

DOES MONEY IN ADULTHOOD AFFECT ADULT OUTCOMES?

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This report examines the evidence on whether money in adulthood has a causal impact on wider adult outcomes. Individuals with less income tend to do worse on a range of indicators than those with more, including measures of physical health and subjective well-being. Would more money in itself make a difference? Or are these differences driven by other, associated factors (education, more satisfying work), or by long-term factors too well-established to shift with a boost to income during adulthood?

This report:

- reviews the evidence, focusing on research that investigates whether the relationship between money and adult outcomes is causal
- uses systematic review techniques to reduce bias and maximise the number of relevant studies identified
- considers a range of important aspects of well-being for adults, including subjective well-being and mental health, physical health and health behaviour, quality and stability of relationships, and social and political participation.

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EXECUTIVE SUMMARY

This report examines the evidence on whether money in adulthood has a causal impact on wider adult outcomes. Individuals with less income tend to do worse on a range of indicators than those with more, including measures of physical health and subjective well-being. But would more money in itself make a difference? Or are these differences driven by other, associated factors, such as higher levels of education or more satisfying work, or by long-term factors too well-established to shift with a boost to income during adulthood?

In this review we examine what empirical evidence tells us about whether adults' individual or household financial resources in themselves contribute to other outcomes in adulthood. We consider evidence across a range of outcomes, chosen as capturing a set of central freedoms for adults: happiness and subjective well-being; health behaviour, morbidity and mortality; quality and stability of relationships; social and political participation; and decisions regarding education and employment.

To identify the relevant research, we used systematic search techniques and supplemented these by following up references from selected studies. We included only studies that present evidence that can reasonably be interpreted as causal, by which we mean studies that make use of randomised controlled trials, natural experiments, instrumental variables, or fixed-effect (or similar) approaches on longitudinal data. It is important to note, however, that despite the strength of these types of studies for identifying causal relationships, because we restrict the evidence in this way, and in particular because we focus on resources during adulthood, the report is not able to capture the impact of long-term differences in resources across the life course.

Because of the vast amount of literature on many of our chosen outcomes, we took a sampling approach to the searches, lifting the top 2,000 search results from each database. This means that our review is unlikely to give a comprehensive picture of all the studies published on each outcome. However, our use of systematic review principles provides confidence that the picture is a representative one: there is no reason to expect bias in the results in either direction.

In total, 54 studies were judged to meet our full inclusion criteria. In addition to using one of the listed methods, studies had to use data for OECD or EU countries; include an abstract in English; examine a measure of individual or household (rather than neighbourhood) resources; and be published in or after 1988. Unpublished studies from 2009 onwards were also included. Of the thousands of studies initially identified by our search strategy, most turned out not to be relevant, while many others, although on the right topic, did not use methods that allowed conclusions to be reached regarding causation.

Of the 54 included studies, the majority were from the US, with some evidence from the UK, Germany, Mexico, Canada, Denmark and Sweden. The larger part of the literature was on happiness and mental health, and on physical health and health behaviour. Very few studies used our included methods to look at social and political participation. Most studies looked at changes in income, with nearly one-quarter using lottery wins or inheritance receipts, the same proportion using longitudinal data to look at within-household changes in income and outcomes, and many others examining the impact of changes in social security benefits that affected different households in different ways. A handful of studies looked at the impact of variation in assets, including changes in housing wealth and variation in debt, for example driven by different student loan policies.

Our review found mixed evidence about the impact of resources in adulthood on wider outcomes. For some outcomes the weight of evidence is clear and convincing, but for others the picture is much less straightforward.

On mental health and happiness, the story is clear. Our review finds strong evidence that additional financial resources during adulthood make people happier and reduce mental health problems such as depression and anxiety. This finding emerges from studies looking at a range of different sources of changing resources, including unusual events such as lottery wins as well as increases in social security benefits and variations in wages. There is also some evidence to support the idea that effects are non-linear, with the same proportional increase in income having a greater effect at the bottom of the distribution. Certainly what several of the studies are picking up is the impact increased resources in low-income households can have in reducing anxiety, stress and depression.

On health behaviours and physical health outcomes, the evidence is far more mixed. There is good evidence from studies looking at social security reforms that an increase in resources improves the health behaviour of parents, especially in relation to smoking. However, evidence from studies of lottery wins and inheritance receipts finds the opposite in relation to the general population: more money can lead to less healthy behaviours such as drinking and smoking more. For health outcomes, including obesity, mortality and morbidity, the evidence is also mixed, with several studies finding positive effects of money on mental health outcomes but not picking up significant effects for physical health. In relation to health in particular, it is important to underline the limits of the evidence that met our 'causal methods' criteria. The mechanisms and pathways that link financial resources to health are likely to be complex, multi-faceted and cumulative across the life course, and the studies in this report cannot reflect this complexity, for reasons we return to below.

The evidence in this report suggests that money gives people more choices in a range of areas of life. More income affects decisions about the types of work people do as well as the number of hours they work. We found very little causal evidence on educational outcomes, but of the four studies included, three found that money widens choices about the types of educational institutions and programmes people attended, as well as the likelihood of attending college and graduate school.

Money also appears to affect relationship transitions, with a boost in income linked both to more relationships ending and to the start of new relationships. Rather than finding these results to be contradictory, we could interpret them as signalling that an increase in income results in an increase in choice over relationship status. There were no studies meeting our criteria that looked at money and relationship satisfaction, but two looked

at women's income and domestic abuse, with both finding that increases in women's income reduced the likelihood of abuse.

In considering these findings, it is essential to remember that the decision to restrict the evidence to studies using only certain methods inevitably narrowed the focus of our review. In particular, because we wanted to be confident that changes to income were not driven by other associated factors (education, satisfying employment, underlying personality traits), we were largely restricted to studies that examined marginal income changes, often fairly small in size (such as adjustments to social security benefits) or short-term, one-off windfalls in unusual circumstances (such as lottery wins or bequests). By design, we were unable to examine the effect of long-term and persistent differences in resources between households.

The impact of marginal changes in resources during adulthood is interesting and important, and the effects identified for mental health show us that money in adulthood certainly matters in crucial ways. But the mixed effects for health suggest that changing things late in life is hard, and underlines the importance of investing early in childhood to affect the long-term drivers of health and well-being. This conclusion is supported by the much stronger and more consistent findings from our companion review on money and children's outcomes, particularly in relation to cognitive and social-behavioural outcomes (Cooper and Stewart, 2013). Early intervention is likely to be a more effective way of changing long-term outcomes.

We end by identifying some gaps for future research. There is currently very little research that meets our criteria from the UK or other European countries, with most of our included studies coming from the US. There are also some outcomes for which there is very little evidence at all, including measures of social and political participation, for which we found just three relevant studies. In addition, it would be valuable to have more studies that include longer follow-up after income increases, and also more studies that get directly at decreases in income, as most of the evidence here looks at positive changes. Finally, although some of our included studies are able to distinguish between men's and women's resources, this is another area where more evidence would be useful.

1 INTRODUCTION

It is well-established that people with lower incomes tend to have worse outcomes across a range of different spheres of life. Individuals with higher incomes have better health, live longer, and report higher subjective well-being than individuals with less (see for example Easterlin, 2001; Marmot, 2010; Mackenbach et al, 2008). There are a number of possible explanations for these gradients. While income in adulthood may itself affect health and happiness, both higher income and improved outcomes could be driven by other common factors, such as higher levels of education, underlying personality traits, or the long-term impact of different circumstances in childhood. Reverse causation may also play a part: for example, health problems may limit economic opportunity.

In this review we examine what empirical evidence tells us about whether adults' individual or household financial resources in themselves contribute to other outcomes in adulthood. We consider evidence across a range of outcomes: happiness and subjective well-being; health behaviour, morbidity and mortality; the quality and stability of relationships; social and political participation; and decisions regarding education and employment. These outcomes were chosen as capturing a broad range of central freedoms or capabilities for adults (Sen, 1985; Burchardt and Vizard, 2011). To identify the relevant research we used systematic search techniques and supplemented these by following up references from selected studies and studies suggested by colleagues. We included only studies that present evidence that can reasonably be interpreted as causal, by which we mean studies that make use of randomised controlled trials, natural experiments, instrumental variables, or fixed-effect (or similar) approaches on longitudinal data.

The review is a companion piece to a similar report we conducted on whether household resources in childhood affect outcomes for children. That study found clear and consistent evidence that household income makes a difference to a wide range of children's outcomes, including cognitive development and school attainment, social-behavioural development, and a range of intermediate outcomes including maternal mental health and the home environment (Cooper and Stewart, 2013). Effects were largest for children in households where income was lower to start with.

For adults, on the one hand, the power of income might seem more self-evident than for children: income increases choice and enables adults to do more of what they want to do, so almost by definition we might expect it to improve adults' lives. For children, in principle at least, what matters most may not cost much – more time with a loving and responsive parent. (In practice, the evidence suggests that by reducing stress and anxiety, more money enables parents to provide this time.) On the other hand, pathways

are likely to be more firmly fixed by the time people reach adulthood. It may be too late for income in adulthood to increase opportunity, with career paths long established and health set long ago.

In policy terms, the question of financial resources during childhood has dominated debates, perhaps in part because each child represents a new start and another chance to get things right, and in part because children bear less responsibility for their situation than adults do and therefore more easily garner sympathy and interest from across the political spectrum. But there are important reasons for policy interest in the relevance of income levels during adulthood. For one thing, many adults are or become parents, and their health and happiness affects their children. In addition, knowing how effective individual or household income is in buying health and happiness and other outcomes (and whether this varies across the income distribution) may be informative in debates around wage inequality, taxation levels, the level of social security benefits at the bottom of the distribution, and the balance between public and private spending.

The report is structured as follows. In the next section we present our methods, discussing our approach to searching and the criteria for inclusion of studies. We go on to summarise the spread and focus of the studies we identified, before discussing what they show in relation to each outcome we looked at.

2 RESEARCH QUESTIONS AND METHODS

Research questions

We set out to evaluate existing research examining whether money has a causal impact on adult outcomes. We searched for evidence on a range of adult outcomes in seven broad areas: subjective well-being and mental health; physical health, mortality and health behaviour; the quality and stability of relationships; social and political participation; educational participation and outcomes; employment behaviour; and being either a victim or a perpetrator of crime. These outcomes were chosen as capturing key aspects of well-being, or what Burchardt and Vizard (2011) have called ‘central and valuable freedoms for adults’, although our coverage falls short of their full 10 domains, omitting legal security and identity and respect.¹ In practice, we found no evidence relating to crime that met our criteria. Outcomes related to parenting and the home environment for children were examined in detail in Cooper and Stewart (2013) and to reduce overlap these are not included in this report.

We took a broad definition of financial resources, including studies that looked at income from wages, benefits and other sources (such as lottery wins); and studies that looked at wealth (both assets and debt). We sought evidence that measured financial resources at the individual or household level, excluding studies that focused on the impact of neighbourhood poverty levels, for example.

Methods

While there is a vast literature on the association between money and a range of adult outcomes, very few studies are able to establish whether money has a causal effect on these outcomes. Two main problems arise. The first is the possibility of reverse causation: income may affect health, but health could also affect income, by reducing hours of work or limiting the types of work that are possible. The second problem is that a third factor could determine both a person’s financial resources and their wider outcomes. For example, an association between money and health could be explained by a personality trait such as optimism: more optimistic people

may be both more likely to pursue better jobs and more positive in how they report their health status. Likewise, when looking at an association between money and relationship stability, it may be that emotional intelligence is rewarded in the workplace with promotions and higher earnings, and contributes to making relationships last.

To address these challenges, we restrict our evidence to studies using one of four types of method. Randomised controlled trials (RCTs) are situations where otherwise similar individuals or households receive different levels of resources by deliberate design, with the intention of evaluating the impact of policy. Because the increase in resources is allocated randomly and not driven by any other household characteristics, any changes between those who did and did not receive the increase can be attributed to the resources received rather than to other, unmeasured factors. RCTs are often considered the gold standard in providing causal evidence, but they are rare in research in this field (Sefton et al, 2002). Our second method, the natural experiment, includes studies where a change in resources affects some households but not other, similar ones as an unintended consequence of policy change or other natural variation. For example, welfare benefits might be phased in gradually meaning some receive extra resources for a longer period than others. Lottery wins can also be treated as natural experiments, provided winners are compared with other people who also play the lottery.

The third type of method is known as an 'instrumental variable' approach. These studies identify a variable (the instrument) that is associated with a person's financial resources but not with individual or household characteristics that could themselves affect the outcomes being measured. An example is variation in earnings due to union membership. Questions about causality remain if the instrument used is in fact correlated with other relevant factors: perhaps union members are happier (or less happy) than people who do not join a union. We therefore examined and discussed studies using this approach carefully and excluded those that did not convince us that the instrument was effective in identifying a source of variation in resources unrelated to other relevant characteristics. Any remaining concerns are discussed in the text.

The final set of studies uses longitudinal data and fixed-effect or similar approaches to measure changes in resources and outcomes for particular individuals or households over time. This approach effectively controls for differences between individuals that we expect to be constant over time, such as personality traits or social class. Factors that change over time such as losing a job, becoming ill or the breakdown of a marriage, can usually be controlled for, although if there are important *unobserved* factors that change over time then the possibility that the observed effect is actually a spurious correlation cannot be ruled out. We applied a broad rule that longitudinal studies needed to control for key time-variant variables (such as employment and marital status), but not a blanket rule that particular variables had to be included. We discuss any concerns about included studies as they arise.

It is worth highlighting that our review on money and children's outcomes found that studies using fixed-effect approaches on longitudinal data were less likely to find significant results than studies using other designs, and that where results were significant, effect sizes were considerably smaller than those for other studies (Cooper and Stewart, 2013). A key reason is likely to be a greater risk of measurement error in the financial resources variable compared with other designs (Griliches and Hausman, 1986). Income in household surveys is subject to error at any point in time, both because of misreporting and because income at the time of the survey may not always

represent the household's typical income. Measuring financial resources at multiple time points and calculating the differences between them will therefore be additionally imprecise and may not even identify the direction of the change accurately.

Inclusion criteria

We imposed other inclusion criteria, for both practical and methodological reasons. In order to reduce bias and maintain reliability and transparency, we decided on these criteria at the outset.

- 1** As described above, studies had to use one of the following methods: RCTs, natural experiments, instrumental variables, or fixed-effects or similar approaches on longitudinal data.
- 2** One of the aims of each study, as stated in the abstract, had to be to test the effect of financial resources on one (or more) of our outcomes of interest. This restriction was intended to keep the search strategy manageable while also reducing bias: including studies that happened to identify an income effect while investigating a different relationship could bias results towards the positive.
- 3** The income or financial resources variable had to be measured at the individual or household level: studies focusing on neighbourhood poverty or national, state or regional poverty rates were excluded.
- 4** We restricted the evidence to studies that looked at financial resources during adulthood, therefore excluding studies that measured the long-term impact of resources in childhood on adult outcomes.
- 5** We excluded studies from countries that are not in the EU or OECD. This was to keep studies focused on contexts most relevant to the UK.
- 6** Studies without abstracts or without English-language abstracts were automatically excluded. Studies in a foreign language but with an English abstract were translated if they appeared to meet our other criteria.

Developing search terms

We developed search terms for each of our outcomes of interest. This was an iterative process that involved discussion of potential terms, trialling different search terms in databases and testing them against the relevant literature already identified to ensure terms were specific but also inclusive. As can be seen in Box 1, each search template had three sections: a set of terms for financial resources; a set of terms for method and causal relationship; and a set of outcome terms. The first two sections of the search template were the same for all searches, while the outcome terms differed (the search terms for each outcome are listed in Appendix 1).

Box 1: Example search template for adult searches

Financial resources	AB(wealth* OR assets OR salary OR salaries OR earning* OR wage* OR pension* OR income* OR “socio-economic status” OR “socioeconomic status” OR SES OR poverty OR poor OR depriv* OR disadvantag* OR hardship OR money OR cash* OR expenditure OR spending OR “standard* of living” OR “living standard*” OR “cost of living”)
Method /casual relationship	AND AB(caus* OR effect* OR determin* OR impact* OR influenc* OR associat* OR correlat*)
Outcome e.g. health	AB(health* OR morbidity OR mortality OR ill* OR sick* OR obes* OR overweight OR underweight OR “life expectancy” OR “Lifespan” OR “Medical condition*” OR Death OR Disease* OR “Chronic condition*” OR hospitalisation OR diabetes OR asthma OR anaemia OR cancer OR “cardiovascular disease” OR nutrition* OR “hospital admissions” OR malaise OR cortisol OR arthritis OR “heart attack” OR “quality of life” OR “QOL” OR “Healthcare” OR “Medical care” OR “exercise” OR “physical *activity” OR “fitness” OR smoking OR alcohol OR drugs or “substance abuse” OR “health screening” OR “mental* health*” OR “Mental* ill*” OR “Mental breakdown” OR Stress* OR Anxiety OR Suicide)

Conducting systematic searches

The databases used for the searches were selected with the aim of including literature from a variety of disciplines, such as economics, sociology, psychology, demography and medicine. The final databases selected were based on those already known to be relevant, advice from colleagues who had completed systematic reviews, and consultation with the LSE Library. After testing the search templates in all databases, we excluded those that were not practical for systematic searches, for example if the database did not allow the export of search results. The final databases included were: EconLit, SocIndex, IBSS (International Bibliography of the Social Sciences), British Education Index, PsychInfo and Medline.

