number of issues, including:

- The nature of the interventions evaluated in previous studies, and the study designs used to evaluate them.
- The data sources used and outcomes measured in previous studies.
- How methodological or implementation issues may have influenced the findings of previous studies.
- How contextual factors may have influenced the findings of previous studies.
- Deriving from all of the above, any practical lessons which might inform the evaluation of the planned Scottish pilots.
Although gendered impacts of basic income are not a focus of this review, we report a brief overview of the evidence on this topic in the discussion section.

**Issues with existing evidence**

Researchers point to gaps in the existing evidence base, primarily due to the difficulty of implementing or evaluating a universal, permanent intervention (Widerquist 2018). Many studies have been of short term interventions with dispersed samples of low income respondents. The effects of such interventions would clearly be very different from those of a ‘true’ BI. In particular, it is likely that labour markets and labour market participation would be affected quite differently by a universal, permanent basic income (BI). People might become more selective when seeking employment, thereby forcing employers to increase wages, or they could settle for lower wages since they already have a baseline income. Increased consumption could also lead to greater demand for labour. Together with higher wages, this could fuel inflation. It is also possible that people would reduce labour market participation more if payments were permanent, as they would have the security of knowing payments would continue.

Other aspects of basic income that would have an important influence on labour supply responses include the value of payments, whether they are made unconditionally, and whether they are withdrawn in response to other income. Clearly, if payments were sufficient to live on and not conditional on work effort, it would be possible for people to withdraw from the labour market. Income thresholds for receipt, and the rate of withdrawal when the threshold was reached, could also be expected to influence decisions around numbers of hours worked. Lack of conditionality would also influence decisions around alternative uses of time, such as caring for relatives, starting a business, returning to education, or volunteering.

As well as these different effects on labour supply and demand, it is likely that a universal, permanent intervention would have spillover effects across every area of society. Spillover effects occur when an intervention has effects on people or outcomes that are not the original target of the intervention. For instance, receiving payments could mean some people are less likely to engage in criminal behaviour, leading to an overall reduction in crime, which in turn could mean fewer injuries, less stress for potential crime victims, and reduced pressure on health, police, and judicial services. Some potential spillover effects such as increased consumption or entrepreneurship could result in higher economic productivity. There could also be cost savings across many public services if, for instance, health improved or crime was reduced. If a BI was permanent, it is also possible that some effects would change over time.

Other issues that are difficult to investigate in a trial setting are the wider effects of making payments to all, regardless of income. How people on higher incomes would respond to payments is not known, but if an actual basic income were implemented, the tax system
would be modified to fund the BI, and payments to high income people would be recovered via taxes. A basic income is also likely to induce attitudinal and cultural changes, such as by altering the perceived value of unpaid work. It would take some time for such changes to become apparent.
6. Methods

Notwithstanding these challenges in understanding the effects of a universal, permanent basic income, there are studies which provide evidence on the effects of similar interventions, and also on their design, implementation, and evaluation. There have been traditional literature reviews describing some of these studies, but no reviews using systematic approaches have been conducted. Scoping reviews use systematic methods to search a number of bibliographic databases for relevant studies, and to extract and present data from the studies. While it is not as comprehensive as a systematic review, the process of data extraction is intended to provide an objective overview of the available evidence.

We conducted a scoping review to identify studies which provided evidence on the design, implementation and impacts of relevant interventions. We followed a framework for scoping reviews developed by Arksey and O’ Malley (2005) and refined further by Levac et al (2010). In line with Levac et al, the research questions were broad at the outset, but following Arksey and O’ Malley, the concept was delineated, and the intended outcome of the review informed the research questions. In this case the review is intended to inform the development of evaluation plans for a Scottish pilot, therefore the research questions sought to locate evidence with practical application.

Study inclusion criteria

We sought evidence from interventions in which individuals or households were unconditionally provided with substantial, regular sums of money on a regular basis. The inclusion and exclusion criteria were refined iteratively, and differ slightly from those laid out in the protocol (Gibson, Hearty, Craig and Watson 2017). While other criteria are often included in definitions of basic income, we deemed universality, permanence, unconditionality, and payments which are fixed in value (in relation to other income), and provide for basic subsistence to be the most important features in terms of understanding the potential impacts of a ‘true’ basic income.

We focus on how far the included studies meet these criteria when considering what can be learned from them. We did not include universality or permanence as there are very few such interventions, and we did not exclude studies with payments which were withdrawn or were below subsistence level, in order to assess the influence of variations in these criteria. Therefore, we focussed on unconditional payments of substantial value made regularly to individuals or households.
We had originally planned to include studies from any country, but we found more unconditional cash transfer (UCT) studies than anticipated, many of which were conducted in low-income countries that experience extreme poverty. Since transferability to the current context is likely to be limited, we narrowed the focus to studies conducted in lower middle, upper middle, and high-income countries (LMIC, UMIC, and HIC respectively). Many interventions were targeted at particularly vulnerable groups of people. We considered these to be of limited relevance, and these were also excluded. Finally, there were some studies that compared UCTs with conditional cash transfers (CCTs). Since our interest is in UCTs compared with existing systems these were excluded.

**Population**

The population of interest was the general population, low income, or unemployed people. This meant that targeted social assistance programmes aimed at people on low incomes were included, as long as there were no other eligibility criteria.

Interventions aimed at particularly vulnerable groups, including children, elderly people, people in extreme poverty, widows, orphans, disabled people, and people suffering from particular diseases were excluded.

**Intervention**

We included programmes involving regular cash transfers, whether this comprised a fixed sum or fluctuated in response to other income. One-off cash payments were excluded.

Cash payments which were conditional upon labour market participation, health service use, educational attendance/attainment, or any other behavioural requirement were excluded.

**Setting**

Using the World Bank classification of countries by income level, studies conducted in low income countries were excluded. Hence, we included studies conducted in lower middle, upper middle and high income countries.

**Comparison**

Studies with comparison groups receiving no intervention were included, as were studies which involved different study arms providing variants of the intervention in addition to a group receiving no intervention.

Studies which compared UCTs with CCTs or in-kind transfers were excluded.
Outcomes

There were no restrictions on study inclusion by outcome reported, but we did not extract impact data on all reported outcomes. The outcomes for which we extracted impact data are listed below under ‘Data Extraction’.

Study design

Evaluation studies with a comparison group, including RCTs and cluster RCTs, quasi-experimental studies, and before and after studies with a separate control or comparison group were included. Process evaluations and qualitative studies were included. Methodological publications that provided information on the role of study or intervention design, implementation or context in relevant completed studies were also included. As there has been a recent upsurge in pilots of basic income and similar interventions, we also mapped ongoing and planned studies.

General commentary and advocacy publications which did not include empirical analysis were excluded. Simulation studies that use existing data to model the predicted effects of policy changes were also excluded.

Search Methods

We developed a search strategy using terms related to basic income, negative income tax, impacts, and study design. Five databases were searched using these terms in April 2017 (see Appendix 2). The information scientist and researchers kept a search diary detailing search terms used and databases searched. On completion of screening the initial database searches, it became apparent that there were many studies of UCTs, including dividend payments such as the Native American casino studies, which the original search had not identified. We therefore conducted further pragmatic searches of a limited number of databases using narrow search terms. The detailed search methods and the search results for the NIT and UCT searches are presented separately.

Negative income tax studies

We hand-searched several relevant sources, including the websites of Mathematica Inc. and the Institute for Research on Poverty, two of the prominent research organisations involved with the evaluations. Both of these sites provide bibliographies of NIT publications, which were also searched. Widerquist (2002) provides a comprehensive bibliography of academic publications relating to the NIT experiments, and this was also hand-searched. Finally, many of the references identified by the database searches were for books that reported the NIT findings. We screened the contents of the books to identify the most relevant chapters.
Unconditional Cash Transfer studies

In addition to screening the original database searches for relevant publications, we conducted further searches of four bibliographic databases and eight specialist international development databases for publications on unconditional cash transfers (see Appendix 1). We used the search terms “unconditional cash transfer”, “tribal casino”, and “Alaska Permanent Dividend Fund”. We also hand-searched the journal Basic Income Studies and the websites of the Basic Income Earth Network and the Citizen’s Basic Income Trust. In addition, we had a number of publications belonging to our own collections.

Selection of studies

The search results were uploaded to Endnote. Inclusion decisions were recorded and reasons for exclusion were noted in Endnote. Search results were screened by one reviewer and a 10% sample was checked by a second.

We did not manually upload all of the NIT references identified in the sources described above. Instead, the screening criteria described below were applied prior to uploading selected references to an Excel spreadsheet for more detailed screening. References duplicated in any source were also excluded at this stage. We then collated the findings from the electronic database searches, our own collections, and the hand searches described in order to identify suitable papers for extraction.

Multiple research organisations worked on the NIT evaluations, and they generated publications running into the thousands. Many publications reported the same outcomes, or interim findings from early in the studies. In addition, the initial findings for some outcomes were subsequently re-analysed, with potentially significant implications for the interpretation of the findings. We adopted a pragmatic approach to selecting publications for detailed extraction. We prioritised papers reporting outcomes of particular interest to this review, but we recorded all outcomes reported by the publications uploaded to Excel. The criteria for selecting references for manual upload are described below.

- We did not upload impact papers which reported early or interim findings.
- The initial findings on labour supply and marital dissolution were later called into question, and several papers reporting differing interpretations were published. We uploaded these.
- Papers reporting any outcomes other than labour supply or marital dissolution were uploaded.
- Papers focusing on methods, design, or contextual issues of particular relevance to our research questions were uploaded.
Data extraction

A data extraction form reflecting the information required by the research questions was developed and refined as necessary. Two reviewers piloted the form independently on a small number of studies. Information was extracted on:

Study design

RCT, quasi-experimental, controlled before and after, qualitative, process evaluation. Sampled at individual or area level, any other relevant information.

Intervention design

The specific intervention content, including the model of basic income evaluated, the value of payments, withdrawal rates (if relevant), the level at which the payment was made (i.e. household/individual), method and timing of payments, etc.

Population

The population targeted by the intervention.

Outcomes

Names of all outcomes reported by the included studies, method of data collection or source of data.

Summary of impacts

We extracted summary impact data on specific outcomes including labour supply, marital dissolution, health and health-related outcomes, child and teen educational outcomes and crime. In addition, where reported, we extracted data on income inequality, poverty, and any evidence of spillover effects. Impact data on reported outcomes not included in these categories were not extracted, but the outcomes were recorded and tabulated.

Methodological issues

Key methodological issues which may have influenced the study findings as reported by the study authors or identified by the reviewers.

Implementation/context

Any information about how implementation issues or intervention/context interaction, including the wider policy context, might have affected study findings.
Scaling up

Any information about the potential effects of a basic income if it were implemented at scale, e.g. if it was paid universally and/or without time limits.

Synthesis

As this is not a systematic review, a full synthesis was not undertaken. Instead, we followed the process recommended by Levac et al (2010), which involves “analysing the data, reporting results, and applying meaning to the results”. We tabulated the characteristics of the included studies and of the interventions evaluated separately, as a number of interventions were evaluated by more than one study. We narratively reported the impacts by intervention type and outcome, and provided a qualitative thematic analysis focussing on how far each intervention met the key basic income criteria and what could thus be learned about the potential effects of a full basic income. We also narratively reported key data relating to methods, implementation and context.

Critical appraisal

As this is a scoping review with a primary purpose of mapping the available evidence, no formal critical appraisal was conducted. However, we comment on the key methods issues identified in the included studies.
7. Search results

Negative Income Tax studies

We identified five NIT interventions. All of these were conducted in North America in the 1970s, and were extensively evaluated by the research teams working on the interventions at the time. We defined all publications from the original evaluations as single studies. We found five later studies analysing the NIT study data conducted by researchers not connected with the original studies, which we defined as separate studies. References meeting the criteria detailed above were selected for detailed consideration and potential extraction. No NIT publications were excluded as such, they simply were not selected for extraction.

The database searches identified 151 references, and all other search methods identified 209 references for upload to Excel. After duplicates were removed, there were 335 remaining references. Of these, 35 records were excluded as they were advocacy or discussion papers, 24 were excluded because they were not about studies of basic income, ten were excluded because they were not about the NIT experiments, 11 were excluded because they were modelling studies, and two were excluded because they were PhD theses. Two hundred and fifty-two references were considered in more detail. Eight references reported labour supply impacts, 97 reported impacts on other outcomes, and 75 reported on aspects of methods, implementation, or context. Of these, 49 publications were selected for extraction. Figure 1 details the progress of the references through the screening process.
Figure 1. Progress of NIT publications through screening

151 records identified through database searching

209 records identified by other means of searching
Bibliographies 51
NIT book chapters 129
Hand searching 29

335 records after duplicates removed

82 records excluded
Advocacy and discussion papers = 35
Not basic income = 24
Not negative income tax = 10
Modelling studies = 11
Theses = 2

335 records screened

252 remaining records tabulated and considered for full extraction

10 studies included in synthesis
49 publications
Unconditional cash transfer studies

The database searches located 1368 records. After deduplication, 1256 remained. Following initial screening, 81 publications were retrieved. We identified 18 studies of unconditional cash transfers, reporting the impacts of five separate interventions or programmes. There were 34 associated publications which reported information about these interventions. Nine publications reported aspects of implementation, design, or context. Two publications were study protocols. Thirty-two studies (47 publications) were excluded; thirteen because they did not report on the intervention of interest, ten because they were systematic reviews, five because they were not of relevant populations, two because they were uncontrolled before and after studies, and two because they were pilot studies. A further four publications were excluded, either because they were general discussions of basic income or because they provided background information which was not relevant. Figure 2 details the progress of the references through the screening process.

We cannot be certain that all relevant UCT studies were found by the searches. Several of the UCTs we found are known by a number of different names or referred to in terms other than those with which we were familiar. For instance, one UCT included in the review is known by a range of terms, including the Cash Transfer Assistance programme (Bantuan Langsung Tunai [BLT]), Direct Cash Transfer programme (Subsidi Langsung Tunai [SLT]), and BBM Compensation programme, which we did not identify until late in the searches. Incorporating all of these into a comprehensive search strategy would be appropriate in the context of a full systematic review, but was not feasible within this scoping review. However, we are reasonably confident that we identified the main interventions which met our inclusion criteria through our existing knowledge and extensive hand searching of international development organisations’ websites.

A number of included publications were working papers published by specialist research organisations, and as such had not been subjected to a full peer review process. It is quite possible that some of their findings would be modified if they were later published in an academic journal, and we found two studies for which the findings reported in a recent journal paper differed substantially from those reported in earlier working papers.
Figure 2. Progress of UCT publications through screening

- 862 records identified through bibliographic database searching
- 506 records identified in specialist databases and websites and through other handsearching

1256 records after duplicates removed

1256 records screened

Excluded studies = 32 (47 publications)
Population inappropriate = 5 (10 publications)
Systematic review = 10 (10 publications)
Not reporting intervention = 13 (16 publications)
Uncontrolled before and after = 2 (6 publications)
Pilot project = 1 (2 publications)
Discussion paper = 2 publications
Background, no relevant information = 2 publications

86 records retrieved

18 studies included
31 publications
Included studies

We identified five Negative Income Tax interventions. These were: the New Jersey Graduated Work Incentive Experiment (typically abbreviated as New Jersey), the Rural Income Maintenance Experiment (RIME), the Gary Income Maintenance Experiment (often referred to as ‘Gary’), the Seattle/Denver Income Maintenance Experiment (SIME/DIME), and the Manitoba Basic Annual Income Experiment (Mincome). All were conducted in the United States excepting the Manitoba Basic Annual Income Experiment, which was conducted in Canada.

We identified five other relevant interventions, with 18 associated studies. Two interventions were UCTs intended to smooth the effects of fuel subsidy reform, two were dividend distribution programmes, and one was a basic income distributed in order to study its effect. These were implemented in Iran, Indonesia, the United States and India.

All the NIT interventions were designed and implemented for the purposes of evaluation, as was the study conducted in India. The remaining studies are of existing policy level interventions which were not implemented for evaluation purposes. For all studies, we report study and intervention design, issues around implementation and methods, and contextual information. We also report an overview of impacts on labour supply, family relationships, health and social outcomes, education, and spillover effects in the text. All outcomes reported by the studies are listed in Table 1.
8. The Negative Income Tax experiments

Background to the experiments

The Negative Income Tax Experiments were the first large scale randomised controlled trials (RCTs) of a social intervention. They were extremely ambitious in their scale, and ground breaking in their use of experimental methods. In the US, they were conceived against a backdrop of widespread support for a guaranteed annual income from economists and politicians of all political persuasions, including free market advocates such as Milton Friedman and prominent Democratic Senator Patrick Moynihan. There was a widespread perception that the existing welfare system disincentivised both work and marriage, as benefits were withdrawn very rapidly as earned income increased, and were usually only available to single parent families. In both the US and Canada, there was also a strong anti-poverty impetus underlying the experiments.

Although it was accepted by all at the outset that there would be a work disincentive effect, the size of the disincentive effect was the major concern for policy-makers. It was seen as morally undesirable that people should be encouraged to reduce work effort by a large amount. No consensus was ever reached on how much of a reduction would constitute ‘t
much’ (Greenberg et al 2003). The findings generated a great deal of controversy, and debate around the correct interpretation continues to this day (Widerquist 2005).

**Study characteristics**

The NIT studies were conducted between 1968 and 1980. The US experiments were funded and overseen at the federal level. Administration of the NIT, and the actual research, were conducted by a range of universities and research organisations. As noted, all of the studies were evaluated using RCTs, with samples of households dispersed across large areas. All except one 5 year arm of SIME/DIME ran for 3 years. The choice of US study sites and populations sampled was purposive, in that the aim was not to be generalisable, but to focus on populations in geographic contexts likely to be eligible for an NIT in the event of national implementation (Watts and Bawden 1978).

The Negative Income Tax provided a guaranteed income to families on low incomes. The value of payments was set at a given threshold. If family earnings fell below the threshold, income would be topped up by the NIT. If earnings were above the threshold, the NIT would be withdrawn at a given withdrawal rate. Each study tested a range of different combinations of guarantee level and withdrawal rate (see Table 2) to gauge the strength of work disincentive effects of each ‘plan’. Although payments were withdrawn in line with earnings, there were no conditions requiring participants to work or look for work. Other welfare provision, which varied by state, was sometimes available to both the experimental and control groups. The relative generosity of available welfare provision would be likely to affect participant’s’ responses to the NIT. Work disincentive or labour supply effects were the main outcomes of interest, but data were collected on a wide range of other outcomes.

While the NIT experiments were clearly conducted in a context quite far removed from present day Western Europe, some key contextual features were comparable. Firstly, in New Jersey, Seattle, and Manitoba, job instability was high (Choudry 1993, Christophersen 1983, Burtless 1986). Secondly, it was common for married women to be in work, and in RIME 50%-75% of the samples worked outside the home. Thirdly, levels of lone parenthood were high in several studies; in SIME/DIME and the Gary experiment respectively, 40% and 60% of the samples were single female heads. A brief overview of each study is provided below. Detailed information on intervention and study characteristics can be found in Table 2 and Table 3.

**The New Jersey Graduated Work Experiment (1968-72)**

The New Jersey experiment targeted white, black and Latino families with a male breadwinner. The study sites, in New Jersey and Pennsylvania, were declining industrial urban areas (Burtless 1986) with areas of concentrated poverty (Greenberg 2004). At the outset of the study, existing welfare benefits (Aid to Families with Dependent Children; AFDC) were only available to single parents in the study sites, but shortly after the study
began, two parent families became eligible for AFDC-Unemployed Parent (AFDC-UP), at levels which were more generous than most of the NIT plans. NIT recipients were not eligible for passported benefits such as Medicaid and food stamps. Both control and NIT group respondents who chose to claim AFDC-UP were eligible for more generous welfare and all passported benefits. Hence, intervention group respondents assigned to lower plans were more likely to claim welfare. In 1971, AFDC-UP was cut severely, so that welfare was no longer more generous than the NIT (Garfinkel 1977). There was political hostility to the experiment, generating a number of problems which we discuss in further detail below (Kershaw and Fair 1976). It is possible this influenced participants’ willingness to remain in the experiment.

**The Rural Income Maintenance Experiment (1969-73)**

RIME was conducted in North Carolina, which was poor and had a large black population, and in Iowa, which was relatively affluent and entirely white. It was the smallest of the studies. At the insistence of policy makers, single parents and disabled family heads were added to the sample, but numbers were too low and the analyses report findings for male-headed families only. The sample was further split by site, ethnicity, and farm or non-farm based respondents (Bawden and Harrar 1978). AFDC-UP was not available in either of the sites, so the control group received no benefits and the experimental group did not have an alternative to NIT.

**The Gary Income Maintenance Experiment (1971-74)**

Gary, Indiana was an urban area dominated by a large steelworks, which was described as a “ghetto” by the researchers (Kelly and Singer 1971). The sample was entirely composed of black families, 60% of whom were single parents. Fifteen per cent of the single parent sample were working, and the remainder were eligible for AFDC. However, all the NIT plans were considerably more generous (Kehrer 1977). Two parent families were not eligible for AFDC (Kehrer 1979). Other co-interventions were provided, including access to social services and childcare, but these were terminated at an unspecified point due to under-subscription (Greenberg 2004).

**Seattle/Denver Income Maintenance Experiment (1970-80)**

Both the Seattle and Denver sample sites were within the urban metropolis. SIME/DIME was the largest of the studies, and it provided the most generous plans. It was the only study to test NITs of varying durations, with respondents assigned to three or five year treatment arms, and one very small group in a planned 20-year treatment arm that terminated after nine years (Greenberg 2004, Widerquist 2005). The sample included blacks, whites, and Latinos, single and two parent families, and also couples without children. Single parents comprised 40% of the sample (Thoits and Hannan 1979). The Seattle labour market was
dominated by the aerospace industry, which was subject to frequent fluctuations. The labour market in Denver was more stable. AFDC-UP was not available, but due to strong cooperation with state level authorities, NIT recipients retained eligibility for passported benefits. SIME/DIME had four treatment arms – NIT only, NIT plus vocational counselling, and NIT plus vocational counselling with either 50 or 100% subsidies for training (Christophersen 1983).

