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WHAT POLICIES AND INTERVENTIONS HAVE BEEN STRONGLY ASSOCIATED WITH CHANGES IN IN-COUNTRY INCOME INEQUALITY?

A SYSTEMATIC REVIEW, JULY 2016

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Picture

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SYSTEMATIC REVIEW SUMMARY

A large body of evidence shows that fiscal policy has a large impact on in-country income inequality in low- and middle-income countries, while trade policy generally has a smaller impact. However, the impact of other types of policies and reforms on income inequality is much less researched and understood.

ABOUT THIS SUMMARY

The overall aim of this systematic review is to identify and synthesise the empirical evidence on the impact of government policies on in-country income inequality. It was commissioned and funded by the UK Department for International Development (DFID). This summary is designed to provide an overview of the key evidence discussed in the review, to assist policy-makers and researchers in assessing the evidence in this field. It is not designed to provide advice on which interventions are more or less appropriate in particular contexts, but to summarise what is known in response to the above question.

SUMMARY

We find evidence that government spending can have a negative impact on income inequality, but only when considering certain types of spending. The largest reductions in income inequality appear to be the result of cash transfer programmes, particularly in Eastern Europe. However, the redistributive effect of cash transfers in other regions of the world, particularly in Latin America, has been limited by their smaller size as a share of national income. We find some evidence that taxation reduces income inequality, but the size of the effect is significant only for a limited number of middle-income countries. More generally, the redistributive effect of taxation in low- and middle-income countries has been limited, for reasons such as the smaller share of direct income tax in total tax revenue. We find very little evidence of an association between trade policy and income inequality, either positive or negative, despite a large number of studies on this issue. For other sorts of government policies, such as labour market reforms, pension reforms, privatisation, and land reforms, there has been much less research, which suggests priorities for further work.

APPROACH

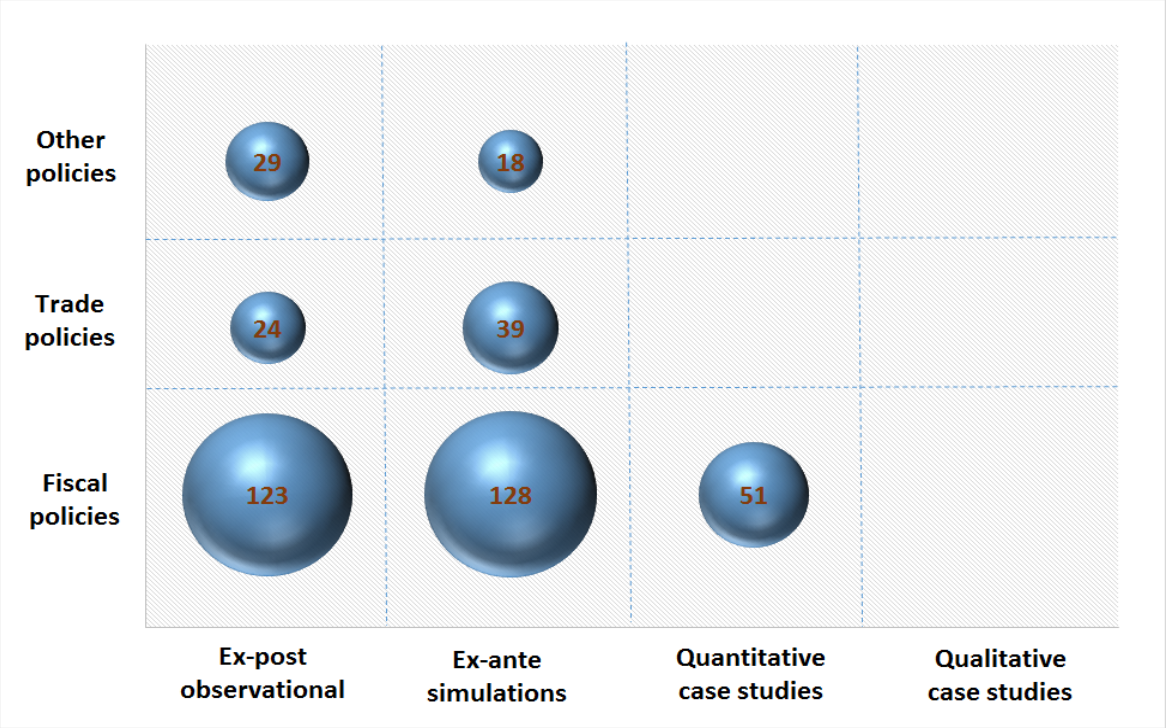
Our review is guided by a clear set of criteria which determine the studies included. It is restricted to studies focusing on low- and/or middle-income countries that use a recognised measure of income inequality, have been produced since 1990 and are written in English, Spanish, or Portuguese. The review draws on a range of evidence: ex-post observational studies (e.g. cross-country econometric analysis), ex-ante simulation studies (e.g. computable general equilibrium models), quantitative case studies, and qualitative case studies.

Following an exhaustive and comprehensive process of searching and screening, including a range of bibliographic databases and websites, we identified a total of 407 separate studies meeting our inclusion criteria. Each of these studies provides evidence regarding the effect of one or more government policies on income inequality, in one or more low- or middle-income countries.

SUMMARY MAP OF EVIDENCE

Figure I gives an overview of the studies included in the review, categorised by the type of government policy (vertical axis) and the study design used (horizontal axis). It makes clear that the majority of the evidence relates to fiscal and trade policy interventions, using either an ex-post observational or ex-ante simulation study design. We did not identify any studies which use a qualitative study design and which address the relationship between government policies and income inequality in sufficient detail to be included in the review.

Figure I: Number of studies by type of policy and study design



Notes: Figures are derived from Tables 2–6 of the main report. Note that some studies focus on more than one type of policy or use more than one study design and are counted twice in this figure; the total number of studies included in the review is 407.

OUTLINE OF EVIDENCE

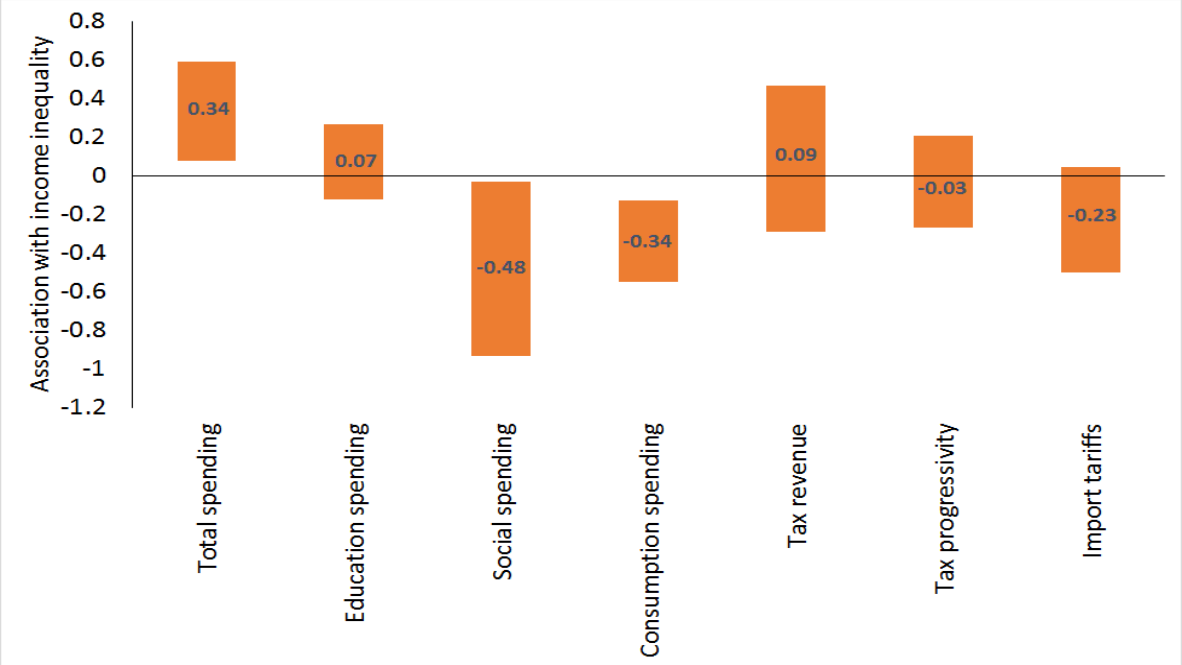
This section provides a more detailed overview of the evidence in this review, organised by study design and broad policy area.

Ex-post observational studies

These studies use econometric analysis to estimate the association between measures of government policy and income inequality. Figure II summarises the evidence from these studies for measures of fiscal and trade policy.

1. We find evidence of a negative and statistically significant association between government spending and income inequality, but only when considering social spending and consumption spending. This is an important finding, indicating the importance of the composition of government spending to the reduction of inequality, and not simply the overall level.
2. By contrast, we find that the association between taxation and income inequality is relatively weak, and not statistically significant. This finding is consistent with evidence from ex-ante simulation studies, and suggests there are limits to the amount of redistribution that can be achieved in low- and middle-income countries via taxation, at least in the short to medium term.
3. We also find no evidence of a statistically significant association between measures of trade policy and income inequality, either positive or negative.

Figure II: Evidence from ex-post observational studies



Notes: The vertical axis shows the direction (positive or negative) and strength of association (values closer to +1 or -1 indicating stronger associations) between each measure of fiscal or trade policy (shown on the horizontal axis) and income inequality. The numbers in each bar indicate the average association across all estimates; the length of the bars indicates the 95 percent confidence intervals around the average association.

Note, however, that it is difficult to say whether or not there is an association between measures of fiscal or trade policy and income inequality on average, since the size and direction of the relationship is affected by a range of other moderating factors.

4. The first is the measure of inequality used: for example, studies using measures other than the Gini coefficient or quintile shares of national income tend to find a smaller negative association between government spending and income inequality.
5. The second is the estimation method: for example, studies using ordinary least squares tend to find a larger negative association between government spending and income inequality, in comparison with studies using more robust analytical approaches, such as panel data methods and instrumental variables estimation.
6. The third is the country composition of the sample: for example, studies including developed as well as developing countries in the sample used for analysis tend to find a larger negative association between measures of taxation and income inequality, in comparison with studies including developing countries only.
7. We also find consistent evidence of publication bias, in that results showing a negative association between government spending or taxation and income inequality appear to be under-reported in the literature. This is an important finding because, unless corrected for, publication bias can lead to significant errors in attempts to summarise empirical knowledge on a given issue. However, although we are able to correct for publication bias, we are unable to pinpoint its precise cause.