Systematic searches were then conducted, using the same overall search template in each database. In order to keep the searches manageable but inclusive, we decided to exclude studies published before 1988 (this was deemed to cover most major research in the field, and indeed preliminary search results showed the majority of relevant studies retrieved were

published after 1990). Because of the very high numbers of results returned we also took the decision to exclude working papers and other unpublished literature dated before 2009, using a filter on the databases where possible, and similarly to filter out dissertations and PhD theses. Working papers dated 2009 onwards were included as they might not yet have had time to be published in journals, but studies that came out in working paper form before that time are not included in our review if they were not subsequently published.

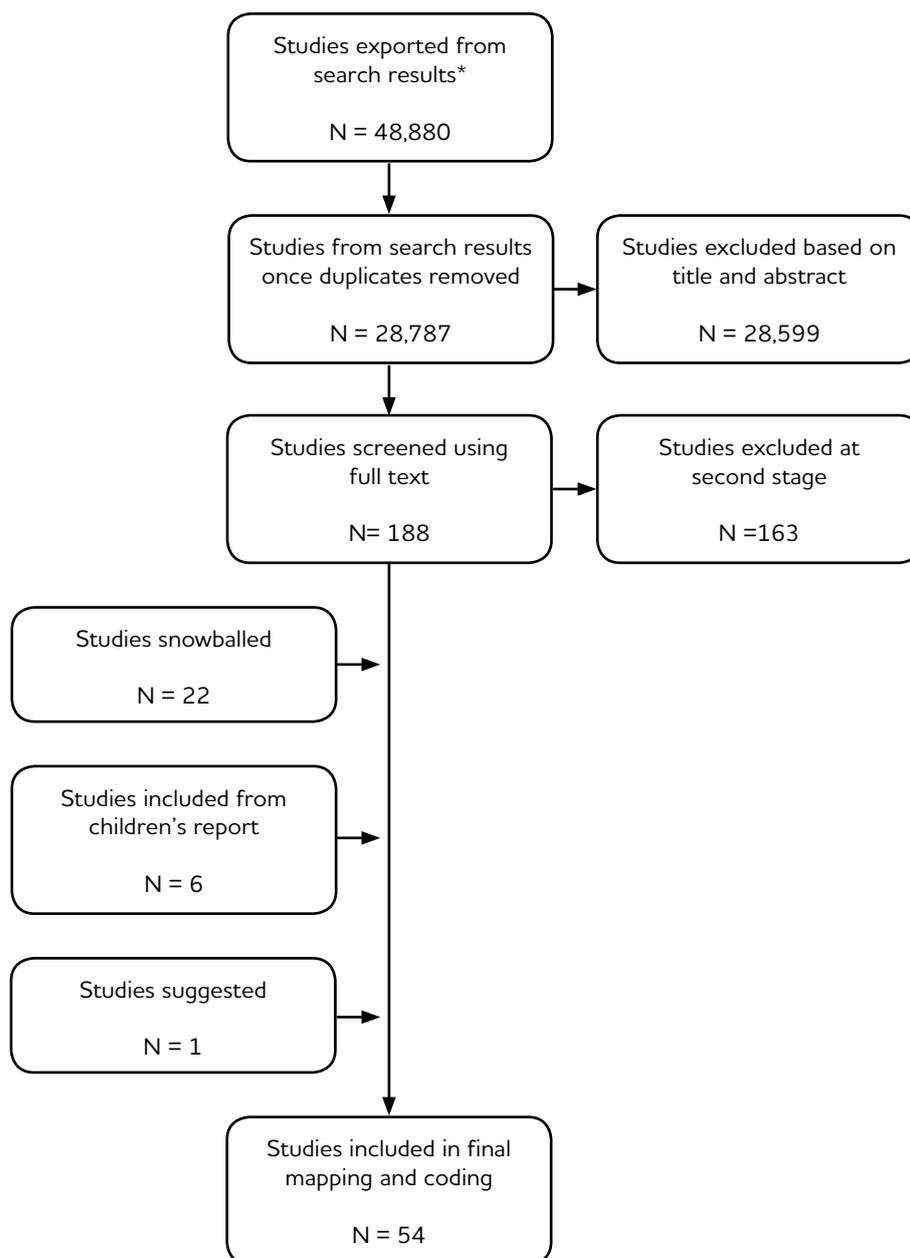
The decision to exclude unpublished literature is an important one. In general, systematic reviews emphasise the importance of including unpublished studies because of the dangers of publication bias: studies that identify significant results are much more likely to be published than those that do not (for example, Dubben and Beck-Bornholdt, 2005). However, we were simply dealing with too many search returns for the study to be manageable without taking this step. We also believe that publication bias is less likely to be a problem in this particular case because the absence of an income effect is a valuable research finding in itself.

A search log (available on request) was kept, recording the details of each search, including any filters used and the number of search results retrieved for each search in each database. Search results were ordered according to relevance; each database has its own algorithm for determining relevance, but in each case it is to do with the number of relevant search terms included in the abstract. We then exported the top 2,000 results from each search (and the top 500 search results from searches for working papers, where it was possible to filter and conduct these separately) and imported them into Endnote where duplicates were automatically removed. This approach is different from that used in our review of children's outcomes, where all search results were imported and all titles and abstracts reviewed by hand (Cooper and Stewart, 2013). Due to the more extensive literature on adults and the constraints of time and resources, we decided that such an inclusive approach was not possible for this review. To compensate, we relied more heavily on following up references cited in studies identified through the searches. Although this has meant that the review is less comprehensive, we believe it to be unbiased. Taking the top 2,000 results can be seen as taking a sample of the full results; a similar approach of sampling the search results has been taken in a previous systematic review by Curran et al (2007). Other than having a higher number of search terms, the studies we exported should not be systematically different in any way from the rest of the results we did not export. Studies followed up from the references of studies identified in the searches should be similarly unbiased.

Screening the search results

There were two stages to screening the search results imported into Endnote. The first stage was based on title and abstract only. If it was clear from the title and abstract that a study did not meet the inclusion criteria, it was excluded. At this stage the number of studies was reduced substantially. As Figure 1 shows, of 28,787 search results, just 188 made it through to the second stage.

Figure 1: Summary of studies excluded at each stage



Source: Adapted from PRISMA 2009 Flow Diagram (see www.prisma-statement.org/statement.htm). Accessed 1 Apr 2014.

Note: Figure 1 takes the top 2,000 published studies and top 500 unpublished studies from search results, ordered by relevance (number of search terms in abstract).

The 188 studies that either clearly met the criteria or required further investigation were imported into a spreadsheet. The full papers were accessed and a decision to include or exclude was made based largely on the methods section. Studies had to use one of the causal methods outlined at the start, to measure financial resources at the individual or household level, and to have a measure of financial resources that was distinguishable from other socioeconomic variables such as education or occupation. Studies that created a socioeconomic status variable using an index measure of income, education and occupation were excluded.

Applying the inclusion criteria based on methodology was not always straightforward, particularly in relation to studies using an instrumental variable approach. We excluded studies that did not convince us that the chosen instrument was both clearly associated with financial resources and independent of other factors that might affect wider outcomes. Potentially interesting studies by Economou and Theodossiou (2011) and Powdthavee (2010) were excluded on these grounds.²

Careful thought and reasoning were also required in relation to the outcome measures. It became clear that it was not enough simply to measure an outcome related to one of our areas of interest; it had to be meaningfully interpretable. This did not necessarily mean we had to be clear about whether an outcome was positive or negative: as discussed in the relationship section, it is often not clear how to interpret indicators of divorce and marriage. But we had to be able to make sense of what the measure meant in relation to well-being. We excluded one study from the health behaviour section that measured changes in the share of expenditure on food overall as we felt unable to interpret the measure without information on the type of food or the level of food insecurity to start with (Attanasio and Lechene, 2010). Similarly, we excluded a study where the outcome was overcrowding in later life, measured using the number of rooms per person (Wolf and Wilmoth, 2010). As 'overcrowding' could decrease if a person's partner died, and increase if a person moved in with family, we felt the measure was too broad to be meaningful.

After the second stage of exclusions, we were left with 25 studies. Details of these studies were entered into the spreadsheet. We included descriptive details of the study, such as dataset used, sample size and method; the measure of financial resources; which outcomes were included, how they were measured and what the results were for each outcome; a summary of overall findings; and any additional notes or concerns about the study's quality. At this stage we also searched for other research referenced in the studies, and if it met the inclusion criteria, added these to the spreadsheet. A further 23 studies were added to the database at this stage (one suggested by a colleague and 22 collected by 'snowballing', meaning following up references from the identified studies). We also included six studies from our report on children's outcomes that covered outcomes for adults in the household (for example, those looking at maternal depression). Five of the six children's studies would have shown up anyway in our searches (the exception was Gregg et al, 2006, which did not include in its abstract any of our listed terms for methods), though of course these five may not have been lifted out in the top 2,000 results. Of the 23 snowballed and suggested studies, 15 would have shown up but were either too far down the 'relevance' list for us to look at them, or in two cases came out in late 2013, after our searches were conducted. Eight would not have shown up, because they did not include the right combination of search terms in their abstract. Four of these (Holtz-Eakin et al, 1993; Gardner and Oswald, 2007; Layard et al, 2008; Van Kippersluis and Galama, 2013) do not include any of the terms from our methods template. One (Kaushal, 2007) examines food stamps but does not include any of our income terms. Three others include 'retirement' (Brown et al, 2010), 'labor earnings' (Imbens et al, 2001) or 'labor supply' (Krueger and Pischke, 1992), none of which features (oddly, in retrospect) in our employment search template.

Aside from highlighting the limitations of some of our search templates, the high proportion of studies that were sourced by snowballing indicates that what we are reviewing in this report is a sample of studies in this field; we do not claim that this is a comprehensive review of all the relevant

literature. Nevertheless, we think there is every reason to believe that the sample gives an unbiased and representative picture. A comparison of results for the searched and snowballed studies shows a strong similarity in the spread of positive, negative and no effects; if anything, the studies that came from the searches are slightly more likely to have found positive money effects than those that were snowballed.

3 AN OVERVIEW OF THE LITERATURE

In this section we map the literature, summarising the evidence found in relation to each of our outcomes of interest, and discussing the spread of studies both by country and by method.

Discussion by outcome

Table 1 gives an overall summary of the literature, showing both how much evidence we found in relation to particular outcomes, and how it falls in terms of the direction of effects. The evidence will be discussed in more detail in the following sections, and we present here only a brief summary. The largest bodies of literature are on physical health and mental well-being. A total of 16 studies look at the relationship between financial resources and indicators of happiness, life satisfaction and mental health. Overwhelmingly, this literature points to positive effects, with strong evidence that more income brings benefits for mental well-being.

The picture for physical health is very different. More than half our studies explore the relationship between financial resources and aspects of health; we split them in Table 1 (and in the discussion below) into studies looking at health behaviour, those looking at body mass index (BMI) or obesity, and those looking at health outcome indicators, which includes both measures of subjective health and objective measures of mortality and morbidity. The evidence about the relationship between resources and health outcomes is very mixed, with a remarkably even spread among studies finding positive money effects, studies finding no effect, and studies suggesting that more money leads to a deterioration in health. The results are similarly mixed for studies looking at money and BMI. Half of the studies on health behaviours find increased resources tend on balance to have a negative effect, unless we focus specifically on the health behaviour of parents, where increased resources have a positive effect. In the section on health we explore the evidence in more detail and try to make sense of these contradictions.

Table 1: Studies by outcome

Nature of outcomes	Studies including outcome	Negative effect	No effect	Positive effects	Mixed results	Depends on interpretation
Happiness and mental health	16		1 (of relative income)	15		
Health behaviours	8	3		4 (parent studies)	1	
BMI/Obesity (including food stamps)	12	4	4	4		
Health outcomes	9	3	2	4		
Relationship stability (new unions and relationship dissolution)	6		1			5
Domestic abuse	2			2		
Social and political participation	3		1	1		1
Employment	9		3			6
Education	4		1	3		
Total	54					

Note: Some studies included more than one outcome. Studies were coded as negative if they found negative effects on any of the outcomes measured, positive if they found positive effects on any of the outcomes measured, no effect if they found no effect on all of the outcomes measured, and mixed if they found a mixture of positive and negative effects depending on the measure used.

We identified six studies examining the relationship between money and relationship status (whether money affects the likelihood of forming or leaving partnerships). One finds no evidence of an effect, and five do find effects but these are not always in the same direction. Our reading of this literature is that, on the whole, money increases choices, and this may or may not lead to partnership formation or dissolution. It also appears to be important whether it is men’s or women’s resources that change. No studies meeting our criteria look at relationship satisfaction; several longitudinal studies showed up in the searches and were considered, but we were concerned that these did not adequately separate the effects of increased income per se from other benefits of paid employment. Two studies look at domestic abuse, both suggesting that if women’s financial resources increase, domestic violence decreases.

That money increases choices is an explanation that may also apply in relation to educational and employment outcomes and pathways, although the studies looking at employment behaviour are a mixed bag, with outcomes (such as decreased labour market participation after a lottery win) hard to classify as either positive or negative. Finally, just two studies look at political participation and two at social participation, and findings are mixed in all cases.

Discussion by country

Table 2 breaks our evidence down by country. The overwhelming majority of studies – more than two-thirds of the total – use data from the US. Evidence for the UK is the next most common, but there are only seven UK studies in all. Three of these look at happiness or mental health, one at mental and physical health, two at different aspects of health behaviour, and one at relationship transitions. A further 10 studies include evidence for six other OECD countries.

Table 2: Studies by country

Country	Number of studies
US	36
UK	5
Germany	4
Mexico	2
Australia	1
Canada	1
Denmark	1
Germany and UK	1
Sweden	1
UK and US	1
US, Germany and EU countries	1
Total	54

Discussion by method

Table 3 summarises the included studies by method. Nearly half the studies make use of either an RCT (four) or a natural experiment (22), including seven lottery studies and six looking at inheritance. Fifteen studies use instrumental variables, while thirteen use fixed-effect or similar approaches on longitudinal data.

Table 3: Studies by method type

Method	Number of studies
RCT	4
Natural experiment	22
of which lotteries	7
of which inheritance	6
Instrumental variable	15
Longitudinal	13
Total	54

Note: The natural experiments include five studies that use inheritances, six studies that use lotteries and one study that uses both lottery wins and inheritances.

There are perhaps three main concerns about the nature of this evidence: the use of lottery studies; the use of inheritance studies; and the broader limitations of studies that mostly focus on short-term changes in money. It is worth saying a few words about each of these at the outset.

Lottery studies have some clear strengths in that the increased income is independent of people's personal characteristics. Lottery players may of course differ from non-players, but the studies we include focus on lottery players only, either by comparing large and small wins across individuals, or by comparing winning versus not winning in the same person. Lottery studies are also useful in that the income gain is truly unexpected, which is especially important when looking at certain outcomes such as relationship status. However, winning a large amount on the lottery is a rather exceptional event, and this poses a challenge to our ability to draw broader conclusions about the impact of money more generally. As Doherty et al (2006) point out, the exceptional circumstances themselves may affect the way that winnings are spent. The thrill and excitement of beating the odds may have an effect, plus there is the possibility that friends and family 'descend on the lottery winner expecting him or her to share this unearned largesse' (Doherty et al, 2006: 446). In addition, if lottery winners are different from the general population, the 'taste for risk' in this group may be unusual and may lead to different reactions to a windfall of money than would be typical (Doherty et al, 2006: 446). For these reasons we have to be cautious about generalising the results from lottery studies to the effect of financial resources in everyday life; winning the lottery may have particular effects that are different from other types of increases in financial resources, such as increased pay or benefits.

There are also concerns about studies that make use of inheritances. Inheritance studies first need to allow for the fact that the loss of a loved one is likely itself to have effects on the mental and physical health of those left behind, as well as on their priorities in life (the realisation, as Brown et al (2010) put it, that one should 'stop and smell the roses'). Included studies seek to allow for this by comparing outcomes for people who inherit larger

and smaller amounts, or by controlling for the death of a relative. Second, it is not clear that people who inherit are representative of the general population. Kim and Ruhm (2012) find in their US study that those who inherit are relatively more highly educated and healthy at baseline, while for the UK, Hills et al (2013) find that those with greater wealth to start with are both significantly more likely to receive an inheritance, and likely to receive a greater amount. This reduces our ability to generalise from inheritance studies to the rest of the distribution. A separate problem is that inheritances may be anticipated even if the exact time of receipt is not clear, and this expectation of inheritance may affect lifestyle choices before the inheritance is even received. This last point dampens our ability to identify inheritance effects, though some studies have information on whether bequests were expected, helping to resolve this problem.