The Manitoba Basic Annual Income Experiment (1975-79)

There were three samples in the Mincome study; one dispersed urban sample in Winnipeg, one dispersed rural sample in the surrounding areas, and one saturation sample in the small town of Dauphin, where all residents were eligible to receive payments if their income fell below the threshold. Single people without children were included in the sample (Hum and Simpson 1991). Study authors reported that employment was very insecure for many participants (Choudry et al. 1993). Forget (2013a) reports that up to 30% of the community received benefits at some point during the study, although payments were usually small. Public and political attitudes to the study were initially extremely positive, but latterly political priorities shifted, and funding was withdrawn before most of the data were analysed (Calnitsky 2016).

Although the NIT in Dauphin was not universal in the sense that everyone received payments, all residents of Dauphin were eligible to claim Mincome if their income fell below the threshold. This means that the Dauphin data can provide insights into spillover and community level effects in a way that none of the other NIT studies can. Very recently, data from the saturated sample in Dauphin have been analysed, using the urban scattered sample and the urban controls as comparison groups. There are also qualitative data from Dauphin which represent the only source of data on how participants experienced taking part in any of the North American NIT experiments, and potentially on the underlying mechanisms linking the intervention and some of the outcomes.

Implementation issues

Many of the implementation issues discussed in reports of the NIT studies relate to the administration of a programme which reduced payments in line with earnings, with complicated rules that were sometimes difficult for respondents to understand. These issues presumably would not arise with a fixed, unconditional BI. Nevertheless, there were some aspects of implementation which may be of relevance in the current context.

Policymakers involved with planning the studies were keen to understand the effects on as many different types of people as possible. For this reason, there were multiple sub-groups in many of the studies, but these were often not large enough for meaningful analysis, and in the Gary study some were excluded from the final results (Greenberg 2004). The researchers did not anticipate that there would be large differences in effects between

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demographic groups (e.g. by ethnicity). This necessitated subgroup analysis by ethnic group, but because this was not planned, the samples were often too small to yield robust findings (Watts and Bawden 1978). Similarly, multiple variants of the intervention, each with small samples, reduced confidence in the findings.

Although the federal authorities were involved in implementing the studies, the state level authorities in New Jersey and RIME were not. In New Jersey, lack of cooperation with this level of government meant that the experimental group continued to be eligible for welfare payments, and very large changes were made to the welfare system during the experiment. Interaction with the wider benefits system was problematic, and NIT recipients lost access to passported benefits such as food stamps and Medicaid. Researchers and some participants were accused of defrauding the local welfare agency, leading to negative publicity for the experiment and stress for the participants (Christophersen 1983, Kershaw and Fair 1976).

The researchers were aware from the outset of the need to maintain respondent confidentiality, but some New Jersey respondents voluntarily took part in media coverage early in the experiment. This led to instances of harassment due to jealousy from non-participants, and by journalists attempting to gain further interviews. Ultimately, a strict policy of no publicity for respondents was adopted. In part due to the controversy surrounding the experiments, researchers came under increasing political pressure to release data and results much earlier than planned. The Senate Finance Committee demanded access to study data, including personal details of respondents (Kershaw and Fair 1976).

**Methods issues**

Critiques of the NIT experiments have focused on a number of methodological issues which are seen to reduce confidence in the findings (for example, see Y Combinator Research 2017). However, not all of these are as serious as often supposed. They are discussed in turn below.

**Conlisk-Watts allocation**

The NIT experiments used an unusual and controversial method of randomly allocating participants to groups, which resulted in people with higher incomes being more likely to be allocated to the more generous treatment plans. This introduced a risk that responses would be due to participant characteristics rather than the effects of the NIT. In analyses by Hum and Simpson (Mincome data; 1991) and Christophersen (SIME/DIME data; 1983) some adjustment was made for the varying probabilities of assignment to the different plans, but it is not clear whether this accounts fully for the imbalances built into the randomisation procedure. These analyses made little difference to the estimates. Hum and Simpson (1991)
concluded that, at least for those studies, the allocation method had not introduced any bias.

**Attrition**

There was evidence that some groups of participants were more likely to drop out of some of the studies, which could also have introduced bias to the findings. Participants with incomes above the threshold for receiving payments had little incentive to remain in the study, meaning effects on those who worked the most were not included in estimates of labour supply effects. However, Hum and Simpson (1991), and Robins and West (1986), used accurate administrative data on earnings for those who dropped out of Mincome and SIME respectively, and found that there was little impact on the estimates.

**Underreporting**

There is evidence that underreporting of income (and therefore employment) led to overestimates of labour supply response in the studies. When models were run using administrative data that measured actual earnings, the effects on labour supply were often smaller than indicated by the self-report data. In SIME/DIME, effects on male heads, second earners, and young people were weaker (Greenberg and Halsey 1983), and in the Gary experiment, effects on employment and earnings virtually disappeared (Burtless 1986). In all cases, this would have led any reductions in work effort to appear larger than they really were.

**Scattered sampling**

The NIT experiments have been criticised because the decision to have respondents scattered over wide geographical areas meant that effects on demand for labour could not be measured (Calnitsky and Latner 2017). However, the studies aimed to identify the effects on individual behaviour, and the researchers therefore explicitly wished to avoid causing any changes to labour market conditions (Watts and Bawden 1978). This does mean that the NIT studies can provide little evidence on their effects on labour demand or any other spillover effects (excepting the Mincome saturated site at Dauphin).

**Lack of generalisability**

For various reasons, the samples in the NIT experiments were not representative of the general population, usually even for the geographical areas in which they were conducted. This can limit the ability to generalise the results. In particular, the respondents were all from low income groups who may have less reason to be attached to the labour market, which could lead to overestimates of the likely labour supply effects in the wider population (Widerquist 2005). However, the populations sampled were diverse in other respects, and the consistency of effects seen across these diverse populations and contexts can increase confidence in the generalisability of results (Greenberg et al 2003).
Non-validated outcome measures

Finally, other than labour supply and marital dissolution, for many outcomes, the development of validated measures was in the early stages when the studies were conducted. Methods of measuring certain outcomes vary across studies, and the measures used may not be very accurate.

Overall, a number of the issues with the NIT studies were addressed in further analyses, and several are likely to have led to overestimates of the labour supply response. Any effect of the allocation method on the results is debatable and difficult to determine with certainty. As with any study which randomises at the individual level, the NIT studies (excepting Dauphin) can tell us little about spillover or community level effects.

Labour supply effects

See also Summary of impacts

Labour supply effects for each study are reported by a number of different authors, each of whom used slightly different analysis methods and samples (Hum and Simpson 1993). Hence, a range of effects is reported for each study. Several authors report weighted averages of all of the studies’ findings, but weighting cannot take account of the many differences between the studies, so these are not robust (Robins 1985, Burtless 1986). Table 4 below shows the labour supply effects from several analyses of each study, as reported in Hum and Simpson (1993). They are presented as the difference in average hours worked per year between the experimental and control groups. These show that for male household heads, the reduction in labour supply reported by the different analyses ranged from 1% to 9%. Very few of these results are statistically significant, in part due to the rather small samples in most studies. Youth labour supply effects are reported alongside educational impacts because the two outcomes are intrinsically linked.

New Jersey

The estimated effect of New Jersey on male heads’ labour supply reported by the different analyses ranges from 1-7% fewer hours worked than the control group. AFDC-UP was not available at the start of the experiment, but was introduced several months after the experiment began. For some time it was more generous than the least generous NIT plans, and most of those treatment groups claimed welfare instead of the NIT. AFDC-UP involved work requirements, so it might be expected that the labour supply response would be underestimated. However, AFDC-UP was later reduced drastically. None of these changes had large or significant effects on labour supply responses, suggesting that the effects were not particularly sensitive to existing welfare arrangements (Garfinkel 1977). Cogan (1999) re-analysed the New Jersey labour supply data including only those who actually received payments in the analysis. In this treatment-on-treated (ToT) analysis, he found that there was a larger reduction in labour supply effects in the intention-to-treat (ITT) analysis.
However, excluding respondents who drop out means that the sample is no longer random, and retaining respondents who drop out is considered to be a better test of how an intervention will perform in a real-world setting (Egbewale 2015).

**Gary**

The majority of the Gary sample were female single parents (60%), few of whom were working before the experiment (Kelly and Singer 1971). The main employer was the local steel works, in which opportunities to reduce hours were limited (Greenberg 2004). The different estimates of the effect on male heads from the different analyses suggest they worked from 2% to 7% fewer hours than controls, and single parents worked 10% to 30% fewer hours. AFDC-UP was not available in Indiana, so there was no alternative form of welfare for two parent families in the NIT group. Single parents were eligible for AFDC, but the NIT was more generous. As noted above, there were significant problems with income underreporting in the Gary study, to the extent that much of the apparent work disincentive effect disappeared when administrative data were used to model the actual effects on earnings (Burtless 1986).

**RIME**

Reported effects on all male heads’ labour supply in RIME ranged from 3-9%. The sample included rural residents who were farmers and those who earned wages. There was no effect on the labour supply of wage earners, but farmers reduced hours worked for income and increased hours worked on the farm. AFDC-UP was not available, so there was no effect of existing welfare provision on two parent families (Orr 1978). Underreporting of income was also a serious issue in RIME (Greenberg 2004), leading to overestimates of labour supply effects.

**SIME/DIME**

Labour supply effects in SIME/DIME were stronger, and more likely to be statistically significant, than those found in the other NIT studies. This is generally attributed to the more generous treatment plans provided in SIME, and to the larger sample (Burtless 1986). Effects on husbands ranged from 7-8%. AFDC-UP was not available, and passported benefits like Food Stamps and Medicaid were available to NIT recipients, so existing welfare provision did not influence two-parent families in the NIT group (Greenberg 2004). Single parents continued to be eligible for AFDC. Effects seemed to be stronger for the 5-year treatment group, leading researchers to conclude that long term programmes would have a greater negative impact on labour supply (Christophersen 1983). However, the 20-year treatment group were no less likely to work than the controls (Robins 1984). Underreporting was again an issue in SIME/DIME.
A later study by Price and Song (2016) analysed administrative data from the SIME/DIME sample 40 years after the study began. They found that, among the experimental group, labour supply and earnings were lower. In addition, claims for disability benefits were higher, but successful claims were lower than the control group. There was no effect on mortality, and no such effects on the children of NIT recipients. However, these results are difficult to interpret given the length of time between intervention and follow-up. Stephens (2007) analysed SIME/DIME data accounting for the variable duration of the job training treatments (not just the NIT treatments, as other analyses had done). He argues that the labour supply differences between the 3 and 5-year samples are not as large when this is done.

Mincome

Mincome’s labour supply findings published in the years after the study ended included data only from the urban dispersed sample (Calnitsky and Latner 2017). Hum and Simpson (1993) published one of the few analyses of labour supply effects produced by the study researchers. They reported that reductions in labour supply were smaller than those found in the US studies, with only a 1% reduction for male heads, 3% for second earners, and 7% for single parents. None of these differences were statistically significant.

Calnitsky and Latner (2017) used a difference-in-difference (DiD) approach to compare changes in labour market participation in the Dauphin saturated site, the dispersed control sample and the dispersed treatment sample. They used a dichotomous measure of labour market participation. Because they only included respondents with no missing data, the sample is reduced from 1050 to 292. They report that labour market participation reduced by 11.3 percentage points more in the Dauphin sample than in the control group, and by 3.1 percentage points more than the dispersed treatment group. This is interpreted to mean that 30% of the labour supply effect in the Dauphin sample was caused by “community context effects”, wherein some people joined Mincome because they knew others who were in it, and also the stigma of receiving payments was reduced.

In an analysis of qualitative data, Calnitsky (2016), and Calnitsky and Latner (2017) draw on the responses to open-ended questions in a survey which was conducted around the mid-point of the experiment to gain some qualitative insights into responses to Mincome. Of approximately 2128 individuals in the Dauphin sample, 407 responded to the survey, of whom 322 answered a question regarding their reasons for participating in Mincome.

The data on potential work disincentive effects of the NIT suggested that it was valued not because it meant respondents could give up work, but because it allowed them to respond flexibly to changing circumstances and remain in work. Respondents who discussed reducing labour force participation typically referred to job insecurity, caring obligations, health problems, and education as reasons for doing so. Many responses emphasised the necessity of working if one was capable, which traditional welfare did not allow them to do.
Standard welfare benefits were available to the study respondents, but the qualitative data indicated that many would not claim them for reasons of pride and self-esteem.

**Labour supply overview**

Interpretations of the size and importance of these effects vary; some describe a reduction of 9% as ‘very small’ whilst others argue that it is large and problematic. It is not generally clear what these assessments of magnitude are based on (Widerquist 2005). The consensus from the study researchers and many subsequent commentators was that the labour supply effects were small and would not be of major concern in the event of universal implementation (Greenberg et al 2003). Many of the estimates were not statistically significant, although this was often due to small sample sizes. Several methodological issues are likely to have caused the labour supply effects to appear stronger than they were or would be in a universal programme, including underreporting, attrition, and potentially weak labour market attachment of the low income participants.

Across all the US NIT studies, the effects for second earners/spouses, and for single female heads, were larger, apparently because these groups used the payments to “buy more time in the home” (Hollister 2005). Although the changes were large in percentage terms, the absolute differences were small, because married women and single parents typically worked few hours to begin with. In the Mincome study, Hum and Simpson (1993) noted that the presence of pre-school children in the home had a stronger effect on labour supply than the experimental treatment, leading fathers to increase their labour supply and mothers to decrease it by a similar amount. Sub-group analysis of the Mincome Dauphin data also suggested that single people, including single parents, were most likely to exit employment (Calnitsky and Latner 2017), while the qualitative data suggested that the ability to remain in work without losing benefits was an important feature of Mincome. In the Gary and SIME/DIME experiments, reductions in hours worked for male heads were more often due to longer spells between jobs than reducing hours worked in an existing job (Burtless 1986), apparently because they spent more time looking for suitable jobs (Watts 2005). Reductions in wage labour in RIME were matched by increases in farm labour. Taken together, these findings suggest that few people choose to simply exit the labour market when provided with an unconditional income at a guaranteed level, and that where working hours are reduced, it is in favour of other productive activities.

**Marital dissolution effects**

Marital stability was measured in all four of the US NIT studies and in Mincome. The issues around analysing and interpreting the findings are exceedingly complex, and we focus here on providing a brief overview of the findings.
The results that were most exhaustively analysed, and which received the most political attention, came from the SIME/DIME study (which was also the only study to include cohabiting couples in the analysis). Contrary to expectations, the headline findings for this study were that NIT caused marital dissolution rates to increase by 40-60%. For couples on the plan with the lowest income threshold, dissolution rates were 58% for black families and 51% for whites. However, dissolution rates were actually lower than controls in the plan with the highest income threshold. The final report authors hypothesised that for a woman considering leaving a marriage, NIT was a more attractive option than AFDC (the control condition) because it was less stigmatised and had fewer transaction costs (Groenveld, Hannan and Tuma 1983). But this analysis only included data from the first 3 years of this experiment, and did not analyse the NIT, training, and NIT plus training groups separately, so could not estimate the effect of the NIT alone. A later reanalysis by Cain and Wissoker (1990) analysed the NIT group separately, and included all 5 years of data in the analysis. This concluded that there were no effects on marital dissolution. While the original authors contest these findings (Hannan and Tuma 1990), the consensus appears to be that the Cain and Wissoker analysis is more robust (Choudry and Hum 1995, Forget 2011).

Knudsen and Shore (1977) reported no impact on any measure of household composition in the New Jersey study. Effects on marital dissolution in RIME appeared to be linked to the generosity of the plan received, with much higher dissolution rates among the low guarantee plans (up to 0.19 per 1000 marriages compared to 0.04 for control), and slightly lower rates for the high guarantee plans (0.03 per 1000 marriages). None of these effects was statistically significant (Hannan 1978). An analysis of marital dissolution in the Mincome study found the NIT did not encourage separations, and had a stabilising effect in the middle of the study period, albeit only briefly (Choudry and Hum 1995). Forget (2011) analysed data from the Mincome saturated site and also found no effects on marriages ending. We did not locate any evidence on the marital effects of the Gary experiment.

Overall, the results suggest that the NIT did not have notable effects on marital stability.

**Effects on other outcomes**

See also [Summary of impacts](#)

**Health and health-related outcomes**

Findings from the NIT studies on mental health were variable. Thoits and Hannan (1979) reported a statistically significant increase in psychological distress in certain demographic groups in the SIME/DIME study. Most groups were not affected though, and the differences from control were very small in all cases. For adults and teens in RIME, Hannan (1978) reported a mild positive effect overall, which appeared to be linked to the value of the income guarantee level, with those on less generous plans having lower wellbeing than controls, and those on more generous plans having higher wellbeing. In the New Jersey
study, Middleton and Allen (1978) reported no effects on a range of psychological scales developed by the study researchers. No reported impacts on psychological outcomes were located for Gary.

Two studies provided some evidence on the psychological impacts of Mincome, both utilising data from the saturated sample site at Dauphin. Forget (2011, 2013, 2013a) found that hospital admissions were 8.5% lower in Dauphin than in the matched control group (propensity score matching was used to identify suitable controls from the Manitoba population), and that this difference was largely driven by admissions for accidents and mental health conditions. Calnitsky’s analysis of qualitative data (2016) suggested that a number of participants felt that Mincome allowed them to retain autonomy and self-respect because they could continue working while receiving it. Taken together, the NIT evidence seems to suggest at least a mild positive effect on psychological outcomes.

Impacts on low birthweight were reported by two studies. In the Gary study, Kehrer and Wolin (1979) found a large and statistically significant increase in birthweight of 0.3-1.2lb (136-544g) among the highest risk groups. Targeted birthweight interventions, such as provision of micronutrients, report increases of 20-50g in birthweight (Potdar et al 2014). Further analysis led the authors to suggest that improved maternal nutrition was the mechanism underlying this improvement. Forget (2011) found no impact on birthweight in her analysis of the effects on all Dauphin residents, possibly because the whole community was included in the analysis, as opposed to only at-risk NIT recipients.

Measures of health service use were reported for New Jersey (Elesh and Lefcowitz 1977), SIME/DIME (Christophersen 1983), and Mincome (Forget 2013). Neither of the US NIT studies found any impact on any measure of health service use. A large and significant reduction of 8.5% in hospital admissions was found in the Mincome Dauphin sample. Forget (2013, 2013a) hypothesises that reductions in poverty may have spillover effects on substance abuse and crime, leading to fewer accidents and beneficial effects on the whole community.

RIME directly reports impacts on diet, finding that the Mean Adequacy Ratio of ten vital nutrients was 3.56% higher for experimental respondents in North Carolina (which had high poverty levels), but there was no difference in Iowa (where poverty levels were low). This may have been due to the relatively good baseline diet in Iowa (O'Connor and Madden 1979). Indirectly, improved diet was identified as a potential mechanism underlying increased birthweight in the Gary study (Kehrer and Wolin 1979).

A range of health outcomes relating to chronic conditions and health-related limitations was reported for New Jersey (Elesh and Lefcowitz 1977; adults and children) and SIME/DIME (Christophersen 1983; adults only). Neither study found any effects on any of these outcomes.
Youth education and labour supply

Effects on measures of educational attainment and attendance were reported by all of the NIT studies. In RIME, Maynard (1977) found that younger children in North Carolina had a 30.5% reduction in absenteeism, 18.9% improvement in SAT scores, and a 6.2% increase in the Grade Point Average. There was no effect on older children or children from Iowa. Effects were stronger in both younger and high-risk children. Reading test scores, the Grade Point Average, and days absent were reported for Gary (Maynard and Murnane 1979). For younger children, there was a 22-point improvement in Reading score. Older children had a slightly worse Grade Point Average than control. There were no effects on the achievement of other groups, or on attendance for any group. The strength of effect was directly related to length of time in the programme, and to lower baseline incomes. No effects on attainment or attendance were found in SIME/DIME (Christophersen 1983).

For New Jersey, a 25-30% increase in high school completion was reported for teens on medium generosity plans, who had 6-12 months more formal education than control by the end of the experiment. New Jersey also reported reductions in labour supply for teens in the experimental group, which were more than offset by these “surprisingly large” increases in educational participation (Mallar 1977). McDonald et al (1977) reported that teenage boys with low family incomes in the Gary experimental group were significantly more likely to stay in school and less likely to work. Only girls in high guarantee plans had similar effects. Venti and Wise reported that youths in SIME/DIME were 11% more likely to complete high school (Venti and Wise 1984, in Hanushek 1986), but Christophersen (1983) reported a substantial reduction in labour supply, and no evidence of a concomitant increase in school attendance. In the Dauphin saturation site, the proportion of pupils completing Grades 11-12 increased from 81% in 1971 to 99-100% in 1975-8; the control site saw a reduction from 99% to 90% in the same period (Forget 2011). There was an 18.9% reduction in employment for those under 30 years reported in Calnitsky and Latner’s analysis of the Dauphin data (2017). Interviews with some Mincome participants suggest that both financial considerations and peer effects played a role in decisions to stay on at school (Forget 2013a).

Crime

Effects on measures of crime were reported for SIME/DIME and RIME. Groeneveld et al (1979) reported whether respondents were arrested during the first 3 years of SIME/DIME. They concluded that the sample was too small to detect small but important effects, although arrests declined in the experimental group relative to controls. A self-report delinquency scale for teens in RIME showed higher crime for low guarantee experimental groups, but substantially lower crime for the highest guarantee group (Hannan 1978).
Summary of NIT findings

See also Summary of impacts

The consistency in labour supply response across the NIT experiments, in spite of the differing populations, contexts, and plan designs suggests that generalisability may be higher than often supposed. Effects on labour supply among main earners were small, while effects on second earners, single parents and teens were larger. Most of the reduction for main earners was due to longer spells between jobs, suggesting that job fit may have improved. Farmers in RIME increased time spent on farm work, and teens in New Jersey, Gary, SIME/DIME and Mincome increased time in education. Qualitative data indicated that receiving an income supplement helped people to cope with job insecurity, health problems and caring obligations without having to give up work, and that this was valued highly.

Most of the US studies reported little or no effect on mental health, excepting RIME which found a relationship with plan generosity. By contrast, using reasons for health service use as a proxy for mental health problems, the results from the Dauphin saturated site suggested a strong positive impact across the whole community. However, the measures used in the US studies had low validity and reliability. While Gary found a strong positive impact on low birthweight in high-risk groups, the Dauphin analysis reported no effect. The latter analysis reported the mean impact for the whole community including non-recipients, which may have obscured effects on recipients. New Jersey and SIME/DIME reported no effect on various measures of health service use, while evidence from Dauphin indicated a large decrease in health service use across the whole community. Evidence on diet from Gary and RIME suggested that there were improvements for NIT recipients, again focused on high risk groups. There was no effect on a range of measures of illness and limitation in either New Jersey or SIME/DIME.