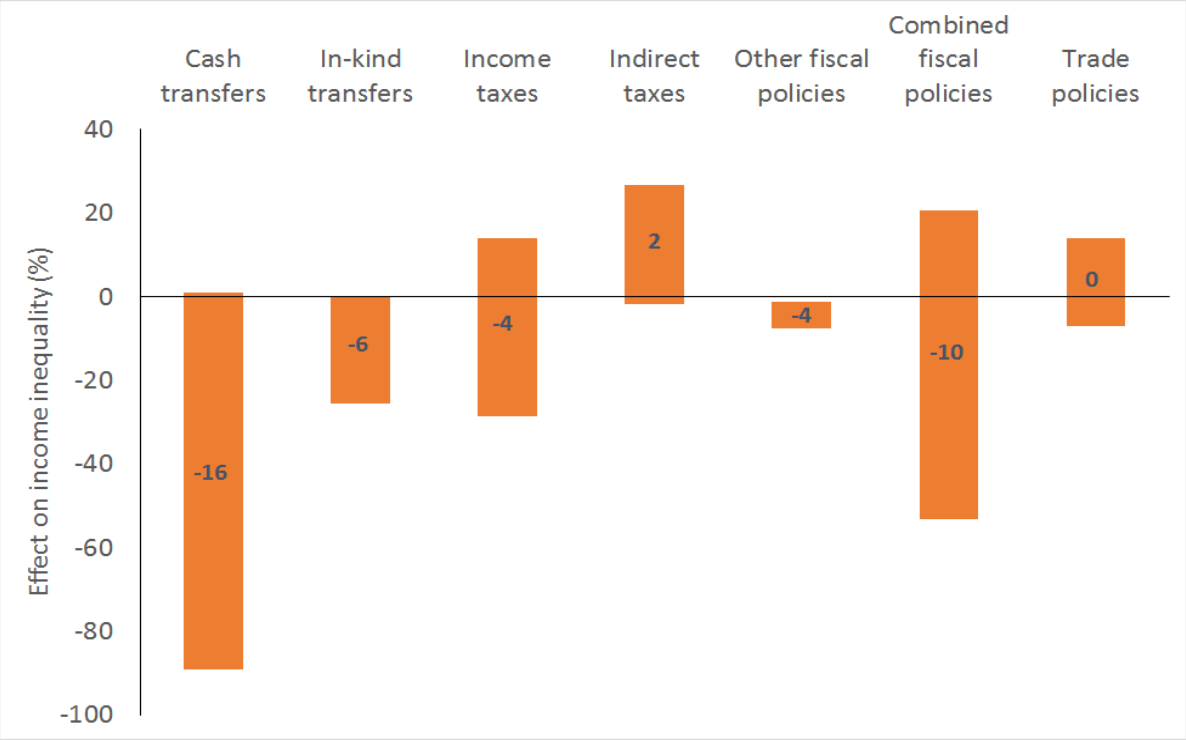
Ex-ante simulation studies

These studies analyse the impact of government policies or spending on income inequality using an economic model applied to recent empirical data for a particular country or region, for example an applied computable general equilibrium (CGE) model. Figure III summarises the evidence from these studies on the effects of fiscal and trade policies on income inequality.

8. Fiscal policy interventions often have a large redistributive effect in developing countries, tending to reduce income inequality, but not in all cases.
9. The redistributive impact of fiscal policy interventions is greater on the spending side of the budget. When considering all regions, the largest reductions in inequality are the result of cash transfer programmes. This is a useful counterpoint to the argument that such programmes are too small to have an impact on income inequality at the national level. However, in some cases the largest contribution to inequality reduction on the spending side does still come from in-kind transfers (e.g. subsidised health, education, housing), particularly in Latin America.
10. The redistributive impact of fiscal policy tends to be smaller on the taxation side of the budget. This suggests that there are structural factors which to some extent limit the amount of redistribution that can be achieved via taxation, particularly in low-income countries. Examples include the smaller share of direct income taxes in total tax revenue, and the higher degree of informality in the economy. Nevertheless, there are some middle-income countries in which direct income taxes do make a significant contribution to the reduction of income inequality.
11. Finally, the evidence from these studies suggests that trade policy reforms typically do not have large impacts on income inequality. This is consistent with the findings of the ex-post

observational studies, which also show little conclusive evidence that trade policies have a large impact on income inequality.

Figure III: Evidence from ex-ante simulation studies



Notes: The height of the bars shows the range of estimated impacts of each policy on income inequality. The numbers in each bar show the average impacts, across all estimates.

QUANTITATIVE CASE STUDIES

These studies use decomposition analysis to analyse the contribution of government taxes or transfers to income inequality, which is comparable with estimates derived from ex-ante simulation studies. The main findings from these studies are as follows.

12. The evidence again shows the large redistributive potential of cash transfers, as evidenced in particular for countries in Eastern Europe. However, the redistributive effect of such programmes in other regions of the world, particularly in Latin America, has been limited by their smaller size as a share of national income.

13. Social insurance programmes (e.g. official pensions, unemployment benefits) are typically found to make a positive contribution to income inequality, often raising inequality by 10 percent or more. Reducing the size of this contribution could therefore make an important contribution to the reduction of income inequality.

RESEARCH GAPS

A number of gaps in the literature were identified. Research has tended to focus predominantly on the effects of government fiscal policies on income inequality: for example, the role of taxes and transfers, and government spending more generally. Trade policy interventions, including the

liberalisation of tariff and non-tariff barriers to trade, are also well covered. But there is much less evidence on the impact of other sorts of government policies, such as labour market reform, pension reform, privatisation, and land reform.

Research has also tended to focus predominantly on middle-income countries. This may simply be the result of data availability, but it is problematic from the point of view of drawing policy implications in low-income countries. In addition, there is a clear reliance on the Gini coefficient as a measure of inequality. This is a potential problem because the Gini coefficient is much more sensitive to inequalities in the middle part of the income distribution than it is to gaps at the top or bottom of the distribution. Other measures, such as the Palma index – the share of the richest 10 percent of households in national income relative to the share of the poorest 40 percent – reflect gaps at the top and bottom of the distribution more closely, and are for this reason often considered more relevant from a policy perspective. These relative gaps in the literature suggest priorities for future research.

Finally, it is worth noting some of the limitations of this review. Because it necessarily focuses on a very wide range of government policies and interventions, and a very large body of literature (over 400 studies), it has not been possible to provide much detail about the underlying processes and mechanisms through which different government policies affect income inequality. Future work could explore this further, for example by synthesising the evidence on the intermediary variables through which the policy interventions covered in this review affect income inequality. In addition, income inequality is just one type of inequality alongside many other important dimensions. This again suggests possible future directions for further synthesis work in this area, which could explore the effects of government policies on inequalities in other important dimensions, such as health or education.

CONTENTS

1. INTRODUCTION

1.1 BACKGROUND

The issue of inequality has been a key issue in development for several decades now. Since the 1970s, a large literature has emerged which documents the many adverse effects of inequality on socio-economic outcomes, including investment and economic growth, poverty, health and well-being, crime, conflict, and social cohesion – see, for example, Easterlin (1974), Williams (1984), Alesina and Perotti (1996), Ravallion (1997), Barber (2001), Luttmer (2005), Eibner and William (2005), Veenstra (2005), Subramanian and Kakawi (2006), Clark et al. (2008), Gravelle and Sutton (2009), Wilkinson and Pickett (2009), Stiglitz (2013), and Ostry et al. (2014). In addition, all societies share a basic, intrinsic concern for equity and justice, and high levels of inequality often conflict with those notions – as, for example, when life chances or opportunities differ significantly between groups defined by gender, inherited wealth, ethnicity, or other accidents of birth (World Bank 2005).

Inequalities are observed in various dimensions, including income, wealth, health, education, and access to employment, in each case within and between countries. This systematic review focuses in particular on inequalities in income within countries. Income is of course only one dimension of welfare, and does not always correlate highly with other important dimensions such as health and well-being. However, it is sufficiently important to merit detailed examination in its own right. In addition, although globalisation makes inequality across countries ever more relevant, within-country inequality is arguably still the most important because a person's own country represents the main social milieu that they interact with and compare with. It is also the dimension of inequality that national governments typically have more direct influence on.

Evidence on the extent of income inequality within low- and middle-income countries has become increasingly available since the 1970s, with the growing body of good-quality, nationally representative household surveys measuring income, or its commonly used proxy, consumption expenditure. Currently, there is no clear overall trend: there are almost as many countries where income inequality is increasing as there are countries where it is decreasing (Olinto and Saavedra 2012). One of the biggest rises in income inequality has occurred in China, where the Gini coefficient increased from around 30 in the early 1980s to around 45 in 2005; income inequality also increased in Vietnam, Indonesia, and Mongolia (ibid.).[1] But in Latin America, the dominant trend over the past decade has been one of falling income inequality – including Brazil, Argentina, Mexico, Peru, and Ecuador (Lustig et al. 2013a).[2]

Nevertheless, income inequality remains high in a large number of developing countries. Over 50 low- and middle-income countries have Gini coefficients of income inequality exceeding 40, above

[1] The (relative) Gini coefficient is one of the most widely used measures of income inequality; it varies between 0, indicating perfect equality of income, and 100, indicating perfect inequality. Other measures of income inequality are discussed further below.

[2] Note, however, that the verdict of falling inequality in Brazil does not hold if an absolute notion of inequality is adopted, where actual income gaps between individuals are taken into account (Anderson and Esposito 2014).

which its potential to undermine progress in key development outcomes, and/or to conflict with basic notions of equity and fairness, is considered significantly greater (e.g. Birdsall 2007). The two developing country regions traditionally associated with high levels of inequality are Latin America and sub-Saharan Africa (Deininger and Squire 1996). However, in the Asia and Pacific region there are now 14 countries with Gini coefficients above 40, including Malaysia, the Philippines, Thailand, Singapore, and Sri Lanka, as well as China (Zhuang et al. 2014).

In addition, recent research has indicated the importance of reductions in income inequality within countries to meeting the new global target of reducing absolute $1.25-a-day poverty to less than 3 percent of the world's population by 2030; economic growth is by itself unlikely to meet this goal (e.g. Basu 2013, Yoshida et al. 2014, Lakner et al. 2014, World Bank 2015). This target was adopted by the World Bank in 2013 and is expected to be adopted by the United Nations in 2015, as part of the new sustainable development goals (SDGs). According to Lakner et al. (2014), for example, with no change in the distribution of income, the global poverty headcount will reach between 5 and 7 percent in 2030, depending on the precise assumptions made about the average rate of economic growth. However, if the incomes of the poorest 40 percent of households grow 2 percentage points faster than average national income – so that the share of the poorest 40 percent in national income rises – global poverty would fall to below 3 percent in 2030, under a range of plausible assumptions about average rates of economic growth.

For several reasons, therefore, inequalities in income are a key policy issue in a large number of low- and middle-income countries around the world. There is a clear demand from policy-makers in national governments and international organisations for accurate, reliable, and up-to-date evidence as to the sorts of policies and interventions that can be used to reduce income inequality, and also which policies and interventions may (in the absence of complementary, offsetting measures) raise income inequality.