Lottery and inheritance studies make up under a quarter of our total evidence, and there are plenty of studies looking at other sources of income and (in a few cases) assets: changes in social security benefits; changes in college financial assistance policies; variations across US states in eligibility for food stamps; variations in housing wealth. However, this brings us to a broader point about the nature of the overall evidence captured in this report. As discussed, we limited ourselves to particular types of methods in order to be confident that the studies we included were isolating a causal role for income and not picking up the effects of other, associated factors. We also decided to focus on resources during adulthood, rather than the long-term effect of resources during childhood (though see our companion review, Cooper and Stewart (2013) for relevant discussion here). These decisions have effectively restricted the evidence used to studies that make use of particular, externally driven changes in income; sometimes one-off, sometimes short term. The impact of these sorts of changes in financial resources during adulthood is interesting and important, shedding light (for example) on how far and in what respects benefit changes, tax changes or wage increases might be expected to affect outcomes. But it is clear that our evidence does not and cannot give us a comprehensive picture of the relationship between money and wider outcomes. In particular, the studies included here do not tell us about the long-term and cumulative impact of money over the life course, a caveat that is arguably particularly significant when assessing the relationship between money and health. This limitation needs to be kept in mind when reading the findings in this report.

4 DOES MONEY BUY HAPPINESS OR BETTER MENTAL HEALTH?

The question of whether and how far money buys happiness has attracted considerable attention from both psychologists and economists, with a rapid increase in the number of published papers mentioning happiness, life satisfaction or subjective well-being since 1999 (Frey and Stutzer, 2002; Diener and Biswas-Diener, 2002; Stutzer and Frey, 2012). The results from cross-sectional research are clear and consistent across countries: richer individuals have higher self-reported happiness than poorer individuals (Easterlin, 1995, 2001; Blanchflower and Oswald, 2000 for the US; Di Tella et al, 2001 for the EU). Additional resources seem to make less difference to households with more money to start with, not just because an extra £1,000 means proportionally less to a richer household; even a proportional increase in income yields a lower increase in happiness at higher income levels (Diener and Biswas-Diener, 2002; Myers, 2002; Frey and Stutzer, 2002).

However, longitudinal data measured at national aggregate level tells a different story, giving rise to what has become known as the Easterlin paradox: despite the strength of the association between income and well-being in cross-sectional research, overall average happiness in rich countries has not risen as income has risen (Easterlin, 1974, 1995; Blanchflower and Oswald, 2000; Myers, 2000; Di Tella et al, 2001; Frey and Stutzer, 2002; Easterlin et al, 2010). In the short run, there is some evidence that economic contractions and recoveries are reflected in corresponding movements in subjective well-being, but this relationship does not seem to hold over the long term (Easterlin et al, 2010).

A plausible explanation of this apparent paradox is that, once basic needs are met, what matters to well-being is not the absolute level of income itself, but how this compares to individuals' expectations, formed by reference both to the income of relevant others (neighbours, friends, peers) and to one's own past income or expected income trajectory. The 'relative income hypothesis' was proposed by James Duesenberry (1949) and has been variously formulated since then as 'social comparison theory', 'aspiration level theory' and 'hedonic adaptation' (Frey and Stutzer, 2002). As Easterlin has argued (1974, 1995, 2001), a central role for relative income would make sense of the fact that, within any given country, people with a higher income are on average happier than people with lower income, but that raising everyone's income does not raise everyone's happiness.

This still begs the question of whether relative income is really one of the causes of better well-being. The association could reflect other confounding

factors (more pleasant and interesting work, for example), and/or there could be a reverse causal relationship at play, with happier people becoming higher earners. (For example, Diener et al (2002) find that 'cheerfulness' at the time of college entry is correlated with income measured 19 years later; see also Oswald et al, 2009.) Despite the huge literature on happiness, studies that make use of our included methods – experimental situations, instrumental approaches or longitudinal individual or household data – are relatively rare. Furthermore, as Gardner and Oswald (2007) point out, there are particular concerns about the ability of longitudinal data to identify causal relationships in this field, as there may plausibly be omitted variables that also move over time – such as status or seniority in the workplace – that drive changes in happiness (Marmot, 2004; Nettle, 2005). This makes natural experiments and instrumental approaches especially important. (An established U-shape association between age and happiness may complicate interpretation of longitudinal studies further, but age is usually observed and can be controlled for.)

We include in this section all the studies we identified that look at aspects of subjective well-being. This includes a range of different indicators. A handful of studies used single questions asking people to provide a general assessment of their well-being, either their happiness or their life satisfaction, but most used indices based on answers to a series of questions regarding sleeplessness, anxiety, self-efficacy and depression, such as the 12-item General Health Questionnaire (GHQ-12) or the maternal depression scale developed by the Centre for Epidemiological Studies (CES). This raises a question about whether all the studies are picking up the same underlying constructs: is 'life satisfaction' the same thing as 'happiness', and do either equate to the absence of stress and anxiety? Psychologists have argued that the broad construct of subjective well-being encompasses many components, including cognitive assessments such as life satisfaction and more emotional assessments such as happiness; these are likely to be positively associated but not synonymous (Diener and Biswas-Diener, 2008). Further, while the absence of depression and other mental health difficulties is associated with these broader assessments of well-being, depression and happiness are not different ends of a single spectrum. For instance, it is hard to have high, positive well-being when one is depressed, but the absence of depression is not enough to guarantee high well-being. For these reasons, we try to be clear in discussion about the measure being used, and these are summarised in Table 4. In practice, though, the evidence points consistently to a causal role for income across all types of measures.³

Table 4: Studies examining financial resources and subjective well-being

Study	Approach	Country	Source of variation in resources	Measure of subjective well-being		Mental health
				Life satisfaction	Happiness	
Gennetian and Miller (2002)	RCT	US	Social security (welfare to work programme)			Maternal depression (Centre for Epidemiological Studies scale)
Gardner and Oswald (2007)	Natural experiment	UK	Lottery wins	Life satisfaction		General Health Questionnaire (GHQ) standard mental well-being score
Apouey and Clark (2013)	Natural experiment	UK	Lottery wins			GHQ standard mental well-being score
Kim and Ruhm (2012)	Natural experiment	US	Inheritance			Depression (CES scale) [See Note to table]
Evans and Garthwaite (2010)	Natural experiment	US	Social security (EITC)			'Bad mental health' days (self-reported)
Pischke (2011)	Instrument	US, Germany, Europe	Industry wage differentials	Life satisfaction	Happiness	
Gathergood (2012)	Instrument	UK	Local changes in housing prices			Psychological health (GHQ, anxiety, depression)
Milligan and Stabile (2011)	Instrument	Canada	Social security (child benefit)			Maternal depression
Frijters et al (2004)	Longitudinal	Germany	Various (linked to reunification)	Life satisfaction		
Dearing et al (2004)	Longitudinal	US	Various			Maternal depression (20-item checklist about last week)
Heflin and Iceland (2009)	Longitudinal	US	Various			Depression (major depressive disorder, based on WHO CDI)
Layard et al (2008)	Longitudinal	UK, Germany	Various	Life satisfaction		

Note: Kim and Ruhm (2012) find inheritances have a significant effect on depression for men but not for the sample overall. All the other studies listed find a significant effect of financial resources on subjective well-being.

Evidence from experiments and quasi-experiments

Five of the studies we identified use situations that could be classified as experimental or quasi-experimental, in which some groups receive an increase in income for reasons that can be considered beyond their control. We discuss here evidence from a single RCT of a welfare-to-work programme in the US; two studies making use of lottery wins; one using inheritance; and one exploiting changes in payments to some family types under the Earned Income Tax Credit in the US. All five studies point to a causal role for money in improving mental well-being, although in one case the effect shows up only for men, not overall.

Gennetian and Miller (2002) examine the Minnesota Family Investment Program (MFIP) in the US, a programme for lone mother families in the mid-1990s that randomly assigned participants to three research groups. The control group continued to receive Aid to Families with Dependent Children, which falls steeply as earnings rise; the second group received financial incentives that allowed them to keep more of their welfare payments as earnings increased; and the third received the same financial incentives but also faced a mandatory requirement to participate in work and training. The financial incentives were found to reduce maternal depression, measured on the 20-item CES depression scale. Adding the mandatory participation requirement made no additional difference. Effects were large: the average annual income rise of \$1,078 appeared to lead to a fall in clinical depression of 8.4 percentage points (18% of a standard deviation).

Two studies examine the impact of a win on the UK National Lottery, both using the British Household Panel Survey. Gardner and Oswald (2007) focus on wins of between £1,000 and £120,000 (1998 pounds sterling) between 1996 and 2003 using the standard mental well-being measure of the GHQ score, which amalgamates responses to 12 questions regarding strain, loss of sleep, self-confidence, happiness and depression. Because there is no way to identify those who entered the lottery but won nothing (and people who play may differ from those who do not), wins of over £1,000 are compared with smaller wins. The authors find a rise in mental stress in the year of winning (0.5 GHQ points on a 36-point scale), but a decrease two years later of approximately 1.4 points relative to those who won nothing or only a small amount. This compares to an effect of widowhood of approximately five GHQ points. If anything, higher income households experience a sharper drop in GHQ than lower income households (though the difference is not clearly significant). Results also hold for a general question on life satisfaction, but the data were too noisy to permit particularly well-defined results. As the authors point out, the sample size is small: only 137 people receive a win of over £1,000.

Apouey and Clark (2009) use the same data to look at lottery wins, but they use 12 waves of data (compared with two in Gardner and Oswald (2007)), include more individual control variables and make use of fixed-effect approaches. The longer panel improves the sample size, with 11,229 wins observed, although only 6% (674 wins) are more than £500, but, like Gardner and Oswald, the paper only estimates effects up to two years after the win. Consistent with Gardner and Oswald, Apouey and Clark find that positive income shocks lead to better mental health, and bigger wins have a significant effect compared with small wins. The impact of lottery wins on the GHQ score seems to be the same for low- and high-income households, but is greater for men than for women.

Evans and Garthwaite (2010) exploit the fact that in the early 1990s payments through the Earned Income Tax Credit (EITC) in the US increased

by more for households with two or more children than for those with just one, with the increase amounting to between \$800 and \$1,327, or as much as 15% of family income. The authors use a 'difference-in-difference' approach to compare changes over time for mothers in these two types of household, focusing on mothers with at most high school education, to capture those likely to be eligible for the EITC. They find that women with two or more children had a 1.4% decrease in probability of reporting a 'bad mental health day' in the month before the interview, and a 7.5% reduction in the number of such days, compared with women with only one child.

Finally, Kim and Ruhm (2012) look at the impact of inheritances among adults aged 51 and over, examining the US Health and Retirement Survey 1992–2006, and comparing health outcomes for those who report receiving a large inheritance (more than \$10,000) since the survey began with those receiving a smaller one; a strategy aimed at controlling for unobserved differences, including the wider impact of the loss of a loved one. They further control for a range of health measures at the start of the period as well as for a number of other observable characteristics. Among many other indicators (discussed in the health section, below) they look at the eight-item CES depression scales, categorising those scoring three or more as depressed. The association between depression and receipt of a large bequest at some time during the last two to 14 years is not significant in the population as a whole, but depression is significantly lower for men who have received such a bequest.

Evidence using instrumental approaches and other exogenous change

We now turn to look at three studies that, without the advantage of an experimental or quasi-experimental situation, identify differences in household financial resources that are beyond household control.

Pischke (2011) uses industry wage differentials as an instrument for family income in a study on three sources of data: the US General Social Survey, the European Social Survey and the German Socio-Economic Panel. The justification is that these differentials reflect rents, rather than unobserved differences between workers in unobserved skills or other characteristics. Controlling for occupation, Pischke finds that workers in high-wage industries are happier and have higher life satisfaction than those in low-wage industries. Acknowledging both that some sorting effects may still remain (in a later paper Pischke and Schwandt (2012) find that industry differentials correlate with mothers' education), and also that jobs differ in other attributes that affect happiness, not just in income, Pischke tests his findings in a series of ways. First, he contrasts results for job satisfaction and life satisfaction, and finds that there is no strong relationship between industry and job satisfaction, only between industry and life satisfaction, suggesting that any industry effect is working through income. Second, he uses individual, fixed-effect methods on workers who switch industries; and third, he examines the happiness of wives using their husband's industry as the instrument. Each of the approaches supports the hypothesis that it is income that explains the correlation between industry affiliation and life satisfaction.

Milligan and Stabile (2011) use data on child benefit levels in Canada, exploiting variation in benefits across provinces and over time to ask whether income affects maternal depression, measured using 12 questions about feelings and behaviour over the past week. They find strong positive

income effects: US \$1,000 is found to bring maternal depression down by 10% of a standard deviation, or by 20% of a standard deviation if the sample is restricted to the low educated.

Gathergood (2012) takes a rather different approach, examining the relationship between housing wealth and psychological health. This is one of the few studies that looks at assets rather than income, and also focuses explicitly on people who are in financial difficulty, rather than on the full distribution. Using UK panel data from 18 waves of the BHPS, and local house price movements as an instrument for housing wealth, Gathergood examines the relationship between 'problem mortgage debt' and psychological health, measured using the GHQ and a list of mental health problems (including anxiety, depression, bad nerves and psychiatric problems). He finds that mortgage holders who enter into arrears on their mortgage debt in localities where house prices are growing (so their home equity 'buffer' is increasing) suffer less deterioration in psychological health, using either measure, than individuals who enter arrears where house prices are falling.

Longitudinal data

Finally, we examine four studies using fixed-effect approaches on longitudinal data, remaining aware that, on the one hand, these carry with them the danger that there may be relevant unobservable factors also changing over time, and on the other that measurement error may bias coefficients downwards.

The first study, Frijters et al (2004), looks at longitudinal data for East Germany post-reunification to examine the impact on life satisfaction of the substantial increases in real household income that resulted. The authors argue that this is an exogenous shock to income, but the regressions include dummies for each year, taking any aggregate income growth out of the picture. (These time dummies pick up a clear improvement in aggregate life satisfaction over the decade, peaking in 1999, but we cannot be sure how much, if any, of this change is explained by income rises rather than simultaneous increases in civil and political liberties.) As Pischke (2011) points out, with this aggregate improvement controlled for, there is little reason to think that individual income changes are any more exogenous for East Germans than for anyone else, so we include the study here rather than in either of the earlier sub-sections. Controlling for the aggregate income shift, the authors find that a one-unit increase in log household income leads to around a 0.5 standard deviation increase in life satisfaction for both men and women, with the greatest effects in the immediate post-reunification years.⁴ According to their calculations, around 35–40% of the increase in life satisfaction in East Germany after reunification was due to the large increase in real household income, with income changes explaining a bigger share of increased life satisfaction for older households.

Two studies use longitudinal US data to look at maternal depression. Dearing et al (2004) use the NICHD Study of Early Child Care and Youth Development, which collected data in six waves between one month and 4.5 years after birth. They find that income gains resulted in the alleviation of symptoms of maternal depression (measured using a 20-item checklist on the presence and frequency of symptoms over the previous week). The authors calculate that a \$10,000 change in income results in a 0.14 decrease in depressive symptoms (4.7% of a standard deviation) – but the

impact was 1.48 times greater if the change in income took a household across the poverty threshold.

Heflin and Iceland (2009) examine the effect of material hardship on depression, using two waves of data from the longitudinal Fragile Families Survey (telephone follow-ups one and three years after birth). The depression measure is an indicator of major depressive disorder, based on the World Health Organisation's Composite International Diagnostic Interview. Heflin and Iceland find that women who reported an increase in problems paying bills and women who had their phones disconnected because of arrears were respectively 1.8 times and 1.5 times as likely to be depressed as women who reported no change in these hardships. Changes in other hardship measures (unstable housing, receipt of free food and lack of medical care) were associated with depression in cross-sectional results but not in the fixed-effect models.

Finally, Layard et al (2008) examine four cross-sectional and two panel datasets (the BHPS for the UK and the GSEOP for Germany) to explore the extent to which the marginal effect of income on life satisfaction falls as income increases, focusing on people aged 30–55. They use fixed effects in their analysis of the panel data, and find income effects that are similar though smaller in size to those in their cross-sectional regressions. Their analysis of marginal effects leads them to conclude that the marginal utility of a proportional increase in income falls as income rises; that is, to get the same boost in life satisfaction as an extra £1,000 would give to someone on £10,000 to start off with, someone starting on £100,000 would need more than £10,000 extra (their calculations suggest the richer person would need around 25% more, or £12,500). Their results are strikingly consistent across countries and between cross-sectional and panel datasets.

Studies focusing specifically on the role of relative income

All the studies discussed so far examine changes in individual or household income over time. In addition, we identified four studies that focus explicitly on the question of relative income, examining whether and how well-being is affected by changes in the income of neighbours or peers (Table 5).