There were strong positive effects on completing or remaining in high school in New Jersey, Gary, SIME/DIME and Mincome. As noted, in a number of studies teens also reduced labour force participation. RIME reported a strong positive effect on school attendance for high risk children, but Gary and SIME/DIME found no effects. Both RIME and Gary found a strong effect on academic achievement for younger children with lower incomes, but no effect was reported in SIME/DIME. SIME/DIME reported a small positive impact on teen crime, but the sample was too small to draw any firm conclusions. In RIME there was again a connection with plan generosity, with a substantial drop in crime for those on the most generous plan but an increase for those on less generous plans.

Overall, there seemed to be little effect on marital dissolution, although in RIME there was a relationship between higher plan generosity and lower dissolution rates. The evidence from Dauphin suggests that spillover effects are important and can feed into positive community level effects, so that non-recipients also benefit. The qualitative evidence also suggested that unconditional payments provided both autonomy and security, which may be
underlying mechanisms influencing health. Overall, there were some substantial positive effects on outcomes which are typically resistant to intervention, with a pattern of greater benefits for those at highest risk.

What can the NIT studies tell us about a universal basic income?

The NIT studies meet some of the criteria for a basic income, but in several areas they fall short. Payments were unconditional, and they provided a guaranteed subsistence level income paid at regular intervals, so the studies provide evidence on the work disincentive effects of a programme in which it was possible to choose not to work. However, they were not universal in that they targeted people on low incomes, and they were only available to the scattered samples included in the experimental group (apart from the Dauphin saturation site). They were also of limited duration. This means that we cannot tell what the effects might be if the whole population were eligible, nor how labour supply and demand might be affected by a long-term programme. Since participants were scattered in the main, and remained anonymous, we know little about community interaction effects. Nonetheless, there is some evidence from the NIT studies which can shed light on these issues.

A number of the study authors noted that three years was likely to be a relatively long time frame for many respondents, due to the high degree of employment instability experienced by many (Watts and Bawden 1978, Choudry and Hum 1995). This would suggest that labour supply response might not differ in a permanent programme. As discussed above, SIME/DIME included groups who were assigned to different treatment durations. The initial analyses of these suggested that labour supply effects were stronger in the 5-year sample (Christophersen 1983), which seemed to indicate that a permanent programme might have stronger work disincentive effects. However, a later analysis of results from the planned 20-year sample suggested that after three years labour supply was not lower than control for male heads or spouses in the 20-year group. There was still a large reduction for single parents, suggesting that time in the home was more important to them (Robins 1984).

Although Mincome was not universal, it was available to every resident of Dauphin whose income fell below the threshold. Despite the fact that only 30% of the community received it, and in many cases payments were small, large and important effects were observed at the community level. Health and educational outcomes improved among the whole community, apparently due to a range of spillover effects (Forget 2011, 2013, 2013a). Labour supply effects among those who received Mincome were stronger in Dauphin, apparently because transfer receipt was less stigmatised than traditional welfare (Calnitsky and Latner 2017).

In relation to the remaining criteria, NIT was unconditional and provided a subsistence level income even to those who were not working. This does not appear to have provoked a large reduction in labour supply among main earners. Availability of alternative welfare provision...
to both intervention and control groups varied across the experiments and sample subgroups (see Table 3). For second earners and single parents, the ability to choose to reduce working hours and balance domestic obligations may have been welcome. As regards fixed payments, again the fact that NIT was withdrawn in line with earnings would be expected to have a work disincentive effect, but this does not appear to be the case. The small labour supply response to a programme with built in work disincentives may indicate that employment effects would be smaller in a programme which did not withdraw payments in response to earnings.

9. Unconditional Cash Transfer interventions

See also Summary of impacts

Indonesian Bantuan Langsung Tunai (BLT) Direct Cash Transfer programme (2005-6, 2008)

In 2005, the Indonesian government reformed its fuel subsidy programme, which was expensive, inefficient, and regressive. Since the sudden removal of fuel subsidies was likely to cause hardship for poorer families, a temporary UCT was developed to cushion the income shock. It was targeted at poor and near-poor households, comprising 30% of the population, and provided quarterly payments of 300,000 Rupiah (IDR; around $30) for one year. The value was equal to 15% of the targeted households’ expenditure. No adjustment was made for household size (Bazzi et al 2012). A later readjustment of fuel subsidies, in 2008, prompted another temporary BLT programme which paid 100,000 IDR per month for 9 months (World Bank 2012). Eligible households were identified using a complex process in which village leaders provided details of struggling families to government officials, who then used a 14-item Proxy Means test measure to estimate the household’s expenditure in the previous year. Payments were collected in person from a post office (Beaton et al 2010). The 2005 BLT programme was conceived, developed, and implemented in 5 months. It caused much controversy, as it was widely believed it would have a work disincentive effect, and that recipients would spend the money on tobacco and alcohol (World Bank 2012).

BLT did not meet the BI criteria of permanence, universality, or providing a subsistence level income. However, it was unconditional, and the value of payments was not affected by other income. BLT was delivered in a lower middle-income country with quite distinctive social relations. Cooperative community work on infrastructure projects and to provide social support is the norm. Given the rapid implementation of the programme, targeting was largely effective, but it is estimated that 37% of recipients were not poor. This appears to have caused quite widespread social conflict and protests, which were often targeted at village leaders. In some cases, this was addressed by semi-formal redistribution of the money to those seen as more needy (Hossain et al 2012).
We found three studies which reported impacts of the BLT programme. Bazzi et al (2012) used data from a nationally representative panel to investigate effects on labour supply and poverty. Difference-in-difference (DiD) methods were used to compare outcomes in incorrectly targeted households, households who received payments at different times, and households of different sizes (as payments were not linked to household size, therefore the relative value varied). They report positive impacts on short and medium-term poverty, and no difference in labour supply between recipients and non-recipients. They did find that labour supply was lower in those who had not received a second payment by early 2006, with a 1.5 hour per week reduction compared to adults who had received their first two payments, but they were unclear as to why this should be the case.

A World Bank report (2012) used a triple-difference strategy to compare a range of outcomes in matched households who did and did not receive the BLT. Using data from a biannual government survey, they found that people in BLT-receiving households were more likely to find a job (36% compared to 30%) and no more likely to leave work. There was a statistically significant reduction of 2.3% in child labour and non-significant increases in school participation of 1.2% for 6-18 year olds and 2.5% for 12-18 year olds. Use of inpatient and outpatient health services increased, perhaps because it was more affordable. In areas with a high proportion of BLT households, the expenditure of non-BLT households increased by 10%.

Hossain et al (2012) conducted a large qualitative study (33 case study areas, 1500 total respondents) investigating the social impacts of the BLT. As noted above, they found quite widespread adverse impacts of BLT arising from jealousy and feelings of injustice around allocation of the payments. Some non-recipients of BLT withdrew their labour from community projects, and there was evidence of a lasting erosion of trust in all levels of government. In 14 of 33 case study areas, funds were reallocated to mitigate these effects, which would most likely have attenuated any impacts observed by the quantitative studies. Most of these issues seem to have been temporary, but it is possible a longer programme would have led to lasting divisions. The money was often used to invest in small businesses or to pay school fees, although there were some verified accounts of men spending it on alcohol, leading to violence and family conflict. In the main, although the money was collected by men, it was handed straight to their wives. In some areas the cost of living was so high that the value of payments was negligible.

The evidence we identified on the impacts of the BLT indicated that there were either no effects or positive effects on labour supply. The transfer seemed to allow children and young people to reduce working and increase participation in education. There was some evidence of positive effects on poverty and health service use, and of an economic spillover effect on non-recipients. Transfers were often spent on school fees and small business investment.
Iran Cash Transfer Programme (2010–)

The Iran Cash Transfer Programme was also developed to replace fuel subsidies. The subsidies encouraged the use of polluting fuels, and benefited the wealthy far more than poor people. Starting in 2010, Iran’s cash transfers were universal, unconditional on work or any other requirement, and paid a fixed monthly amount. They were paid to the household but adjusted for household size. The per capita sum was equivalent to $90 monthly, which initially equated to 28% of median income, and was higher than the monthly expenditures of 2.8 million Iranians (Salehi-Isfahani 2014). When implemented, the programme was intended to be permanent. As such, it comes the closest of any of the included interventions to the definition of a ‘true’ BI. Inflation in Iran soared in the years following implementation, due to international sanctions and a steep decline in the value of oil. As a result, within 7 years the value of the transfer was only one third of the original value (Salehi-Isfahani 2017).

We identified one study meeting our inclusion criteria, which reported short-term impacts on hours worked and labour force participation. Salehi-Isfahani and Mostafavi-Dehzooei (2018) exploited variations in implementation timing and in the relative intensity of treatment (defined as the proportion of household income represented by the transfer) to conduct DiD and fixed effects analyses of nationally representative panel survey data. For administrative reasons, there was a 3 month lag between the first 70% and remaining 30% of the population registering for the programme. The DiD analyses found no effects on the labour supply of men in the bottom 40% of the income distribution, and a positive effect on hours worked and employment among poorer women. They cannot account for this increase but note that in a CCT evaluation a similar increase was attributed to greater affordability of childcare. Among self-employed workers, there was a positive effect on hours worked, which the authors suggest is because the transfer allowed them to invest in their businesses.

Overall, effects on labour supply appear to have been negligible. This may suggest that a programme which is permanent, universal, unconditional, and subsistence level would not have a strong effect on labour supply. However, public opinion polling indicated that only 38% of people believed that the transfers would be permanent (Tabatabai 2012), and this may have influenced the labour supply response. Further, the value of payments decreased very rapidly, and may have ceased to provide a subsistence level income not long after implementation.

The revenues raised by reforming fuel subsidies were insufficient to cover the costs of the transfer. It ran at a deficit of 1.1% of GDP and was funded by printing money (Salehi-Isfahani 2014). The transfer is widely believed to have work disincentive effects (Salehi-Isfahani and Mostafavi 2018), and President Rouhani’s 2018-19 budget proposes to target payments at the poorest. This decision is enthusiastically supported by the IMF (IMF 2017), but is
believed to have contributed to widespread protests in Iran in early 2018 (Salehi-Isfahani 2018).

**The Madhya Pradesh Unconditional Cash Transfer Pilot (MPUCT; 2011-2012)**

A basic income intervention was trialled in Madhya Pradesh, India in 2011. The study was conducted in conjunction with the Self Employed Women’s Association (SEWA Bharat), an Indian trade union that represents poor self-employed women and works to promote the empowerment of women. A basic income was paid to all residents of the treatment villages, irrespective of income. For the first 12 months of the study every adult and every child received 200 rupees and 100 rupees per month respectively, equal to 30% of average income for extremely poor people. For the remaining five months, this was increased to 300 and 150 rupees. Payments were made via standard bank accounts in some villages, and through ‘doorstep’ banking in others. Implementation of the BI was very efficient, with 98.3% of respondents receiving payments quickly.

MPUCT met the definition of a basic income in that it was paid to everyone in the sample area, was not conditional on work effort, and was not affected by other income. However, it was not permanent, and the income provided represented only 30% of the average income of a person in severe poverty. Although India is now categorised as a LMIC, levels of poverty were very high. It was rare for villagers to have toilets, and open defecation was the norm. Many respondents were in semi-feudal or bonded labour type relationships with landowners and other employers.

We found two studies reporting impacts of MPUCT. The main study, conducted by the researchers who developed the intervention, is reported in SEWA Bharat (2014) and Davala et al (2015). MPUCT was a cluster randomised trial including a sample of randomly selected villages. One hundred respondents were also sampled for in-depth case studies. To test the effects of a BI both independently, and in conjunction with a ‘collective voice’ organisation (i.e. SEWA), the 20 sampled villages were randomly allocated to four groups: 4 basic income only, 4 basic income plus SEWA, 6 control, and 6 control plus SEWA. There were also two tribal villages, one BI and one control. A total of approximately 15,000 people were included across all the samples. Data were collected primarily through a survey designed for the study and qualitative interviews. Data were collected on a very wide range of outcomes at three time points, although 25% of the first wave data were lost (Beck et al 2015). It is not always clear whether the analysis method used could control for some quite large differences in baseline characteristics between the BI villages and the control villages. We provide a brief overview of impacts on key outcomes for the non-tribal villages. A list of the other outcome categories reported by the study is provided in Table 1.

Food sufficiency appears to have been better in the BI villages, particularly for some at risk groups (scheduled tribes and scheduled castes). There was a larger increase in the
percentage of children who were the normal weight for their age in the BI villages, particularly among girls (an increase from 39.4 to 64.7% in BI villages compared with an increase from 48 to 59.7% for control). Alcohol purchasing increased in the control villages, but there was no change in the BI villages. School enrolment was higher for 14-18 year olds of both genders, but the difference was particularly large for girls; 65% of girls in BI villages were enrolled at school, compared to 36% of those in control villages. There was a 20 percentage point reduction in waged child labour in BI villages, compared to a five percentage point reduction for control. There was no evidence of impact on waged labour for adults, but there was evidence of an increase in non-waged labour and investment in productive assets. Qualitative evidence indicated that items such as sewing machines, livestock and seeds had been purchased to support income-generating activity.

A further study of MPUTC (Beck et al 2015) analysed health data from the non-tribal villages using propensity score matching and logistic regression. They report impacts on: any illness or injury in the household requiring treatment short of hospitalisation in the preceding three months, any illness or injury requiring hospitalisation in the preceding twelve months, and child vaccination coverage. They found that the odds of illness/injury requiring treatment short of hospitalisation in the preceding three months were 46% lower in the intervention villages. There was no impact on the more serious category of illness or injury, nor on rates of child vaccination. The authors suggest that a longer study or higher payments may be required to have an impact on more serious health outcomes. Child vaccination rates were already very high for both groups, so there was little room for improvement.

MPUCT appears not have affected participation in waged labour, but to have increased participation and investment in independent enterprises. The evidence suggests that there were improvements in food sufficiency, children’s nutrition, alcohol consumption, school enrolment and child labour. However, as noted, most of these analyses do not control for baseline differences between groups, so confounding cannot be ruled out.

In regards to universality, the increase in business investment suggests that a BI could have economic spillover effects. Wealthier people expressed doubts about receiving payments before the trial started, but the reports do not discuss the effects of providing payments to all income groups. The short duration of the programme could have influenced labour supply responses – because they knew payments were time limited, respondents could have put more effort into developing their own business as quickly as possible. In a context where wage labour was often highly exploitative, incentives to become self-employed would be very high.

The payment method mattered, with doorstep banking increasing take-up and financial inclusion for women in SEWA villages. Respondents were involved in all stages of designing the study. They were happy with the study duration and felt that receiving payments for a year would be better than not receiving them. They were also content with the principle of
randomisation at a community level. Individual randomisation was rejected for ethical reasons. Researchers conducted focus groups after the study and found that both beneficiaries and non-beneficiaries were very positive about the study.

**Alaska Permanent Dividend Fund (APDF; 1982- )**

The Alaska Permanent Dividend Fund is a resource dividend scheme which distributes a proportion of the revenues from the state’s oil wealth amongst its citizens. It was instituted in 1982 following the discovery of large oil reserves, and is paid to people who have been resident in the state for at least a year. The APDF is extremely popular with Alaskans, who view it as a right based in their common ownership of oil resources (Widerquist and Sheahen 2012).

The APDF is universal, and to all intents and purposes, permanent. It is not conditioned on work or any other requirement, and although it fluctuates, the value has been fairly stable in recent years. Payments are not sufficient to cover subsistence, but since it is paid per capita, the value to a family of four can be over $5000 p.a., equal to the sums provided through the US equivalent of tax credits. It is important to note that the APDF was not designed with the aim of reducing poverty or income inequality. The APDF was implemented in conjunction with the abolition of state level income taxes, meaning that transfers are not recovered from wealthier people via the tax system, and the overall effect of the programme is regressive (Zelleke 2012). We could not establish whether welfare claimants are permitted to receive full benefits, or the APDF payments are deducted, as with Child Benefit in the UK.

Four studies reporting the effects of the APDF on relevant outcomes were included in the review. Two further studies reported outcomes not included in our specified categories, therefore we do not report them here (Hsieh 2003; consumption, Olsen and O’Brien 1990; intertemporal utility maximisation). All of the extracted studies used quasi-experimental methods to analyse routinely collected data. Chung et al (2016) used DiD methods to identify the effects of APDF on birthweight and AGPAR score (a composite measure of newborn health) in the US-wide Natality Detail File between 1978 and 1984, using 44 states as a control group. They found that the incidence of low birthweight was 0.7 percentage points lower compared to the US states used as a control group. Birthweight increased by 17.7g for every $1000 increase in income. Earlier pre-natal care and longer gestation appeared to explain the increases in birthweight. The effects were strongest among those with the least education.

Evans and Moore (2011) analysed mortality data for 2000-2006 from the Multiple Causes of Death (MCOD) database (a census of all deaths in the United States) using DiD methods. All other US states were used as a control group. They found that in the week the annual payment was received, there was a large, statistically significant increase in mortality of 13% among urban Alaskans compared to the control states. They attribute 8% of these additional
deaths to substance abuse (2.6 out of 33 additional weekly deaths). They suggest that other causes of death are most likely related to increased activity and consumption. There was a concomitant decrease in mortality in the 4 weeks following the payment, suggesting that some of the initial increase is caused by a displacement effect linked to the increase in activity at the time of payment. The mortality decrease did not fully offset the initial increase. However, the authors also report increases in mortality in response to US Social Security payments, tax rebates and military salary payment schedules.

The impact of APDF on income inequality was investigated by Kozminski and Baek (2017) using Interrupted Time Series modelling to analyse a 49-year data series (1963-2012). They evaluated the effects on the Gini Coefficient, the Relative Mean Deviation (RMD) and the Thiel’s Entropy Index, finding that the APDF increased all three types of income inequality in the study period. They posit this may be due to the ability of wealthier individuals to use the payments for investments which can increase income over the medium term, while those on lower incomes have to use the dividend for more pressing needs. As noted above, payments to wealthy people are not recovered through the tax system, and the APDF is known to be regressive for this reason.

Jones and Marinescu (2018) investigated the effect of the APDF on labour supply using synthetic control methods to construct an appropriate control group. Labour force participation and the rate of part-time employment were analysed using data from the Current Population Survey between 1982 and 2014. They found that there was no decrease in labour force participation due to the dividend payments, but part-time employment was 17% higher (a difference of 1.8 percentage points). The effect on part-time employment was stronger for women. Further analysis suggested that the lack of impact on labour force participation may be due to an increase in consumption resulting from the payments, since increased consumption leads to increased demand for labour.

The results suggest that a universal, unconditional, permanent cash transfer of substantial value has little effect on labour supply and a positive effect on new-born health. There is evidence of a short-run negative effect on mortality, which is partially offset by a reduction in subsequent weeks and also occurs in response to other regular cash payments. There is a negative effect on income inequality, because the wealthy are not taxed at a higher rate. There is also some evidence of economic spillover effects leading to increased consumption and offsetting potential work disincentive effects.

**Tribal casino dividends**

In 1988, the US Indian Gaming Regulatory Act (IGRA) created a legal framework for Native American nations to run gambling businesses in various forms. In a context of very high poverty and unemployment among Native Americans (Akee et al 2015), the IGRA had an explicit purpose of “promoting tribal economic development, self-sufficiency, and strong tribal governments”. Over 200 Native American nations now have established casinos.
(Smith and Taggart 2010), and approximately 120 distribute a proportion of the dividends to tribal members on a per capita basis (Akee et al 2015). Individual payments have generated controversy among many tribal leaders who fear that they will encourage substance abuse and dependency, and erode tribal cultures.

Per capita casino dividends are similar to basic income in that they are intended to be permanent, they are unconditional on employment or any other behaviour, and their value is not affected by any other income. The value varies between tribes, but in some cases they are paid at subsistence level or well above. Since only tribe members are eligible for payments, dividends are not truly universal, but Native American tribes function as discrete political units in which all citizens are entitled to dividend payments. However, the density of Native Americans living in a given area varies, so the extent to which community level effects are possible is variable. Dividend payments diverge from basic income in that young adults typically receive a large lump sum around 18 years. In addition, casinos are often accompanied by substantial investments in infrastructure and services, as well as increased employment opportunities and college scholarships. We found five studies of per capita payments from tribal casino dividends, using a range of methods.

**The Great Smokey Mountains Study**

The Great Smokey Mountains Study (GSMS) used a range of quasi-experimental methods to analyse the impact of per capita payments on children of the Eastern Cherokee nation. The study began in 1993 and ended in 2003, collecting ten waves of data from a representative sample of Indian and non-Indian children aged 9, 11 and 13 years at baseline. A casino opened on the Eastern Cherokee reservation in 1996, providing an opportunity for a natural experiment of the impact of cash transfers on youth mental health in the Native American population. The taxable transfers are worth on average $4,000 annually, comparable to established government cash assistance programs such as Temporary Assistance for Needy Families (TANF) or the Supplemental Nutrition Assistance Program (SNAP). They represent a 20-40% increase in average household income. They are paid twice yearly to individuals, and children’s payments are placed in a trust fund which they can access when they reach 18 years. The transfers do not affect eligibility for in-work tax credits.

Although the Eastern Cherokee nation had higher than average rates of poverty prior to the casino opening, they were lower than those found in other Native American tribes, and the reservation was relatively well integrated into the local economy. After the casino opened, poverty fell more rapidly in Indian than non-Indian families, and the availability of jobs increased. GSMS collected data on child anthropometric measures (Akee et al 2013), child personality traits (Akee et al 2018), child and young adult personality traits and behaviour disorders (Costello et al 2003), psychiatric and substance use disorders (Costello et al 2010), educational attainment, and offending behaviour (Akee et al 2010). They also collected data on parenting quality, parental employment, mental health and marital status (Akee et al
The comparison groups were non-Indian children and older cohorts of Indian children.