1.2 AIMS OF THE REVIEW

The overall aim of this systematic review is to identify and synthesise the existing empirical evidence about the association between government policies and within-country income inequality. The more specific objectives are as follows:

1) to map the available evidence that seeks to evaluate or better understand the effects of government policies and interventions on income inequality, in low- and middle-income countries;

2) to establish whether any particular types of policies or interventions tend to reduce or increase income inequality on average: in other words, whether there are any consistent and generalisable findings or results across contexts and methods;

3) to explain heterogeneity in the estimated effect of government policies or interventions on income inequality, across countries, regions, or over time ('structural' heterogeneity), or research methods used ('method' heterogeneity);

4) to understand better the processes and mechanisms through which government policies and interventions affect income inequality.

The first aim is to map the research field. By mapping, we simply mean identifying and documenting all of the evidence relevant to the review question, and categorising the available evidence according to key descriptive information, namely:

- the country (or countries) of focus
- the type of government policy or intervention
- the method(s) used to assess the impact on income inequality
- the measure(s) of income inequality used.

Mapping is a useful output in its own right. In the words of Gough et al. (2013, p 16):

> Systematic maps of research fields can also highlight gaps in research. [They] can be used to compare policy and practice on the ground with what has been studied in research; they may reveal that only a specific sub-set of policy and/or practice has been studied.

Through mapping the research field, therefore, we aim to provide an important resource for researchers working on income inequality, and organisations involved in the commissioning of research on income inequality – for example, indicating types of government policies where evidence of impacts on income inequality is relatively scare. As noted below, mapping is a particularly important component of this review, given the very broad nature of the underlying question.

The second and third aims both relate to the synthesis of the evidence. We aim to establish whether any specific government policy interventions are associated with reductions or increases in income inequality on average, and to explain any heterogeneity in the estimated effects of particular policies on income inequality, by structural characteristics or by research method. For example, do some types of policies work more effectively in some contexts rather than others – for example, in middle-income compared with low-income countries? This will be done using meta-regression analysis (MRA), applied to studies which use an appropriate counterfactual in assessing the relationship between different sorts of government policies and income inequality. The aim of MRA is to provide a reliable and objective way of summarising conflicting research findings (Stanley and Doucouliagos 2012). Even the best, most rigorous empirical research will produce a wide variation in estimates of a particular relationship, or the effect of a particular policy intervention. Without a reliable way of summarising the results of such research, informed policy actions are impossible (ibid.). MRA uses multiple regression analysis to uncover the reasons why estimates vary, and to correct for some of the biases that can result in the way that policy-relevant relationships are estimated and reported. It is for this reason widely regarded to be an essential part of evidence-based policy-making.

The final aim of the review is to understand better the processes and mechanisms through which government policies and interventions affect income inequality. This will involve synthesising the results of detailed case studies of income inequality in particular low- or middle-income countries, or particular regions within such countries. Such studies allow us to explore in detail the various assumptions in our conceptual framework about the ways in which government policies affect inequality, and to identify and explore any unanticipated effects.

1.3 SCOPE OF THE REVIEW

Many of the systematic reviews that have been carried out in the field of international development to date have focused on a specific policy intervention or a narrowly defined set of policy interventions; examples include microcredit programmes, conditional cash transfers, school feeding programmes, and farmer field schools (White and Waddington 2012, Table 1). This systematic review, by contrast, does not focus on a specific policy intervention, nor on a narrowly defined set of policy interventions. Instead, it potentially includes *any* government policy intervention associated with income inequality.

The sorts of policies and interventions which can affect income inequality are recognised to be broad. Perhaps most obviously, governments influence the level of income inequality through fiscal policy – that is, through choices with regard to the level and structure of taxes on the one hand, and the level and composition of government spending on the other. But governments also undertake a range of other policies which can also affect income inequality (Martinez-Vazquez 2004, Claus et al. 2012). It is widely accepted, for example, that trade policy – choices with regard to the level of import tariffs and quotas, export taxes and subsidies, and so on – can have a large impact on income inequality (Anderson 2005). Other policies that are recognised to have significant direct and indirect potential impacts on the distribution of income include price controls, minimum wages, interest rate controls, land reform, anti-discrimination legislation, affirmative action, regional policy, and various other interventions designed to promote 'employment-friendly' growth (Martinez-Vazquez 2004, Rhee et al. 2014). Macroeconomic policies, such as monetary and exchange rate policy, and the overall fiscal balance, can also have a significant impact on income distribution, for example through tackling high inflation, which can have a regressive impact (Martinez-Vazquez 2004).

This broad nature of the review gives rise to the danger that the range of policies and interventions covered by the review will be too diverse, preventing meaningful and interesting comparisons of the effects of similar types of policies and interventions across different countries and contexts. We respond to this danger by dividing the review into two main stages. The first stage – the 'mapping stage' – involves identifying and documenting all studies looking at the impact of *any* government policy or intervention on income inequality (subject to meeting various other inclusion criteria, discussed further below). The second stage – the 'synthesis stage' – involves synthesising the results from a particular subgroup of the studies identified in the mapping stage, namely all studies looking at the effects of **fiscal policy or trade policy interventions** on income inequality. The results of the mapping indicate that there is a sufficiently large body of comparable studies on these policy areas which can be subjected to meaningful synthesis using meta-regression analysis. By contrast, for most other intervention types (e.g. finance or labour market reforms), the number of studies is too small to allow meta-analysis, which can only be applied if there is a sufficiently large body of comparable studies which all relate to a particular type of policy or intervention.

We also restrict the synthesis to studies focusing on **income inequality at the national level**, as opposed to subnational level (e.g. within regions, or within urban or rural areas), to further reduce the heterogeneity of studies. The majority of studies do this, but some look only at inequality at the level of regions within a country (e.g. states or provinces), or in urban or rural areas, or sometimes at a very localised level (e.g. the village). Inequality at these subnational levels is not directly comparable with inequality at the national level (nor are they comparable among themselves).

The two-stage approach adopted in our review – mapping the research field, as a prior stage to synthesis – has been an important part of many systematic reviews. In the words of Gough et al. (2013, p 16):

> The studies contained within a research field may be too numerous or heterogeneous for meaningful synthesis; it might be methodologically too difficult or just take too much time. The map provides an opportunity to select a sub-group of studies for synthesis. This can involve undertaking a single synthesis based on a narrowed review question and set of inclusion criteria; or undertaking a series of syntheses … Syntheses can also be restricted to studies employing specific research methods.

It is also worth stressing that studies looking at policies other than fiscal and trade policy, and at inequality at the subnational level, will remain documented in our review, as part of the mapping exercise.

1.4 CONCEPTUAL FRAMEWORK

In the Protocol to this review, we set out a broad conceptual framework which showed the range of government policies and interventions that can affect income inequality, and the main transmission mechanisms involved. In Appendix 1 of this final report, we set out a more condensed framework, focusing on the two broad policy areas that form the basis of the synthesis stage of our review, namely fiscal policy and trade policy. In this framework, we show how fiscal and trade policy interventions can affect inequality in four different concepts of income, namely:

- **market income**, meaning income from all 'private' sources, such as wages and salaries, profits from owned enterprise, investment earnings, private transfers (e.g. remittances), and private pensions, *before* taxes have been subtracted and transfers have been added;
- **disposable income**, defined as market income *after* direct income taxes have been subtracted and direct cash transfers have been added;
- **real income**, defined as disposable income after taking into account the prices of goods and services consumed, including the effects of indirect taxes and subsidies;
- **final income**, defined as real income plus the value of in-kind transfers and publicly provided goods and services received, in areas such as health, education, water, and sanitation.[3]

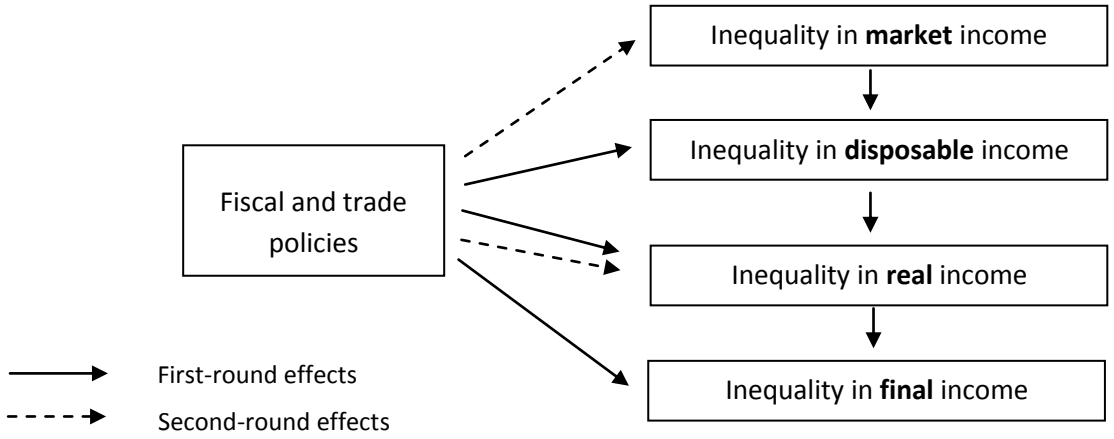
This is shown in Figure 1.

We also distinguish between the short-run or 'first-round' effects of fiscal and trade policy interventions on income inequality, and the medium-term 'second-round' effects that occur as a result of households' behavioural responses to the interventions, which in turn have potential spill-over or knock-on effects throughout the rest of the economy. A key issue when it comes to assessing the evidence on the effects of fiscal and trade policy on income inequality is whether and to what

[3] These income definitions closely match those used by other researchers in this field, such as Lustig (2011), Hemming and Hewitt (1991), Goni et al. (2011), Castelletti (2013), and Ostry et al. (2014). They are discussed in more detail in the Protocol to this review (see pages 7–9 in particular). Note that we depart slightly from the Protocol in this final report by using the term 'real income' rather than 'post-fiscal income' for the third concept; this is for reasons discussed in Appendix 1.

extent studies take into account the second-round, indirect effects. For example, Goni et al. (2011) show that direct income taxes and cash transfers tend to reduce inequality in disposable income in Latin America, even though the size of the impact is quite small in comparison with Western Europe. In particular, the Gini coefficients of disposable income in Argentina, Brazil, Chile, Colombia, Mexico, and Peru are on average 2 percentage points lower than they are for market income, compared with 15 percentage points lower in Western Europe. However, these estimates refer only to the direct, 'first-round' impacts of direct taxes and transfers on inequality; it is not immediately clear whether the same conclusions hold when taking into account the potential 'second-round' effects.