Table 5: Studies examining relative financial resources and subjective well-being

Study	Country	Reference group	Measure of well-being		Result
			Happiness	Mental health	
Ferrer-i-Carbonell (2005)	Germany	People of similar age and education in same region	Happiness		Relative income matters: the larger an individual's income in relation to the reference group, the happier they were found to be. Upward comparisons were strongest.
Luttmer (2005)	US	Local, based on local data on industry and occupation and national earnings data	Happiness		Relative income matters: having neighbours with higher earnings was associated with lower levels of happiness, controlling for individual's own income.
Bechtel et al (2012)	Australia	Others in the survey from the same neighbourhood and with higher income (upward comparison only)		Mental health (Short Form 36 index)	Relative deprivation had a very weak effect: small and significant in some specifications only.
Blanco-Perez (2012)	Germany	People with the same occupation in the same region (age and education also tried with no effect)		Mental health (Short Form 12 index)	There were no significant effects of relative income.

Ferrer-i-Carbonell (2005) examines the impact on happiness of own income in comparison to that of a reference group, using German panel data (the GSEOP 1992–97). The reference group is assumed to be people of a similar age and education level living in the same region. The larger an individual's own income is in comparison with that of the reference group, the happier they are found to be. For West Germans and for the full German sample, effects are asymmetric, with comparisons mostly upwards: poorer individuals' happiness is negatively influenced by the fact that their income is lower than the reference group, while richer individuals do not get happier from having above-average income. This is as predicted by Duesenberry (1949), who hypothesised that people largely compare themselves upwards rather than downwards.

Luttmer (2005) uses two waves of US panel data (1987–1988 and 1992–1994), matching in data on local earnings estimated from national data on industry and occupational earnings and local data on industry and occupation mix. He experiments with controlling for area house prices, and also uses individual fixed effects to check that the explanation is not simply that (for some reason) happier people are being selected into lower income neighbourhoods. Luttmer finds that, controlling for individuals' own income, having neighbours with higher earnings is associated with lower levels of self-reported happiness. An increase in neighbours' earnings has an effect on happiness similar in size to a decrease in one's own income. Effects are strongest for those who report socialising more with neighbours (but no stronger for those who socialise a lot with relatives or those outside the neighbourhood). However, neighbours' earnings have no significant impact on the Radloff depression scale (reflecting things like loneliness and sleep deprivation). In investigating the mechanism, Luttmer considers satisfaction with material possessions but finds no evidence of an effect; he suggests that instead it may be that when neighbours' income rises, people spend less time on leisure and friendships in order to 'keep up with the Joneses'.

Bechtel et al (2012) use the Australian Household, Income and Labour Dynamics panel survey (HILDA) to examine the impact of relative income and income inequality on mental health. Fixed-effects techniques are used on eight waves of data, from 2001 to 2008. Mental health is measured using the mental health component of the Short Form 36 (SF-36), covering areas regarding being 'nervous, down in the dumps, peaceful, sad and happy'. Relative deprivation is measured by the average difference between the respondent's income and that of others in the survey who are within their own neighbourhood and have a higher income (so an upward comparison only). This is found to have a very small significant effect in some specifications only, while inequality within the neighbourhood or within the city or state is not found to be significant at all. However, there are only between 10 and 55 people in each neighbourhood in the dataset, so the relative deprivation measure may not be well-identified.

Finally, Blanco-Perez (2012) looks at both relative deprivation (upward comparisons) and relative satisfaction (downward comparisons) using fixed-effect methods on five waves of panel data from the German Socio-Economic Panel 2002–2010. After trying a number of alternatives, she settles on a combined occupational/geographical reference group (people working in the individual's occupation, in their area of Germany). Her results for health (discussed below) are unexpected and counter-intuitive, and may plausibly reflect a poorly identified reference group, but for mental health she finds no significant effects for either relative deprivation or relative satisfaction.

Summary

In sum, there is very strong evidence that money has a causal impact on subjective well-being, measured using either general indicators of happiness and life satisfaction, or indicators of poor mental health (measures of depression, anxiety and stress). Four out of five studies using experimental or quasi-experimental methods, covering the UK and the US, found clear evidence of positive income effects, with the fifth, Kim and Ruhm's (2012) study of inheritance in the US, finding a positive effect on depression for men but not overall or for women. All three studies using instrumental variable approaches point to a positive role for income, with evidence for Canada, Germany, the US and the UK. And all four longitudinal studies point to positive effects of income on life satisfaction or depression.

The consistency of this story is striking, given not only the range of measures and methods used, but also the different sources of changing resources (summarised in Table 4). Winning the lottery seems to improve one's well-being, but this is a rather exceptional circumstance that might leave us wary of generalising to other income gains. But we also see the positive effects on maternal mental health of social security benefits, including increases in in-work support and child benefits; differences in life satisfaction that appear to be driven by industry wage differentials; and improvements in psychological health linked to rises in housing wealth. The effects identified in the longitudinal studies reflect the combination of reasons that household income changes over time, though these studies also leave unanswered questions about the role of possible unobserved factors.

The story in regard to the relationship between relative income and subjective well-being is less clear (and there are only four studies in total). Both studies looking at relative income and happiness found that happiness is greater where one's own income is higher relative to that of a reference group, but studies looking at mental health or depression found little or no evidence of an income effect. One hypothesis is that the difference in results is driven by the different aspects of subjective well-being being measured, but the number of studies is really too small to draw such conclusions, especially as identifying the right comparator group within the confines of household survey data is difficult: some studies may have been more effective in doing this than others.

As noted at the outset of this section, cross-sectional evidence on the relationship between income and happiness has pointed to a clear, non-linear relationship – the same proportional change in income appears to have a bigger effect on lower income households. Among our included studies, Layard et al (2008) is the only one to focus on this issue explicitly, concluding that the marginal effect of a proportional change in income on life satisfaction indeed falls as income rises (and, interestingly, finding very similar results for the cross-sectional and panel datasets examined). The evidence on income and maternal depression is also suggestive that money has more impact at the bottom of the distribution, although these studies look at the effect of an income change in dollar rather than proportional terms: Milligan and Stabile (2011) find that a given income change has twice the effect among low-educated mothers than among the full sample, while Dearing et al (2004) find the same income gain has an effect 50% larger if it lifts a family over the poverty line (defined using income-to-needs ratios against the US poverty line). The other studies of maternal mental health, all of which find positive income effects, examine lower income households only: Gennetian and Miller (2002), Heflin and Iceland (2009) and Evans and Garthwaite (2010).

The two lottery studies, however, look for evidence of non-linearity but do not find it; indeed, if anything, Gardner and Oswald find a sharper drop in GHQ scores for lottery winners from high-income than low-income households. This may reflect the unusual nature of lottery wins.

5 DOES MONEY CHANGE HEALTH BEHAVIOUR OR HEALTH OUTCOMES?

There is a clear income–health gradient in the UK and other developed countries: people with higher incomes have longer life expectancies and enjoy better health across their lifetime (Marmot, 2010; Mackenbach et al, 2008). Explaining this relationship is difficult as so many factors affect health, among them childhood circumstances; health behaviours such as drinking, smoking and diet, which are themselves associated with socioeconomic status; and access to health services (McGinnis et al, 2002; ONS, 2013; Department of Health, 1999).

In their theoretical review on the impact of money on health, Benzeval et al (2014) distinguish three main pathways: material (the ability to afford better diet, housing and perhaps health care), psychosocial (stress-related factors) and behavioural (the effect on smoking, drinking, exercise and so on). These pathways suggest that both absolute and relative resources are likely to be important in affecting health, with absolute resources perhaps mattering more in relation to material pathways and relative resources to psychosocial (see Wilkinson and Pickett, 2010 for example). At the same time, Benzeval et al underline the importance of a long-term approach; many aspects of health inequality start in childhood and widen over the life course (see for example Ben-Shlomo and Kuh, 2002). The methods we include in our review only really allow us to get at quite short-term shifts in income, and it might be implausible to expect these to undo the effects of differences in health that have accumulated over a long period of time. This may explain why the evidence described in this section is so mixed.

We divide our health studies into three categories: studies that test for an effect of financial resources on health behaviours such as smoking, drinking and exercise; studies that test the effect of resources on obesity and BMI, including a batch of studies focused on food stamps in the US; and studies that test the effect of money on health outcomes, such as morbidity measures and life expectancy. Evidence for each type of outcome is discussed in turn, followed by an overall summary of the evidence on money and health.

Health behaviours

It is difficult to predict the impact of money on health behaviours. On the one hand, having more money enables greater consumption of unhealthy goods such as cigarettes and alcohol (in fact we already know that those with higher incomes drink more alcohol (ONS, 2013)); on the other hand, if these behaviours are related to stress, and stress is linked to low income, more resources may make a positive difference. We found eight studies that test the effect of financial resources on one or more health behaviours, including smoking, drinking and exercise. Two studies are from the UK, five from the US and one used data from both countries.

Seven of the eight health behaviour studies used natural experiments (Table 6). The first two also look at measures of mental health and were discussed in Section 4. Apouey and Clark (2013) test the effect of income on smoking and alcohol consumption in the British Household Panel Survey (BHPS), using lottery wins as a measure of exogenous income variation. Because it is not possible to distinguish lottery players from non-lottery players in the data, the authors compare the effects of winning and not winning for the same individuals (using fixed-effect models), as well as comparing those who won large amounts with those who won less. They find that increased income from lottery wins increases the number of cigarettes smoked, but that it does not significantly increase the probability of smoking. They also find winning the lottery increases the probability of frequent social drinking (although this measure is fairly crude, capturing only the frequency of drinking in pubs or clubs, with 'once a week' the highest – and most common – category). They find the impact of winning the lottery is the same for high- and low-income individuals, and that the effect on the number of cigarettes smoked is greater for men than for women.

Kim and Ruhm (2012) use inheritances to measure the effect of changes in wealth on smoking, drinking and exercise, in the US Health and Retirement Survey (HRS). The authors find no effect on smoking or exercise (although the latter was measured as vigorous exercise at least three times a week). Receiving an inheritance did increase the probability of drinking alcohol and the amount of alcohol consumed, but the increase was for light (1–7 drinks a week) and moderate (1–14 drinks a week) drinking, and there was no significant increase in heavy drinking (more than 14 drinks a week). The increase in light consumption of alcohol was found to be greater for women than for men.

Table 6: Studies examining financial resources and health behaviour

Study	Country	Method	Negative effect	No Effect	Positive effect
Apouey and Clark (2013)	UK	Natural experiment – lottery wins	Increased number of cigarettes smoked Increased alcohol consumption	Probability of smoking	
Van Kippersluis and Galama (2013)	UK	Natural experiment – lottery wins	Increased alcohol consumption	Probability of smoking Number of cigarettes smoked	
	US	Natural experiment – inheritance	Increased prevalence of smoking Increased number of cigarettes smoked Increased alcohol consumption	Exercise	
Kim and Ruhm (2012)	US	Natural experiment – inheritance	Increased alcohol consumption	Smoking Exercise	
Gregg et al (2006)	UK	Natural experiment – welfare reform affecting parents			Reduced expenditure on cigarettes and alcohol Increased expenditure on fruit and vegetables
Strully et al (2010)	US	Natural experiment – variation in EITC payments affecting parents			Reduced smoking during pregnancy
Averett and Wang (2013)	US	Natural experiment – variation in EITC payments affecting parents			Reduced maternal smoking (for low-educated white mothers)
Cowan and Tefft (2012)	US	Natural experiment – variation in EITC payments affecting parents			Reduced maternal smoking (unmarried mothers with less than a college degree)
Kenkel et al (2013)	US	Exogenous income variation – uses changes in EITC payments and differences between states	Increased probability of smoking Increased number of cigarettes smoked		

Van Kippersluis and Galama (2013) use the same data as Apouey and Clark (2013) for the UK (lottery wins in the BHPS) and the same as Kim and Ruhm (2012) for the US (inheritances in the HRS), but in both cases reach slightly different conclusions. For the UK, the authors support Apouey and Clark's finding that increased income from lottery wins increases the probability of drinking out, but find no significant effects for either smoking or the number of cigarettes smoked. The authors suggest that the difference here is due to their fixed-effect model, which controls for unobserved differences (that do not change over time) between individuals who win larger and smaller amounts.⁵ They explain that Apouey and Clark's OLS results for smoking do not control for unobserved differences between those that win larger amounts and those that win smaller amounts. (When they replicate Apouey and Clark's analysis they confirm their results but find 'substantial differences between the winners of small and large lottery amounts'; this effect disappears when they use a fixed-effect model). The authors do not suggest an explanation for the difference between those who win large and small amounts. Van Kippersluis and Galama's (2013) models also suggest that the probability of engaging in sports at least once a month increases with a lottery win (although the effect size is small). They further find that the effects on drinking out and engaging in sports are greater for those aged over 50.

In contrast to Kim and Ruhm's (2012) results, Van Kippersluis and Galama find that wealth from inheritance increases the prevalence of smoking and the number of cigarettes smoked (although this seems to be due to previous smokers not quitting, similar to previous findings). Again, they explain the difference by their use of fixed-effect models; they find substantial differences in the characteristics of those that inherit an amount above and below US\$10,000 (Kim and Ruhm's cut-off point) and argue that these differences are likely to have affected Kim and Ruhm's results. Similarly to Kim and Ruhm, the authors find that inheritances increased the prevalence of drinking alcohol, although this time there was no significant effect on the number of drinks consumed each week. Also, as with Kim and Ruhm, Van Kippersluis and Galama find no significant effect of inheritance on light physical activity. The effect of inheritances on drinking and smoking are found to be larger for the least wealthy.

Gregg et al (2006) exploit changes in UK welfare reforms since 1998 that favoured low-income families over higher income families, and favoured families with children aged under 11 over families with older children. The authors analyse expenditure patterns before and after the reforms and find increased income was associated with significantly reduced spending on alcohol and cigarettes as well as an increase in expenditure on fruit and vegetables. This appears to be in contradiction with other evidence that suggests more money leads to a rise in cigarettes smoked, but Gregg et al's focus is on the spending of families; parents may respond differently to increased financial resources than people without children. It may also be that an increase in regular income through the social security system has a different effect on adults in low-income households than income received as a one-off windfall through a lottery win or bequest.

Our last four studies all make use, in different ways, of variation in Earned Income Tax Credit (EITC) in the US. Three focus specifically on the health behaviours of mothers, exploiting ways in which particular groups were treated differently at different times and in different states. Strully et al (2010) examine the effects of prenatal poverty on smoking during pregnancy and infant health, comparing women in states with an EITC programme with similar women (unmarried mothers with a high school

degree) in states without. The authors find that living in a state with EITC available reduced the odds of smoking during pregnancy by about 5%. (Reduced maternal smoking was also found to account for part of a decrease in low birthweight.) Averett and Wang (2013) and Cowan and Tefft (2012) exploit the fact that in the early 1990s, changes in EITC payments were made significantly more generous to families with two or more children than families with one child. Both find higher EITC payments reduce maternal smoking, although Averett and Wang find this is only for low-educated white women and Cowan and Tefft find the effect is significant for both black and white women, but the greatest impact is on single mothers with some college experience (although no college degree). All three studies face the same two difficulties. First, they estimate the income effects by identifying those likely to be eligible for EITC payments (focusing on women with low education) but are not able to identify actual receipt of EITC and therefore may underestimate the effects. Second (and more significantly), as EITC also increases employment it is difficult to separate out employment effects from income effects. These three studies present their results as the joint effect and acknowledge that employment may also decrease smoking, for example through a reduction in free time or restrictions on smoking in the workplace. This second issue means these results may overestimate the effect of income.

The final study on health behaviours, by Kenkel et al (2013), also uses the EITC to look at smoking, but in this case uses maximum benefit levels in the state in that year as an instrument for income (rather than comparing the eligibility of specific groups). In contrast to the other EITC studies, Kenkel et al found that smoking was higher where EITC payments were more generous: having more money increased the probability of smoking as well as the number of cigarettes smoked. As they point out, this study looks at all low-income individuals and not just mothers, which is one possible explanation of why the findings are so different from those of the other three EITC studies.

Overall, the evidence on health behaviours is very mixed. Around half the studies find that an increase in financial resources leads to a worsening of some health behaviours, with increases observed in the number of cigarettes smoked and the probability of drinking. The other half find that increases in resources lead to improvements in health behaviours, in particular reductions in maternal smoking.

There are two plausible explanations for these contrasting findings. First, all the studies that find improvements in health behaviours focus on parents; it may be that parents respond differently to increased financial resources than people without children. Second, all but one of the studies that find negative effects on health behaviours make use of lottery wins and inheritances; these are unique routes to increased resources (as discussed in Section 3 above); receiving such unexpected windfalls may influence how the additional money is spent. All the studies that find positive health behaviour effects focus on government transfers, which may be considered more permanent and stable changes in income.

Intermediate health outcomes: body mass index and obesity

We now turn to summarise evidence from studies that look at body mass index (BMI), calculated by dividing a person's weight in kilograms by their height in metres squared. A person with a BMI above 25 is classified as

overweight, and above 30 as obese; both outcomes are strongly predictive of other health problems including heart disease, stroke and diabetes. Having a BMI below 18.5 also carries health risks but in richer countries this is much less common than obesity. Just one of our studies (focused on pregnant women) looks at the impact of income on being underweight as well as overweight.