Several of the GSMS analyses focused on child and adolescent mental health outcomes. Costello et al (2003) used generalised estimating equations to compare changes in the Child and Adolescent Psychiatric Assessment among Indian children who were never or always poor with those who moved out of poverty over the 4-year study period. They report mean symptom scores of behavioural, emotional, and all symptoms, finding that compared to children who were persistently poor, children who moved out of poverty had a large decrease in psychiatric symptoms which brought them in line with never poor children. This effect was seen only in behavioural symptoms. No effect was found on emotional symptoms (anxiety or depression). A similar pattern of effects was observed in non-Indian children who moved out of poverty. Mediation analyses indicated that increased parental supervision explained the improvement in behavioural symptoms, with a suggestion that a reduction in lone parent families, increase in households with two working parents, and decrease in demands on parents’ time accompanied this shift. However, these analyses are limited by small sample sizes.

Costello et al (2010) compared prevalence of any DSM-IV psychiatric, behavioural, emotional, or substance use disorders in Indian young adults (19 and 21 years) from the youngest study cohort with Indian young adults from the older cohorts and with non-Indian young adults. Using generalised estimating equations, they found that the youngest Indian cohort was significantly less likely to have any adult psychiatric disorder (31.4% compared to over 41% for both of the older cohorts), and that all Indian cohorts were significantly less likely to have any psychiatric disorder than non-Indian young adults (30.2% compared to 36%). This effect was driven by reductions in alcohol and cannabis abuse, but no effect was found for abuse of nicotine or other drugs, or emotional and behavioural disorders. Mediation analysis suggested that association with delinquent friends was the most important factor underlying these differences. It is not possible to tell whether differences between Native American cohorts were due to a cumulative effect, or to the age at which children were exposed, but it is known from the previous study that these respondents received greater parental supervision as children.

Akee et al (2010) used differences in the length of exposure (6 years vs 2 years) to per capita payments to conduct a DiD analysis of differences in years of education, school attendance, probability of being arrested and probability of dealing drugs. To test for potential mechanisms explaining any changes, they also looked at changes in parental labour force participation, probability of arrest, supervision of children, and quality of parent-child relationships. They found that children with longer dividend exposure were 22% less likely to have been arrested at ages 16-17 (with no difference at 18-21 years) and 7% less likely to have dealt drugs by age 21. For children who were in poverty at baseline, $4000 p.a. in extra income was associated with completing an extra year of education, and school attendance increased by 4 days per quarter. There was no effect on maternal or paternal labour force
participation, but there was a reduction in parental arrests. Parental supervision improved by 3-5% and child reports of parent-child interactions indicated that maternal relationships improved by 4%. The authors describe both of these effects as large, and hypothesise that improved parenting quality due to reduced financial stress is the mechanism underlying improvements in child outcomes. All of these effects appeared to increase in magnitude over time.

A further DiD analysis of GSMS data investigated the impact of dividends on young adult BMI, height, weight and obesity, comparing changes in younger and older cohorts of Indian children (Akee et al 2013). The authors report a decrease in the probability of obesity of 2-4% at age 21 for every $5000 increase in annual household income. However, they found heterogeneous effects depending on initial household socio-economic status (SES), with BMI increasing among children from lower income households. They explored a number of potential mechanisms to explain this difference, concluding that initial SES was the key explanatory factor. They note that this suggests that not all negative effects of poverty in early years can be addressed by cash transfers beginning in early adolescence.

Akee et al (2018) used a triple difference approach to investigate the impact of dividend payments on a range of child and parental outcomes, comparing both the different age cohorts of Indian children, and Indian children with non-Indian children. They found large differences in child and parent-reported trait conscientiousness (+21% of a standard deviation), trait agreeableness (+27% SD) and trait neuroticism (+26% SD), and in emotional (-37% SD) and behavioural (-23% SD) distress. In all cases these results represent large improvements, excepting neuroticism, for which an increase is harder to interpret. They also reported improvements in parental supervision, parental and parent-child relationships, and parental mental health, which they identified as the mechanisms underlying the improvements in child outcomes. There was no effect on several measures of parental employment or time use, nor on marital status. They found that effects were strongest in those children with the greatest deficits at baseline, but no differences by initial poverty level as in previous GSMS analyses.

The GSMS study pre-dates infrastructure investment on the reservation, and the older Indian cohort were equally affected by other changes resulting from the casino opening, therefore the findings are not confounded by these factors. However, the study is limited by small sample sizes; the full sample of Indian children is 350, and most of the analyses depend on this being divided into smaller subgroups. There appear to be some contradictions in findings across the analyses, possibly due to the differing methods used in each paper.

**Bruckner 2011**

A further study investigated the impact of casino dividends on the Eastern Cherokee nation. Bruckner et al (2011) utilised aggregate data for accidental deaths from three counties of
North Carolina, with a total population of 57,000 including non-Indians. They used Poisson regression to analyse observed versus expected monthly accidental deaths over 204 months between 1990 and 2006, finding that the risk of death after the biannual dividend payments was over twice as high as in other months (relative risk 2.62, CI 1.54-4.47). Motor vehicle-related injuries accounted for over half of the deaths. To identify mechanisms linking dividend payments to increased risk of death, they analysed data from an earlier ethnographic study conducted by one of the study authors. From 70 interviews and 36 focus groups with people aged 15-25 years, they found that the most frequent references to spending per capita cheques involved purchasing vehicles. Buying alcohol and drugs was also discussed. It is not clear what the original purpose of this study was, and the analysis appears to involve simple frequency counting, which is not typically used in qualitative data analysis.

A question on per capita payments from a further small-scale survey of Cherokee youth (n=125, aged 15-25) also found that the majority referred to purchasing vehicles, with a significant minority discussing alcohol and drugs. The authors speculate that receipt of the ‘big cheque’ at 18 years may be linked to more driving, in combination with a heightened possibility of intoxication. It is notable that a considerable majority of the deaths in fact occurred among the 25-55 age group (66.7% compared to 28%); it is not clear whether age was adjusted for in the regression analysis. The authors note that the low incidence of accidental deaths during the study period limits the ability to base firm conclusions on their findings, and argue their findings do not negate the GSMS findings on the health promoting effects of dividends, particularly for the younger children in GSMS who were exposed to dividends earlier and for longer than this sample. Further, as Evans and Moore (2011) reported, this pattern is observed following large payments of any nature.

Kodish et al (2016) conducted qualitative interviews with tribal leaders (n=12) and tribal members (n=24) to investigate their perceptions of the mechanisms linking tribal casinos with health. The interviews were conducted in California, with respondents drawn from 23 of 109 Californian tribes. Californian gaming tribes distribute revenues to tribes with no or limited gaming to reduce inter-tribal income inequality. The tribes sampled varied in size, extent of gaming, and size of per capita payments. Three pathways linking casinos with health were identified; improving the tribal economy, altering the built environment, and disrupting the social landscape. Many of the themes within these related to the impact of casinos per se, as opposed to per capita payments, so we focus only on the perceived impacts of per capita payments. Key amongst these, many tribal members reported that per capita payments led to higher disposable income, which in turn improved ability to pay rent and bills, eat healthier food, and participate in physical activity. More negative perceptions of payments usually came from people who were resentful because their payments had been reduced, or they no longer received payments. Tribal leaders were less positive,
reporting that in some cases dividends exacerbated existing issues with gambling and substance abuse. In addition, some respondents reported that payments were spent on more expensive unhealthy foods. There were also concerns about a culture of entitlement and dependency being fostered among the young. It is not possible to tell in this study how much respondents were receiving in dividends, nor how long they or other tribal members had been exposed to them.

**Foley 2005**

The Meskwaki nation of Iowa opened a small casino in the mid-1980s, which has expanded over time and as at 2005 generated $65 million in profits annually. This was disbursed to adult tribal members in monthly payments of $1,800. In addition, young people who had completed high school received a lump sum of up to $200,000 on reaching 18 years. The Meskwaki per capita payments cannot be seen in isolation, as they were accompanied by the large lump sum payment, substantial investment in services, free housing, college scholarships, and infrastructure, as well as increased availability of jobs. Nonetheless, a qualitative study conducted by Foley (2005) does provide some insight into the perceived impacts of per capita payments specifically.

Foley (2005) sampled 60 tribal members active in tribal affairs, and 20 white business people, teachers and casino employees. He also interviewed key actors, such as the county sheriff and attorney, and utilised data collected by the tribe. The casino did not appear to have had any impact on adult labour force participation, but some parents reported that their teenage children were living off their dividends. Many people reported direct observation of increased drug and alcohol abuse. On the other hand, local law enforcement reported substantial drops in drunk driving, robbery, and other petty crimes, and education staff reported increased enrolment in post-school education. The particularly large lump sum payment would presumably play a role in how young people respond to the intervention. There was an increase in non-Indian women marrying into the tribe, and adults seeking to be ‘adopted’, which some believed was due to a dividend pull factor. There was thus conflict over how to define tribal membership, with some demands to institute a system based on blood purity. The study provides little information on methods, and the use of key informants appears to be somewhat ad hoc.

**Conner and Taggart 2013**

Conner and Taggart (2013) used a method called “robust regression” to analyse aggregate US census data on a total of 330 Indian nations with no gaming operations (n=190), lower stake gaming (n=29), full gaming without per capita payments (n=120), and full gaming with per capita payments (n=41). They compared a range of economic outcomes for each group in 1990 and 2000, but only three outcomes were available solely for Native American residents of tribal areas. Controlling for a range of tribe level contextual characteristics, they found that only tribes with individual dividend payments experienced large improvements.
in per capita income (near $4,000 increase) or the unemployment rate (reduced by 3%). However, the proportion of the population in the labour force fell by over 7%, for reasons that could not be discerned. Further subgroup analysis suggested that the effects were even stronger for nations sited in large, wealthy states with no other commercial gambling. The weaker effects in nations with full gaming but no per capita payments may suggest that the improvements reported in other studies are not necessarily confounded by infrastructure investment and employment. However, the sample of Indian nations is small, and the subgroup analyses in particular must be interpreted with caution. In addition, information on the specific interventions, including the size of payments, is not available in these data. There were also strong positive effects on the variables including both Indian and non-Indian residents of tribal lands with per capita payments, possibly suggesting some spillover effects of individual payments not linked to higher employment or infrastructure investment.

**Tribal casino summary**

The casino studies provide a body of evidence on the effects of permanent, unconditional payments which are available to all members of a political unit. However, in a number of cases payments are well above subsistence, and teenagers also receive large lump sums on reaching the age of majority. There is also the possibility of confounding by multiple other casino-related changes, but the GSMS was not affected by this, and analysis of census data seems to indicate that improved outcomes were linked to individual payments rather than any other changes. When assessing what the findings mean, it is important to consider the timing of the studies in relation to the interventions, as the age at which an individual becomes exposed to the intervention appears to moderate the effects. Methodologically, all of the quantitative studies suffer from small samples, and methods information for some of the qualitative evidence is very limited.

The quantitative evidence from GSMS did not suggest that there was any reduction in labour supply for mothers or fathers, and analysis of census data for all Indian nations indicated that unemployment decreased in per capita gaming nations. The GSMS data also showed a strong effect on remaining in education for poorer children. However, the census data also suggested a reduction in the proportion of people in the labour force, and there were a number of qualitative reports of young people who were not engaged in employment or education and were using drugs and alcohol more. Some of these apparent contradictions in the data may be to do with the timing and nature of the intervention. Positive effects in GSMS were observed in those who were exposed to the intervention from the age of 12 years, compared to older cohorts of young people. Some of the contradictory data come from older samples, or from tribes where per capita payments and trust funds are very large.

There was some evidence from the GSMS of strong positive effects on a range of child and teen mental health outcomes. Further analyses suggested that improved parental mental
health, increased parental supervision, and fewer delinquent peers in adolescence were the mechanisms whereby increased income impacts on child outcomes. More evidence on mechanisms came from a number of respondents in Kodish et al (2016), who reported that per capita payments reduced stress associated with financial hardship and facilitated physical activity and healthier eating. Again however, there were some reports from qualitative respondents of an increase in unhealthy eating and substance abuse.

Among the Eastern Cherokee, accidental death increased immediately after payments were received, although this effect may have been connected to lump sum payments.

There was no indication of any effect on marital status in one GSMS analysis, although another suggested that there was a decrease in lone parent families. Reductions in criminal arrests for both parents and teens were also found in the GSMS, and a general reduction in crime was reported among the Meskwaki by local law enforcement officials. There was some suggestion of community level or spillover effects in the census data, which indicated that a number of outcomes improved for non-Indians living on tribal lands.

Summary of UCT findings

See also Summary of impacts

The UCT studies suggest that there was little overall effect of unconditional payments on labour supply. In Iran self-employed people increased hours worked, probably in their own small businesses. Hours worked also increased among women. In MPUCT, there was no reduction in waged labour, but there was an increase in personal business activity. There was no evidence of an effect on labour supply overall in BLT, or in the casino studies for either mothers or fathers, although there were some qualitative reports of disengagement from the labour market in other tribal casino studies. The APDF had no effect on labour force participation overall, but there was an increase in part-time working, particularly among women.

Studies which reported other economic outcomes found a reduction in poverty (BLT), evidence of increased consumption and thus demand for labour (APDF), and a potential economic spillover effect on overall expenditure (BLT). Two studies reported increases in small business investment (BLT, MPUCT). There was evidence that income inequality increased in Alaska due to the known regressive nature of the intervention. One study which reported economic impacts on non-Native Americans resident in tribal lands suggested that they experienced improvements potentially attributable to spillover effects from per capita payments.

Low birthweight was substantially less common in Alaska, and new-born AGPAR scores were higher, with benefits concentrated among the least educated. The GSMS found evidence of large positive effects on a range of young adult and child psychiatric disorders. However, child BMI increased in the GSMS survey sample, and there was some qualitative evidence of
an increase in unhealthy eating linked to casino dividends. There was also evidence of increased mortality after payments in both APDF and a tribal casino study, although this appears to be linked to the timing and value of payments.

There was a substantial reduction in minor illnesses in MPUCT, but no effect on more serious illnesses. Food sufficiency increased and the proportion of children who were the normal weight for their age was higher in villages receiving the basic income payments. There was also evidence of lower alcohol consumption. Potential mechanisms for health impacts identified by a qualitative study of casino dividends included a reduction in stress due to decreased financial strain, and increased ability to purchase healthy food and participate in sports.

In BLT, the GSMS, and MPUCT, there was evidence of higher participation in education, of up to a year’s extra schooling in the GSMS. Child labour was lower in both BLT and MPUCT. In the GSMS, rates of arrests and drug dealing were lower for teenagers. This and other positive effects on children in the GSMS appeared to be mediated by improvements in circumstances for their parents, including a lower rate of arrests and mental health problems. There were some qualitative reports of increased substance abuse and social problems in BLT and in some tribal casino studies. In BLT, conflict and resentment arose due to the nature of the targeting process, and reports of increased alcohol use were rare. In the qualitative tribal casino studies, there was also some evidence of problematic substance use funded by dividend payments, although this may have been influenced by differences in length of exposure to, or the value of, cash transfers.

What can the UCT studies tell us about a universal basic income?

The extent to which the UCT studies meet the criteria for a full basic income varies. The Iran cash transfer is the closest example we found, although it seems that most people were aware that permanence was unlikely. Nonetheless, it was universal, unconditional, initially subsistence level, not related to other income, and at the time of writing it had been running for eight years. Unfortunately, the only evidence available is for short-term impacts on labour supply. The tribal casino dividends provide payments which are permanent, unconditional, fixed, and in many cases at subsistence level or above. They are available to everyone who is defined as a member of a gaming nation, meaning that the geographic density of recipients varies. The APDF is permanent, unconditional, and universal within the state. Payments fluctuate, although they have been relatively predictable in recent years. They are not at subsistence level, but they are substantial at a household level. MPUCT was universal, unconditional and fixed, but payments were received for only 17 months and did not cover subsistence. There was an income threshold for BLT, and the below subsistence level payments were temporary, but it was unconditional and payments were fixed.

In all cases, effects on labour supply were minimal. In some studies small business owners increased their hours worked. There was no effect on labour force participation in Alaska,
although there was an increase in part-time working, mostly among women. There was also evidence that the APDF increased consumption, which in turn increased demand for labour. BLT recipients were more likely to find work than non-recipients, and women in Iran increased their average hours worked. There was no change in maternal or paternal employment in the GSMS, and census data indicated that unemployment fell in gaming nations with individual payments, although there was some qualitative evidence of possible dependency on payments. There was evidence of increased time spent on non-waged labour in MPUCT, but no effect on waged labour. Thus it appears that labour supply responses to both temporary and permanent transfers are quite limited, contrary to common assumptions.

Duration of the intervention appeared to lead to a cumulative effect on adult and child outcomes in the GSMS, with improvements in child psychiatric outcomes increasing in relation to the length of exposure. This seemed to be mediated by improved parental relationships and reduced parental problems, evidenced by reductions in parental arrests, which also intensified over time. None of the other studies provide evidence on longer term effects. In terms of spillover or community level effects, consumption increased in Alaska, possibly leading to an increase in labour market demand. There was evidence of an economic spillover effect of the Indonesian BLT, as non-recipients’ expenditure increased by 10%. Finally, census data indicated that there were improvements in a number of outcomes for non-Indians resident in tribal lands with individual payments.
10. Design and implementation

Intervention design

The studies we found were conducted in widely divergent settings, ranging from advanced economies in North America to low and middle income countries in South, West, and South East Asia. They span a period ranging from 1968 to 2015, and include very diverse populations. All of the interventions shared the fundamental characteristic that they provided income to individuals or households that was not conditional on employment or any other behaviour. Several different types of intervention were evaluated. There were five Negative Income Tax interventions, two dividend distribution programmes, two unconditional cash transfers in response to fuel subsidy reforms, and one universal basic income. The NIT and basic income interventions were implemented for the purposes of evaluation, while the remainder were existing interventions which provided opportunities for evaluation.

Three interventions (APDF, Iran cash transfer, MPUCT) were universal, and a further three (Mincome saturated site, BLT, tribal casinos) were available to everyone who met income or ethnicity criteria. The NIT studies involved scattered samples of people on low incomes. Three interventions were permanent (APDF, tribal casinos, and putatively Iran) and had been operational for between 1 and 30 years at the time of data collection. The others varied in duration, from 9 months to 5 years.

The value of payments varied; all of the NIT studies ensured a subsistence level income. BLT, MPUCT, APDF, and the Eastern Cherokee dividend were below subsistence level, but the latter two were considerable at a household level. Other casino studies involved varying values, which could be substantially above subsistence. The Iran cash transfer was initially at subsistence level, but the value was eroded very quickly. Payments were reduced in line with other income in all of the NIT studies, but not in any other intervention. Payments were made on a per capita basis but paid to the (usually male) head in the NIT studies and Iran. In the BLT, a single value of payment was made to the male household head, irrespective of household size. MPUCT, APDF, and tribal casino dividends were paid to individuals. Full details of the interventions are provided in Table 5.

Study design

All of the NIT studies reported by the original researchers were evaluated using RCTs. Later, Stephens (2007) and Price and Song (2016) used the RCT design to analyse study and administrative data respectively. The Indian MPUCT study used a cluster RCT to evaluate impacts, selecting a sample of 20 villages and randomly allocating them to the intervention or control group (SEWA Bharat 2014).

Later researchers used original NIT study data and routine data to evaluate impacts using quasi-experimental designs. Forget (2011, 2013, 2013a) used administrative data to analyse the effects of Mincome on health service use and education using an Interrupted Time
Series (ITS) model. Calnitsky and Latner (2017) used a difference-in-difference (DiD) approach to analyse labour market participation data from the original study. Four studies of the APDF used quasi-experimental methods to analyse routinely collected data. Chung et al (2016) and Evans and Moore (2011) used DiD analysis of routine data to estimate the impacts on birthweight and mortality respectively. An ITS approach was used by Kozminski and Baek (2017) to investigate the effects of APDF on income inequality, and Jones and Marinescu (2018) used a synthetic control method to estimate the impacts on labour supply. The effects of the APDF on consumption were analysed by Hsieh (2003; not reported here) using routine time series data and variations in the value of payments to different household sizes to identify effects.

The GSMS utilised DiD, triple difference, and controlled before and after approaches, using non-Native American children and older Native American children as control groups (Akee et al 2010, 2013, 2015). Data were collected using a study survey. Bruckner et al (2010) analysed a time series of monthly mortality data for the Eastern Band of Cherokee Indians, and Conner and Taggart (2013) used a controlled before and after study design to analyse Census data on 330 Native American nations with and without per capita payments before and after casinos opened.

Triple-difference (World Bank 2012) and DiD (Bazzi et al 2012) strategies were used to evaluate the impacts of the Indonesian BLT using routinely collected data. Bazzi et al (2015; not reported here) also used quasi-experimental methods to analyse the effects of BLT on consumption. A DiD approach using existing survey data was used to analyse the effects of the Iran cash transfer programme (Salehi-Isfahani 2017).

Calnitsky (2016) analysed qualitative data collected in open-ended survey questions during the Mincome study. Hossain et al (2012) conducted a large qualitative study to investigate the social impacts of the BLT programme. Kodish et al (2016) collected qualitative data from tribal members and leaders to investigate the mechanisms whereby casinos influenced health. Bruckner et al (2011) used data from an earlier ethnographic study of Cherokee young people. There was also a large qualitative component in MPUCT (SEWA Bharat 2014). Full details of the study designs can be found in Table 3 and Table 6.

Outcomes

Labour market participation and labour supply outcomes were reported by all of the NIT studies. Various measures of labour market participation were also reported for APDF, the Indonesian BLT, the Iran Cash Transfer, MPUCT, and for tribal casino studies by Conner and Taggart (2013) and Akee et al (2010, 2018). A number of NIT publications reported labour supply impacts for particular subgroups, or specific aspects of labour market participation such as decision making around employment transitions, which we did not extract impact data for. These are not detailed here, but they are listed in Table 1.

We extracted impact data on other economic outcomes including income inequality (APFD), income (SIME), community expenditure (BLT), poverty (BLT), investment in own business (MPUCT), and ownership of productive assets (MPUCT). Economic outcomes we did not

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report in detail included measures of expenditure (BLT), consumption (APDF, BLT), home
ownership, assets and debts (Gary, RIME, SIME/DIME, MPUCT), business decisions and
performance (Gary), and savings activity (BLT).

We reported measures of marital dissolution for New Jersey, SIME/DIME, RIME and
Mincome. Other related outcomes we did not extract from the NIT studies included family
formation (New Jersey, SIME/DIME), fertility (New Jersey, SIME/DIME), and family
relationships (SIME/DIME). We also reported a measure of marital status and several
parenting measures from the GSMS.