Figure 1: Basic conceptual framework



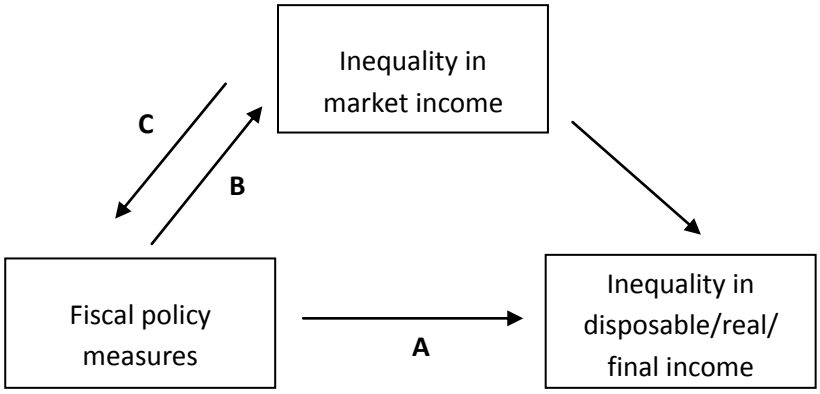
Notes: Note that not all fiscal and trade policy interventions affect all four measures of income; some affect only one or two. This is discussed further in Appendix 1, which distinguishes different types of fiscal and trade policy interventions.

The framework shown in Figure 1 assumes that fiscal and trade policy interventions have an impact on income inequality, but not vice versa. However, there is an argument that countries with higher levels of inequality in market income tend to engage in more redistributive activity (Meltzer and Richard 1981, Alesina and Rodrik 1994, Persson and Tabellini 1994). The idea is that when market incomes are unequal, governments face political pressures to redistribute income. In a democratic system, for example, a larger share of the population will stand to gain from income taxes and transfers, and a political majority emerges in favour of redistribution. Even in a non-democratic system, similar effects may operate, for example through popular mass protest in support of redistributive political movements. Some evidence in support of this argument has been found by Milanovic (2000) and Ostry et al. (2014), although it has been challenged by other authors (e.g. Perotti 1996, Benabou 2000, de Mello and Tiongson 2006), who argue that governments in countries where inequality is high tend to redistribute less, because political power is firmly in the control of higher income groups, who are able to resist any redistributive measures.

Thus, although the evidence remains mixed, it is clear that the direction of causality between fiscal policy interventions and income inequality can run in both directions. This is shown in Figure 2. On the one hand, fiscal policy interventions affect inequality in disposable, real, or final income, holding the level of market inequality constant (Arrow A). Fiscal policy can also affect inequality in market

income (Arrow B). On the other hand, inequality in market income can affect the use of fiscal policy (Arrow C).

Figure 2: Fiscal policy and income inequality: a two-way relationship



From the viewpoint of policy, the main interest is in the causal effect of fiscal policy measures on income inequality. Nevertheless, it is clearly important to be aware of the possibility of reverse causation, for two reasons. First, it helps highlight some of the circumstances which make national governments more or less willing to pursue redistributive policy measures. This is of interest to non-government actors in both developing and developed countries, including aid donors, domestic and international NGOs, and other civil society organisations seeking to influence policy and engage national governments in constructive dialogue around the issue of income inequality. Second, ignoring the possibility of reverse causation may lead to biased estimates of the causal impact of fiscal policy measures on income inequality. This is particularly so for cross-country econometric studies, where it is associated with the so-called 'endogeneity' problem.

In summary, although the precise question of this systematic review refers to the *association* between government policies and income inequality, we try to be clear whether and to what extent the evidence is indicative of the *causal impact* of fiscal and trade policies on income inequality, as opposed to the mere association, which could be explained by effects in either direction.

1.5 IMPORTANCE OF THE REVIEW

As noted in Section 1.1, inequalities in income and other dimensions are a key policy issue in a large number of low- and middle-income countries around the world. There is a clear demand from policy-makers for accurate, reliable, and up-to-date evidence as to the sorts of policies and interventions that can reduce income inequality, and also which policies and interventions may (in the absence of complementary, offsetting measures) raise income inequality.

There is of course already a very large literature on income inequality and development. This includes the large and still increasing number of studies on the effects of inequality on development outcomes (see references in Section 1.1 above), but there is also a large literature looking at the determinants of income inequality, much of it using cross-country econometrics. Much of the early work focused on testing the Kuznets curve, the hypothesis that inequality first rises and then falls with economic development (e.g. Ahluwalia 1976), although subsequent research has tested a much

broader range of factors, including a country's resource endowments, its openness to trade, the level of government spending, the nature of credit markets, and the level of financial development, democracy, good governance, and so on (e.g. Bourguignon and Morrison 1990, Edwards 1997, Li et al. 1998, Leamer et al. 1999, Spilimbergo et al. 1999, Barro 2000, White and Anderson 2001, Dollar and Kraay 2002, Lundberg and Squire 2003).

Despite this very large literature, there is still arguably no clear consensus about the sorts of policies and interventions that governments can use to reduce income inequality. The results from the econometric literature are often inconclusive, particularly for variables closely related to government policy choices, such as the level of government spending or barriers to international trade.[4] The recent study by Dollar et al. (2013), for example, based on a large cross-country dataset, finds no robust evidence that measures of pro-poor government policies (e.g. government spending on health and education) have any impact on the share of the poorest 40 percent of households in national income, which is one common measure of income inequality. However, other recent studies have found evidence that government spending on social welfare, education, and health has a significant negative effect on income inequality, for example Martinez-Vazquez et al. (2012), Claus et al. (2012).

There has to our knowledge been only one *systematic* review of the evidence on the determinants of income inequality. This is the study by Abdullah et al. (2015), which compares the results of 64 mainly cross-country econometric studies looking at the effects of education on income inequality.[5] Their analysis shows that indicators of education do on average have a negative effect on income inequality, and that the heterogeneity of results can be explained – via meta-regression analysis – by a combination of differences in econometric specification, and differences in measures of inequality and education. However, this study focuses only on the effects of educational attainment on income inequality, and does not relate more specifically to the association between government policies and income inequality, since educational attainment is influenced only partly by government policy choices.

For various reasons, therefore, there appears to be a clear need for a systematic review of the evidence relating to the relationship between government policies and interventions and income inequality in low- and middle-income countries.

[4] In the words of Killick (2002, p. 5): 'A substantial number of interventions [for reducing inequality] have been identified … but none are new and not all are of great proven potency. But it would be wrong to conclude that there is little that can be done, as there are many examples of diminishing income disparities.'

[5] There have, by contrast, been a number of systematic reviews of the evidence on the determinants of economic growth, including Doucouliagos and Paldam (2008, 2009, 2014) on the effects of foreign aid, and Josheski (2011) on the effects of infrastructure investment.

2. METHODS

2.1 CRITERIA FOR INCLUDING STUDIES IN THE REVIEW

TYPES OF STUDIES

Study designs: We include studies using any one of four main research methods:

a) ex-post observational studies, using econometrics
b) ex-ante simulation studies
c) quantitative case studies, using decomposition analysis
d) qualitative case studies, which draw on primary data.

An extended discussion of these study designs is provided in Appendix 2. Their main features are as follows.

Ex-post observational studies (Study Design A) use econometric analysis to estimate the effects of government policies on income inequality. They involve estimating a regression in which a measure of income inequality is the dependent variable and the explanatory variables include one or more measure of government policy. The vast majority of studies using this approach are cross-country econometric studies using panel data, although there are also some single-country studies using time series analysis.

Ex-ante simulation studies (Study Design B) analyse the impact of government policies or spending on income inequality using an economic model applied to recent empirical data for a particular country or region, for example an applied computable general equilibrium (CGE) model. The model contains a set of assumptions about how households and firms respond to government interventions, and can be used to simulate the impact of those interventions on different measures of income inequality.

Quantitative case studies (Study Design C) use decomposition analysis to analyse the contribution of government taxes or transfers to income inequality. This can also provide an estimate of the effect of taxes or transfers on income inequality, which is comparable with estimates derived from ex-ante simulation studies.

Qualitative case studies (Study Design D) make use of primary data collected by the researcher, such as small-scale household surveys, focus group discussions, and semi-structured interviews. Unlike the quantitative study designs, they do not attempt to establish a counterfactual and cannot therefore be used to assess impact; they can, however, be used to shed light on the processes and mechanisms through which government policies and interventions affect income inequality.

With these four study designs we aim to include a diverse range of methods in our review. One important goal of the synthesis will be to compare the results from the ex-post observational studies (e.g. cross-country econometrics) with ex-ante simulation studies (e.g. CGE models). While ex-post studies are often preferred since they represent external data validation, cross-country econometric studies unavoidably work at a high level of aggregation and as a result provide little evidence on the effects of specific policies on income inequality – a change in a key tax rate or import tariff, for

example. By contrast, CGE and other simulation-based studies are able to analyse much more specific policy changes. We aim to show how the results of studies using ex-ante simulation compare to those using econometrics, and to discuss the likely reasons for any systematic differences between these two different research approaches.[6]

Note, however, that we are still excluding many types of studies from our review, in particular:

theoretical studies which contain no analysis of actual empirical data, and instead contain only purely theoretical derivations, or numerical simulations using hypothetical data;

review articles which summarise or synthesise existing research on income inequality, but which do not present any new evidence;

quantitative case studies which report and discuss quantitative data on income inequality, but which do not analyse these data using econometrics, applied simulations, or decomposition analysis;

qualitative case studies which rely on narrative methods to assess impacts of government policies on inequality, and which do not make use of primary data.

Publication status: We include published and unpublished studies, including refereed and non-refereed journal articles, working papers, conference proceedings, book chapters, government reports, NGO reports, and other technical reports. We exclude comments and media briefings, review articles, and dissertations (PhD and MA). The exclusion of dissertations is mainly due to time and budgetary constraints: although our searches did identify a number of dissertations which are potentially relevant to the review, these are on the whole not available electronically. The financial and opportunity costs of obtaining hard copies of each dissertation for full text screening would therefore be very high and detract from the review and synthesis of the other publication types.

Time-frame: We restrict the review to studies published since 1990. This is mainly on the grounds that reliable, cross-country data on income inequality have only been available since the early 1990s, so that any studies before this date would not meet basic requirements in terms of data quality. In addition, studies published before the 1990s are generally not available electronically; this again drives up the financial and opportunity costs of the screening process.

Language: We include studies published in English, Portuguese, and Spanish.

TYPES OF PARTICIPANTS (POPULATION)

The review is restricted to studies of low-income countries (LICs) and middle-income countries (MICs) at the time of the government intervention; studies which focus only on high-income countries are excluded. The World Bank definitions of LICs and MICs are used in applying this criterion. In the Protocol to this review we listed three groups of countries:

[6] We are not the first to include ex-ante simulation studies (e.g. CGE models) in a systematic review. For example, the DFID-funded systematic review on the effects of trade liberalisation on employment and fiscal revenue by Cirera et al. (2011) specifically included such studies, alongside more conventional econometric analysis; the study by McCorriston et al. (2013) on trade liberalisation and food security also took this approach.

a) those which have always been low or middle income, since classifications began
b) those which have been low or middle income in some years but not all
c) those which have always been high income.

Studies of countries in group (a) are always included, while studies of countries in group (c) are always excluded. Studies of countries in group (b) are included if the intervention being studied took place while the country was a low- or middle-income country.