A priori, the direction of the relationship between financial resources and obesity is difficult to predict. In the UK, there is an association between income deprivation and the risk of being overweight or obese for women, but the same is not true for men (HSIC, 2012). In theory, if low-income households are choosing food that has a high calorie–price ratio because of budget constraints, an increase in income might enable them to afford foods that are lower in calories but higher in nutrients, such as fresh fruit and vegetables. Food hardship has also been found to predict binge eating, which is itself related to obesity; a more secure income could lead to more stable consumption patterns (Townsend et al, 2001). On the other hand, across the income distribution, an increase in income could simply lead to higher food intake overall without changes in diet. (The impact of income on exercise could also be relevant, but there is very little evidence on this, as discussed above.)

We include 12 studies in this sub-section. Five examine the relationship between cash income and BMI. A further seven focus specifically on the impact of the Food Stamp Program (FSP) in the US.

Income and BMI

Of the income studies, three use US data, one looks at Germany and one at Sweden. Three of the five use natural experiments: inheritances in the US, lottery wins in Sweden, and an error in US social security payments which led to older people born before a certain date receiving higher pension payments. A fourth study uses variations in the US Earned Income Tax Credit (EITC) over time and state as an instrument for income; and a fifth study examines longitudinal data for Germany. This last study focuses on the role of relative income, exploring whether one's income in relation to that of regional and occupational peer groups has an impact on one's BMI. Together, this small group of studies provides inconclusive evidence about whether additional financial resources have a measurable impact on BMI. Perhaps in part, the mixed results reflect differences across studies in the characteristics of the population studied, though there are too few studies to draw clear conclusions in this regard (Table 7).

Table 7: Studies examining financial resources and BMI or obesity

Study	Country	Method	Negative effect	No effect	Positive effect
Cawley et al (2010)	US	Natural experiment (social security benefits notch)		There was no significant difference between those that received extra payments and those who did not.	
Schmeiser (2009)	US	Instrument and longitudinal (differences in EITC across states and over time)	Negative effect on women already overweight or obese – US\$1,000 extra associated with BMI increase between 0.14 and 0.21	Income was not significant for men's BMI.	
Kim and Ruhm (2012)	US	Natural experiment – inheritances			Bequests over US \$10,000 predict substantial declines in obesity and severe obesity (but imprecisely estimated – large standard errors).
Blanco-Perez (2012)	Germany	Longitudinal		There was no effect on BMI.	
Lindahl (2005)	Sweden	Natural experiment – lottery wins			Larger lottery win decreases the chance of being overweight.

Just one study out of the five finds clear evidence that additional income increases BMI, specifically for women who were already overweight or obese. Schmeiser (2009), examining the EITC in the US, finds that additional income is associated with an increase in BMI for this group of women. No effect was found for men (and it is also worth noting that pregnant women were excluded from the sample).

At the same time, just one study finds clear evidence that higher income results in *improvements* in BMI. Lindahl (2005), examining longitudinal data on lottery winners in Sweden, finds that winning a large amount on the lottery decreases the chance of being overweight compared to winning a smaller amount.

The remaining three studies find no significant income effect. Kim and Ruhm (2012), looking at inheritances among white 51–61 year olds in the US, find that bequests over US\$10,000 predict substantial but ‘imprecisely estimated’ declines in both obesity and severe obesity: that is, the effect appears large but is not significant because of large standard errors. Cawley et al (2010), exploiting a windfall in social security payments enjoyed by retirees born between 1915 and 1917 as the result of an indexing error in the 1970s, finds no significant difference in weight between those that receive the extra payments and those that do not. Finally, Blanco-Perez (2012), examining German longitudinal data, finds no evidence that relative income position (income compared with that of occupational and regional reference groups) affects BMI.

US Food Stamps Program (FSP)

Food stamps are a very specific form of money, but they are an important component of household financial resources in US families that receive them. The FSP (now in fact called SNAP, the Supplementary Nutrition Assistance Program) was designed in the 1960s to provide a nutritional safety net for low-income households. There is evidence that the stamps have indeed been successful in reducing food insecurity and food hardship (DePolt et al, 2009; Borjas, 2004). However, the fact that female (though not male) FSP participants have a higher risk of obesity than eligible non-recipients has led to concerns that the FSP has been too successful in boosting food consumption and is contributing to the rise in obesity in the US (see for example discussion in Townsend et al, 2001; Chen et al, 2005; Fan, 2010).

Of the seven studies that examine food stamps and BMI, two use longitudinal fixed-effect approaches (that is, examining whether individuals are more likely to be overweight during periods in which they receive food stamps). One uses a difference-in-difference approach, comparing changes in obesity for part-time and full-time participants over time. Three use instrumental variables, exploiting state-level variation in various aspects of FSP eligibility and outreach, and one uses a natural experiment, in which some but not all states stepped in to extend programmes to legal immigrants after a federal law removed their entitlement.

Table 8: Studies examining the US Food Stamps Program and obesity

Study	Method	Negative effect	No effect	Positive effect
Baum (2012)	Instrument – state variation in food stamp eligibility laws		There was no effect on obesity.	FSP decreases the probability of gaining insufficient amount of weight during pregnancy.
Baum (2011)	Longitudinal – Fixed effect	Receipt significantly increases the obesity gap for women. Long-term receipt significantly increases obesity gap for men and women, and probability of obesity for women (by almost 5 pp).	There was no effect on obesity for men in the short term.	
Fan (2010)	Longitudinal – Difference-in-difference		There was no effect on obesity for men in the short term.	
Gibson (2003)	Longitudinal – Fixed effect	Low-income women are more likely to become obese when participating in FSP.		
Jo and Lim (2009)	Instrument: uses state-level variables on FSP take-up		There were no significant effects on BMI.	FSP reduces the prevalence of obesity.
Kaushal (2007)	Instrument – exploits different state responses to a 1996 law denying immigrants access to food stamps		There was no statistically significant relationship between FSP and BMI. A 0.3% increase was recorded for low-educated unmarried mothers, but was not significant.	
Meyerhoefer and Pylypchuk (2008)	Instrument – uses expenditure on FSP outreach for women, and toughness of requirements for recertification for men	Women participants are 6.7% more likely to be obese.	There was no effect on men.	

As was the case for the studies examining cash income and BMI, the overall picture is far from clear-cut. Three of the seven FSP studies indicate that food stamps increase obesity, consistent with the finding in Schmeiser's analysis of the EITC. Meyerhoefer and Pylypchuk (2008) use an instrumental approach on a short panel, the Medical Expenditure Panel Survey (MEPS 2000–2003). They exploit differences across states in spending on outreach to improve take-up (for women) and in the toughness of requirements for recertification (for men), and find female but not male FSP participants are more likely to be obese than non-participants. Using fixed-effect techniques on longitudinal data from the National Longitudinal Survey of Youth 1979 (NLSY79), Gibson (2003) also finds a negative effect for women but not men. Also using fixed-effect methods, and the same data as Gibson (though a longer panel, 1985–2000 instead of 1985–1996), Baum (2011) investigates timing and dynamics more closely. He finds that short- and medium-term receipt significantly increases the obesity gap but not the likelihood of obesity itself (so the effect is only on women who are already obese). Long-term receipt (at least 24 months) significantly increases both the probability of obesity and the obesity gap for women, as well as the obesity gap for men. Baum's is the only study that identifies any negative effect of the Food Stamps Program on men (Table 8).

In contrast, however, three studies find no evidence of effects on obesity. Focusing on low-income expectant mothers in the NLSY79, using an instrumental approach that exploits state variation in food stamp eligibility laws, Baum (2012) finds no effect on obesity (alongside a positive effect on reducing the likelihood of being underweight during pregnancy). Kaushal (2007) makes use of differences in the way state regulations treated immigrants after a 1996 law removed their right to qualify under federal law. Using a large dataset from the National Health Interview Survey, she shows that immigrant women had higher food stamp use in states that had substitute programmes (as expected), but that the increases were associated with negligible and statistically insignificant changes in BMI. Finally, Fan (2010) uses a difference-in-difference approach on NLSY79 data, comparing changes for full-time and part-time participants, and finds no significant effect on obesity.

The seventh study, by Jo and Lim (2009), finds a positive effect of food stamp participation, with food-stamp participation reducing the risk of obesity. However, this study uses degree of FSP participation at state level as an instrument, and finds positive effects also for non-low-income households (who should not be eligible for the FSP). This suggests that FSP take-up may be acting as a proxy for other changing state factors, meaning the result should be treated with caution.

Even setting Jo and Lim (2009) aside, how do we make sense of the differing findings of the other six studies, with three finding negative effects of FSP participation, especially for women, and in some cases for women who are already overweight, and three finding no such effects? There do seem to be plausible mechanisms through which food-stamp participation may increase BMI: participation does seem to increase food expenditure, with some research suggesting that it does so more than other forms of cash benefit (Gibson, 2003; Fraker, 1990; Whitmore, 2002). Chen and Zhang (2011) point out that if the level of FSP benefits meets nutritional needs for a group of recipients, it must exceed these needs for others. The monthly administration of the stamps has also been put forward as a potential mechanism, as it may result in binge eating, which may lead to weight gain over the long term (Townsend et al, 2001; see discussion in Fan, 2010, and DeBono et al, 2012).

On the other hand, two of the three studies that find that FSP increases obesity use fixed-effect techniques, and both Fan (2010) and Kaushal (2007) argue that the finding could be driven by changes in unobserved characteristics associated with both FSP participation and weight. The fixed-effect approach controls for any such variables that remain fixed over time (for example, an excessive love of food), but if there are factors that affect both participation and obesity that change over time (such as depression or food insecurity), this is a problem for the technique. This would make the results from the instrumental and natural experiment studies more reliable, and three out of four of these studies find no negative effect.

In sum, there is extremely mixed evidence about whether food-stamp participation increases the likelihood of being overweight or obese. Three studies find such effects, especially for women, supporting the finding of Schmeiser's (2009) study of the effect of an increase in cash incomes through the EITC. These studies suggest that the mechanism could be the income itself, and/or the practice of paying the transfers monthly, which may result in binge eating. However, it has been argued that two out of the three studies may be subject to selection effects, with hidden characteristics explaining the association. Of studies that use instrumental and natural experiments, three out of four find no significant increase in obesity, with one study finding that food stamps reduced the probability of low-income women gaining insufficient weight during pregnancy.

Health outcomes

Next we consider the evidence for physical health outcomes. Having found such mixed evidence for health behaviours and BMI, it is not clear that we would expect to find a clear and positive story on health outcomes either. On the other hand, health behaviours are just one of the three pathways through which resources may affect health; a positive income effect may be identifiable that operates through the material pathway in ways not picked up above (for example via housing conditions or heating) or through the psychosocial pathway. Still, we should keep in mind that these pathways may operate over the long term, and we are largely restricted here to studies that investigate relatively short-term changes in resources.

We found nine studies that investigate the effect of financial resources on self-reported general health, morbidity or mortality. Most of the evidence comes from natural experiments, but there is also one study that takes an instrumental approach, and two that use longitudinal data (Table 9).

Table 9: Studies examining financial resources and health outcomes

Study	Country	Method	Negative effect	No effect	Positive effect
Snyder and Evans (2006)	US	Natural experiment – social security notch	Increased mortality		
Evans and Garthwaite (2010)	US	Natural experiment – EITC payments			Improved self-reported health; reduced risky conditions
Lindahl (2005)	Sweden	Natural experiment – lottery wins			Improved self-reported health status; decreased mortality (but not for those aged 60 years+)
Apouey and Clark (2013)	UK	Natural experiment – lottery wins	Problems with arms, legs and hands; diabetes	Self-reported health; blood pressure, breathing problems, skin conditions, hearing and sight	
Kim and Ruhm (2012)	US	Natural experiment – inheritance		Mortality; self-reported health status; difficulties with activities of daily living (ADLs); difficulties with instrumental activities of daily living (IADLs)	Reduction in difficulties with IADLs (for men only)
Meer et al (2003)	US	Natural experiment – inheritance		Self-reported health; symptoms	
Milligan and Stabile (2011)	Canada	Exogenous variation – child benefit		Self-reported health	
Frijters et al (2005)	Germany	Longitudinal			Improved self-reported health satisfaction (for men only)
Blanco-Perez (2012)	Germany	Longitudinal	Relative satisfaction shows decline in self-reported health; decline in objective health measures		

Evidence from natural experiments

Evidence from the six studies using natural experiments is mixed: two studies find increased financial resources lead to improvements in health outcomes; one indicates worse health outcomes for those on higher incomes; and three find no effect. Snyder and Evans (2006) make use of a social security ‘notch’ in the US, whereby those born after 31 December 1916 received much lower social security benefits than those born before. Contrary to expectations, the cohort who received higher social security payments had a higher mortality rate.⁶ The authors suggest that the explanation may be changes in labour supply: those with lower payments responded by increasing post-retirement work effort, which was itself associated with decreased mortality. The authors argue that this may be due to the reduction in social isolation from working, referring to evidence that social isolation is associated with increased mortality.

Evans and Garthwaite (2010) use variation in Earned Income Tax Credits (EITC) in the US as a natural experiment to test the effect of income on self-reported health and medically measured biomarkers. They compare the outcomes of mothers with two or more children, who received generous increases in EITC payments, with those of mothers of one child whose EITC payments increased by much less. They find significant improvements in self-reported health, with a 1.35% increase in the probability of reporting very good or excellent levels of health and a 23% reduction in risky conditions (such as high blood pressure or high cholesterol). Because the authors are not able to identify those who actually received EITC payments, they used a ‘likely eligible’ sample based on education level, so these results may be an underestimate due to measurement error.

Two of the studies used lottery wins as natural experiments. Apouey and Clark’s (2013) UK study, described above, finds no significant effect of increased income on self-reported general health status. They also find mostly insignificant results for a number of specific health problems such as blood pressure, breathing problems, skin conditions, and deterioration in hearing or sight. However, they find some weak evidence that lottery wins have a negative effect on problems with arms, legs and hands and on diabetes.

Contrary to Apouey and Clark, Lindahl’s (2005) study of lottery wins in Sweden finds significant positive effects on health. Lindahl constructed a standardised index of poor health based on 48 questions on health symptoms and found that an income increase of 10% improves health by 4–5% of a standard deviation and reduces the probability of dying in the next five or 10 years by 2–3 percentage points. When the sample was divided by age, the health effects were not significant for respondents over 60 years old.

Finally, two of the natural experiments use inheritance to test the impact of financial resources on health. The US study by Kim and Ruhm (2012), described above, finds that increased wealth from inheritance has no significant overall effect on mortality or self-reported health status, nor on difficulties with ‘activities of daily living’ (for example, bathing, dressing, eating) or ‘instrumental activities of daily living’ (for example, difficulty answering the phone, managing money, shopping and preparing meals without help). However, when the sample is separated by sex, they find that inheritances do lead to a significant decrease in difficulties with instrumental activities of daily living for men. This study is based on data from the Health and Retirement Survey (HRS) with respondents aged 51–61; as Lindahl’s study suggests, we may expect less significant effects of income on the health of older people.

Similarly Meer et al (2003) find no significant effect of inheritance on self-reported health status or reporting having a physical or nervous disability, using the US Panel Study of Income Dynamics (PSID), even when controlling for initial wealth. However, the authors do not rule out a longer term impact on health, as they only consider up to five years after receipt of the inheritance. In addition, the self-reported health variable is dichotomous, with excellent, very good or good health all classified as 1. This will make it more difficult to identify changes in health than in studies that use a continuous variable.

Evidence from exogenous income variation

One study uses other exogenous changes in financial resources to test the effect of money on health outcomes. Milligan and Stabile (2011) exploit variation in child benefit levels across different Canadian provinces, over time and for different family types, and simulate the benefits that a random sample of families would be eligible for in each province, year and number of children combination between 1994 and 2004. While some significant positive effects of higher benefit levels are identified for children's outcomes, as well as reductions in maternal depression (noted above), the study finds no effect on adults' self-reported health status.

Evidence from longitudinal studies

Our final two studies on health outcomes use longitudinal data to measure changes within households over time. This cancels out differences between households, holding unobserved characteristics (that do not change over time) constant. Frijters et al (2005) use longitudinal data (the GSOEP) straddling the period of German reunification, which resulted in large income increases to most of the population in the GDR. They find that increased income positively affected health satisfaction for East German men but not women. However, the size of the effect is described as very small: a one log point increase in income is associated with a 0.083 improvement in health satisfaction on a scale of 0–10. A similar significant impact of income on health satisfaction was found for West German males and females.

Finally, using the same data, Blanco-Perez (2012) focuses on the role of relative income for health outcomes, using a measure that distinguishes between the effects of upwards and downwards comparison. Blanco-Perez tested this for a range of different reference groups people may compare themselves with, including region, age, education and occupation. When occupation and region combined are used as the reference group, results show relative deprivation to have a positive effect on self-reported health and quasi-objective health measures, while being richer than others appears to have a negative effect on health. When other reference groups were used the results were not significant. These results appear counterintuitive, but Blanco-Perez maintains that the findings are in line with other literature that argues that a so-called 'tunnel effect' explains the relationship: as the reference group is occupation-based, being in a position of relative deprivation within the group gives people positive expectations about their future income. However, this would indicate not that relative deprivation is good for one, but that improvements in pay in one's sector are encouraging, even when they have not yet affected one directly. Another interpretation is that occupation is simply proxying individual income, and shows up as significant because individual income is measured with error in the survey.