We reported all health outcomes identified in the included studies. These included: low
birthweight (Gary, Mincome, APDF), AGPAR score (APDF), various measures of mental
health (New Jersey, SIME/DIME, RIME, Mincome, GSMS), mortality (APDF), and accidental
death (GSMS). Measures of dietary sufficiency were extracted from (RIME, Gary), as were
measures of child BMI, weight and obesity (GSMS), and child weight-for-age measures
(MPUCT). A qualitative tribal casino study reported mechanisms linking the intervention to
health. Measures of health service use were reported by New Jersey, SIME/DIME, Mincome
MPUCT, and BLT. Finally, New Jersey, SIME/DIME, and MPUCT reported by a range of
measures of illness and limitation

Measures of child and teen educational attainment including attendance, enrolment,
completion, and academic achievement were reported by Gary, Mincome, New Jersey,
SIME/DIME, RIME, BLT, and the GSMS. All of these were reported in the review. Mincome
and SIME/DIME reported educational outcomes for adults, but these are not reported here.
New Jersey, Gary and SIME/DIME reported youth participation in the labour market. Child
labour was reported by BLT and MPUCT. Teen crime was reported by SIME/DIME, RIME and
the GSMS, which also reported parental crime.

A number of studies used qualitative approaches to gain deeper understandings of the
impact of interventions on communities. A large qualitative study of BLT investigated its
broader impacts on community relations. Attitudes to Mincome and the role of stigma in
shaping them were explored using qualitative data. Social impacts and mechanisms linking
increased income to health were researched by two qualitative studies of tribal casinos.
MPUCT gathered a large amount of qualitative data exploring experiences of expenditure,
diet, health, education, indebtedness, employment and farming.

A range of other outcomes which were reported by the included studies are not reported in
detail here. They are listed in Table 1. Some NIT publications reported impacts on labour
market demand (not included due to using simulation approaches), wages, time preference,
migration, housing demand, house purchasing, use of social services, and several other
social outcomes. In addition to those we report here, MPUCT also reported many outcomes
in areas including living conditions, other health-related outcomes, economic activity,
indebtedness, women’s empowerment, and gendered effects on education, health, and
labour force participation.
Methods issues

Despite mainly using “gold standard” RCT designs, most of the studies of interventions implemented for the purposes of evaluation were limited by small samples and over-complicated interventions. Due to the inclusion of multiple study arms and subgroups, the analyses were often complicated and underpowered. Of the quasi-experimental studies, some of the tribal casino studies had similar issues. The NIT studies had other methodological limitations, including the use of the Conlisk-Watts allocation method.

Many of the studies demonstrated innovative approaches to using quasi-experimental methods and existing administrative or study data. In combination with large samples, these rigorous study designs provide credible evidence of effect. Some of the qualitative studies were well conducted, with clearly described methods and sampling strategies. These provided very useful evidence on mechanisms linking interventions to outcomes, community reactions to the interventions, and the impact of the transfers on community relations. Others were not so well reported, so it was not possible to be sure what methods were used, how respondents were recruited, or how data were analysed.

The NIT experiments used scattered samples and targeted payments at people on low incomes for reasons that were consistent with the study aims. BLT was designed to assist poor people, so it also targeted low income groups. The use of scattered samples and targeting limits what can be learned about spillover and wider economic effects, or the impact of a truly universal basic income. It is not possible to get a sense of the wider community impacts if the sample is geographically dispersed at low density or the intervention only includes low income groups. Studies with a high density of treatment group respondents, or interventions that are implemented universally, can provide evidence on spillover and wider economic effects.

Implementation issues

In many cases, implementation issues were specific to the context or to the nature of the interventions. We did identify several issues that recurred across studies or which may have wider relevance. Primarily due to concerns about promoting economic dependency on cash payments, many of the interventions were highly controversial and faced resistance from politicians, the media, and the public, which in some cases may have contributed to the demise of the intervention. The US NIT studies received hostile media coverage, and both respondents and researchers were accused of fraud. Politicians demanded results well before any data had been analysed. The tribal casino studies, BLT, and the Iran cash transfer were all predicted to cause laziness and increase substance abuse.

By contrast, the APDF is and always has been extremely popular. This is in part due to the source of the funds — the oil is seen as belonging to everyone, and thus the people believe the dividend is their right, not a form of social security. At the outset, Mincome was also very popular with the press and the public, who supported both the anti-poverty aims and the principal of experimentation to identify effective policy. The economic climate changed during the study period, and Mincome was abandoned due to loss of interest and lack of

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funds before completion. Thus it seems that framing is very important, and expectations around the cost and duration of such an experiment need to be managed carefully.

Lack of cooperation with all relevant authorities in some of the US NIT studies caused a great deal of problems, including issues with claiming other benefits, hostility from welfare agencies, and inability to maintain the experimental conditions for the intervention group. Where there was cooperation, respondents were able to claim passported benefits, and authorities routed saved welfare payments into the study funds.

Publicity and interaction with the media led to unwelcome attention and hostility for respondents in some studies, even where they had entered into it voluntarily. The media could be extremely vociferous in their attempts to identify and obtain interviews with respondents. Depending on the density of the sample and the nature of the intervention, maintaining anonymity was challenging. Relatedly, where there was wider knowledge of an intervention, community interaction sometimes led to jealousy and conflict with negative effects on respondents, intervention personnel, or even on the implementation of the intervention.

Ethical considerations, including transparency and consultation with respondents, are important. MPUCT took steps to ensure the project was acceptable and all respondents were content before and after the study. Both intervention and control group participants were fully aware of the implications of randomisation and study duration. The NIT researchers put a great deal of thought into the exit strategy when payments ended, which seemed to assist with drawing the experiment to a close.

Targeting and means-testing caused major problems in several of the studies. Most implementation issues identified in the NIT studies were concerned with monitoring income and ensuring respondents were paying the correct tax rates. Iran’s initial attempt to target its cash transfer proved too complex and was abandoned. The Indonesian BLT was paid to many non-poor people and failed to reach many who were poor, causing widespread social unrest. Establishing eligibility for Native Americans casino dividends is also challenging and a source of conflict.

Payment arrangements can influence outcomes. For people who were previously unbanked, special arrangements such as the ‘doorstep’ banking employed in MPUCT seemed to reduce barriers to inclusion and increase uptake of payments. The timing and value of payments can also influence effects on outcomes such as accidental death.
11. Potential effects of a universal basic income

Although no studies fully meet all the criteria for a basic income, they do provide useful evidence on some of the key outstanding questions. In particular, there is some evidence on the effects of permanent or long term cash payments on labour supply, the potential for cumulative effects in long term interventions, and for possible spillover and community level effects in interventions that are universal or include large populations.

Intervention duration and labour market participation

The available evidence on the effects of a permanent basic income on labour supply suggests that effects on most groups would be small. In Iran, where payments were at subsistence level and touted as permanent, labour supply did not change apart from a small increase for poorer women and the self-employed. In Alaska, where payments are permanent but do not cover subsistence, there was no effect on labour supply overall, but there was an increase in part-time working, particularly among women. The analysis suggested that any work disincentive effects were offset by an increase in labour demand fuelled by consumption. The GSMS findings indicate that the Eastern Cherokee nation’s permanent and substantial dividend payments have not changed maternal or paternal employment levels. Evidence on the 20 year SIME/DIME sample also suggested that labour supply effects on main and second earners were no stronger than those for the 3 and 5 year samples. There is some qualitative evidence from the casino studies of reductions in productive activity, particularly among young people who receive a large lump sum at 18 years.

Intervention duration and other outcomes

In terms of the potential effects of permanence on other outcomes, the GSMS reported that improvements in child mental health were strongest for the cohort with the longest exposure to the intervention. It appeared that this was mediated by improved parental relationships and reduced parental problems, which also developed cumulatively. Young people who had been exposed to the intervention for the longest remained in education for a year longer, and had lower rates of offending and substance use, impacts which are likely to have wider, cumulative social benefits.

Intervention density and spillover effects

The studies provide some evidence on the community level and spillover effects of cash transfers, which can offer insights into the potential effects of a universal transfer. The evidence on labour supply from Alaska suggests that a universal payment could lead to higher consumption, which in turn would lead to increased demand for labour. There did appear to be a larger reduction in labour market participation for the Mincome saturated sample, suggesting that community effects increased the acceptability of receiving the supplement. But individual decisions to reduce labour market participation were driven by health issues, caring responsibilities, employment insecurity and educational pursuits.
Although BLT was not universal, in areas with a high proportion of recipients there was evidence of an increase in non-recipients’ expenditure, again suggesting the existence of economic spillover effects. Positive economic impacts for non-Indians living in tribal lands suggest that individual casino dividends had some spillover effects on the wider community.

The Dauphin saturated sample also provides some evidence on the potential effects of a universal payment on other outcomes. Despite only 30% of the community actually receiving payments, there was a large reduction across the community in health service use for accidents and mental health issues. This may result from the indirect effects on non-recipients of overall reductions in poverty-related stress, alcohol abuse, and violence. There is also some evidence that for teens, decisions to remain in school for another year were influenced by their friends’ choices, which were influenced by the availability of the income supplement.

**Potential indirect effects**

In a number of areas there is evidence of direct impacts on specific outcomes which are known to have indirect effects on a wide range of other outcomes over the longer term. To take just one example, longer time spent in education and higher educational attainment have substantial long term effects on individual and societal outcomes, including higher income (Crawford and Cribb 2013), better adult health (Hahn and Truman 2015), higher cognitive ability in later life (Banks and Mazzonna 2012), and lower mortality (Hahn and Truman 2015). Large numbers of people remaining in education is associated with higher economic growth, but large increases in education expenditure yield only modest increases in retention (Jackson et al 2015). Improvements in other indicators, such as low birthweight, are similarly associated with substantial improvements in many individual and societal indicators over the long term. Interventions directly targeting such outcomes typically have modest effects (Allen et al 2017, Mallik and Spiker 2017). Reductions in health service use and in crime are also likely to have wider long term effects, including cost savings.

**Effects of subsistence level or withdrawn payments**

The studies provide some evidence on the effects of differing withdrawal rates versus fixed value payments. The withdrawal of payments in response to other income is generally expected to disincentivise working, but this was not observed among male heads in the NIT experiments. Similarly, subsistence level payments of a value which would permit labour market withdrawal did not seem to have that effect on male heads. As noted, all of the interventions were unconditional, and this does not appear to have affected labour supply responses. There is some evidence to suggest that unconditionality in Mincome gave people the ability to choose how to respond flexibly to changing circumstances, without having to give up work. The sense of autonomy and security thus engendered seemed to reduce stress, which is known to contribute to better mental health.

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12. Discussion

See also Summary of impacts

We conducted a scoping review of studies of interventions which meet some criteria of a basic income. We found 28 studies of ten interventions, providing evidence on the impact of unconditional payments to individuals or households on a wide range of outcomes. Some of the studies were small or included multiple subgroups, but a number used large samples and rigorous quasi-experimental methods. The included studies are highly heterogeneous. The interventions occurred in different economic, social and political contexts, and targeted different populations. Some were temporary, some were permanent, some were received by everyone and others by specific populations or dispersed samples. Existing social protection and interaction with taxes and other benefits were widely divergent. The underlying purpose of the interventions varied, but all provided quite substantial amounts of additional income to individuals or households with no conditions attached.

Across the studies, the effect of providing unconditional payments on the labour supply of most groups appears to be minimal, even where work disincentives were deliberately built in. In groups with larger reductions in labour market activity, it seemed that time gained was often channelled into other productive activities, such as education, providing care, and business activity. In the NIT studies, small reductions in male heads' labour supply arose from spending more time searching for work, which could lead to improved job fit. There was also consistent evidence that children and young people reduced labour market activity and spent more time in education. Given that this evidence comes from studies of differing interventions, in widely varying contexts, and including diverse populations, the consistency of effects increases confidence in the findings.

There were modest to strong positive effects on a number of health outcomes, including low birthweight, adult and child mental health, service use, and diet. Some studies suggested mechanisms underlying these improvements, including increased parenting quality and reduced financial strain. These effects were less consistent than those for labour market activity and educational participation, although this could be in part due to differences in measurements used or other study characteristics. Several studies reported reductions in criminal behaviour. There did not seem to be any effect on rates of marital dissolution, but one study reported strong positive effects on parenting quality and other measures of family relationships. Many studies reported stronger effects on health and educational outcomes in the most disadvantaged groups.

There is evidence to suggest that there could be important spillover and community level effects, and that impacts on some outcomes could strengthen over time. For instance, there appears to be an increase in consumption in Alaska, which in turn stimulates increased demand for labour. Improvements in mental health in Dauphin seemed to benefit the whole community. There was some evidence of increased business activity, which could have important economic effects if replicated at scale. Positive effects on child mental health and educational outcomes in the GSMS casino study appeared to be mediated by reduced

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parental problems and improved parenting, which became stronger over time. In addition, there is evidence from the wider literature that the indirect effects of improvements in outcomes such as education and low birthweight have widespread societal implications.

Some adverse impacts were reported, including increases in accidental mortality related to receipt of transfers. Increased substance abuse was implicated in these increases, but the authors of one study reported that this pattern is observed after any large payments, including salaries and social security benefits. There were also some qualitative reports of increased substance abuse, particularly for the tribal casino studies. It should be noted these have the added component of a very large lump sum for young adults which is likely to play a role in such substance abuse and also in reported reductions in productive activity. Cash transfers were linked to conflict in some communities, but this was engendered by issues around eligibility and resentment in a context of targeted payments.

The interventions were evaluated primarily using quasi-experimental methods, in addition to a number of RCTs, several qualitative studies and one controlled before and after study. The studies demonstrate the potential for using quasi-experimental methods to analyse policy level interventions using existing administrative or routine survey data. Qualitative methods were also used effectively to investigate the impact of transfers on communities, and to identify mechanisms connecting the payments to effects on health. A number of studies were limited by small samples or multiple intervention arms. Where it was possible to use a saturation sample, or to evaluate the effects of a universal intervention, useful data about community level and spillover effects were generated.

Many implementation issues identified focused around framing and perceptions of the intervention. The belief that people would withdraw from the labour market was common, and could give rise to negative publicity and political opposition. Where the source of payments was believed to be held in common, views were much more positive. Public and political understanding of the purpose of social experimentation reduced controversy around experimental studies. Conversely, lack of understanding of the purpose, costs, and need for lengthy timelines could lead to friction and loss of interest in study findings. Lack of cooperation between all relevant agencies and levels of government caused serious problems for the earlier NIT experiments. Where payments were restricted to certain groups, non-participants could be jealous of recipients. In policy level interventions, difficulties with establishing eligibility could lead to social conflict. In some cases, recipients were targets for hostility or persistent pursuit by the media.

We also identified five pilot studies which are ongoing or planned and one which has been terminated, which are reported in detail in Appendix 1. Many issues identified in the pilot studies were similar to those found in the completed studies. Small samples and overly complicated interventions will hamper inference in a number of cases. All but one of the pilots is using dispersed samples of low income individuals, which will again limit understanding of spillover or community level effects. One study is randomising at village level, and making payments to all residents of the study villages, but generalisability to Europe will be limited as it is conducted in a LMIC. None of the pilots is using quasi-

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experimental methods and only one is evaluating the economic effects of the intervention.

Several studies illustrate practical and ethical issues around differing approaches to recruitment and to maintaining anonymity. Some studies have encountered difficulty in recruiting low income respondents using door knocking or mail outs. Intense media interest has been a problem for at least one of the pilots. All of the pilots demonstrate that it is possible to set up payment systems and negotiate ways of operating with existing tax and benefit systems. As with the completed studies, political resistance has been a major challenge, leading to the termination of one pilot and the curtailment of two others. The pilots will generate more evidence on the effects of cash transfers paid to spatially dispersed low income individuals, but understanding of the community level effects of a basic income paid to all will be limited.

Gendered impacts of basic income

The question of how a basic income might affect gender relations is controversial. Some argue that basic income risks entrenching unequal relations by increasing expectations that women should stay in the home and engage in caring and domestic labour, whilst others believe that it would increase equality by giving women an independent source of income which would permit them to leave unhappy relationships (Smith and Shanahan 2018). The evidence from these studies is somewhat conflicting. Although there does not appear to be any evidence that cash transfers increase rates of marital dissolution (in the US NIT studies, Mincome, and GSMS), the effects on female labour force participation vary across the studies.

In the NIT studies, including Mincome, female employment did decrease more than men’s because the payments enabled women to spend more time in the home. The biggest reductions in the NIT studies were seen among female heads. Lone parents have worse than average health which is attributable to poverty, stress, and role strain (Gibson et al 2018). It is likely that the opportunity to work less and spend more time with children or in the home would be beneficial in such circumstances. Similarly in Alaska, although labour force participation did not decrease overall, there was an increase in part-time employment, mostly driven by women. By contrast, no effects on mothers’ employment were seen in the GSMS, and there was an increase among poorer women in the Iran cash transfer study. Why these effects should differ is not clear, but it seems that labour supply responses for women may be more context dependent than those of men.

Whether the payment is made at a household or individual level is expected to influence the effects on gender relations, but in Iran female employment increased in spite of household level payments. By contrast, individual payments in Alaska were associated with an increase in part-time employment. It is also interesting to note that in Indonesia, which has a relatively high gender inequality index, payments were made to men, but reportedly handed straight to their wives in most cases.

Gaps in the evidence

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While this review provides some insights into the potential effects of basic income, given the complexity and scale of the issues involved there are inevitably many outstanding questions. We did not identify any studies which assessed the potential for cost savings from reduced use of health services, lower crime rates or any other of the outcomes affected by the interventions. Few studies provide evidence on macro-economic impacts in areas such as productivity, consumption, demand for labour, wages, or inflation. A full economic evaluation would provide important evidence on such issues and thus a more accurate picture of wider social and economic impacts.

While there is evidence that some effects are stronger in disadvantaged groups, little is known about the wider social and economic impacts of people across all income groups receiving payments. These and other issues should be considered in any future evaluation design, but it is also possible that there are data from existing studies and interventions which could help to answer these questions. In particular, the Alaskan oil fund and the tribal casino dividends could provide more evidence on macro-economic effects, and we are not aware of any analysis of the health impacts of the Iran cash transfer.

Ultimately, any study using individual or community level samples may be unable to fully capture the emergent effects produced by unpredictable behavioural responses within multiple complex interacting systems. Simulation modelling approaches, such as Agent Based Modelling (ABM), have the advantage that they are able to model the emergent effects produced by the interaction of behavioural responses with wider social systems. As such, they may be particularly useful for assisting in the understanding of the potential effects of a full basic income.

**Strengths and limitations**

This is the first review of which we are aware that has used scoping review methods to systematically search for, and extract and synthesise data from, studies of interventions which unconditionally provide income to individuals or households in middle and higher income countries. In addition to insights into methods, implementation and contextual considerations, it provides an overview of the impacts of the included interventions. None of the included studies meets all of the conditions of ‘true’ BI. However, by employing a relatively inclusive definition of basic income and including a wide range of study designs, we have been able to locate evidence from interventions which share important features with basic income, including some which unconditionally provide payments to whole populations on a permanent basis.

In line with accepted scoping review methods, we conducted pragmatic searches within the constraints of available resources. Thus, we cannot guarantee the searches are comprehensive, and indeed there were search terms we did not incorporate into searches because they were not identified until late in the searches. However, we are reasonably confident that we identified the main interventions which met our inclusion criteria through our existing knowledge and extensive hand searching of international development organisations’ websites.

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A number of included publications were working papers or reports published by specialist research organisations, and as such had not been subjected to full peer review. The findings of such papers can be modified if they are later published in an academic journal, and indeed we found two studies for which the findings reported in a journal paper differed significantly from those reported in an earlier working paper. This is an issue which can affect any review which includes grey literature.

We reported information on methodological issues that were readily apparent. However, this does not constitute formal critical appraisal at the level employed in a full systematic review. Judgements regarding the quality of evidence indicate the degree of confidence that can be placed in the findings, therefore the discussion of methods we report does not provide a formal assessment of confidence. As discussed, consistency of impacts found across diverse studies and contexts serves to increase confidence in the findings.
13. Conclusions

While none of the included studies exactly replicates a ‘true’ basic income, they have important features which collectively provide evidence on the effects of unconditional, universal and permanent payments in a wide range of contexts. Taken overall, studies of interventions which unconditionally provide appreciable sums of money indicate that effects on labour market participation are small. There is evidence of strong positive effects on educational participation and on some health outcomes. The data point to the possibility of spillover and cumulative effects which could result in increased economic growth and reduced public service costs.