TYPES OF INTERVENTIONS

This review is not restricted to any one type of policy or intervention; all government policies and interventions are relevant to the review. We include studies of policies and interventions by any level of government, including local, state, and national. We do, however, exclude any studies of interventions by non-government and private sector organisations; for example, we exclude studies of microcredit programmes operated by non-governmental organisations. We also restrict our attention to 'unilateral' government policy interventions; we exclude studies of bilateral or multilateral policies, such as the forming of a free trade agreement with trading partners, or joining the World Trade Organization (WTO). In addition, while the review as a whole is not restricted to any one type of policy intervention, the synthesis is restricted to fiscal and trade policy interventions.

The precise measures of government policies included in the review differ somewhat according to the study design (see Appendix 2). For Study Design A, we require that the regression analysis includes one or more explanatory variable which is clearly and closely influenced by government policy. We refer to these as 'policy variables': examples include government spending on health, education, or social welfare, as a share of GDP, the average tax rate in the economy, and the average level of import tariffs. We exclude econometric studies that only look at the broader determinants of income inequality which are not clearly and closely influenced by policy; examples include international trade or foreign direct investment as a share of GDP, inflation, and the underlying institutional environment, such as control of corruption, political stability, and so on.

For Study Design B, we include studies which simulate the effect of a change in a variable that is directly controlled by the government. We refer to these as 'policy simulations'; examples include a change in the rate of income tax or VAT, or in the level of a cash transfer. We exclude any studies where the simulations refer only to the effects of external or internal shocks on income inequality; examples include a change in a country's terms of trade, or an increase in productivity. Studies using Study Design C (decomposition analysis) are limited to those which include estimates of the contribution of government taxes or transfers to income inequality. For Study Design D, we simply require that the study addresses in depth and details the processes and mechanisms through which one or more government policy or intervention affects income inequality.

2.1.4 TYPES OF COMPARISON GROUPS

The control or comparison group for assessing the impact of government policies and interventions is constructed using either an ex-post observational approach or an ex-ante simulation-based approach. The former involves comparisons of inequality across countries and/or over time, using panel data. The latter involves comparisons of the observed level of inequality in a country before a

particular intervention and the simulated level of inequality after the intervention. We also include studies focusing on income inequality in regions or states within a country, as well as those that focus on income inequality at the national level. Thus the unit of analysis may be the country as a whole, or a region or state within the country. However, the synthesis is restricted to studies focusing on income inequality at the national level.

TYPES OF OUTCOME MEASURES

We include studies that focus on inequality in a comprehensive measure of income that includes income from all sources (e.g. wages and salaries, business profits, investment earnings, rental income, transfers); we exclude any studies that focus on inequality in one source of income (e.g. wages). We also require that data on income or expenditure be drawn from a representative household survey covering all of the relevant population. We exclude any estimates which are derived from the national accounts, or from household surveys that cover only a subset of the relevant population.

We include studies that focus on any of the four definitions of income set out in Section 1.4, namely market, disposable, real income, and final income. We also include studies which focus on inequality in total consumption expenditure, since the latter is often considered to be a more reliable indicator when data on income are difficult to collect.

We also include any measure of inequality in income or consumption expenditure. This includes:

- global measures, being those which seek to capture the dispersion across the whole distribution and utilize all values of the underlying indicator (e.g. income) in its evaluation. Examples include the coefficient of variation, the relative mean deviation, the variance, the Gini coefficient, the Atkinson family of measures, and the generalized entropy class of measures (e.g. Theil index, mean log deviation);

- partial summary measures, including i) percentile ratios, such as the ratio of the 90th to the 10th percentile of the income or expenditure distribution, or the ratio of the 90th percentile to the median of the distribution; ii) income shares, such as the share of the poorest 10 percent, 20 percent or 40 percent of households in national income; and iii) income share ratios, such as the ratio of the income share of the richest 20 percent of households to that of the poorest 40 percent (the so-called Palma index).

In each case, inequality may be measured across households or individuals; in the former case, average household income or expenditure may be expressed per capita or per adult equivalent.

We only include studies estimating the impact of government policies on measures of overall inequality across individuals or households, and exclude studies of income inequality between spatial units within countries (e.g. states, provinces, regions), studies of income gaps between urban and rural areas, and studies of income gaps between demographic groups, as defined, for example, by gender or ethnicity. In addition, the focus of this review is on the size distribution of income, not the so-called functional distribution of income. While the former refers to the distribution of income across individuals, the latter refers instead to the share of national income received by each factor of

production, that is labour, capital, land, and so on. We exclude any studies which only report impacts of government policies on the functional distribution of income.

Finally, certain measures of the size distribution of income are related to inequality but do not themselves constitute measures of inequality. Examples include measures of:

- relative poverty, such as the percentage of the population with incomes less than 50 percent of mean or median income;
- the size of the middle class, such as the percentage of the population within a certain range of median income, or the share of the middle three quintiles in national income;
- income polarisation, which refers to the amount of 'between-group' dispersion of income relative to 'within-group' dispersion, with the groups themselves defined by income – see, for example, Duclos et al. (2004).

We exclude any studies which focus only on one of these indicators and none of the global or partial summary measures of inequality listed above.

2.2 SEARCH METHODS

In order to select appropriate databases for this review we followed the Campbell Collaboration guide on key online databases for systematic reviews in international development (Campbell Collaboration 2012). This list was complemented with additional databases and websites used by other systematic reviews on questions relevant to this review. The electronic databases that were searched for relevant studies are listed in Appendix 3. We also reviewed relevant websites of key institutions and conference proceedings; a full list is contained in Appendix 3.

Each database was searched using a combination of the search terms indicated in Table 1. This shows three sets of concepts (A, B, and C), each of them containing a list of associated terms or synonyms that were used in our search. When using foreign language databases, each of the terms was translated into the appropriate language (i.e. Portuguese or Spanish). Due to the fact that some search engines only allow a limited number of operators, two search query strings were used: a long version and a short version. The long version follows the equation:

A + (B W/n C)

Thus the terms within columns A, B, or C in Table 1 were combined with 'OR', columns B and C were combined with the proximity operator *W/n*, (where *n* is the number of words that separate the terms from the two columns) and column A was combined with the combination of B and C using the *AND* command.[7] The precise search terms used for each database are listed in Appendix 3.

[7] Our strategy is to use n=1 to capture concepts such as 'distribution of income', 'inequality of income', as well as 'income distribution' and 'income inequality'.

Table 1: Key terms for search strategy

A	B	C
Policy	**Income**	**Inequality**
Polic*	Income*	*Equal*
Intervention*	Expenditure*	*Distribut*
Program*		Disparit*
Instrument*		Differen*
Tool*		Gap*
Reform*		*Equit*
Legislation*		Share*
Govern*		Ratio*
		Gini

Notes: * is included as a truncation symbol to capture automatically conjugated forms of each word; thus *equal* captures 'inequality' as well as 'inequalities'; *distribut* captures 'distribution' as well as 'redistribution'.

In addition to these electronic searches, we carried out some searches using handsearching. First, we checked all of the full text reports identified for another related systematic review that we are carrying out, on income poverty.[8] Second, we checked all of the chapters of the edited volumes identified by our electronic searches of this review, to see if there were multiple studies relevant to our review from any one particular volume. Third, we checked the reference lists of all published peer-reviewed academic journals identified via our electronic searches to see if there were any other additional searches relevant to our review which we had missed.

2.3 DATA COLLECTION AND ANALYSIS

Data extraction took place in two main stages. In the first stage we extracted descriptive information about all studies meeting the inclusion criteria, in the following four areas:

1. context and population
2. type of intervention
3. study design and methods used
4. outcome measures

Data extracted in this first stage was used for the research mapping exercise, allowing us to provide a descriptive survey of all the evidence relevant to the question, categorising and cross-tabulating the available evidence in interesting ways, for example the overall balance of studies between intervention types, outcome indicators, country groupings, and study designs.

A further extraction stage was used to extract additional information required for the quality appraisal and synthesis (including meta-analysis), in particular:

5. study results and findings
6. quality of research methods.

[8] Anderson E, D'Orey M, Duvendack M and Esposito L (forthcoming) Which policies and interventions have been strongly associated with the translation of growth into reductions in income poverty? A systematic review of the evidence.

As discussed in Section 1.3, only those studies focusing on the effects of fiscal policy or trade policy interventions on income inequality at the national level were selected for inclusion in the synthesis stage. This was designed to avoid the problems stemming from the very broad question of this systematic review. All studies included in the synthesis were assessed for their quality, otherwise referred to as 'risk of bias'.

2.4 DATA SYNTHESIS

We carry out the synthesis and meta-analysis separately for each study design, as was the case in the systematic reviews by Cirera et al. (2011) and McCorriston et al. (2013). For the ex-post observational studies (Study Design A), we use meta-analysis, following the approach taken by Abdullah et al. (2015). Their study examines the impact of education on income inequality using a meta-regression approach. This is discussed further in Section 4 below. For the ex-ante simulation studies, we rely on a combination of narrative synthesis (using descriptive statistics) and some simple meta-regression analysis, following the broad approach used by Hess and von Cramon-Taubadel (2008), who synthesise the results from ex-ante simulation studies (including CGE models) on the impacts of multilateral trade reform. These authors use meta-regression to show how the results of simulations vary according to the different modelling assumptions used. For the quantitative case studies we also rely on narrative synthesis techniques, as suggested in Arai et al. (2007) and Rodgers et al. (2009).

3 RESULTS

3.1 SEARCHING AND SCREENING

The initial phase of the electronic search process consisted of registering the databases which allowed exporting of results to Endnote, and only such databases were considered in this screening phase. Figure 3 summarises the screening process. In total, 19,684 reports were exported into Endnote. After removing 6,146 duplicates, a rapid initial screening process was conducted to exclude studies that dated from before 1990 or were Masters' theses. In the end 11,557 reports were left for screening.[9]

A comprehensive list of inclusion and exclusion criteria was used and it was decided to exclude 9,544 reports based on screening of the abstract. Of the remaining 2,013 reports, 1,709 were obtained in full text, either electronically (soft copy) or in hard copy via the UEA library or Inter-Library Loan (ILL).[10] The remaining 304 studies were not found and were not available via ILL. Of the 1,709 reports in full text, a further 1,417 were excluded in a second phase of screening based on the full text of the article. This left a total of 292 reports which were deemed to meet the inclusion criteria. Following a further round of checking (during the process of data extraction), a further 36 reports were deemed not to meet the inclusion criteria. This left a total of 256 reports which were deemed to meet the inclusion criteria.