Overall, evidence on the effect of financial resources on health outcomes can only be described as mixed, with around one-third of studies finding improvements in health, just over one-third finding no effect and two studies

finding that increased income leads to a deterioration in health outcomes. This is the case for a range of measures, including self-reported health status, mortality and specific health conditions. Rather than call into question the relationship between financial resources and health outcomes, these mixed findings are more likely to be indicative of the limits of the evidence available, which is capturing relatively short-term impacts of changes in financial resources that are sometimes quite small. Given that health outcomes are not only influenced by multiple factors but also accumulate across the life course, with trajectories starting even before birth, it is perhaps unsurprising that an increase in resources during adulthood does not appear to significantly alter life expectancy or health conditions. Unlike health behaviours that appear to be more malleable, health outcomes are presumably more difficult to change.

Summary

In sum, the evidence on money and health behaviours is split. There is some evidence that increased financial resources have a negative effect on health behaviours, with studies that mostly make use of lottery wins and inheritances pointing to increases in moderate (but not heavy) drinking as well as in the number of cigarettes smoked. This latter finding stands at odds with the extensive cross-national evidence that individuals in poor households are more likely to smoke than those in richer households (see for example Lynch et al, 1997). It may be that what matters is low income over the long term, or that the link reflects other associated factors; either way an increase in resources in adulthood does not appear to help. However, a crucial caveat is that for parents, the story in relation to health behaviour looks very different, with extra money resulting in improvements, particularly in relation to smoking.

The evidence on BMI is very mixed, with nearly half of all the studies finding no effect of increased cash or food stamps on BMI or obesity, and the rest split between finding a positive and negative effect. However, in some of these studies it has been argued that there may still be unobserved characteristics (such as depression) that drive the association. Of the experimental and instrumental studies (ie, those that are more likely to avoid this problem), most find no effect on obesity and one study finds that food stamps protect against insufficient weight gain during pregnancy for low-income women.

Finally, the evidence on mortality, morbidity and self-reported health is similarly mixed, although it is worth noting that several of these studies look at mental health too and find positive effects (as discussed in Section 4). Perhaps the main conclusion to be drawn from this section is that the types of evidence that pass our test on using 'causal methods' are limited in what they are able to reveal about the relationship between money and health. They capture mostly relatively short-term effects of changes in resources on patterns of behaviour and health outcomes; they are not designed to pick up the long-term, cumulative nature of health and the importance of resources across the life course.

6 DOES MONEY AFFECT THE QUALITY OR STABILITY OF RELATIONSHIPS?

The impact of financial resources on the quality and stability of relationships is difficult to predict and there are a number of theories whose conclusions run in opposite directions. First, we may expect increased income to improve relationship quality and stability by reducing economic stress, as predicted by the family stress model (Conger et al, 2000). This model posits that financial hardship causes stress, which increases husbands' and wives' hostility towards each other and reduces warmth (Conger et al, 1990).

However, whether it is the husband or wife that receives the increased financial resources may be significant in determining the effects of the resources. Historically, men and women have had segregated gender roles, in which men have mostly earned the money and women have mostly looked after children and the home. It has been argued that an increase in women's income destabilises relationships by interfering with gender roles and also providing women with greater economic independence and therefore less reliance on the relationship (Becker, 1981). This may both undermine men's role, causing hostility, and reduce the attractiveness of marriage for women. Under the independence hypothesis, then, we might expect a rise in women's income to decrease the likelihood of marriage and increase that of divorce, and an increase in men's income to have the opposite effect.

On the other hand, this theory has been criticised for over-simplifying money as equating independence and thereby decreasing the value of marriage, while the desirability of role specialisation in relationships has also been called into question (Oppenheimer, 1997). Indeed, there is some evidence that role-sharing has a positive effect on relationships for cohabiting couples (Brines and Joyner, 1999). Kalmijn et al (2004) find that traditional gender roles are only stabilising for relationships in which the wives hold traditional gender values. This leads to a final criticism: as relationship and family patterns continue to change, the relevance of the theory for most couples today becomes questionable. It is likely to be entirely redundant when considering the effect of money on same-sex couples (although evidence on this is lacking, and none of the causal studies we found include analysis of same-sex couples).

When considering the impact of money on domestic abuse, predictions are also unclear. The family stress model (Conger et al, 2000) would suggest more resources reduce domestic abuse by reducing stress and improving

relationship interactions, but the independence hypothesis would predict that an increase in women's income would lead to both an increase in domestic abuse, as men's roles come under threat, and a decrease as women gain greater independence and are more likely to leave relationships. The prediction of a rise in abuse is in line with theories of 'male backlash' whereby men use violence as a means of reinstating their authority (Macmillan and Gartner, 1999). However, we might also expect increases in women's income to increase women's bargaining power and lower their exit threshold; this might reduce the amount of domestic abuse if partners are aware that they have a lower threshold and are likely to tolerate less abuse (Aizer, 2010). The causes of domestic abuse and women staying in or leaving abusive relationships are undoubtedly complex and involve other significant factors that theories do not account for. As with predictions of relationship stability and quality, there is disagreement among the theoretical explanations and it is not clear what the expected effect of increased financial resources would be.

Beyond the theoretical arguments, there are practical obstacles to assessing the impact of money on relationships. First, it is difficult to confirm the direction of any causal link. The sequencing of events cannot be relied on to help with this: there is some evidence that women increase their work hours (and thereby their income) when a relationship breakdown is anticipated, so although it may appear that increases in income lead to divorce, risk of divorce may actually lead to increases in income (Özcan and Breen, 2012). Furthermore, it is difficult to separate out employment effects from income effects, as increases in work may also bring increased opportunities to meet other people as well as increased confidence; both could be factors in the dissolution of a current relationship (ibid). It is for these reasons that in this section we have had to take an even stricter approach to the inclusion of studies and have excluded a number of interesting studies on relationship outcomes (Rogers, 1999; Rogers and Deboer, 2001; Gibson-Davis, 2009; Dew, 2008 and Benson et al, 2003) which did use longitudinal data but were not able to rule out the possibility that changes in employment accounted for changes in relationship satisfaction. Our final studies should therefore avoid both these problems (reverse causality and unobserved variable bias) as they use methods that are able to isolate the impact of money alone.

A second complication is that a lot of the evidence on this topic actually evaluates the impact of specific welfare programmes that provide financial incentives or disincentives (often different for different groups) either to marry or to become/remain single (for example Gennetian and Knox, 2004; Herbst, 2011; Moffit, 1990; Ellwood, 2000). This literature is not included here as we are interested in the impact of financial resources on wider outcomes, not in the effectiveness of financial incentives to prompt particular behaviours, ie not in money as a motivating factor. This is an interesting question in itself, but not the focus of this report.

Finally, when evaluating the evidence, it is very hard to draw conclusions about the positive or negative effects of money on relationships, not just because the evidence is fairly mixed, but also because it is unclear whether an increase in divorce is a good or bad outcome. Each case will depend on the specific nature of the relationship. Arguably, we could interpret an increase in both marriage and divorce, if resulting from an increase in money, as a positive outcome, because both suggest an increase in choice over relationship status. With the domestic abuse studies, the interpretation is obviously much more straightforward.

In total, seven studies are examined here. Six test the effect of money on divorce and marriage, and two focus on domestic abuse (one of these studies measures both outcomes). The evidence is described below for each relationship measure, starting with the most high-quality causal evidence. The majority of the studies find that money does have a significant effect on the quality and stability of relationships, although the results are not always in the same direction. As predicted by some of the theories outlined above, it does appear to make a difference whether it is men's or women's money that increases.

Relationship formation, stability and dissolution

Six of our studies attempt to assess the impact of financial resources on relationship stability and the likelihood of relationship dissolution for those in relationships, as well as the likelihood of new unions for those not in relationships, either in the form of marriage or cohabitation. The evidence comes from three RCTs, two natural experiments, and one study that uses an instrument to measure an exogenous shock to financial resources.

Evidence from RCTs

Two of the RCTs find that an increase in financial resources leads to an increase in relationship transitions: that is, an increase in divorce or dissolution for those previously in relationships, and an increase in new unions for those previously single. Bobonis (2011) makes use of experimental data from Mexico's conditional cash transfer (CCT) programme Progresa, which provided families with increased income conditional on children's school attendance and health check-ups. Although the conditions attached to such programmes sometimes make them redundant in terms of testing for the impact of the cash incentive received, in this case the conditions are unlikely to affect our outcomes of interest, namely new unions and relationship dissolution. Of 506 communities in Mexico, 320 communities were randomly assigned to receive the programme, with the remaining 186 as the control group. A poverty-proxy means test was used to identify households in poverty, and only eligible households in communities assigned to the treatment group received the programme (with the cash paid to the mothers). Bobonis found that although the overall rate of unions did not vary between those classed as eligible for the programme in treatment and control communities, families that were intact at baseline experienced a significant increase in relationship dissolution (0.32 percentage points higher, which is large compared with the separation rate in the control group), and mothers who were single before the programme significantly increased their union rates (by 3.1 percentage points in the first year and 2.2 percentage points by the second year). The impact of the CCT was also found to vary amongst different groups: relationship dissolution rates were not affected for non-indigenous women and there was a bigger impact on dissolution rates for younger women. Separated or divorced women with lower education were more likely to form a new union as a response to the cash transfers, as were single women aged under 35.

Eriksen (2010) used an RCT from the US to test the effect of increased assets on relationship stability. Individual development accounts (IDAs) were randomly offered to some eligible households and not others; these accounts were aimed at low-income households and provided matching payments when balances were withdrawn for particular purposes; for example, withdrawing money to buy a house had the highest matching rate

of 2:1. (The average matched withdrawal was US\$844 for down payments on a house and US\$576 for other allowed uses; including the match, this is US\$2,532 and US\$1,152 respectively.) Participants were interviewed immediately before the random assignment as well as 18 and 48 months afterwards. The estimation of the effect of the increased assets is likely to be less accurate with this study since although 98% of those offered IDAs did take them up, not everyone who had the accounts made use of the matched withdrawals or kept them open for the duration of the study. After 48 months the IDAs were found to have significantly increased the marriage rate for those previously not in relationships (42% higher than for the control group) and after 18 months those with IDAs were also significantly more likely to get divorced: there was a 149% increase in the likelihood of being divorced for the treatment group and when the sample is restricted to women or women with children, this increases to a 423% increase in divorce.

Both these RCTs thus find increased financial resources result in an increase in relationship transitions in both increased new unions and dissolution. The third RCT only measures the proportion of women married so is unable to reveal anything about relationship dissolution, but the results are not in contradiction to the first two RCTs. Gennetian and Miller (2002) estimate the impact of money on marriage and domestic abuse using a randomised controlled trial of the Minnesota Family Investment Program in the US, (discussed above in relation to happiness). The programme was aimed at lone mothers and randomly assigned participants to three research groups: a control group that continued to receive AFDC, which reduced significantly as earnings increased; one treatment group that provided financial incentives by allowing mothers to keep more of their welfare payments as their earnings increased; and a second treatment group, which provided the same financial incentives but also required participants to take part in mandatory work and training activities. The authors find the incentives-only treatment significantly increased the proportion of mothers who were married at the 36-month follow-up by 4 percentage points (Table 10).

Table 10: Studies examining financial resources and relationship stability and domestic abuse

Relationship stability						
Study	Country	Method	Change in men's or women's income?	Decreases	No effect	Increases
Bobonis (2011)	Mexico	RCT – <i>Progresa</i> conditional cash transfer programme	Women's income			Relationship transitions: both relationship dissolution rates and new unions (for both marriage and cohabitation)
Eriksen (2010)	US	RCT – Individual Development Accounts (IDAs) with matched savings	Does not distinguish			Relationship transitions: significant increase in both marriage and divorce
Gennetian and Miller (2002)	US	RCT – MFIP welfare to work programme	Women's income			Proportion of women married at 36-month follow-up
Boertien (2012)	UK	Natural experiment – lottery wins	Examines both men's and women's income, but results significant for men's income only			Likelihood of remaining together three years after win (for males only)
Hankins and Hoekstra (2011)	US	Natural experiment – lottery wins	Examines both – women's wins significant for getting married but men's not	Women's likelihood of getting married	Likelihood of divorce for men or women; men's likelihood of getting married	
Zhang (2013)	US	Instrument – college debt	Both		No effect of debt on marriage	
Domestic Abuse						
Study	Country	Method	Change in men's or women's income?	Decreases	No effect	Increases
Gennetian and Miller (2002)	US	RCT – MFIP welfare to work programme	Women's income	Rates of domestic abuse (5 self-report measures)		
Aizer (2010)	US	Instrument – changes in gender wage gap	Women's income	Decline in gender wage gap leads to reduction in violence against women (measured by hospitalisations)		

Evidence from natural experiments

Boertien (2012) uses the BHPS to analyse lottery wins in the UK as a natural experiment, in a similar manner to studies discussed earlier in this paper which compare players who won a lot to players who won a little. The relationship outcome of interest was whether partnered respondents remained in their relationships three years after winning the lottery. Boertien found that men who won more than £4,000 were significantly more likely to have remained with their partner three years later, while there was no significant effect for women lottery winners. The effect identified for men was only for men who earned up to around £18,000 and whose partners were also active in the labour market. The larger the earnings share for men, the smaller the marginal effect of lottery wins. In addition, the effect depended on relationship satisfaction in the first place and was not significant for men who were entirely happy with their partner a year before winning the money. This study highlights the importance of the timing of measurements and time lags: no effect was observed just two years after the win and by five years afterwards, the significance had faded again, so there appears to be a very specific window in which the effect is observed. On the other hand, this specific timing might be seen as calling into question the existence of an effect.

Usefully, this study goes some way towards trying to understand the potential mechanisms that explain these results, exploring measures of satisfaction and consumption after winning. Boertien (2012) found that when men win the lottery, they become more satisfied with their income, social life and leisure time, whereas when women win they do not become significantly more satisfied. Partners of men and women who win have no significant increase in satisfaction; from this, it is suggested that lottery wins are not shared. In probing these results further, it was found that when men win, they increase their spending on leisure time and eating out, while when women win, they save the money or spend it on consumer durables (although the differences with men are not significant). They also find that the effects of lottery wins on satisfaction and consumption were only temporary. Using structural equation modelling, the author concludes that it is increased leisure expenditure and decreased economic hardship that leads to improved satisfaction with income, leisure and partners for men. This provides some support for the family stress model and also highlights behavioural reasons why increased financial resources may have a different impact on men and women.

A second natural experiment uses lottery wins, this time on US data. Hankins and Hoekstra (2011) use administrative data on two lottery games in Florida, linking them to public records of marriage and divorce in two counties (using the name of the winner and excluding common names that were in the phone book more than once). The authors find that winning higher amounts (US\$25,000–US\$50,000 compared with winning less than US\$1,000) did not affect the likelihood of divorce for men or women. However, women who won higher amounts were significantly less likely to marry in the next three years (by between 2.9 and 3.4 percentage points) than women who won less. The authors argue that the differences between women who win a lot and a little are large, representing 41–48% reductions in marriage relative to the baseline marriage rate among all female lottery players of 7%. There is no effect of winning larger amounts on men's likelihood of marrying.

Evidence from instruments

Finally, Zhang (2013) uses college aid policies in the US to examine the effect of student college debt on a range of educational and employment outcomes as well as marital status. Zhang finds increases in debt influence educational choices (as discussed in Section 8 below), but there was no significant impact on the likelihood of getting married either in the short- or long term.

The evidence then for relationship stability suggests that money does have an effect (all but one of the six studies found a significant effect), but results run in both directions (relationships ending and new relationships forming), and differ depending on whether the money goes to men or women. Arguably, relationship break-ups and new unions are extreme outcomes. Money may have an influence on relationships that is less visible and more subtle. Another way of measuring the impact of money on relationships is to consider people's relationship satisfaction. Unfortunately, none of the studies that met our full criteria measured relationship satisfaction, so this is an area where more research is needed.⁷

Domestic abuse

Two studies, both from the US, examined the influence of money on domestic abuse. The RCT of the MFIP by Gennetian and Miller (2002), described above, found that for the treatment group that received financial incentives, domestic abuse reduced by 10 percentage points (or by 16% compared with the control group), as reported by mothers based on five different types of abuse (hitting, yelling, feeling controlled, sexual abuse and being threatened).

Aizer (2010) finds similar evidence for the impact of money on violence against women. The author exploits changes in demand for labour in female-dominated industries relative to male-dominated ones in order to test the effect of a reduction in the gender wage gap on violence against women. Violence against women is measured by the number of female hospitalisations caused by assault for the state of California from 1990 to 2003, and the analysis controls for secular trends over time in both violent crime and hospitalisations. Although hospitalisations are an imprecise measure as the figures will include non-intimate violence, Aizer reasons that three-quarters of violence against women is intimate and trends in non-intimate violent crime can be controlled for. It is found that the narrowing of the gender wage gap does significantly reduce violence against women: the decline in the wage gap of 3.6 percentage points explains 9% of the reduction in violence against women.