A number of the completed studies and the pilots were limited by small samples and overly complicated interventions. Any future evaluations of basic income should involve large samples and seek to test a simple intervention. The innovative use of quasi-experimental methods in a number of the studies demonstrates that it possible to robustly evaluate an intervention at community or national level without necessarily randomizing individuals or communities to receive payments. An economic evaluation would provide important evidence about net costs and benefits arising from changes in areas such as educational participation, economic activity, and service use. Key lessons about implementation of an evaluation include the importance of political support and the need to ensure that participants are not vulnerable to hostility or interference from non-recipients or other agencies.
<table>
<thead>
<tr>
<th>Intervention</th>
<th>Study</th>
<th>Outcomes reported</th>
</tr>
</thead>
</table>
| Negative Income Tax | New Jersey Graduated Work Incentive Experiment | **Impacts discussed in this review:**  
Labour supply: annual hours worked, % difference from control group.  
Reported for male heads, second earners, teens.  
Marital dissolution  
Education levels completed education, school enrollment, college attendance  
Anonym scale, Control of Future scale, Community Efficacy Scale, Psychosomatic and Nervous Symptoms Scale, Self-Esteem Scale, Worry Items, Quality of Life, General Happiness, Feeling of nothing to do  
Health and health service use:  
ADULTS: head’s and spouse’s number of chronic conditions, number of work days lost, number of days spent in a hospital, and number of physician visits.  
CHILDREN: per capita number of chronic conditions per capita number of days spent in bed, whether any child has spent at least one night in a hospital in the year previous to the interview, per capita number of visits to a physician  
**Impacts not discussed in this review:**  
Labour supply - effects of health on  
Labour supply - job selection  
Labour supply - psychological factors  
Labour supply - risk taking  
Labour supply - unemployment duration  
Labour supply - whole family  
Consumption  
Family formation  
Fertility  
Housing - purchasing  
Housing - demand  
Social outcomes  
Wages |
| Negative Income Tax | Rural Income Maintenance Experiment | **Impacts discussed in this review:**  
Labour supply: annual hours worked, % difference from control group.  
Reported for male heads, second earners, female heads, teens.  
Marital dissolution  
Mean Adequacy Ratio of 10 vital nutrients (MAR);  
Self-report delinquency scale; number of times in last 2 years committed theft, received stolen property, trespassed, committed assault, extortion, used marijuana or other narcotics. Another scale developed to take account of seriousness of offences.  
Attendance, comportment  
Grades, academic grades and Standardized Achievement Test score  
Psychological well-being; scales similar to New Jersey  
**Impacts not discussed in this review:**  
Labour supply - employees  
Labour supply - farmers  
Labour supply - business performance  
Assets/debts  
Consumption  
Employment decision making  
Mobility  
Political participation |
| Negative Income Tax | Gary Income Maintenance Experiment | **Impacts discussed in this review:**  
Labour supply: annual hours worked, % difference from control group.  
Reported for male heads, second earners, female heads, teens.  
Marital dissolution  
Reading Test Scores  
Academic Grade Point Average  
Days Absent  
Birth weight  
**Impacts not discussed in this review:**  
Labour supply - black family heads  
Labour supply - farm families  
Labour supply - underreporting  
Assets/debts  
Business decisions  
Housing services  
Social service use |
| Negative Income Tax | Seattle/Denver Income Maintenance Experiment | **Impacts discussed in this review:**  
Labour supply: annual hours worked, % difference from control group.  
Reported for male heads, second earners, female heads, teens.  
Marital dissolution  
Achievement scores, academic grades, and absence rates  
Remaining in school  
(1) number of work days lost due to illnesses, (2) number of hospital stays and (3) number of days hospitalized in the last 2 years, (4) number of work days missed in the last 6 months, (5) presence of a functional limitation on doing household tasks, (6) presence of a chronic condition that limits activities of daily living or (7) market work, (8) the duration of the chronic condition, (8) a mental health index, and (10) self perception of overall health  
Psychological distress - "a close variant of the Macmillan Health opinion survey index"  
**Impacts not discussed in this review:**  
Labour supply - childcare  
Assets/debts  
Consumption  
Employment decision making  
Employment transitions  
Family formation  
Family life  
Fertility  
Housing demand  
Job satisfaction  
Marital formation |
| Negative Income Tax | Manitoba Basic Annual Income Experiment | Labour supply: annual hours worked, % difference from control group.  
Reported for male heads, second earners, female heads, teens.  
Marital dissolution |
<table>
<thead>
<tr>
<th>Negative Income Tax</th>
<th>Price 2016 (SIME/DIME)</th>
<th>Reported for SIME participants and their adult children: Income, Labour force participation, Disability benefit claims, Disability benefit claims % successful, Mortality</th>
</tr>
</thead>
<tbody>
<tr>
<td>Negative Income Tax</td>
<td>Calnitsky 2016</td>
<td>Reason for joining Mincome Role of perceived stigma in decisions to claim Mincome/standard welfare benefits</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Forget 2011, 2013, 2013a (Mincome)</td>
<td>Total hospital separations (hospitalisation rate per 1000 people) Hospital separations for “accidents and injuries” Hospital separations for “mental health diagnoses” Overall physician claims and physician claims for “mental health diagnoses”. % progressing to Grade 11/12 high school Fertility (not reported here) Low birthweight Family dissolution</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Calnitsky and Latner 2017</td>
<td>Labour force participation</td>
</tr>
<tr>
<td>APDF</td>
<td>Hsieh 2003</td>
<td>Consumption - seasonal variations in expenditure (not reported here)</td>
</tr>
<tr>
<td>APDF</td>
<td>Chung 2017</td>
<td>Birthweight AGPAR score</td>
</tr>
<tr>
<td>APDF</td>
<td>Evans 2011</td>
<td>Accidental mortality</td>
</tr>
<tr>
<td>APDF</td>
<td>Kozminsky 2017</td>
<td>Income inequality (Gini Coefficient, Relative Mean Deviation (RMD) and Thiel’s Entropy Index)</td>
</tr>
<tr>
<td>APDF</td>
<td>Olson 1990</td>
<td>Utility maximisation (not reported here)</td>
</tr>
<tr>
<td>APDF</td>
<td>Jones 2018</td>
<td>Employment rate Part-time employment Consumption</td>
</tr>
<tr>
<td>Tribal casino</td>
<td>Great Smoky Mountains Study</td>
<td>Child BMI, height, weight, and obesity Psychiatric disorders amongst children and adolescents - emotional (anxiety or depression) and behavioural (conduct or oppositional defiant disorder) Substance use disorder early adulthood. Child and Adolescent Psychiatric Assessment Young Adult Psychiatric Assessment Trait conscientiousness Trait agreeableness Trait neuroticism Educational attainment (years of completed education at age 21, whether finished high school, whether has high school diploma/general equivalency degree) School attendance Criminal arrest figures (adult and child) Marital status Parental employment Parental mental health Parent-child relationship quality Parenting quality Parental supervision</td>
</tr>
<tr>
<td>Tribal casino</td>
<td>Bruckner 2010</td>
<td>Accidental mortality</td>
</tr>
<tr>
<td>Tribal casino</td>
<td>Foley 2005</td>
<td>Community perceptions of impacts of casinos</td>
</tr>
<tr>
<td>Tribal casino</td>
<td>Kodish 2016</td>
<td>Mechanisms linking casinos to health</td>
</tr>
<tr>
<td>BLT</td>
<td>World Bank 2012</td>
<td>Expenditure (not reported here) Community-wide expenditure Child labour School participation Inpatient and outpatient utilisation Finding a job Hours worked Leaving work</td>
</tr>
<tr>
<td>BLT</td>
<td>Bazz 2012</td>
<td>Expenditure-Based Poverty Transitions Labour supply: “total hours worked per household divided by the number of working age adults not currently enrolled in school.”</td>
</tr>
<tr>
<td>BLT</td>
<td>Hossain 2012</td>
<td>Broad range of social and community impacts, targeting effectiveness</td>
</tr>
<tr>
<td>BLT</td>
<td>Bazzi 2015</td>
<td>Consumption smoothing (not reported here)</td>
</tr>
<tr>
<td>BLT</td>
<td>Hasyid 2015</td>
<td>Community participation (not reported here)</td>
</tr>
</tbody>
</table>

**Impacts discussed in this review:**
- Health: Food sufficiency, children's nutrition, weight-for-age, alcohol purchasing
- Education: school enrollment, waged child labour
- Productive activity: investment in farm or business, productive assets, number of hours worked

**Impacts not discussed in this review:**
- Living conditions: water source, access to toilet, fuel used for cooking and lighting, ownership of assets, consumption
- Health-related outcomes: illness, perceived improvement, reason for perceived improvement, type of health service used, purchase of medicines, health insurance, health-related indebtedness
- Education: spending on school-related items
- Productive activity: income-earning work, second economic activity
- Gendered: nutrition, health, education, labour force participation
- Borrowing and indebtedness: levels and sources of debt

**MPUCT 2014:**
- SEWA Bharat

**MPUCT 2015:**
- Beck

**Iran Cash Transfer 2012:**
- Salehi-Islahani

| Number of hours worked |
| Labour force participation |

**Any hfd illness or injury lasting more than 24 hours and requiring treatment short of hospitalisation in preceding 3 months**

**Any hfd illness or injury requiring hospitalisation in preceding 12 months**

**Complete child vaccination coverage**
<table>
<thead>
<tr>
<th>Study/ intervention name</th>
<th>Location/s</th>
<th>Intervention dates</th>
<th>Duration</th>
<th>Type of programme</th>
<th>Population</th>
<th>Level of implementation</th>
<th>Payed to household or individual</th>
<th>Income threshold</th>
<th>Guarantee level/tax rate or level of payment</th>
<th>Co-interventions</th>
</tr>
</thead>
<tbody>
<tr>
<td>New Jersey Graduated Work Incentive Experiment</td>
<td>Trenton, Jersey City, New Jersey, and Scranton, Pennsylvania.</td>
<td>1968-72</td>
<td>3 years</td>
<td>NIT</td>
<td>Black, white, and Latino, two-parent families in urban areas with a male nondisabled head aged 18-58, and at least one dependent; unclear whether this had to be a child</td>
<td>Individual, scattered sample</td>
<td>Household</td>
<td>Below 150% of poverty line</td>
<td>Provided subsistence level income for those with no other income source. Eight combinations of guarantees and tax rates (1) 50% of poverty line guarantee, 30% tax rate; (2) 50% guarantee, 50% tax rate; (3) 75% guarantee, 30% tax rate; (4) 75% guarantee, 50% tax rate; (5) 75% guarantee, 70% tax rate; (6) 100% guarantee, 50% tax rate; (7) 100% guarantee, 70% tax rate; (8) 125% guarantee, 50% tax rate</td>
<td>None</td>
</tr>
<tr>
<td>Rural Income Maintenance Experiment</td>
<td>Duplin County, North Carolina, and Pocahontas and Calhoun Counties, Iowa.</td>
<td>1969-72</td>
<td>3 years</td>
<td>NIT</td>
<td>Rural, low-income families, in which the male head was 18-58 and not disabled. Female headed and aged or disabled headed households were also sampled but excluded from analyses because numbers were too low</td>
<td>Individual, scattered sample</td>
<td>Household</td>
<td>Below 150% of poverty line</td>
<td>Provided subsistence level income for those with no other income source. Five combinations of guarantees and tax rates (1) 50% of poverty line guarantee, 50% tax rate; (2) 75% guarantee, 30% tax rate; (3) 75% guarantee, 50% tax rate; (4) 75% guarantee, 70% tax rate; (5) 100% guarantee, 50% tax rate</td>
<td>None</td>
</tr>
<tr>
<td>Gary Income Maintenance Experiment</td>
<td>Gary, Indiana</td>
<td>1971-74</td>
<td>3 years</td>
<td>NIT</td>
<td>Black families with at least one child under the age of 18. 60% welfare dependent female headed families</td>
<td>Individual, scattered sample</td>
<td>Household</td>
<td>Below 240% of poverty line</td>
<td>Provided subsistence level income for those with no other income source. Four combinations of guarantee and tax rate (1) 75% of poverty line guarantee, 40% tax rate; (2) 75% and 60%; (3) 100% and 40%; (4) 100% and 60%</td>
<td>None</td>
</tr>
<tr>
<td>Seattle/Denver Income Maintenance Experiment</td>
<td>Seattle, Washington, and Denver, Colorado.</td>
<td>1970-80</td>
<td>3, 5 and 20 years</td>
<td>NIT</td>
<td>Couples (with or without children) or single heads of households with at least one dependent child younger than 18; and including a nondisabled husband, 18 to 58 years old, or a single, nondisabled, female head of household, 18 to 58. Black and White in Seattle, Black, White and Latino in Denver 40% were single parents</td>
<td>Individual, scattered sample</td>
<td>Household</td>
<td>$9000 p.a. for single worker, or $11000 for two worker households</td>
<td>Provided subsistence level income for those with no other income source. Tapered withdrawal rate which changed for each $1000 of earned income. The change in withdrawal rate is listed last in the following: 11 combinations of guarantee, tax rate, and change in tax rate for $1000 income increase: (1) 95%,50%,0%; (2) 95%,70%,0%; (3) 95%,70%,2.5%; (4) 95%,80%,2.5%; (5) 120%,50%,0%; (6) 120%,70%,0%; (7) 120%,70%,2.5%; (8) 120%,80%,2.5%; (9) 140%,50%,0%; (10) 140%,70%,0%; (11) 140%,80%,2.5%</td>
<td>3 co-interventions were included in the study: Free vocational counselling Free counselling plus a 50% tuition subsidy for education or career-related training Free counselling plus a 100% tuition subsidy</td>
</tr>
<tr>
<td>Manitoba Basic Annual Income Experiment</td>
<td>Winnipeg, Manitoba scattered sample site, Dauphin, Manitoba saturation site</td>
<td>1975-79</td>
<td>3 years</td>
<td>NIT</td>
<td>Low-income households with able-bodied heads under 58 years of age. Included single people with no children (21% of all men in the sample were single)</td>
<td>Individual, two scattered samples and one saturation site</td>
<td>Household</td>
<td>Below 150% of poverty line</td>
<td>Provided subsistence level income for those with no other income source. 8 combinations of maximum benefit level and tax rate (CAD $) (1) 3800, 35% (2) 3800, 50% (3) 4800, 50% (4) 2,800, 50% (5) 2,800, 75% (6) 2,800, 75% (7) 2,800, 75% (8) 2,800, 75%</td>
<td>None</td>
</tr>
<tr>
<td>Intervention</td>
<td>Study</td>
<td>Evaluation method</td>
<td>Data source</td>
<td>Sample size at baseline</td>
<td>Study dates</td>
<td>Comparison group</td>
<td>Analysis method</td>
<td>Methodological issues</td>
<td>Implementation</td>
<td>Context</td>
</tr>
<tr>
<td>------------------------------</td>
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<td>------------------------------------------------------------------------</td>
</tr>
<tr>
<td><strong>Negative Income Tax</strong></td>
<td>Calnitsky and Latner 2017</td>
<td>QE; DID, qualitative</td>
<td>Study data</td>
<td>Saturation sample n=147 (Dauphin), scattered sample n=45 (Manitoba treatment), control n=100 (Manitoba control).</td>
<td>1973-78</td>
<td>Dispersed treatment and control study samples in Manitoba. Income Assistance available to intervention and control groups</td>
<td>Regression modelling</td>
<td>Only respondents with no missing data in two separate study datasets are included in the analysis. This substantially reduces the sample size. Potential selection bias is not addressed. Some analyses are based on very small samples.</td>
<td>See Calnitsky 2016 See Calnitsky 2016</td>
<td></td>
</tr>
<tr>
<td><strong>Negative Income Tax</strong></td>
<td>Forget 2011, 2013, 2013a</td>
<td>QE; Interrupted time series</td>
<td>Routine data</td>
<td>Multiple controls selected from surrounding rural areas using propensity score matching. Income Assistance available to intervene and control groups</td>
<td>1971-85</td>
<td>40 surveys completed by Manitoba welfare recipients and 98 surveys completed by non-recipient &quot;controls&quot; from various small Manitoba towns, with incomes that would have qualified them for Mincome if they lived in Dauphin. Income Assistance available to intervention and control groups</td>
<td>Negative binomial distribution model</td>
<td>The analyses include all residents of Dauphin, whether they received were eligible for Mincome. It is not possible to tell whether effects were concentrated in recipients (30%) or spread across the whole sample.</td>
<td>See Calnitsky 2016 See Calnitsky 2016</td>
<td></td>
</tr>
<tr>
<td><strong>Negative Income Tax</strong></td>
<td>Calnitsky 2016</td>
<td>Qualitative</td>
<td>Study survey data - open-ended question responses</td>
<td>321 Mincome recipients, 98 non-recipients and 40 Manitoba welfare recipients</td>
<td>1976</td>
<td>40 surveys completed by Manitoba welfare recipients and 98 surveys completed by non-recipient &quot;controls&quot; from various small Manitoba towns, with incomes that would have qualified them for Mincome if they lived in Dauphin. Income Assistance available to intervention and control groups</td>
<td>Line-by-line and axial coding</td>
<td>Based on an analysis of responses to open-ended survey questions. Only a small proportion of respondents completed these questions. Potential selection bias is not addressed. Some analyses are based on very small samples.</td>
<td></td>
<td>Attitudes towards Mincome were very positive. Mincome was seen as morally superior to normal welfare. Application process was also much less onerous and stigmatising</td>
</tr>
<tr>
<td><strong>Negative Income Tax</strong></td>
<td>New Jersey Graduated Work Incentive Experiment</td>
<td>RCT</td>
<td>Study data</td>
<td>Total n=1216</td>
<td>1968-72</td>
<td>Usual care - AFDC-UP available to 2 parent families (initially more generous than most NIT plans, later reduced dramatically)</td>
<td>Conlisk-Watts method of allocation</td>
<td>The analyses conducted by NIT research staff used a range of methods, but most appear to be regression or ANOVA based.</td>
<td></td>
<td>Declining urban industrial areas. AFDC-UP implemented in New Jersey and Pennsylvania soon after intervention started. Intervention and control respondents eligible. Almost all experimental families eligible for lower level guarantees claimed welfare. Welfare are also passported to Medicaid and food stamps, but did include work requirements. Value of AFDC-UP was substantially reduced in 1971. The experiment generated a great deal of controversy at the time, leading to calls from the NJ senator for its cessation due to suspicions of fraud.</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Rural Income Maintenance Experiment</td>
<td>RCT</td>
<td>Study data</td>
<td>Total n=609; 587 male headed families retained in primary analyses.</td>
<td>1969-73</td>
<td>Usual care - no welfare for 2 parent families</td>
<td>The analyses conducted by NIT research staff used a range of methods, but most appear to be ANOVA regression based.</td>
<td>Conlisk-Watts method of allocation</td>
<td>Small sample size, further stratified to include single female headed households, households with an aged head, farm and nonfarm residents, as well as across 2 very distinct geographical areas. Few estimates significant and generalisability very limited.</td>
<td>The state govs of Iowa and North Carolina were not involved in delivering the NIT, which caused many problems for the families and the staff. Policy makers were keen to include many different subgroups in the study. However, the resulting subgroups were too small for meaningful analysis, leading to their exclusion from the analyses.</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Gary Income Maintenance Experiment</td>
<td>RCT</td>
<td>Study data</td>
<td>Total n = 1799</td>
<td>1971-75</td>
<td>Usual care - no welfare for 2 parent families, AFDC for single parent families.</td>
<td>The analyses conducted by NIT research staff used a range of methods, but most appear to be regression or ANOVA based.</td>
<td>Conlisk-Watts method of allocation</td>
<td>Not used. However, there were a number of other issues with representativeness of the sample</td>
<td>The involvement of state level authorities in delivering the programme assisted with implementation.</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Seattle/Denver Income Maintenance Experiment</td>
<td>RCT</td>
<td>Study data</td>
<td>Total n = 4800</td>
<td>1970-80</td>
<td>Usual care - no welfare for 2 parent families, AFDC for single parent families.</td>
<td>The analyses conducted by NIT research staff used a range of methods, but most appear to be regression or ANOVA based.</td>
<td>Conlisk-Watts method of allocation</td>
<td>Large number of treatment groups greatly weakened the precision of the estimates. Allocation method was particularly complex due to different duration of treatment groups and inclusion of training/counselling treatment groups</td>
<td>The involvement of state level authorities in delivering the programme assisted with implementation. NIT recipients were eligible for passported benefits</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Manitoba Basic Annual Income Experiment</td>
<td>RCT</td>
<td>Study data</td>
<td>Sources conflict; total n appears to be around 1700</td>
<td>1975-79</td>
<td>Usual care - Income Assistance, eligibility conditions unclear. Dauphin saturation site was not included in early analyses</td>
<td>ANOVA methods</td>
<td>Conlisk-Watts method of allocation</td>
<td>Attrition 36%, but this does not appear to have biased the estimates</td>
<td>See Calhoun 2016</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Price 2016</td>
<td>RCT</td>
<td>Study and routine data</td>
<td>Total n=2280</td>
<td>1978-2013</td>
<td>Usual care - no welfare for 2 parent families, AFDC for single parent families.</td>
<td>Least squares regression</td>
<td>In the absence of interim data, it is difficult to attribute any differences to the intervention after such a long period (40 years)</td>
<td>See Seattle/Denver Income Maintenance Experiment</td>
<td>Forty years after the initial intervention</td>
</tr>
<tr>
<td>Negative Income Tax</td>
<td>Stephens 2007 (SIME/DIME)</td>
<td>RCT</td>
<td>Study data</td>
<td>Total n=1923</td>
<td>1970-80</td>
<td>Usual care - no welfare for 2 parent families, AFDC for single parent families.</td>
<td>Regression modelling</td>
<td>See Seattle/Denver Income Maintenance Experiment</td>
<td>See Seattle/Denver Income Maintenance Experiment</td>
<td>SIME/DIME had NIT and job training interventions, both of which could run for 3 or 5 years. Previous analyses accounted for the variation in NIT duration, but not the variation in job training</td>
</tr>
</tbody>
</table>

**DID = difference-in-difference**  
**QE = quasi-experiment**  
**RCT = randomised controlled trial**
<table>
<thead>
<tr>
<th>Study</th>
<th>Author</th>
<th>Husbands</th>
<th></th>
<th>Wives</th>
<th></th>
<th>Single Female</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Annual hours</td>
<td>% difference</td>
<td>Annual hours</td>
<td>% difference</td>
<td>Annual hours</td>
<td>% difference</td>
</tr>
<tr>
<td>New Jersey</td>
<td>Keely (1981)</td>
<td>—116</td>
<td>7</td>
<td>—75</td>
<td>33</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>Robins (1985)</td>
<td>—34</td>
<td>2</td>
<td>—56</td>
<td>25</td>
<td>n/a</td>
<td>n/a</td>
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<tr>
<td></td>
<td>Burtless (1986)</td>
<td>—21</td>
<td>1</td>
<td>—56</td>
<td>25</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td>Rural</td>
<td>Keeley (1981)</td>
<td>?</td>
<td>9</td>
<td>?</td>
<td>29*</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>Robins (1985)</td>
<td>—56</td>
<td>3</td>
<td>—178</td>
<td>28</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>Burtless (1986)</td>
<td>—56</td>
<td>3</td>
<td>—178</td>
<td>28</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td>Seattle-Denver 3 year</td>
<td>Keeley (1981)</td>
<td>—147</td>
<td>8*</td>
<td>—139</td>
<td>21*</td>
<td>—155</td>
<td>15*</td>
</tr>
<tr>
<td>study</td>
<td>Robins (1985)</td>
<td>—113</td>
<td>7*</td>
<td>—141</td>
<td>21*</td>
<td>—163</td>
<td>16*</td>
</tr>
<tr>
<td></td>
<td>Burtless (1986)</td>
<td>—144</td>
<td>8</td>
<td>—107</td>
<td>17</td>
<td>—85</td>
<td>9</td>
</tr>
<tr>
<td></td>
<td>Robins (1985)</td>
<td>—35</td>
<td>2</td>
<td>—58</td>
<td>20</td>
<td>—37</td>
<td>10</td>
</tr>
<tr>
<td></td>
<td>Burtless (1986)</td>
<td>—114</td>
<td>7</td>
<td>*14</td>
<td>5</td>
<td>—112</td>
<td>30</td>
</tr>
<tr>
<td>All U.S. experiments:</td>
<td>Robins (1985)</td>
<td>—89</td>
<td>5</td>
<td>—117</td>
<td>21</td>
<td>—123</td>
<td>13</td>
</tr>
<tr>
<td></td>
<td>Burtless (1986)</td>
<td>—119</td>
<td>7</td>
<td>—93</td>
<td>17</td>
<td>—133</td>
<td>17</td>
</tr>
<tr>
<td>Mincome</td>
<td>Hum &amp; Simpson (1993)</td>
<td>—17</td>
<td>1b</td>
<td>—15</td>
<td>3</td>
<td>—79</td>
<td>7</td>
</tr>
</tbody>
</table>