In addition to the electronic searches, a number of other relevant databases and websites of key institutions were reviewed. The screening process followed the same approach as for the electronic searches, with the exception that the screening by abstract was conducted entirely online due to the impossibility of exporting the results to Endnote. Here 6,945 studies were screened online and it was decided to exclude 6,641 reports based on screening of the abstract. The remaining 304 reports were then exported to Endnote in order to screen by full text.[11] Of these, 292 were obtained and screened in full text, and the remaining 12 were not found. Of the 292 reports in full text, a further 196 were excluded in a second phase of screening based on the full text of the article. This left a total of 96 reports which were deemed to meet the inclusion criteria. Following a further round of checking (during the process of data extraction), a further 10 reports were deemed not to meet the inclusion criteria. This left a total of 86 reports which were deemed to meet the inclusion criteria.

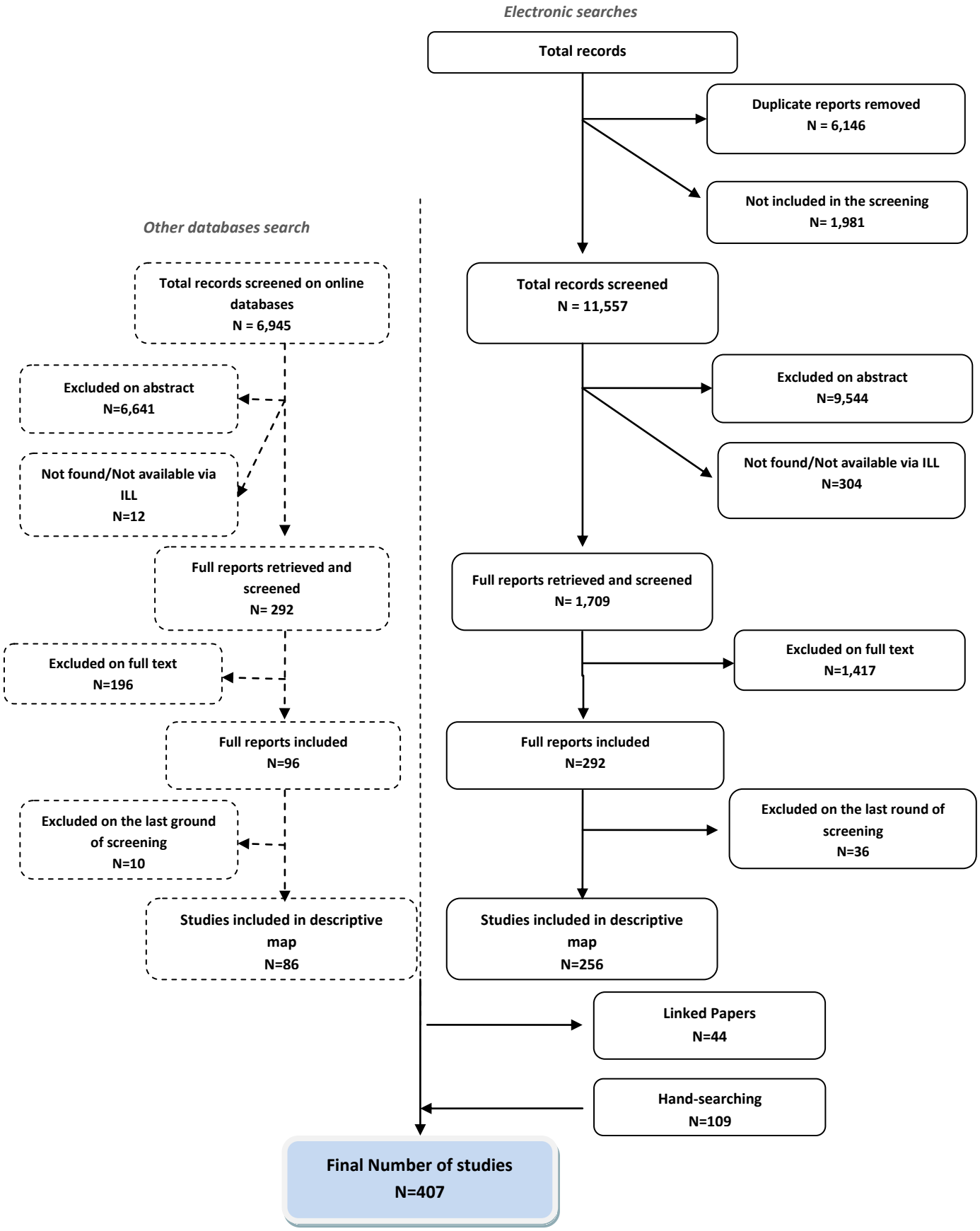
In addition to these searches, we also identified 109 additional studies relevant to the review via handsearching.

[9] The results of the screening process are recorded in a series of Excel spreadsheets, which are available on request from the authors.

[10] A total of 135 ILL requests were submitted.

[11] In order to export the results to Endnote the studies were first exported into Mendeley using the web importer bookmarklet to save references. After saving the studies in the Mendeley library the results were exported to Endnote.

Figure 3: Flow of literature through the review: the PRISMA diagram



In total, therefore, we identified 407 studies which meet our inclusion criteria. In Table 2 below we give a summary of the types of studies included in our review, by the four main study designs, and from each type of search.

Table 2: Included reports by study design

Study design	Electronic searches	Other database searches	Linked papers*	Handsearching#	**Total number of studies**
A	103	44	17	46	**176**
B	110	26	15	53	**174**
C	38	15	12	10	**51**
D	0	0	0	0	**0**
Mixed	5	1	0	0	**6**
Total	**256**	**86**	**44**	**109**	**407**

Notes: *Note that we subtract the number of linked papers when calculating the total number of studies. Two or more linked papers are treated as one study for the purposes of mapping and synthesis. #This figure refers only to new, 'non-duplicate', and non-linked studies that were found via handsearching.

Note that we did not identify any studies which meet our inclusion criteria and use Study Design D. We had initially identified a number of studies with this study design (e.g. Hy Van and Unger 1998, Adger 1999, Enriquez 2003, Sekar and Randhir 2007, Copestake 2008, Van Hue and Scott 2008). These studies all use a case study approach (Peru, India, Vietnam, China, and Cuba), collecting primary data through interviews or small-scale surveys. However, although these studies do all mention income inequality, and typically provide basic descriptive information about levels or trends in income inequality, we felt on further scrutiny that they did not address in sufficient depth or detail the processes and mechanisms through which government policies and interventions affect income inequality. We therefore decided to exclude them.

We did find a small number of studies using 'mixed methods', but these use a combination of different quantitative methods rather than a combination of quantitative and qualitative methods. Four of these studies use a combination of Study Designs B and C, one uses a combination of A and B, and another uses a combination of A, B, and C. These studies therefore feature in the mapping and synthesis for each study design used.

3.2 MAPPING

EX-POST OBSERVATIONAL STUDIES (STUDY DESIGN A)

The results of the mapping exercise for the ex-post observational studies are shown in Table 3. As outlined above, 176 papers were identified from the search process which use Study Design A. Of these, 52.3 percent of the studies have been published in peer-reviewed journal articles, and the majority have been published since the year 2000 (92 percent of the total). The studies after 2008 predominantly draw on the UNU-WIDER database as well as World Bank's WDI but using different subsamples of countries as the basis for analysis. The UNU-WIDER database was first used by some studies in 2001; until then the Deininger and Squire (1996) database was frequently used. The time

periods covered by the studies have an average start year of 1977 and an average end year of 2000, with an average length of 23 years.

Table 3: Results of mapping exercise, Study Design A

	Number of studies	% of total
Source		
Peer-reviewed journal	92	52.3
Working papers, conference proceedings	74	42.0
Book chapters	10	5.7
Publication year		
1990s	14	8.0
2000s	94	53.4
2010 onwards	68	38.6
Country coverage		
Multiple	141	80.1
Single	35	19.9
Analytical approach		
Dynamic panel: GMM estimations	33	18.8
Panel data estimations (fixed or random effects)	44	25.0
IV/2SLS and related approaches	30	17.0
OLS and related approaches	58	33.0
Others	11	6.3
Outcome measure		
Gini	148	84.1
Income shares	23	13.1
Others (Theil index/Atkinson, Watts, EHHI)	5	2.8
Type of policy variable		
Fiscal policy	**123**	69.9
- *Government spending measures*	114	64.8
- *Tax measures*	9	5.1
Trade policy	**24**	13.6
- *Import tariffs*	10	5.7
- *Sachs-Warner index*	7	4.0
- *Others*	7	4.0
Other policy variables	**29**	16.5
- *Financial reform*	4	2.3
- *Labour standards*	2	1.1
- *Others*	23	13.1
TOTAL	**176**	100.0

Note: Many studies use various analytical approaches and multiple outcome measures. The most dominant for each paper is listed here. Similarly, some studies report a number of valid policy variables, and the most prevalent one is reported here.

The majority of studies are cross-country in nature (80.1 percent of the total), covering all regions and including low-, middle-, and high-income countries. The studies focusing on single countries cover 15 different countries (China 7 papers, Brazil 7, Pakistan 6, Indonesia 2, India 3, then 1 paper

each for Mexico, Chile, Malaysia, Thailand, Iran, Kazakhstan, Korea, Russia, Philippines, Taiwan).[12] The most common analytical method is ordinary least squares (OLS), although a substantial number of studies do use instrumental variables (IV), two-stage least squares (2SLS), or panel data methods (e.g. GMM, fixed effects, random effects). The Gini coefficient is used in 84.1 percent of the studies as the outcome variable.

The most common type of policy variable is fiscal policy (69.9 percent of the total) followed by other policy variables (16.5 percent) and trade policy (13.6 percent). The studies within 'other' examine a range of policies, including measures of financial reform, labour standards (e.g. minimum wages), and privatisation. Of the 123 papers focusing on fiscal policy, 114 papers focus on government spending, while 9 focus on taxation. Of the papers that focus on government spending, 84 measure government spending as a share of GDP. This is useful from a meta-analysis viewpoint, since it suggests a relatively homogenous approach to measuring government spending across different studies.

EX-POST OBSERVATIONAL STUDIES (STUDY DESIGN B)

The results of the mapping exercise for Study Design B are shown in Tables 4 and 5. In total, we identified 177 studies which use this study design and which meet all of the other inclusion criteria for this review.[13] Of these studies, 89 are peer-reviewed journal articles, 49 are working papers, 31 are book chapters, 6 are policy reports, and 2 are conference papers (see Table 4). The majority (162 out of 177) have been published since 2000; only 15 studies date from the 1990s, and just under half (87 out of 177) have been published since 2010. This goes some way toward justifying our decision to restrict the review to studies published since 1990.