Both Gennetian and Miller (2002) and Aizer (2010) focus on the effect of an increase in women's income, which has been predicted to increase women's economic independence and lower the exit threshold; this is expected to cause men to reduce their usual levels of violence or abuse in the knowledge that they have less power and that women are in a stronger position to leave the relationship. This explanation is supported by Andberg et al's (2013) research on unemployment and domestic violence; they find that although there appears to be no significant association between unemployment rates and domestic violence, if a distinction is made between male and female unemployment, it becomes evident that the effects operate in opposite directions: an increase in male unemployment is associated with a decrease in abuse while an increase in female unemployment is associated with an increase in abuse.

Summary

The overall findings suggest that money does have a significant effect on the quality and stability of relationships. Increases in financial resources appear to lead to an increase in relationship transitions – that is both in relationships ending and new relationships starting. Rather than finding these results to be contradictory, we might interpret them as signalling that an increase in income results in an increase in choice over relationship status.

The evidence is unclear as to whether it matters whether men or women experience the increase in money. Two studies of relationship stability focused on women's income only, as Bobonis (2011) and Gennetian and Miller (2002) made use of programmes where the money was paid to women. They found similar effects to a third RCT examined by Eriksen (2010), which did not distinguish between men and women's income: all found an increase in relationship transitions. Two of the remaining studies, however, provide a different story for men and women. Boertien (2012) finds that it is only men's lottery wins that significantly increases the likelihood of a couple remaining together after three years, while Hankins and Hoekstra (2011) find no significant effect of men's lottery wins but that women who won larger amounts on the lottery were less likely to get married.

Of the two studies that look at money and domestic abuse, both focus on an increase in women's income and find that this decreases domestic abuse. There are no studies to compare with an increase in men's income, but these results are in line with predictions that when women have more economic independence, they are more likely to leave abusive relationships and men are therefore more likely to reduce abusive behaviour to avoid this. Furthermore, although the evidence base is thin, these two studies are supported by findings from research on unemployment and domestic violence (Andberg et al, 2013).

Most of the studies focus on low-income groups only, and just one tests explicitly whether there is a non-linear relationship: Boertien (2012) finds that lottery wins have a significant effect on the likelihood of remaining married after three years only for men who earned £18,280 or less.

7 DOES MONEY AFFECT SOCIAL OR POLITICAL PARTICIPATION?

Our searches for this section aimed to identify studies that looked at the effect of having more money on measures of social participation (such as volunteering) and political participation (including voter turnout as well as more active measures such as campaigning or standing for election). We identified very few studies that used the included methods – just three in total – and the results of these are mixed. Two studies look at social participation, one finding no significant effect of home ownership, and the other suggesting that deprivation increases participation. Two studies look at political participation, with the same home ownership study finding no significant results, but a paper examining a sample of African-American mothers finding a positive effect of income on voter turnout using fixed-effect methods.

Corman et al (2012) use fixed-effect techniques on Fragile Families data⁸ to look at how financial insecurity affects participation in organisations, including church-related groups, charities and community groups. Financial security is measured using material deprivation indicators and access to financial buffers, including ability to get a bank loan and ability to borrow money from a friend or relative. Financial buffers are not significant at conventional levels once other controls are included. In contrast, however, material deprivation indicators are significant but in the opposite direction to that expected: increases in the number of material hardships increases the propensity to participate in any organisation. The authors suggest that this may be because participation can act as a coping mechanism, with people experiencing hardship reaching out to organisations for assistance and support.

Engelhardt et al (2010) are interested in whether home ownership affects a range of measures of social and political participation, including voting, writing to a public official, supporting a candidate for public office with time or money, and a number of indicators of volunteering or helping friends. The approach taken uses the same randomly assigned field experiment used by Eriksen (2010) to look at assets and relationship stability. Low-income households were offered individual development accounts (IDAs), with payments matched when balances were withdrawn for particular purposes, including home purchase. Assignment is used here as an instrument for home ownership, because among the 217 renters in the

treatment group, home ownership increased by between 25% and 30% over the four years of the study than among the 220 control group renters. For each of the political involvement measures, the impact of home ownership was found to be zero or negative (but not significant) over these four years. For helping and volunteering, results were also not significant but, in the authors' view, not conclusive, because results could possibly have been significant with a larger sample size.

Plutzer and Wiefek (2006) use fixed-effect methods on a dataset that tracked the mothers of all first-grade children in Woodlawn, an inner-city area with a high African-American population, between 1967 and 1976. They found that income loss was not significantly associated with voter turnout, but an increase in income was: for a single mother with a 60% probability of voting in 1967, an increase in income of US\$3,500 was linked to a probability of voting of 66%. However, about one-third of this effect disappears once employment and education transitions and other controls are included, while further robustness tests suggest that part of the remaining effect may reflect other, unmeasured traits (voting in 1967 is found to predict future income growth, even after including a host of controls, suggesting unobserved variables may be at play). Table 11 summarises these studies.

Table 11: Studies examining financial resources and social or political participation

Study	Method	Country	Measure		Result
			Social participation	Political participation	
Engelhardt et al (2010)	Instrument – Individual Development Accounts (IDAs) that increase home ownership	US	Volunteering, helping friends	Voting, writing to a public official, supporting a candidate	No significant results
Plutzer and Wiefek (2006)	Longitudinal	US		Voting	Increase in income increased participation; decrease in income had no effect
Corman et al (2012)	Longitudinal	US	Participation in organisations (church-related, charities, community groups)		Increases in hardship increased propensity to participate

The main conclusion here is that more research would be useful. There appears to be very little evidence about the impact of more money on either social or political participation, and the evidence that does exist is inconclusive.

8 DOES MONEY AFFECT DECISIONS REGARDING EDUCATION OR EMPLOYMENT?

In this section we explore the evidence looking at the effect of financial resources first on educational choices or outcomes, and then on decisions regarding labour supply or career pathway. The studies between them look at the impact of a range of different forms of resources, including income from cash transfers, pensions, lottery wins and bequests, as well as variation in assets (housing wealth and college debt).

Education

Four studies look at educational outcomes or choices, one for Denmark and three for the US. The Danish study uses a natural experiment in which some high school students received more in cash transfers than others, depending on their date of birth; this study finds no effect of higher transfers on academic performance. The three US studies use instrumental approaches – two looking at housing wealth, one at college debts – and all three conclude that these affect decisions about college attendance. In all three cases there is evidence of non-linearity, with asset effects making more difference in lower income households.

Humlum and Vejlin's (2013) study examines the effect of monthly cash transfers made to high school students in Denmark after they turn 18. Because students only receive the payment from the beginning of the quarter after the quarter in which they turn 18 (April, July, October or January), students with birthdays just a few days apart can receive very different amounts: those born just before the end of a quarter end up receiving around 4,500 Danish krona (or US\$560) more than those born just afterwards. The authors exploit this difference to look at the impact of the payments on student employment (discussed below) and on academic choices and results; grade point average, choice of advanced maths, probability of dropping out of high school and choice of college enrolment. They find what they interpret as 'essentially zero effects' on all measures: estimates are small and not significant at 5%.

Of the US studies, two look at the effect housing wealth has on decisions about going on to college. Both take the same approach, using change in the value of housing wealth during the four years before a child is college age as a source of wealth variation beyond household control. The aim is to compare the decisions made by 18–19 year olds who reach this age at different times in different states, and so have experienced different changes in housing wealth during high school. Housing values are measured from self-reported data, and the models include time and area (state and city) dummy variables to control for other time or area effects. Using PSID data, Lovenheim (2011) examines the decision about whether to attend college for cohorts of young people who reached 18–19 years old in 2001, 2003 and 2005, a time of rapid house price rises. He concludes that there is a ‘sizeable positive relationship’ between house prices and the decision to stay in education: a US\$10,000 increase in home equity increases the likelihood that a child goes to college by 0.71 of a percentage point (a 1.37% marginal effect). Effects are largest for households earning less than US\$70,000 a year.

Lovenheim and Reynolds (2013) conduct a similar analysis on data from the NSLY79 to look at whether housing wealth affects the choice of college and the probability of graduation for children who were aged between 12 and 18 in 1997 and whose parents are homeowners. House prices are only collected in 1997, so the authors use a state- and area-level house price index to predict house price change in the relevant time period. They conclude that a US\$10,000 rise in house prices increases the likelihood of attending a public flagship university by 2% compared with a non-flagship institution, and decreases the likelihood of attending a community college by 1.6%. There is no effect on selection into private institutions. Again, the effects are larger for students from low- and middle-income households (those earning less than US\$75,000 a year), and seem to be driven by changes in the pattern of applications rather than admissions. For the lowest income households only, short-run housing price growth is also associated with a greater probability of graduating: each US\$10,000 increases the likelihood of graduation by 1.8%.

While year and area control variables are included in all the models in these studies, the possibility remains that sorting effects within areas could explain the results: households with high-ability children may live in houses that appreciate more over time. Both studies take a series of steps to check this possibility and conclude that there is little evidence that such sorting exists or explains the results.

Zhang’s (2013) study focuses on the next stage of education: he asks whether students’ college debts affect the probability of their attendance at graduate school and graduate school choice. This study, also described in Section 6 above in relation to relationship transitions, makes use of differences in college aid policies to create an instrument for debt that is not driven by hidden student characteristics. The data, the Baccalaureate and Beyond Survey, is a longitudinal study with baseline data collected in 1992–1993 (when students graduated from college) and follow-ups in 1994 and 1997. The authors use the percentage of students from each students’ college who receive aid as an instrument for the level of debt faced on graduation, while controlling for college characteristics that might be associated with both the level of aid provided and the ability of the student body (the extent of selectivity, and whether the institution is a research university or a liberal arts college focused on undergraduates). A number of additional robustness checks are included to check the validity of the instrument. The results indicate that higher levels of debt do have an impact,

but the negative effects are concentrated on students attending public colleges. For these students, debt has a significant and negative effect on the likelihood of attending graduate school at all, and debt also decreases the likelihood of choosing a doctoral, MBA or first professional (FP) programme. For students at private colleges there is no overall effect, while higher debt seems to increase the likelihood of entering an MBA or FP programme. Zhang suggests that the difference between private and public students may reflect an underlying difference in willingness to incur debt for human capital investment, revealed by students' initial choice of type of college (Table 12).

Table 12: Studies examining financial resources and educational choices or outcomes

Study	Method	Country	Measure	Result
Humlum and Vejlin (2013)	Natural Experiment – cash transfers to adolescents, using differences in eligibility due to birthday	Denmark	Grade point average; choice of advanced maths; dropping out of high school; college enrolment	There was no effect.
Lovenheim (2011)	Instrument – housing wealth, using variation over time and across areas	US	Staying in education at 18	Increases in home equity increased the likelihood of going onto college, with larger effects for households earning less.
Lovenheim and Reynolds (2013)	Instrument – housing wealth, using variation over time and across areas	US	Choice of college and probability of graduation	Increases in home equity increased the likelihood of going to a ‘flagship’ public institution, with larger effects for lower income households. There was a positive effect on graduation for lowest income households only.
Zhang (2013)	Instrument – variation across colleges in financial aid policies	US	Attendance and choice of graduate school	Higher debt decreased graduate school entry and choice of a higher ranked graduate programme for students at public colleges only.

Employment and labour supply

The studies that look at the effect of financial resources on employment choices are a mixed bag, and findings are also mixed. Two studies explore whether US college debt has an impact on later career choices. Six studies look in different ways at whether unearned income affects the labour supply decision: one is the Danish student programme discussed above (Humlum and Vejlin, 2013), one examines conditional cash transfers in Mexico, and four look at either lottery or inheritance in the US. A final study examines whether variation in pension receipt due to a coding error affected labour market behaviour, using the US social security ‘notch’ natural experiment. Table 13 summarises these studies.

Of the two studies examining college debt and career choices, one is Zhang’s (2013) study, discussed above, which uses the same instrument for college debt to look at early career outcomes. No effect is found for any of the measures, which include annual salary, the probability of being employed in the public or non-profit sector rather than the private sector, and the probability of becoming a teacher. The second study, by Rothstein and Rouse (2011), comes to very different conclusions. The authors exploit a natural experiment in which an expensive and selective university introduced a no-loans policy in the early 2000s, replacing financial aid loans with grants. The authors compare career pathways for students before and after the change in policy, and find that under the new scheme, aid recipients shifted away from industries with high average salaries (such as banking and consulting) and into lower salary public service industries (non-profit, government and education sectors). There was no change in the composition of jobs for students not receiving aid. In contrast to Zhang’s findings, this analysis suggests that carrying debt constrains career choice. The authors suggest that this could be because graduates are debt-averse, or because the debt makes it more difficult for graduates to borrow further money later, which places limits on their choices.

Six studies explore the impact of unearned income on labour supply decisions. Skoufias and Di Maro (2008) ask whether the Mexican conditional cash transfer programme, Progresa, has affected work incentives. Once a family is selected for the programme, payments are conditional on children’s school enrolment and attendance at health clinics, but remain unaffected by adults’ work decisions or income, marking it out from many means-tested programmes in other countries. This, and the fact that transfers are substantial in size – 20% of pre-programme consumption – make Progresa an ideal setting to examine the impact of cash transfers on labour supply decisions. In addition, the experimental design of the programme, with randomisation at area level, makes this the study in this section that is best able to make use of a pure control group. The authors track labour market behaviour from a baseline of 1997 through to 1999, and find no evidence that the programme has significantly affected either labour force participation or leisure time. There is some indication that, early on, individuals may have used the transfers to reduce their labour in family enterprises and to look for (perhaps more profitable) salaried employment, but this effect seems to disappear over time.

Table 13: Studies examining financial resources and employment and labour supply

Study	Method	Country	Measure	Result
Skoufias and Di Maro (2008)	RCT – Progresa conditional cash transfers	Mexico	Labour supply decisions	There were no effects.
Humlum and Vejlín (2013)	Natural experiment – cash transfers to adolescents, using differences in eligibility due to birthday	Denmark	Labour market participation of high school students	Transfer decreased labour market participation and number of hours worked.
Rothstein and Rouse (2011)	Natural experiment – change from college loans to grant system	US	Career pathways of students after college	Recipients shifted away from industries with high average salaries to lower-paid public service industries.
Imbens et al (2001)	Natural experiment – lottery wins	US	Labour market earnings of lottery winners	Large win significantly reduced labour market earnings. There were larger effects for those closer to retirement.
Holtz-Eakin et al (1993)	Natural experiment – inheritance	US	Labour force participation and earnings	Large bequests decreased labour force participation and earnings.
Joulfaian and Wilhelm (1994)	Natural experiment – inheritance	US	Labour force participation	Bequests led to small but significant reductions in labour supply.
Brown et al (2010)	Natural experiment – inheritance	US	Retirement (study focuses on older workers)	Inheritance receipt increased the probability of retirement; effect was larger with size of bequest.
Krueger and Pischke (1992)	Natural experiment – social security benefits (pensions) notch	US	Labour supply near pension age for those with higher/lower pension entitlement	There were no effects.
Zhang (2013)	Instrument – variation across colleges in financial aid policies	US	Annual salary, probability of working in public or non-profit sector, probability of becoming a teacher	There were no effects.

Humlum and Vejlin's (2013) study of cash transfers to Danish students found no effect of higher transfer receipt on a range of educational outcomes, as discussed above, but the study did find significant effects on students' labour force participation. Receiving 4,500 Danish krona (US\$560) more on average decreases labour market participation by 1.9 percentage points, and reduces the number of months worked by 0.26 (7–8 days). Young people whose parents are on a low income were more responsive to the transfer: an extra 1,000 Danish krona decreases participation by 1.2 percentage points more for those in the bottom quintile than for those in the top. As the authors point out, the fact that there are no effects on academic performance suggests that students are spending their extra time on leisure rather than study, so the policy is meeting an intermediate goal but not its ultimate one.

The four other studies look at the effects of unearned income in the US. Imbens et al (2001) examine the impact of lottery wins in the US state of Massachusetts. Using their own original survey, the authors compare season ticket holders (regular players) who have won small prizes (US\$100 to US\$5,000) with winners of prizes between US\$22,000 and US\$10 million, which are paid out in yearly instalments over 20 years. (Season ticket small prize winners were chosen as the comparison group because no records were kept for all players, or for season ticket holders who won nothing.) As part of the survey, respondents were asked to authorise the release of their social security earnings records, giving the authors accurate earnings data for 496 people, 237 of whom had won large amounts. The sample is a little more highly educated and slightly older than the population at large. The results indicate that a large win leads to significantly lower labour market earnings, with a marginal propensity to consume leisure of around 11%. Effects are similar for men and women, but are greater for those closer to retirement age.

Three studies explore the effect inheritances have on labour supply. Holtz-Eakin et al (1993) frame this as a test of the 'Carnegie conjecture' – industrialist Andrew Carnegie's assertion in 1891 that 'the parent who leaves his son enormous wealth... tempts him to lead a less useful and less worthy life than he otherwise would' (Carnegie, 1891/1962, p.56). The approach matches US Inland Revenue Service (IRS) data on inheritance taxes in 1982 and 1983 with personal income tax returns to examine whether labour force participation is affected by inheritance receipt. They find that recipients of large bequests were significantly more likely to exit the labour force between 1982 and 1985 than those receiving smaller amounts. For example, families with one or two earners who received inheritances above US\$150,000 were about three times more likely to reduce their labour force participation to zero over this period than families with inheritances below US\$25,000. Conditional on remaining in the labour force, high-inheritance families experienced lower earnings growth than low-inheritance families, consistent with the idea that inheritance reduces work hours.