*a* 3-year experiment only.  
*b* Includes single individuals (21% of all men in sample).  
* Statistical significance at the 5% level or lower. In some cases, statistical significance is not reported or is mixed (the result is an average of several results, some of which are significant). Burtless (1986) does not report statistical significance.
<table>
<thead>
<tr>
<th>Study/intervention name</th>
<th>Location/s</th>
<th>Intervention dates</th>
<th>Duration</th>
<th>Type of program</th>
<th>Population</th>
<th>Individual/area level implementation</th>
<th>Paid to household or individual</th>
<th>Income threshold</th>
<th>Guarantee level/tax rate or level of payment</th>
<th>Co-interventions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alaska Permanent Dividend Fund</td>
<td>Alaska</td>
<td>1982</td>
<td>Ongoing</td>
<td>UCT; resource dividend</td>
<td>Whole population of Alaska</td>
<td>National</td>
<td>Individual</td>
<td>n/a</td>
<td>Variable; can reach up to $5,000 p.a. for a family of 4. Equivalent to sums paid through family tax credits</td>
<td>Alaska abolished state level taxes when APDF was implemented</td>
</tr>
<tr>
<td>Bantuan Langsung Tunai</td>
<td>Indonesia</td>
<td>2005-2006; 2008</td>
<td>2005 - 1 year</td>
<td>UCT; fuel subsidy reform</td>
<td>Poorest 30% of the population</td>
<td>National</td>
<td>Household</td>
<td>Below 1.2 times the official region specific poverty line - IDR 175,000 in 2010</td>
<td>2005 - IDR 300,000 per quarter for 4 quarters 2008 - IDR 100,000 for 9 months. Paid to head of household. Value equal to 15% of targeted households’ consumption expenditure</td>
<td>Several interventions were launched at the same time. It is not clear how these might have interacted with BLT</td>
</tr>
<tr>
<td>Californian Native American nations tribal casino dividend (Kodish et al 2016)</td>
<td>California, USA</td>
<td>Various</td>
<td>Ongoing</td>
<td>UCT; casino dividend</td>
<td>American Indian gaming communities</td>
<td>Population is often concentrated on reservation land, but some tribal members live outside reservation</td>
<td>Individual</td>
<td>n/a</td>
<td>Varied across Indian nations. No detail reported</td>
<td>Investment in health and social services on reservations. Most young adults receive a large lump sum at 18</td>
</tr>
<tr>
<td>Eastern Cherokee Nation tribal casino dividend (GSMS)</td>
<td>11 counties in North Carolina</td>
<td>1996-</td>
<td>Ongoing</td>
<td>UCT; casino dividend</td>
<td>All members of Eastern Cherokee tribe - historically high poverty and unemployment on average</td>
<td>Population is often concentrated on reservation land, but some tribal members live outside reservation</td>
<td>Individual</td>
<td>n/a</td>
<td>Initially approximately $4,000 per person annually; $9,000 a year by 2006. Represented 20-40% of households’ income. Paid biannually. Children’s cash transfers are banked for them until age 18</td>
<td>Investment in health and social services on reservation was subsequent to data collection. All young adults receive a large lump sum at 18 (approx $30,000 in 2011) or 21 if they do not have a high school degree</td>
</tr>
<tr>
<td>Iran Cash Transfer</td>
<td>Iran</td>
<td>2010 -</td>
<td>Ongoing at time of writing</td>
<td>UCT; fuel subsidy reform</td>
<td>Whole population of Iran</td>
<td>National</td>
<td>Household</td>
<td>n/a</td>
<td>Approximately $90 pcm. Initially represented 20% of median household income</td>
<td>None reported</td>
</tr>
<tr>
<td>Meskwaki nation tribal casino dividend</td>
<td>Iowa, USA</td>
<td>Mid 1980s</td>
<td>Ongoing</td>
<td>UCT; casino dividend</td>
<td>All members of Meskwaki tribe - historically high poverty and unemployment on average</td>
<td>Population is often concentrated on reservation land, but some tribal members live outside reservation</td>
<td>Individual</td>
<td>n/a</td>
<td>$1800 pcm. 75% of children’s cash transfers are banked for them until age 18 if they complete high school leaving exams. 25% is paid to parents</td>
<td>Investment in health, housing, and social services on reservation. All young adults receive a large lump sum at 18 of up to $200,000</td>
</tr>
<tr>
<td>The Madhya Pradesh Unconditional Cash Transfer Pilot</td>
<td>State of Madhya Pradesh, India</td>
<td>2011-12</td>
<td>12-17 months</td>
<td>UCT; basic income</td>
<td>All residents of study villages</td>
<td>Area</td>
<td>Individual</td>
<td>n/a</td>
<td>General pilot villages: first 12 months 200 p/m per adult, 100 p/m per child, following 5 months 300 p/m, adults, 150 p/m children. Tribal pilot villages: 300 p/m adult, 150 p/m child throughout. Represented 30% of average income for a person in severe poverty</td>
<td>None reported</td>
</tr>
<tr>
<td>Various tribal casino dividends (Conner &amp; Taggart 2013)</td>
<td>USA</td>
<td>Various</td>
<td>Ongoing</td>
<td>UCT; casino dividend</td>
<td>All members of 330 Indian Nations</td>
<td>Population is often concentrated on reservation land, but some tribal members live outside reservation</td>
<td>Individual</td>
<td>n/a</td>
<td>Varied across Indian nations. No detail reported</td>
<td>Investment in health and social services on reservations. Most young adults receive large lump sum at 18</td>
</tr>
<tr>
<td>Intervention</td>
<td>Study</td>
<td>Evaluation method</td>
<td>Data source</td>
<td>Sample size at baseline</td>
<td>Study dates</td>
<td>Comparison group</td>
<td>Analysis method</td>
<td>Methodological issues</td>
<td>Implementation</td>
<td>Context</td>
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<tr>
<td>APDF</td>
<td>Chung 2017</td>
<td>QE: DiD</td>
<td>Routine data</td>
<td>7.7 million live births (52,346 in Alaska)</td>
<td>1978-1984</td>
<td>44 US states</td>
<td>Regression model</td>
<td>None known</td>
<td>Not reported</td>
<td>See Kozminsky 2017</td>
</tr>
<tr>
<td>APDF</td>
<td>Evans 2011</td>
<td>QE: DiD</td>
<td>Routine data</td>
<td>Not reported; all US deaths during study period.</td>
<td>2000-2006</td>
<td>Other US states</td>
<td>Regression model</td>
<td>None known</td>
<td>Not reported</td>
<td>See Kozminsky 2017</td>
</tr>
<tr>
<td>APDF</td>
<td>Kozminsky 2017</td>
<td>QE: Interrupted time series</td>
<td>Routine data</td>
<td>Annual observations of income inequality for all US states during the study period</td>
<td>1963-2012</td>
<td>Time series data 1963-2012</td>
<td>Autoregressive distributed lag (ARDL) and the Johansen cointegration approach to ITS</td>
<td>None known</td>
<td>Not reported</td>
<td>Interaction with the tax system means the APDF is regressive, as payments are not recovered from higher earners.</td>
</tr>
<tr>
<td>APDF</td>
<td>Jones 2018</td>
<td>QE: Synthetic control</td>
<td>Routine data</td>
<td>Employment data: 48,686,169 observations for the study period</td>
<td>1977-2015</td>
<td>average of control states to best match Alaska for the outcome of interest and other observable characteristics before the dividend payments begin.</td>
<td>Synthetic control</td>
<td>None known</td>
<td>Not reported</td>
<td>See Kozminsky 2017</td>
</tr>
<tr>
<td>BLT</td>
<td>World Bank 2012</td>
<td>QE: Triple-difference</td>
<td>Routine data</td>
<td>Total n = 9648 (intervention group 4444)</td>
<td>2005-2009</td>
<td>“(1) before and after the introduction of BLT, (2) in households receiving the BLT transfer and those not receiving the BLT transfer, and (3) in districts characterized by high levels of a chosen environmental feature versus those districts characterized by low levels of the same environmental feature.”</td>
<td>Regression modelling</td>
<td>The effects on the findings of informal redistribution of payments is unknown</td>
<td>BLT was designed and implemented over a period of 5 months. Half of respondents to household survey were aware of poor people who didn't receive the BLT, and a quarter were aware of non-poor people who did receive it.</td>
<td>There was much controversy about BLT because people believed the poor would stop working/spend the money on alcohol etc</td>
</tr>
<tr>
<td>BLT</td>
<td>Bazzi 2012</td>
<td>QE: DiD</td>
<td>Routine data</td>
<td>not reported</td>
<td>2005-2007</td>
<td>“1) before and after the introduction of BLT, (2) in households receiving the BLT transfer and those not receiving the BLT transfer, and (3) in districts characterized by high levels of a chosen environmental feature versus those districts characterized by low levels of the same environmental feature.”</td>
<td>Regression model (ToT analysis)</td>
<td>The effects on the findings of informal redistribution of payments is unknown</td>
<td>Due to fuel subsidy cuts, inflation increased to 17.9% at the same time as BLT was implemented</td>
<td>Indonesia is a strongly communal society, and people are expected to contribute to communal works. Hence it was possible for village leaders to redistribute payments to more needy people. This is likely to have diluted the impacts reported in quantitative studies. Cost of living varies so much that in some regions the payments were worth very little</td>
</tr>
<tr>
<td>BLT</td>
<td>Hossain 2012</td>
<td>Qualitative</td>
<td>Study data</td>
<td>1500 respondents from 33 communities</td>
<td>2010-11</td>
<td>Non-recipients of BLT</td>
<td>Analysis workshop and open-ended coding</td>
<td>Little detail is provided on how respondents were recruited. Targeting was problematic, and there was much resentment and conflict due to perceived unfairness in distribution.</td>
<td></td>
<td>Cost of living varies so much that in some regions the payments were worth very little</td>
</tr>
<tr>
<td>Iran Cash Transfer</td>
<td>Salehi-Isfahani 2018</td>
<td>QE: DiD, fixed effects</td>
<td>Routine data</td>
<td>Total n = 37,751</td>
<td>2010-11</td>
<td>30% of the population received payments 3 months later than the remainder. This group was used as a control in the DiD analyses. Households where the payment represented a smaller proportion of household income were used in the fixed effects analyses.</td>
<td>Regression modelling</td>
<td>High attrition from the survey (45%); weighting used to correct for this. It was initially intended that the cash transfer would be targeted at poorer people, but this was scrapped due to protests and because targeting proved too complex to administer</td>
<td></td>
<td>There was opposition to the policy due to its anticipated work disincentive effect. The majority of people did not believe that the programme would be permanent. Very high inflation rapidly eroded the value of the payments.</td>
</tr>
<tr>
<td>Study</td>
<td>Sample Method</td>
<td>Study Design</td>
<td>Sample Size</td>
<td>Study Years</td>
<td>Intervention</td>
<td>Outcome</td>
<td>Data Type</td>
<td>Study Follow-up</td>
<td>Notes</td>
<td></td>
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</tr>
<tr>
<td>MPUCT</td>
<td>Cluster RCT</td>
<td>Study data</td>
<td>Total n = 19000</td>
<td>2011-12</td>
<td>Control villages not receiving intervention</td>
<td>Unclear; some regression analyses used to correct for this.</td>
<td>The samples were small</td>
<td>Not reported</td>
<td>See SEWA Bharat 2014</td>
<td></td>
</tr>
<tr>
<td>Tribal casino dividend</td>
<td>Great Smoky Mountains Study</td>
<td>QE; DiD, triple difference, controlled before and after</td>
<td>Study data</td>
<td>1,420 children aged 9, 11, and 13 years at baseline (350 Native American, 1070 non-Native American)</td>
<td>Non-Native American children in households not receiving the UCT, older cohorts of Native American youth</td>
<td>Robust regression</td>
<td>The sample population; the analysis sample was small (n=75).</td>
<td>Results suggest that the timing and value of payments influences effects on mortality</td>
<td>Young people receive a large lump sum on reaching 18</td>
<td></td>
</tr>
<tr>
<td>Tribal casino dividend</td>
<td>Conner 2013</td>
<td>Controlled before and after</td>
<td>Routine data</td>
<td>300 Native American nations in 32 US states</td>
<td>Nations without gaming, Class II gaming nations, Class III gaming nations without per capita payments</td>
<td>Robust regression</td>
<td>The sample population; the analysis sample was small (n=75).</td>
<td>Results suggest that the timing and value of payments influences effects on mortality</td>
<td>Young people receive a large lump sum on reaching 18</td>
<td></td>
</tr>
<tr>
<td>Tribal casino dividend</td>
<td>Bruziner 2010</td>
<td>QE; Interrupted time series, ethnography</td>
<td>Routine data</td>
<td>Total population of study area = 57000</td>
<td>Monthly data series from 1990-2006</td>
<td>Poisson regression</td>
<td>The sample population; the analysis sample was small (n=75).</td>
<td>Results suggest that the timing and value of payments influences effects on mortality</td>
<td>Young people receive a large lump sum on reaching 18</td>
<td></td>
</tr>
<tr>
<td>Tribal casino dividend</td>
<td>Foley 2005</td>
<td>Qualitative interviews</td>
<td>None</td>
<td>None</td>
<td>None</td>
<td>Not reported</td>
<td>Methods are not clearly described. Recruitment of key actors seems somewhat ad hoc</td>
<td>There are issues around who is defined as a 'pure blood' tribal member for establishing eligibility, which can lead to conflict</td>
<td>Meskwaki payments are particularly large, and young people receive a very large lump sum on reaching 18</td>
<td></td>
</tr>
<tr>
<td>Tribal casino dividend</td>
<td>Kodish 2016</td>
<td>Qualitative interviews</td>
<td>None</td>
<td>None</td>
<td>None</td>
<td>Not possible to tell whether respondents are receiving per capita payments, nor how much they are</td>
<td>There appear to be some issues around transparency of dividend management</td>
<td>Sample drawn from 23 of 109 tribes in California.</td>
<td>Californian tribes operate a system of redistributing dividends to non-gaming tribes</td>
<td></td>
</tr>
</tbody>
</table>

**DiD = difference-in-difference, QE = quasi-experiment, RCT = randomised controlled trial**
<table>
<thead>
<tr>
<th>Study name</th>
<th>Location/s</th>
<th>Intervention dates</th>
<th>Duration</th>
<th>Type of programme</th>
<th>Population</th>
<th>Individual/area level implementation</th>
<th>Payed to household/individual</th>
<th>Income threshold</th>
<th>Level of payment</th>
<th>Co-interventions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kela Basic Income Experiment</td>
<td>Finland</td>
<td>2017-2019</td>
<td>2 years</td>
<td>UCT</td>
<td>Unemployed, age 25-58</td>
<td>Individual</td>
<td>Individual</td>
<td>Not reported, but it is targeted at unemployed thus by definition those on low incomes</td>
<td>€560 per month, which is lower than standard unemployment benefit. The payment is not withdrawn in relation to other income. Payment can be topped up with other benefits if participant is unemployed</td>
<td>None reported</td>
</tr>
<tr>
<td>GiveDirectly Basic Income Experiment</td>
<td>Kenya</td>
<td>2017-2029</td>
<td>2 or 12 years</td>
<td>UCT</td>
<td>All residents of 290 villages. Approximately one-third of households live below the Kenyan poverty line of less than US$15 per capita</td>
<td>Individual</td>
<td>Individual</td>
<td>None - the transfer is provided to all residents of study villages, regardless of income</td>
<td>3 intervention groups: approximately $0.75 per day paid to adults for 2 years or 12 years, or the total 2 year payment provided as a lump sum at the start of the experiment. The transfer is equivalent to half the Kenyan average income</td>
<td>None reported</td>
</tr>
<tr>
<td>Social Assistance Experiments</td>
<td>Utrecht, Groningen, Wageningen, Tilburg, Deventer, Nijmegen, Amsterdam</td>
<td>2017-2019</td>
<td>2 years</td>
<td>UCT/CCT</td>
<td>Current welfare benefit recipients</td>
<td>Individual</td>
<td>Individual</td>
<td>Not reported, but targeted at unemployed</td>
<td>Amounts are not reported but seem to be at the same level as current welfare benefit. Some groups will be allowed to retain 50% of additional income (up to the €199 maximum), whilst others will have the standard withdrawal rate of 75%</td>
<td>None reported</td>
</tr>
<tr>
<td>Y Combinator Research</td>
<td>2 US states, yet undecided, but will be announced</td>
<td>Not announced as yet</td>
<td>3 or 5 years</td>
<td>UCT</td>
<td>Age 21-40, with total income in the year prior to enrolment not exceeding the area median</td>
<td>Individual</td>
<td>Individual</td>
<td>Income below area median</td>
<td>$1,000 per month for either 3 or 5 years. Not withdrawn. Researchers are seeking waivers and exemptions so that participants continue to receive benefits for which they are eligible based on earned income.</td>
<td>None reported</td>
</tr>
<tr>
<td>Ontario Basic Income Experiment</td>
<td>Ontario, Canada</td>
<td>2018-2018</td>
<td>3 years</td>
<td>NIT</td>
<td>Age 18-64 and resident in a test region for at least 12 months</td>
<td>Household; a single payment is made to couples, which is lower than the value for 2 individuals.</td>
<td>Individual</td>
<td>$34,000 per year for a single person and $48,000 per year for a couple</td>
<td>Payments are set at 75% of the Low Income Measure. Participants receive up to $16,989 per year for a single person and $24,027 per year for a couple, less 50% of any earned income. People with a disability will also receive up to $500 per month on top</td>
<td>None reported</td>
</tr>
<tr>
<td>B-MINCOME Experiment</td>
<td>Barcelona, Spain</td>
<td>2017-2019</td>
<td>2 years</td>
<td>Guaranteed minimum income</td>
<td>Low income people aged 21-40 in receipt of various social services and resident in Besos, Barcelona since 2015</td>
<td>Individual</td>
<td>Household</td>
<td>A threshold is alluded to but the value is not reported</td>
<td>The amount of the GMI will depend upon household composition and financial status, and is expected to range from €100 to €1,676 per month per household. For the group receiving unconditional payments two withdrawal rates will be tested</td>
<td>25% of the transfer will be made in a new local currency. There are an additional 4 treatment groups receiving various social and economic interventions</td>
</tr>
<tr>
<td>Intervention</td>
<td>Study</td>
<td>Evaluation method</td>
<td>Data sources</td>
<td>Sample size at baseline</td>
<td>Study dates</td>
<td>Comparison group/s</td>
<td>Outcomes of interest</td>
<td>Methodological issues</td>
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</tr>
<tr>
<td>UCT</td>
<td>Kela Basic Income Experiment</td>
<td>RCT? Unclear how participants allocated to intervention and control groups.</td>
<td>Administrative data</td>
<td>Total n = 177,000 (2,000 in treatment group)</td>
<td>2017-2019</td>
<td>Remainder of unemployed claimant population</td>
<td>Primary outcome: labour supply. Secondary outcomes: health and well-being, medication expenditure, use of health services, social relations, and income</td>
<td>Use of entire unemployed population as control group is unsatisfactory - unclear if intervention group were randomly assigned.</td>
<td>Mandatory participation in trial is ethical problematic and not necessary to avoid selection bias.</td>
<td>Centre-right government introduced trial. Plans for further trials have now been shelved.</td>
</tr>
<tr>
<td>UCT</td>
<td>GiveDirectly Basic Income Experiment</td>
<td>Cluster RCT</td>
<td>Study survey</td>
<td>290 villages, 21,000 will receive a cash transfer, with 5,000 receiving payments for 10 years</td>
<td>2017-2029</td>
<td>In addition to a control group receiving no payments, 2 further groups are receiving payments for 2 years, or the equivalent as a lump sum</td>
<td>Individual level: Economic status (income, consumption, assets and food security), time use (employment, education, leisure, community participation), risk-taking (e.g. migration or business start-up), gender relations (particularly female empowerment), aspirations and outlook. Community level: Economic effects, access to health, education/water facilities, road access, community engagement, crime.</td>
<td>Ad-hoc accounts are being collected from study participants, whose details are made public on the study Twitter feed. This has the potential to alter respondents' responses to the intervention.</td>
<td>The use of study participants' personal details to publicise the study has ethical implications.</td>
<td>The study is conducted in a LMIC with high levels of poverty, limiting generalisability.</td>
</tr>
<tr>
<td>UCT/CCT</td>
<td>Dutch Social Assistance Experiments</td>
<td>RCT</td>
<td>Not reported</td>
<td>Utrecht: 200 each in 4 treatment groups. Unclear for other study sites</td>
<td>2018-2020</td>
<td>For Utrecht: 1) usual conditions plus increased support, 2) usual conditions plus allowed to retain mere of earned income, 3) usual conditions. Other municipalities vary</td>
<td>Labour market behaviour, health and well-being, social participation, financial circumstances, recipient and caseworker satisfaction, and costs of the scheme.</td>
<td>Small samples and multiple intervention arms. Respondents with no conditionality will continue to have high withdrawal rates. However, they will still be subject to conditionality.</td>
<td>Lengthy negotiations with central government to include unconditional treatment groups.</td>
<td>Municipal dissatisfaction with increasing conditionality imposed by central government.</td>
</tr>
<tr>
<td>UCT</td>
<td>Y Combinator Research</td>
<td>RCT</td>
<td>Administrative data, web-based surveys, qualitative interviews, bio-markers, financial tracking apps, data from credit reference agencies, and cognitive function tests</td>
<td>1,000 in treatment group, 2,000 in control group</td>
<td>Not announced as yet</td>
<td>Receive $50 pcm for study participation</td>
<td>Only categories and sub-categories reported here: TIME USE OUTCOMES Employment Human Capital Investment Unpaid Productive Activity Leisure HEALTH-RELATED OUTCOMES Physical Health and Well-being Food security Housing quality and stability Crime victimization MENTAL HEALTH Cognitive Functioning and Stress (cortisol levels) FINANCIAL OUTCOMES Assets, Savings, Borrowing, and Investment Behaviour Cash Flow Financial Health POLITICAL AND SOCIAL OUTCOMES Political and Social Attitudes and Behaviours Social Capital Anti-Social Behaviour Time and Risk Preferences SPILLOVER AND NETWORK EFFECTS</td>
<td>Study researchers are concerned about the risk of differential attrition, non-representativeness, and low uptake.</td>
<td>A pre-pilot is being conducted to refine approaches to recruitment, data collection, payment, and reducing attrition. Door-to-door recruitment proved challenging in the pre-pilot.</td>
<td>Funded by Silicon Valley start-up. Study locations unclear as yet.</td>
</tr>
<tr>
<td>NIT</td>
<td>Ontario Basic Income Experiment</td>
<td>RCT? Unclear how participants allocated to intervention and control groups.</td>
<td>Study survey, administrative data</td>
<td>4,000 in treatment group, 2,000 in control group</td>
<td>2018-2018</td>
<td>Usual care</td>
<td>Food security, stress and anxiety; mental health; health and healthcare usage; housing stability; education and training; employment; labour market participation.</td>
<td>Unclear how participants allocated to intervention and control groups.</td>
<td>There was a lengthy period of consultation with potential recipients and stakeholders.</td>
<td>The study began in April 2018 and was terminated by the incoming Conservative provincial administration in July 2018.</td>
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</table>
| UCT/CCT | B-MINCOME Experiment | Study survey, ethnographic data | 1,000 households in treatment group, 1,000 in control group. | 2017-2019 | There are 4 additional intervention groups. The control group is 1,000 households. Unclear how these were recruited. | Labour market participation, food and housing security, economic well-being, educational attainment, community outcomes, health and well-being, demand on social services. | Extremely complicated intervention. Unclear whether recruitment process resulted in representative sample. Not clear how control group were recruited. Multiple subgroups resulting in small samples. Many aspects of reporting are unclear. | Complex recruitment process. | }
Bibliography

Publications from studies included in this review


SEWA Bharat (2014). A little more, how much it is... piloting basic income transfers in Madhya Pradesh, India. New Delhi, SEWA Bharat and UNICEF.