The majority (151 out of 177) are studies of single countries. The 26 multiple-country studies look at more than one country, although in each case the analysis is carried out separately for each country included in the study (as opposed to cross-country econometric studies which pool data for different countries in the same analysis). The region with the largest coverage is Latin America and the Caribbean (74 studies covering 19 countries), followed by East Asia and Pacific (35 studies covering 6 countries), Eastern Europe and Central Asia (31 studies covering 19 countries), sub-Saharan Africa (29 studies covering 16 countries), South Asia (9 studies covering 5 countries) and Middle East and North Africa (5 studies covering 7 countries). The countries with the largest number of studies are Brazil (23 studies), followed by Mexico (16 studies), China (15 studies), and South Africa (15 studies). A total of 72 countries are covered, of which the majority (52 out of 72) are middle-income countries.[14]

A range of different measures of inequality are used, including the Gini coefficient, the Theil index, the extended S-Gini coefficient, quintile and/or decile shares of the income distribution, percentile ratios, the Atkinson inequality measures, and general entropy measures. Over half of the studies

[12] The study of Taiwan refers to when it was still a middle-income country.

[13] This number includes the 3 studies using Study Design B alongside Study Design A or C (see Table 2).

[14] Note that we measure income group status at the time of the policy intervention covered by the study, not the current status nor the status at the time of publication.

(108 out of 177) in fact report results for more than one measure of inequality. However, virtually all studies (167 out of 177) use the Gini coefficient, either as the only measure of inequality or one of the measures of inequality used. The next most common measures are the Theil index, which is included in 24 studies, and income quintile shares, which are included in 15 studies. In addition, the vast majority of studies (160 out of 177) focus on inequality at the national level, either as the only level of analysis or alongside subnational analysis. This suggests that heterogeneity of outcome measure is unlikely to be a major constraint to the meta-analysis.

The two most common analytical approaches used are standard fiscal incidence analysis (101 studies) and CGE modelling (61 studies). We found only 10 studies using some form of behavioural incidence analysis; a further 5 studies use a range of other modelling approaches, such as a farm household model, or a social accounting matrix. It is worth noting that the vast majority of studies (171 out of 177) use only one analytical approach; we found only 6 studies that use two approaches, and we compare the results between them to see if they generate significantly different findings. Examples include the study by Debowicz and Golan (2014), who compare the effects of the Oportunidades cash transfer programme on inequality in Mexico using both a partial and a general equilibrium approach, and the study by Roelen (2010), who compares the effects of social transfers on inequality in Vietnam, using both standard and behavioural incidence analysis. This suggests that there is a role for synthesis in terms of comparing the results of different analytical approaches.

Finally, each of the studies using Study Design B includes one or more policy simulation. Taking into account the fact that most studies carry out more than one policy simulation for each country, we identified a total of 536 policy simulations from the 177 studies (see Table 5). In terms of the broad policy category, the majority of these simulations were changes in fiscal policy (343 simulations from 128 studies). The next most common were changes in trade policy (126 simulations from 39 studies). The remaining 67 simulations from 18 studies cover a wide variety of other policy areas, including labour market reforms, land reforms, privatisation, and macroeconomic policies, such as exchange rate devaluation.

Table 4: Results of mapping exercise, Study Design B

	Number of studies	% of total
Publication type		
Peer-reviewed journal	89	50.3
Working paper	49	27.7
Book chapter	31	17.5
Policy report	6	3.4
Conference proceedings	2	1.1
Publication year		
1990s	15	8.5
2000s	75	42.4
2010 onwards	87	49.2
Country coverage		
Multiple	26	14.7
Single	151	85.3
*By region**		
LAC	74	41.8
EAP	35	19.8
EECA	31	17.5
SSA	29	16.4
SA	9	5.1
MENA	5	2.8
Outcome measure*		
Gini coefficient	167	94.3
Theil index	24	13.6
Quintile shares of the income distribution	15	8.5
Unit of analysis		
National	160	90.4
Subnational only	17	9.6
Analytical approach*		
Standard incidence analysis	101	57.1
Behavioural incidence analysis	10	5.6
Applied computable general equilibrium (CGE) model	61	34.5
Other approaches	5	2.8
Policy category*		
Fiscal policy	128	72.3
Trade policy	39	22.0
Other policy areas	18	10.2
Total number of studies	**177**	100.0

Notes: *Items under these headings sum to more than the total number of studies because some studies include more than one country, outcome measure, analytical approach, or policy category.

Table 5: Policy simulations by category

Category of simulation	*Number of policy simulations*	*% of total*
Fiscal policy	**343**	**64.0**
Direct taxes (e.g. income tax, corporate tax)	66	12.3
Indirect taxes (e.g. VAT)	39	7.3
Domestic subsidies (e.g. fuel, food items)	18	3.4
Cash transfers (e.g. social assistance, social insurance)	121	22.6
In-kind transfers (e.g. value of education, health services)	23	4.3
User fees	2	0.4
Other government spending (e.g. infrastructure)	14	2.6
Some combination of the above	56	10.4
Trade policy	**126**	**23.5**
Import tariffs	86	16.0
Export subsidies	10	1.9
Export taxes	6	1.1
Some combination of the above	24	4.5
Other	**67**	**12.5**
Energy reforms	26	4.9
Labour market reforms	8	1.5
Land reforms	3	0.6
Pension reforms	7	1.3
Financial reforms	1	0.2
Macroeconomic policies	10	1.9
Privatisation	12	2.2
TOTAL	**536**	**100**

3.2.3 QUANTITATIVE CASE STUDIES USING DECOMPOSITION ANALYSIS (STUDY DESIGN C)

The results of the mapping exercise for the 56 studies using this study design are shown in Table 6. Here, 27 are published journal articles, 17 are working papers, 9 are book chapters, and 3 are policy reports. In total, 10 studies were published in the 1990s, 24 during the 2000–09 period and a further 22 since 2010. It appears, therefore, that this study design retains popularity among researchers as a way of analysing income inequality.

The vast majority of studies (51 in total) are single-country studies; the remaining 5 carry out comparative analysis, in most cases between pairs or small groups of countries. In addition, virtually all studies focus on middle-income countries; the only exceptions are the studies by Osmani and Sen (2012) on Bangladesh, Oyekale et al. (2007) on Nigeria, and Khan et al. (1993) on China, when the country was still classified as low income. In terms of regional coverage, there are most studies for countries in East Asia and Pacific, Latin America and the Caribbean, and Eastern Europe and Central Asia; there is limited coverage of South Asia and sub-Saharan Africa (other than South Africa). The

countries covered most frequently are: China (12 studies), Brazil (9 studies), and South Africa and Mexico (6 studies each).

The majority of studies (49) use the Gini coefficient as the measure of income inequality, either following the three-way decomposition outlined by Lerman and Yitzhaki (1985) or the simpler two-way decomposition outlined by Shorrocks (1982) (see Appendix 2.3). In terms of policy interventions, the majority of studies (51 in total) include estimates of the contribution of one or more government cash transfer programmes to income inequality. A smaller number of studies (8 in total) also provide estimates of the contribution of income taxation to inequality, and in-kind government transfers (4 in total). Finally, 41 studies focus on inequality at the national level; the remaining 15 focus only on income inequality in rural areas or in urban areas.

Table 6: Results of mapping exercise, Study Design C

	Number of studies	% of total
Source		
Journal articles	27	48.2
Working papers	17	30.4
Book chapters	9	16.1
Policy reports	3	5.4
Publication year		
1990s	10	17.9
2000s	24	42.9
2010 onwards	22	39.3
Country coverage		
Multiple	5	8.9
Single	51	91.1
*By income category**		
LICs	3	4.4
MICs	65	95.6
*By region**		
EAP	12	17.6
LAC	21	30.9
EECA	27	39.7
SSA	6	8.8
Others	2	3.0
Outcome measure		
Gini coefficient	49	87.5
Other	6	12.5
Type of fiscal policy*		
Cash transfers (e.g. social assistance, social insurance)	51	81.0
Direct taxes (e.g. income tax, corporate tax)	8	12.7
In-kind transfers (e.g. value of education, health services)	4	6.3
Unit of analysis		
National	41	73.2
Subnational only	15	26.8
TOTAL	**56**	100.0

Notes: *Items under this heading sum to more than the total number of studies because some studies (5 in total) focus on more than one country or type of fiscal policy intervention. Note also that this study design refers only to the effects of fiscal policy interventions on income inequality.

3.3 SUMMARY

Through the search, screening, and mapping exercise we have assembled and mapped out a substantial body of evidence. Through an exhaustive and wide-ranging search process we have identified a total of 407 research studies, all of which provide evidence about the effects of one or more government policy or intervention on income inequality, in one or more low- or middle-income countries.

Simply by mapping the research field in this way, our research has generated some important findings. Research has tended to focus predominantly on the effects of government fiscal policies on income inequality: for example, the role of taxes and transfers, and government spending more generally. To a lesser extent, trade policy interventions, including the liberalisation (i.e. reduction) of tariff and non-tariff barriers to trade, are also reasonably well covered. But there is much less evidence on the impact of other sorts of government policies, such as labour market reforms, pension reforms, privatisation, and land reforms. With regard to the ex-ante simulation studies, for example, trade and fiscal policy simulations account for 88 percent of the total number of policy simulations carried out across the 177 studies we identified using this approach.

Research has also tended to focus predominantly on middle-income countries, particularly for the single-country studies. For example, for the 56 studies we identified using decomposition analysis, 65 of the 68 countries covered are middle income. This may simply be the result of data availability, but it is problematic from the point of view of drawing policy implications in low-income countries. In addition, there is a clear reliance on the Gini coefficient as a measure of inequality. This is a potential problem because the Gini coefficient is by no means the 'best' measure of inequality, nor is it necessarily the most relevant from a policy perspective. In fact, there are many recognised drawbacks with the Gini coefficient as a measure of inequality: for example, it is much more sensitive to inequalities in the middle part of the income distribution than it is to gaps at the top or bottom of the distribution (Cowell 2000).

Since these relative gaps in the literature suggest priorities for future research, our mapping exercise provides an important resource both for researchers and for policy-makers involved in the commissioning of research.

Our mapping exercise also shows that the studies relevant to the review question are very diverse. We deliberately chose not to restrict our search to any one type of evidence, and the 407 studies span a wide variety of different study designs, including cross-country econometric studies, ex-ante simulation studies (e.g. fiscal incidence analysis, CGE models), and decomposition analyses. Even when we disaggregate between these main study designs, there is a high degree of diversity across studies, in terms of the measures of inequality used, the spatial unit of analysis, and in particular the type of government policy or intervention being considered.

This high degree of diversity is challenging from the perspective of synthesis and meta-analysis. Nevertheless, the results of the mapping do indicate that there is a sufficiently large body of

comparable studies on the effects of fiscal and trade policy which can be subjected to meaningful synthesis using meta-regression analysis. By contrast, for most other intervention types (e.g. finance or labour market reforms), the number of studies is currently too small to allow meta-analysis, which can only be applied if there is a sufficiently large body of comparable studies which all relate to a particular type of policy or intervention. As discussed in Section 1.3, therefore, for the purposes of synthesis and meta-analysis we restrict the focus to studies reporting estimates of the effect of fiscal or trade policy on income inequality at the national level. Through these refined inclusion criteria we aim to synthesise studies that are more similar in terms of their level of analysis and policy focus.