Joulfaian and Wilhelm (1994) find a small but statistically significant effect of inheritance on labour supply, using fixed-effect techniques on data from the Michigan Panel Study of Income Dynamics (PSID) and the US Treasury's Estate-Income Tax Match (EITM) sample, which captures the top of the distribution, not well represented in PSID. They point to an increase in the probability of being retired for older men of less than half a percentage point for each US\$10,000 inherited, and to very small reductions in hours worked for younger men and married women (a few hours annually for each US\$10,000 received). Effects are larger for men (but not women) if they expect additional bequests in the future. An examination of the EITM sample

(those who receive the highest inheritances) gives very similar results once outliers are excluded.

Focusing on older people in particular, Brown et al (2010) also conclude that an inheritance effect exists, especially where the bequest is unexpected. They examine data for 1994–2002 from the US Health and Retirement Study. Inheritance receipt is found to increase the probability of retirement, and the effect increases with the size of the bequest. Receiving an inheritance raises the probability of retirement over an eight-year period by 4 percentage points, or 7% relative to baseline retirement levels. Raising the value of the inheritance by US\$100,000 raises the probability of retirement by 3.8 percentage points. If the bequest is unexpected, an extra US\$100,000 increases the probability of retirement by 10.3 percentage points. The authors control for the death of a parent to try to ensure that the effect is not driven by a change in priorities after such a death (for example, the realisation that time is limited and that one should ‘stop and smell the roses’). They also explore whether the effect is bigger in liquidity-constrained households (those with lower assets to start with); they find no evidence of this but are hesitant to draw conclusions as they are not confident of the accuracy of their measure of liquidity constraint.

Finally, Krueger and Pischke (1992) make use of the social security ‘notch’ natural experiment, already discussed earlier in the report with regard to health (Snyder and Evans, 2006). A 1970s amendment to the US Social Security Act resulted in an unexpected reduction in social security entitlement for individuals born after 31 December 1916 compared with those born on or before that date. Krueger and Pischke examine whether this led to an increase in labour supply among the younger cohort, those with reduced social security wealth and the prospect of lower benefits in retirement, and find no evidence that it does. On the other hand, we know from Snyder and Evans’ (2006) examination of the same experiment that post-retirement work effort increased for the younger cohort. Snyder and Evans posit that this extra work effort (and the social contact it entailed) may explain why mortality was lower among the cohort with lower entitlement. It seems then that pre-retirement behaviour and retirement age were unaffected by the lower payments, but they did lead more pensioners to continue working part time after they retired.

Summary

The number of studies covering each of the areas of behavioural change considered in this section is small, so conclusions must be tentative. However, the evidence reviewed appears to illustrate the role financial resources can play in widening choices. More assets (or lower debts) enable wider educational choices and – in one study though not in another – choices about career pathway. In Denmark, higher cash transfers allow students to work fewer hours, though the students do not appear to spend this extra free time studying. Unexpected (and large) income gains, through lottery wins or inheritance, appear to lead to individuals reducing their labour supply, but reductions in pension payments as a result of error in the US social security system did not seem to affect the decision to take early retirement. (The lower payments did, however, lead to increases in post-retirement employment, with positive effects on mortality, as discussed in Section 4.) Cash transfers to low-income households in Mexico do not appear to have affected labour supply in these households, but there is some evidence that they enabled individuals to look for different types of work.

9 CONCLUSIONS

There are two main conclusions to be drawn from this report. First, the causal evidence identified suggests that money in adulthood does itself matter for wider adult outcomes, but that this is clearer for some outcomes (mental well-being) than for others (for example physical health). Second, although the studies included meet our criteria of being 'causal', they are only getting at part of the story and still fall short of fully answering the question of whether money affects adults' outcomes.

Our review finds strong evidence that additional financial resources make people happier and reduce mental health problems such as depression and anxiety. This finding emerges from studies looking at a range of different sources of changing resources, including unusual events such as lottery wins as well as increases in social security benefits and variations in wages. There is also some evidence to support the idea that effects are non-linear, with a greater effect of a proportional increase in income at the bottom of the distribution. Certainly what several of the studies are picking up is the impact increased resources in low-income households can have in reducing stress and depression. Given striking figures about the number of people who experience mental illness in the UK and other rich nations (see for example Layard and Clark, 2014), this is an important conclusion. It is particularly significant in the current context of austerity policies, in which pay freezes and tax-benefit reforms have meant real cuts in incomes in many households, with largest effects at the bottom of the distribution (Cribb et al, 2013). Changes to the rules for the uprating of benefits over time mean that low-income households will feel an increasing squeeze in income over the next decade (Brewer et al, 2013).

The evidence from this report also suggests that money gives people more choices in a range of areas of life. Increases in money are likely to increase relationship transitions and reduce domestic violence (although whether it is men or women that receive the increase in money is significant). Increases in money also appear to affect decisions about the types of work people do as well as the number of hours worked. We found very little causal evidence on educational outcomes, but of the four studies included, three found that money widens choices about the types of educational institutions and programmes people attended, as well as the likelihood of attending college and graduate school.

For health outcomes the evidence is less clear. There is strong evidence from studies looking at social security reforms that an increase in resources improves the health behaviour of parents, especially in relation to smoking. However, evidence from studies of lottery wins and inheritance finds the opposite in relation to the general population: more money can lead to less healthy behaviours such as drinking and smoking a greater number of cigarettes. For health outcomes, including obesity, mortality and morbidity, the evidence is also mixed. We suggest that this may in part be explained by the limits of the

evidence that met our ‘causal methods’ criteria. The mechanisms and pathways that link financial resources to health appear to be complex, multi-faceted and cumulative across the life course (Benzeval et al, 2014). The studies in this report are not able to reflect this complexity.

This brings us to our second overall conclusion: that the types of evidence we have included only reveal part of the answer, and this is for two reasons. First, in seeking to test the causal effect of money, we limited ourselves to a very narrow set of studies that meant we could be confident that it was money itself that was making the difference to outcomes, and not other associated factors. This criterion has inevitably restricted us to studies that look at marginal changes in income and wealth; often short term, sometimes relatively small and sometimes large one-off windfalls in unusual circumstances. Long-term and persistent differences in financial resources may well be important to all of the outcomes we look at, but it is simply not something we can examine with this set of studies. Further, by design, the report focuses on the difference that resources during adulthood can make, and does not include studies looking at the long-term effect of money in childhood on adult outcomes, though some such research exists (see for example Shea, 2000).

The evidence needs to be seen then, as telling us about fairly limited variation in resources in adulthood, rather than about the impact of resources across the life course. The effects identified for mental health show us that money in adulthood certainly matters in crucial ways. But the mixed effects for health suggest that changing things late in life is hard, and underline the importance of investing early in childhood to affect the long-term drivers of health and well-being. This conclusion is supported by the much stronger and more consistent findings from our companion review on money and children’s outcomes, particularly in relation to cognitive and social-behavioural outcomes (Cooper and Stewart, 2013). Of course, children tend to live with adults, and raising income for adults is usually the way to reach children. Indeed, not surprisingly, money appears to affect children in part because of the way it affects adults, for example through reductions in parental smoking and maternal depression. Investment in children and in adults is related, then, not dichotomous. The key point though is that earlier intervention seems to be a more effective way of changing long-term outcomes, certainly in relation to physical health.

We end by identifying some gaps for future research. One obvious gap is that there is little research that meets our criteria on the UK or other European countries, with most of our included studies coming from the US. There are also some outcomes for which there is very little evidence at all, including measures of social and political participation. It would be valuable to have more studies that include longer follow-up after income increases, and also more studies that get directly at decreases in income, as most of the evidence here looks at positive changes. Finally, though some of our included studies are able to distinguish between men’s and women’s resources, this is another area where more evidence would be useful.

More broadly, more evidence using causal methods to examine the relationship between financial resources and wider outcomes would be welcome. Examples of research that makes innovative use of natural experiments or instruments are increasing, as the relatively high share of our studies from 2010 onwards testifies. Researchers need to be on the look-out for these sorts of opportunities. In the meantime, careful longitudinal use of panel and cohort studies should not be undervalued; while many such studies did not meet our causal criteria, in the absence of other evidence and combined with theory (such as Benzeval et al’s (2014) theoretical review of why money matters for health), these studies make an important contribution.

APPENDIX 1 SEARCH TERMS

This is the search syntax we have used for each outcome in all databases (EconLit, IBSS, Medline, PsychInfo, SoclIndex, British Education Index). Below is a breakdown of the search templates as outlined in Box 1 in the report, followed by a list of the search terms used for each outcome.

Financial resources:

AB(wealth* OR assets OR salary OR salaries OR earning* OR wage* OR pension* OR income* OR “socio-economic status” OR “socioeconomic status” OR SES OR poverty OR poor OR depriv* OR disadvantag* OR hardship OR money OR cash* OR expenditure OR spending OR “standard* of living” OR “living standard*” OR “cost of living”)

+ Causal relationship/method:

AND AB(caus* OR effect* OR determin* OR impact* OR influenc* OR associat* OR correlat*)

+ Outcome:

*see below for search terms used for each outcome

Subjective wellbeing

AB(wellbeing OR “well-being” OR happiness OR happy OR happier OR unhappiness OR unhappy OR unhappier OR fulfil* OR unfulfil* OR satisf* OR dissatisf* OR “Cantril ladder-of-life scale” OR content* OR “subjective welfare” OR “positive feelings”)

Physical and mental health

AB(health* OR morbidity OR mortality OR ill* OR sick* OR obes* OR overweight OR underweight OR “life expectancy” OR “Lifespan” OR “Medical condition*” OR Death OR Disease* OR “Chronic condition*” OR hospitalisation OR diabetes OR asthma OR anaemia OR cancer OR

“cardiovascular disease” OR nutrition* OR “hospital admissions” OR malaise
OR cortisol OR arthritis OR “heart attack” OR “quality of life” OR “QOL” OR
“Healthcare” OR “Medical care” OR “exercise” OR “physical *activity” OR
“fitness” OR smoking OR alcohol OR drugs or “substance abuse” OR “health
screening” OR “mental* health*” OR “Mental* ill*” OR “Mental breakdown”
OR Stress* OR Anxiety OR Suicide)

Stability and quality of relationships

AB(relationship N/3 qualit* OR relationship N/3 satisf* OR relationship
N/3 stabil* OR relationship N/3 break* OR relationship N/3 separat* OR
relationship N/3 abus* OR relationship N/3 violen* OR relationship N/3
interaction OR partner* N/3 qualit* OR partner* N/3 satisf* OR partner
N/3 stabil* OR partner N/3 break* Or partner N/3 separat* OR partner n/3
interaction OR partner N/3 abus* OR partner N/3 violen* OR famil* N/3
qualit* OR famil* N/3 satisf* OR famil* N/3 stabil* OR family N/3 break*
OR family N/2 separat* OR family N/3 interaction OR family N/3 abus* OR
family N/3 violen* OR marriage N/3 qualit* OR marriage N/3 satisf* OR
marriage N/3 stabil* OR marriage N/3 break* OR marriage N/3 separat* OR
marriage N/3 interaction OR marriage N/3 abus* OR marriage N/3 violen*
OR marital N/3 qualit* OR marital N/3 satisf* OR marital N/3 stabil* OR
marital N/3 break* OR marital N/3 separat* OR marital N/3 interaction OR
marital N/3 abus* OR marital N/3 violen* OR spous* N/3 qualit* OR spous*
N/3 satisf* OR spous* N/3 stabil* OR spous* N/3 break* OR spous* N/3
separat* OR spous* N/3 interaction OR spous* N/3 abus* OR spous* N/3
violen* OR conjugal N/3 qualit* OR conjugal N/3 satisf* OR conjugal N/3
stabil* OR conjugal N/3 break* OR conjugal N/3 separat* OR conjugal N/3
interaction OR conjugal N/3 abus* OR conjugal N/3 violen* OR husband
N/3 qualit* OR husband N/3 satisf* OR husband N/3 stabil* OR husband
N/3 break* OR husband N/3 separat* OR husband N/3 interaction OR
husband N/3 abus* OR husband N/3 violen* OR wife N/3 qualit* OR wife
N/3 satisf* OR wife N/3 stabil* OR wife N/3 break* OR wife N/3 separat*
OR wife N/3 interaction OR wife N/3 abus* OR wife N/3 violen* OR
divorce OR “intimate partner violence” OR “domestic abuse” OR “domestic
violence”)

Social participation

AB(social* N/3 part* OR social* N/3 activit* OR social* N/3 protest* OR
social* N/3 engag* OR social* N/3 involv* OR civic* N/3 part* OR civic* N/3
activit* OR civic* N/3 protest* OR civic* N/3 engag* OR civic* N/3 involv*
OR commun* N/3 part* OR commun* N/3 engag* OR commun* N/3 involv*
OR social* N/3 network* OR social* N/3 support* OR social* N/3 contact*
OR friend*)

Political participation

AB(political* N/3 particip* OR political* N/3 activit* OR vot* OR suffrage OR
ballot OR political* N/3 protest* OR political* N/3 lobb* OR political* N/3
engag* OR political* N/3 interest* OR political* N/3 part*)

Education

AB(Education OR Literacy OR Reading OR “writing skills” OR Numeracy OR math* OR Qualification* OR Grades OR Exams OR Graduate* OR Post-graduate OR Degree OR “School completion” OR NEET OR Post-compulsory OR Postcompulsory OR Post-16 OR “Sixth form” OR College)

Employment

AB(Job* OR Work* OR Employ* OR Unemploy* OR Profession OR Career OR occupation)

Crime

AB(crim* OR delinqu* OR offending OR arrest* OR convict* OR victim)

Key

AB= in abstract

*=truncated endings - includes alternative endings

?= includes alternative letter e.g. to include American spellings of some words with ‘Z’ instead of ‘S’

“ “ = to search for an exact phrase

() = nesting of search terms

N/3 = each of the terms specified have to appear within 3 words of one another

AND = to only retrieve if both words/ sets of search terms included

OR = to retrieve if contains any of the search terms included

NOTES

- 1 Our headings map roughly but not perfectly onto Burchardt and Vizard's domains: they have separate domains for life expectancy and health but include mental health under the broader health heading; we have split aspects of physical security between crime and domestic abuse; and we have split productive activities between employment and social participation. It is also important to underline that neither our searches nor – more patently – the studies we identified do justice to the range of important aspects of life that they suggest under each domain heading.
- 2 Economou and Theofossiou's (2011) study of income and health includes whether a respondent pays privately for his/her children's education or has bought valuable paintings as instruments for income, arguing that these will be correlated with household income but not otherwise with health. But these factors may well reflect – as indeed the authors suggest – long-term socio-economic characteristics dating back to childhood rather than income in adulthood per se. Powdathavee (2010) uses whether a respondent shows his/her payslip as an instrument for income in studying income and life satisfaction; this is likely to address the problem of measurement error but not the endogeneity of income.
- 3 In their review of measures of subjective well-being for the Office of National Statistics, Dolan et al (2011) draw a distinction between 'evaluation' measures (including overall assessments of one's happiness or life satisfaction), 'experience' measures ('how relaxed did you feel yesterday?') and 'eudemonic' or 'worthwhileness' measures ('to what extent do you feel that the things you do in life are worthwhile?'). Dolan et al suggest that the GHQ-type questions can be grouped as 'evaluation' measures, though some of the questions appear to capture aspects of experience. None of our studies captures indicators of worthwhileness.
- 4 Income is frequently transformed into log form for use in regression analysis because this effectively makes the percentage change in income the explanatory factor, rather than the absolute change in income. A one-unit increase in log income is a large change, approximately a doubling of income.
- 5 Note that Apouey and Clark (2013) also use fixed-effect methods but only when comparing 'winning' with 'not winning' for the same individual. They do not use fixed effects when comparing large and small wins across individuals.
- 6 This difference in mortality rates was not accounted for by the older age of the 1917 cohort, as the authors used a difference-in-difference method, comparing those born in the last quarter of 1916 with those born in the first quarter of 1917, as well as using control groups to rule out the possibility that the small difference in age or the particular timing of birth explains the difference in mortality rates (see Snyder and Evans, 2006: 487–8 for a more detailed explanation of how they rule out age as a confounding factor).
- 7 We identified several studies using longitudinal methods to look at this question, but these were excluded because they did not control for employment effects, and a change in employment status or working hours might itself be expected to affect relationship satisfaction independently of income change (see Rogers, 1999; Rogers and Deboer, 2001 and Dew, 2008). They also pointed to a problem of reverse causation: while Rogers (2001) suggests that an increase in income was significantly associated with an increase in marital satisfaction, both this study and Rogers (1999) found that marital discord itself led wives to increase their income, perhaps in anticipation of possible separation.
- 8 See www.fragilefamilies.princeton.edu

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