Other publications referenced in this review


Basic Income Earth Network. (Undated). "What is basic income?" Retrieved 26/9/18, from https://basicincome.org/basic-income/


Appendix 1 Current Basic Income Pilots and Experiments

Pilot study methods

To find material on ongoing or planned pilot studies, we screened the searches for completed studies, consulted the websites of the Basic Income Earth Network and a number of international development organisations (see Appendix 2), and conducted Google searches of terms relating to the pilots we identified. The inclusion criteria were the same as those used for the completed studies. Data extraction was not as exhaustive as that for the completed studies, but an extraction form was developed for the key information required. One author extracted data from the pilots and another author checked it. Evaluation and intervention characteristics were tabulated.

As with the completed studies, the pilots were analysed and interpreted in light of the research questions, focusing on what might be learnt about approaches to intervention and study design, implementation and potential methods issues. We also considered what it might be possible to learn about the effects of a ‘true’ basic income given the studies’ characteristics. Although no formal critical appraisal was undertaken, the analysis considered any readily apparent methodological issues and how these might influence the utility of the findings.

Information about the putative effects of some of the ongoing studies has entered the public domain, either through media interviews with respondents (Finland), blogs written by people connected with the study (Ontario), or Twitter feeds run by the study researchers (Kenya). Prior to completion of data collection and analysis, we consider this information anecdotal and have not included it in the pilot study findings.

Pilot study findings

We identified eight ongoing or planned trials of interventions with similarities to a basic income in various locations around the world. Two of these trials, in Uganda and Stockton, USA, have very small samples and it is unlikely they will provide any useful evidence. In addition to the trials identified, an intervention in Brazil provides R$20 per month to 42,000 residents of Marica, and an additional R$110 to the poorest families. We could not locate any further information in English on this scheme, including whether an evaluation is being conducted.

We provide a brief overview of the main features of the remaining six trials, any known issues with the evaluation, and how they compare to a ‘true’ BI, below. In a number of cases it was difficult to locate recent or verifiable information on the studies, and few have published full study protocols in line with contemporary requirements. It should also be
noted that this is a fast-moving field, and the below reflects the situation at the time of writing (August 2018). An overview of intervention and evaluation characteristics is provided in Table 7 and Table 8.

Finland: Kela Basic Income Experiment, 2017-18

The Finnish government are conducting a two year trial of unconditional monthly payments. The randomly selected sample of 2000 recipients is drawn from unemployed people aged 25-58, who are permitted to keep all of the payments if they do find work. The payments are set at €560 per month, which is lower than standard unemployment benefit although unemployed people can claim the difference in benefits. The remainder of the unemployment claimant population (175,000) acts as the control group. To avoid selection bias, participation is mandatory. Labour supply effects are the primary outcomes of interest, but data will also be collected on health and well-being, medication expenditure, use of health services, social relations, and income. No data are to be analysed until the trial is completed, and most data used will be administrative, in an attempt to avoid ‘observer bias’. However, some respondents have been interviewed by the media, leading to misleading early reports of the study’s effects, and potentially influencing participants’ responses to the experiment.

The Finnish intervention is not universal or permanent, and does not cover subsistence. The sample is dispersed throughout Finland, so there will be no evidence about spillover effects. It is however unconditional and fixed, so its value is not influenced by decisions around whether or how much to work. It should therefore provide some useful evidence on the labour supply effects of unconditional payments. The researchers state that the sample is not large enough to provide sufficient power and the use of the entire unemployed population as a control group is not satisfactory. The mandatory nature of the intervention raises ethical issues. Randomly allocating participants to intervention and control groups after they have been given an opportunity to refuse to participate should prevent selection bias, while preserving respondents’ freedom of choice. Contrary to recent media reports, the experiment has not ended early and will continue until December 2018 as planned. However, amid political controversy over the potential to disincentivise working, previously proposed follow-up studies are not going ahead. Results are expected in 2019.

United States: Making Ends Meet 2019-2024

Y Combinator is a Silicon Valley based start-up company which provides seed funding for small companies. They are motivated to pilot basic income by concerns about increases in income inequality, in-work poverty, and the potential for mass unemployment due to automation. The study will be managed by researchers from the University of Michigan, and is now expected to begin in early to mid-2019. They have recruited research staff and published a research protocol on the proposed project, but it is not yet clear where the study will be conducted. Details of study locations will not be revealed to attempt to protect the anonymity of participants. The study proposes to recruit 3,000 participants in two US
states, of whom 1,000 will be randomly allocated to the intervention group and receive $1,000 per month for 3 or 5 years. Payments will be unconditional and fixed. The control group will receive $50 per month throughout. The intervention will be targeted at people aged 21-40 with an income below the median for their area of residence. The researchers are going to some lengths to ensure that interactions with other benefits do not leave respondents worse off, including attempting to secure waivers from state and local agencies to preserve entitlements to existing means-tested benefits.

The study plans to draw a stratified random sample of census tracts with median household income below the area median. Within the tracts, they will randomly select addresses, screen them for eligibility, and then draw a random sample of no more than 1% of eligible individuals stratified on race, gender, and income, who they will attempt to recruit using door-to-door methods. They plan to use a wide range of data collection methods to collect data on a broad suite of outcomes. The outcome categories include time use, health and well-being, financial health, risk-taking behaviour, political and social behaviour, crime, effects on children, and an attempt to measure social spillover effects by analysing some data from respondents’ social networks. In addition to administrative data, they intend to use a number of frequent web-based surveys, qualitative interviews with a subsample of 200, bio-markers, financial tracking apps, data from credit reference agencies, and cognitive function tests. While the researchers are clearly aware of the risk of research fatigue, the volume of data they intend to collect seems likely to place a heavy and potentially intrusive burden on participants.

A very small pre-pilot (n=100), which is designed to refine recruitment methods and intervention implementation, has been underway for some time. No impact data will be reported from this study. The project protocol reports that pre-pilot recruitment in low income areas has been very challenging, as housing often has high security, and the 45 minute enrolment process was too time-consuming. The study has been delayed due to the pre-pilot taking much longer than expected, and gaining ethical approval has also been very time consuming. The researchers discuss their decision to target people on low incomes, arguing that the smaller expected effects on high income individuals would dilute the mean impacts of BI. They also acknowledge that the dispersed sample design precludes understanding of macro-economic and spillover effects, but argue that a saturated sample would be too expensive, it would be too difficult to identify a suitable control community, and it would risk causing conflict with non-recipient communities. They are also concerned about the risks of differential attrition, low uptake, and lack of representativeness. The media will not be granted access to respondents at any time during the study.

The pilot is similar to a basic income in that it will provide an unconditional payment which is not affected by other income. It is clearly not universal, as it is targeted at a dispersed sample of people on low incomes. Its value will depend on the cost of living in the chosen study sites, but $1,000 per month is unlikely to cover subsistence. Evidently it is not permanent, but it does have a longer duration arm of 5 years. The sample size is reasonably
large, and powered to detect quite small effects in the primary outcomes, but it does not leave much scope for subgroup analyses. The study will provide useful evidence on the effects of a regular, substantial and unconditional payment on isolated low income individuals across some interesting, novel outcomes. Understanding of the effects of a ‘true’ basic income is not likely to be advanced due to the dispersed sample design.

The Netherlands: Social Assistance Experiments 2018-2020

Plans are underway for pilots in seven Dutch cities. Conditionality on benefit receipt has recently been increased in Holland, but legislation has been passed which permits experimentation at municipal level. Early proposals to test unconditional payments to existing social assistance claimants were thwarted by the terms of the legislation. These require that any experimental group with less stringent conditionality be matched by a group with stricter work requirements, and limit the amount earned without affecting other benefits to €202 per month. Researchers have gained assent to go ahead with six separate trials in Utrecht, Groningen, Wageningen, Tilburg, Deventer and Nijmegen. A seventh trial in Amsterdam has so far been delayed by the city’s refusal to comply with the terms of the legislation. Information in the public domain is limited, but all of the trials are described as RCTs targeting existing social assistance recipients, and seem to be 18-24 months in duration.

The Utrecht study webpage states that the trial began recruitment in February 2018 and data collection in June. It appears that eligible Utrecht welfare recipients will be informed of the study by letter and given the opportunity to volunteer. There are one control and three treatment groups with 150-200 respondents randomly assigned to each group. Group 1 has no conditionality and standard withdrawal conditions for earned income, Group 2 has normal conditionality plus extra support, Group 3 has normal conditionality but can earn more without affecting their benefits, and Group 4 is the control group, retaining normal benefit conditions. However, it appears that even those in the nominally unconditional study arms will have their job search activity monitored every 6 months, and will be removed from the trial if they have not looked for work. The study will collect data on labour market behaviour, health and well-being, social participation, financial circumstances, recipient and caseworker satisfaction, and costs of the scheme. Data sources were not reported, but it is likely that the main source will be administrative data.

As at October 2017, the BIEN website reported that the Tilburg and Wageningen trials were similar to Utrecht, while Groningen had a fifth group which could select from any of the three treatments. Nijmegen has two groups; one has no conditionality and the other has stricter conditionality but with greater choice of work-related activity. Both groups can keep 50% of earned income up to the monthly maximum. These latter trials began between October and December 2017. Participation in all of the studies is voluntary. We could not find any information on sample sizes for these studies.
The Dutch experiments are again targeted at existing welfare recipients, and have only small samples with limited conditionality. Payments presumably cover basic subsistence, but the ‘unconditional’ groups and most are only allowed to retain 25% of earnings for a maximum of 6 months. This will introduce work disincentive effects over and above those of receiving unconditional payments. Thus the groups with no conditionality are small and face high withdrawal rates. In this light, it seems unlikely that the Dutch experiments will add much to the existing evidence base on basic income.

Spain: B-MINCOME, 2017-2019

Researchers in Barcelona are testing an intervention which aims to transform the delivery of public services, and to tackle “passivity and dependency” among benefit recipients. A collaboration involving several universities and many NGOs and public bodies is undertaking the evaluation. The study is being conducted in the Besos district, which has very high rates of poverty and unemployment. There are quite complex eligibility criteria pertaining to age, length of residence in the area, receipt of other social services, and a commitment to remain in the same household for the 2 year duration of the study. An income threshold is also alluded to, but we could not locate the value.

The intervention is very complicated, with many different subgroups receiving various social and economic interventions in addition to a guaranteed minimum income. Following a highly complex recruitment procedure, 1000 households were randomly selected from the 1527 applicants who met the eligibility criteria. These were then randomly allocated to one of many subgroups. It would appear that 450 households were allocated to receive payments without conditions or any co-interventions. This group was further subdivided into two groups with differing withdrawal rates for additional income. How the control group were recruited is not stated. Outcomes include labour market participation, food and housing security, economic well-being, educational attainment, community outcomes, health and well-being, and some measures of demand on social services. It seems that data will be collected via dedicated study surveys, along with a large ethnographic component.

The payments are intended to cover all living expenses including housing and utilities. There is a complex calculation method based on number of household members, current income and household costs. Payments may range from €100 to €1,676 per month per household, of which 25% will be paid in a new local currency developed for the intervention. As noted, payments are withdrawn in relation to additional income. It seems that in one group the withdrawal rate is 25 or 35%, depending on the household’s income at the start of the experiment. The rate for the other group is not clear. One of the project reports appears to suggest that the transfers will not affect any other benefits received.

The B-Mincome intervention is very complicated, and has multiple small subgroups. The group receiving an unconditional payment is somewhat bigger, but this is subdivided by the withdrawal rate, and a quarter of the payment is made in a new local currency. It is not clear whether the recruitment method will have achieved a representative sample. It is also
unclear whether the unit of analysis is the individual or the household. In common with the other pilots, it targets people on low incomes, and is relatively short term. It is possible some useful data may be gleaned from the unconditional payment group, although the local currency co-intervention and varying withdrawal rates will complicate interpretation.

**Ontario Basic Income Pilot 2018-2018**

Although the newly elected Conservative administration of Ontario recently announced that they are terminating the Basic Income Pilot, there is still relevant information on study design and intervention implementation, which we report in brief below.

The Ontario Basic Income Pilot began collecting data from 4,000 intervention group and 2,000 control group participants in April 2018. There was a lengthy consultation process during the development stage of the study to consider the best approach to intervention and study design. The outcome of this was a study which was very similar in design to the earlier Canadian Mincome study; an NIT which was paid at a household level to working age individuals or couples whose income fell below a given threshold (Can$34,000 for a single person, Can$48,000 for a couple), and which was withdrawn at a rate of 50% on any income above Can$16,989 for a single person and Can$24,027 for a couple. Payments were to have been made for a period of 3 years. It provided a guaranteed income equivalent to 75% of the Low Income Measure. Additional payments were available to people with disabilities, up to a maximum of Can$500 per month. It was not conditional on working or seeking employment and was available to people in education. We could not locate any information on how respondents were allocated to the intervention and control groups.

Like Mincome, there were three sites, two with scattered samples in Hamilton and Thunder Bay, and one saturated sample in Linsay, where anyone in the area could claim the NIT if they met the income criteria. Data were to be collected via study surveys on a range of outcomes including: food security; stress and anxiety; mental health; health and healthcare usage; housing stability; education and training; and employment and labour market participation. In addition, the saturation site was designed to provide data on spillover and community level effects. Participants have been informed that they will receive their last payment in August 2018. It is not clear whether there are plans to analyse any of the study data.

The study struggled to recruit respondents using a 40 page enrollment package that was mailed to eligible households. The pilot transfer payments appear to have been efficiently integrated into the existing tax and benefits system. Participants could continue to receive Child Benefit payments, existing contributions-based benefits reduced the NIT dollar for dollar, while recipients of means-tested subsistence payments had to withdraw from those programmes. It is not clear whether there was an assessment process for extra disability payments. If the pilot had not been terminated, it would have provided unconditional, subsistence level payments to a large sample of low income people for 3 years. Notably, only one withdrawal rate was to be tested in this study, presumably due to the issues with
small sub-samples in the early NIT experiments. The saturation sample would have provided insights into community level effects of a targeted payment. As such, the results would have provided a useful update to the 1970s’ findings, but left several important questions unanswered.

**Kenya: GiveDirectly Basic Income Experiment, 2017-2029**

Building on previous studies (which did not meet the scoping review criteria), this large US charity is providing payments to all residents of 190 villages for up to 12 years. Using a cluster RCT design to randomly allocate treatment at the level of villages rather than individuals, the study will test the effect of providing substantial payments to all individuals for a considerable period of time. GiveDirectly began making payments in 2018 using a mobile money service which is popular in Kenya. Freely available information is limited to brief overviews on the websites of involved organisations.

There are three intervention arms. In Group 1, the adult residents of 40 villages will receive $0.75 per day for 12 years. The 80 villages in Group 2 will receive payments for 2 years, and in Group 3, 70 villages will receive the total value of Group 2 payments as a one-off lump sum. A control group of 100 villages will receive no payments. The intervention groups combined consist of 21,000 people, with 5,000 receiving the long term cash transfer. The value of the payment is approximately half the Kenyan average income. Data will be collected via a study survey in 2019 and every 3-5 years thereafter. Effects to be measured include income, consumption, assets and food security, time use (employment, education, leisure, and community participation), risk-taking (e.g. migration or business start-up), gender relations (particularly female empowerment), aspirations and general perspective on life. Community level effects on the economy, access to health, education, and water, community engagement and crime will also be measured.

GiveDirectly has considerable experience in delivering such interventions in LICs and LMICs. In addition, they have conducted a small pre-pilot to refine study methods and payment mechanisms. As part of the project publicity, the researchers are conducting ad hoc interviews with some study respondents, which are disseminated via Twitter along with interviewees’ photographs and other details. This risks influencing how people to respond to the intervention and is ethically questionable. This is the only pilot which is targeted at all income groups, but it is not clear how wealthy non-poor recipients might be. The poverty rate in the two counties hosting the experiment is 40%, but those above the poverty threshold may be considered poor in the context of an HIC. Unfortunately, while this is a large, well-conducted trial of ‘true’ basic income which will be able to measure community level effects over the long term, its location in an LMIC limits generalisability to an HIC context. The long duration, while highly desirable, obviously means the effects will not be known for a considerable time. Early findings are expected in 2019 or 2020.
Pilot study conclusions

We found five ongoing or planned pilots of interventions similar to BI, and one which has been terminated. Few of these provided published protocols. Several involve small samples and complicated interventions. Three studies are definitely RCTs, and the other three appear to be, but we could not confirm whether respondents were randomly assigned to intervention and control groups. No studies use quasi-experimental methods, and none reported plans to conduct an economic evaluation. Approaches to data collection included using routine data, face to face surveys, web-based surveys, mobile apps, qualitative interviews, and taking physical samples of bio-markers. The use of routinely collected administrative data will save on study costs and minimise research burden on respondents.

Only one study met all of the criteria of a ‘true’ BI (as far as possible in the context of a trial), in that it is available to all residents of a defined area, regardless of income and will run for 12 years. This study will provide data on longer term and community level effects, but the LMIC context limits generalisability. The remainder are targeted at dispersed samples of poor or unemployed respondents (apart from the cancelled Ontario saturation site), and so will not provide evidence on community level effects. One dispersed sample study plans to collect data on spillover effects among respondents’ social networks.

In terms of implementation, the studies demonstrate that it is possible to develop efficient payment systems, and to find ways of operating interventions alongside existing tax and benefit systems. This may be worth examining in more detail, while bearing in mind that every system is different and will involve different challenges. Preparation for trials included consultation with potential participants and other stakeholders, or in some cases very small pre-pilots to refine recruitment, payment systems etc. Studies which reported on recruitment adopted differing approaches. Attempts to recruit low income respondents via door knocking or by mailing out lengthy enrolment packages proved very challenging. Another study simply selected a random sample of the study population for mandatory participation, with concerning ethical implications. Ethical considerations are also highlighted by the decision to interview respondents and publicise their details in the Kenyan study. The Finnish experience underlines the importance of managing communications with the media to maintain respondent anonymity and avoid influencing responses to the intervention.

A major obstacle for several of the studies has been political resistance to an intervention which many believe will disincentivise labour market participation. This has resulted in the premature termination of the Ontario trial, the foreshortening of research plans in Finland, and considerable difficulty implementing the Dutch pilots. It is likely that a BI trial implemented in almost any developed country will generate considerable controversy. These pilot studies will provide evidence on the effects of an unconditional cash transfer targeted at samples of low income individuals, although in some cases inference will be
hampered by small samples and complicated interventions. However, evidence from most of the studies on the effects of a universal, long term BI will be limited.

**Pilot study sources**

**All studies**


**Finland**


**United States**


**The Netherlands**


Spain


Ontario


Kenya

GiveDirectly (2017) Basic Income https://www.givedirectly.org/basic-income


GiveDirectly Twitter page (2018) Available at: https://twitter.com/Give_Directly
Appendix 2 Search diary

Initial bibliographic database search terms

"basic income"
"citizens basic income"
"citizens income"
"guaranteed annual income"
"income maintenance"
"negative income tax"
"universal basic income"
"universal income guarantee"
"universal allowance"

"guaranteed income"

"income-maintenance programs"

Implement*

Impact Trial
Consequence
Intervention*
Result*
Evaluat*
Experiment*

Web of Science 7.3.17

With phrase above and

Experiment or impact

Planex

"basic income"

"citizens income"

Scopus 28.3.17

"basic income"

AND

Experiment or impact

"basic income"
AND
Trial
All terms above in appropriate combinations

**Copac 5.4.17**
"basic income"

**WorldCat 5.4.17**
"basic income"

**Subsequent bibliographic database search iterations**

**6/11/17**
PubMed
WoS Core collection
IBSS
Ovid Medline

“unconditional cash transfer”

**23/11/17**
PubMed
WoS Core collection
IBSS
Ovid Medline

“Tribal casino”

**1/12/17**
PubMed
WoS Core collection
IBSS
Ovid Medline

“Alaska Permanent Dividend Fund”
### Specialist database searches

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