4. RESULTS (II): SYNTHESIS, EX-POST OBSERVATIONAL STUDIES

4.1 INTRODUCTION

As discussed in Section 1.3, for the purposes of synthesis and meta-analysis we restrict the focus to studies reporting estimates of the effect of fiscal or trade policy variables on income inequality at the national level. There are a total of 125 studies which meet these more refined criteria, of which 102 studies include estimates of government spending variables on inequality, 18 studies include estimates of tax variables, and a further 26 include estimates of trade policy variables.[15] Some of these studies do not report sufficient information to allow us to calculate our chosen effect size measure (the partial correlation coefficient), which explains why there are slightly fewer studies in the meta-regression analysis (see below). We also exclude a small number of studies which include interaction or quadratic terms, since the coefficients in this case are not directly comparable with those obtained from studies using a linear framework.

We carry out our meta-regression analysis separately for government spending variables (Section 4.2), tax variables (Section 4.3) and trade policy variables (Section 4.4). In each case, we follow the MAER-NET guidelines to report the findings of our meta-regression analysis (see Stanley et al. 2013, p. 393). Our approach also follows Abdullah et al. (2015), who examined the impact of education on income inequality using a meta-regression approach. Before presenting the meta-regression findings, however, we first discuss the results of our risk of bias assessment (Section 4.1.1), our effect size measure (4.1.2), our initial tests for publication bias (4.1.3), and our overall modelling approach (4.1.4).

RISK OF BIAS ASSESSMENT

We adapted the risk of bias tool developed by Duvendack et al. (2011, 2012) for the purpose of our risk of bias assessment. We began by categorising each study by its proclaimed research design and analytical method. Following Duvendack et al., each study was scored depending on its design and analytical approach. In a next step each of these scores was combined in an index. An arbitrary threshold of 2 was applied; that is, a study with a score of equal to or less than 2 was classified as low risk of bias, while a study with a score above 2 was classified as medium risk of bias (Duvendack et al. 2011, 2012, 2014).

The studies included at the synthesis stage can be split into two dominant analytical approaches – 1) panel data techniques and 2) regression-based techniques – and these formed the basis for adapting the risk of bias tool of Duvendack et al. (2011, 2012). Below we provide a brief description of these two broad approaches before presenting the findings of our risk of bias assessment.

[15] Note that some studies report more than one type of estimate and are therefore relevant to more than one synthesis or meta-analysis exercise.

Panel data analysis

Within panel data analysis, generalised method of moments (GMM) and fixed- as well as random-effects models prevail among the included studies. There are also some studies that employ instrumental variables (IV) approaches in the panel data context (e.g. Clarke et al. 2006, Dollar and Kraay 2002, Wagle 2012, and others).

Fixed-effects models are used when the focus is on exploring impacts of variables that change over time. For example, the relationship of predictor and outcome variables across countries is examined where each country has its own individual characteristics that could potentially affect the predictor variables. The underlying assumption of the fixed-effects model is that something within those individual characteristics affects the outcome variables in a fixed way modelled by a difference in the intercept. The fixed-effects model allows controlling for those time-invariant characteristics and thus provides the net effects (common to all countries) of the predictor variables on the outcome variables.

Unlike fixed-effects models, the assumption underlying random-effects models is that the variation across countries is random and uncorrelated with the predictor or independent variables included in the model:

> …the crucial distinction between fixed and random effects is whether the unobserved individual effect embodies elements that are correlated with the regressors in the model, not whether these effects are stochastic or not. (Greene 2008, p. 183)

Random effects are based on less restrictive assumptions than the fixed-effects model and used when the differences across countries believed to influence the dependent variable are drawn from a population of effects with a given statistical distribution.

Fixed- and random-effects models cannot fully account for the numerous biases that may exist even when combined with IV, which is supposedly an improvement of basic fixed- and random-effects models but it suffers from weak instrument bias, which can make the results worse than those of other methods (see Caliendo 2006, Caliendo and Hujer 2005). Thus, more recently, dynamic panel data models have been widely adopted, such as the approach of Arellano and Bond (1991), which is based on the notion that an IV approach does not fully exploit all of the information available in the sample. Thus, in a GMM context it may be possible to construct more efficient estimates of the dynamic panel data model. See Hansen (1982) for more details on GMM, who first developed this estimation technique.

From a risk of bias perspective it is very difficult to judge which one of these analytical approaches is affected more or less by biases; there are other factors that play a role in the risk of bias assessment, such as model specifications (i.e. are adequate control variables included, how well was the method of analysis executed?). (See, for example, the Campbell Collaboration for more guidance: www.campbellcollaboration.org/ID_Resources/Methods_Resources.php.)

Regression-based analysis

Among the regression-based studies, ordinary least squares (OLS) dominate. Cross-country regression-based approaches have been criticised widely (see, for example, Beck et al. 2000, Graff 2002). Jalilian and Kirkpatrick (2002) summarise Beck et al.'s critique of cross-country approaches as follows:

> (i) time series dimension of data is generally ignored; (ii) parameter estimates may be biased because of omission of cross country differences; and (iii) no control for endogeneity of regressors. An additional shortcoming of this approach is that it cannot be used for causal inference. (p. 99)

Jalilian and Kirkpatrick (2002) further argue that advances in analytical approaches such as dynamic panel estimations can correct for the drawbacks of cross-country approaches. Hence, in our risk of bias tool we coded studies using panel data analysis or 2SLS/IV differently to studies pursuing purely regression-based approaches: the former were coded as one, while the latter were coded as 2. As a result, most studies employing panel data analysis were classified as having a low risk of bias, while the regression-based studies were all labelled as medium risk of bias due to their shortcomings as outlined above.

Table 7 presents the results of our risk of bias assessment. Included studies are ranked by research design and analytical method using scores 1–3, where 1 implies low risk of bias and 3 high risk of bias.

Table 7: Distribution of studies by research design and analytical method

		Statistical methods of analysis		
		Panel data analysis (GMM, fixed and random effects), IV, PSM, 2SLS/LIML, DID, RD	**Regression-based approaches/OLS/ error correction models**	**Others (correlations)**
Research design	**Scores**	1	2	3
RCT	1	0	0	0
Pipeline	2	0	0	0
Panel (cross-country/time series)	3	83	38	0

Legend	Low score	83	Medium score	38

Source: Duvendack et al. (2011 and 2014, see 2012 for an adaptation).

Based on an initial literature search, we expected that most of the Study Design A studies would be scored 3 for research design, as the majority use panel data sets, and 1 or 2 for analytical method, as panel data analysis and cross-country regression approaches dominate. This point further motivates the use of the Duvendack et al. tool as it assesses risk of bias by providing a combined score for research design as well as analytical technique. For example, a study might get a score of 3 when using cross-country panel data but can considerably improve its score when using a sophisticated analytical approach. A combined score per study will reflect this and provide an overall risk of bias score. We can see this in our sample of included studies; many of them are classified as low risk of bias due to the use of advanced panel data techniques, which considerably improved their score. However, we still have a substantial number of medium risk of bias studies which all use basic regression-based approaches.

We are aware that the Duvendack et al. tool is subjective (see Duvendack et al. 2014, footnote 7 for an explanation) and the cut-off figures are arbitrary, but at least an indication is given as to how well studies deal with risk of bias issues. We explored alternative risk of bias tools; for example, many Campbell Collaboration systematic reviews now use adaptations of the ICDG risk of bias tool (see, for example, Baird et al. 2013). However, the ICDG tool was developed with microeconometric studies in mind (as was the Duvendack tool) and is a checklist approach which requires expert knowledge as well as a high degree of in-depth information from the included studies. We felt that the implementation of the ICDG tool in our particular context would be as problematic as the tool we opted for.

EFFECT SIZES

All regression-based estimates were converted into a comparable measure, the partial correlation coefficient, which was the best choice given our particular context. The partial correlation measures the strength of association between income inequality and government spending, holding all other factors constant. It is calculated as follows:

$$r = \frac{t}{\sqrt{t^2 + df}}$$

where *t* is the t-statistic of the regression coefficient and *df* is the degrees of freedom from the t-statistic (Stanley and Doucouliagos 2012).[16] If the t-statistic was not reported we calculated it by dividing the regression coefficient by its standard error. We had a few studies that did not report the t-statistic or the standard error but we had the exact p-value and the degrees of freedom. In these cases we used the TINV function in Excel, which allowed us to calculate the t-statistic using the p-values as well as the degrees of freedom (see Stanley and Doucouliagos 2012, footnote 45). In some cases we did not even have the exact p-value and only the levels of statistical significance were given, such as * (for 10 percent), ** (for 5 percent) and *** (for 1 percent). Stanley and

[16] The t-statistic was multiplied by -1 when the variable referred to the income share of the poorest/bottom or the average income of the poor. This is because a rise in this income share means a fall in income inequality.

Doucouliagos (2012) argue that in such cases the analyst will have to decide whether or not the estimates should be included. We decided to include them and followed the simplest approach suggested by Stanley and Doucouliagos (2012, p. 31), namely to assume that the p-value is 0.01 if the significance level is given as ***, 0.05 if the significance level is given as **, and so on. We then used these p-values as well as the degrees of freedom to calculate the t-statistic using the TINV function in Excel again. We excluded any study that did not report any of the above statistics and therefore did not enable us to calculate the partial correlation coefficient.

We should note that a number of effect size measures exist in the meta-analytical context, such as standardised mean differences, odds, and risk ratios, as well as partial correlation coefficients. We narrowed down the list of possible effect size calculations by closely looking at the data reported in the studies we included in our meta-regression approach. The vast majority of the included studies reported regression coefficients, t-statistics, and standard errors. Hence, we chose the partial correlation coefficient as it can be calculated easily from regression output, requiring only limited information. It is a unitless measure allowing comparisons within and between studies as well as comparisons involving variables using different scales such as Gini coefficients and income shares (Stanley and Doucouliagos 2012, Abdullah et al. 2015). It is often argued that the partial correlation coefficient should be converted into Fisher's *z* scale as the partial correlation coefficient is truncated at -1 and +1, which can cause problems. These problems can be overcome by running the meta-regression on the Fisher's *z* transformations, though Hunter and Schmidt (2004) cast doubts about using this transformation. Despite these doubts we used the command corrci in STATA to transform our partial correlation coefficients to Fisher's *z* scale, but this made little difference to our results, which is not surprising if one follows the arguments set out by Stanley and Doucouliagos (2012) and Abdullah et al. (2013).

Table 8 provides an overall description of the sample of included studies as well as the distribution of their results. For the government spending variables, we were able to extract 987 estimates of the partial correlation coefficient from 87 studies. Of these, 521 recorded positive partial correlations between a government spending variable and income inequality, with 243 of these being statistically significant. On the other hand, 466 of the estimates recorded negative partial correlations, with 180 of these being statistically significant. For the tax variables, we were able to extract 128 estimates from 15 studies. Of these, 60 recorded positive partial correlations between a tax variable and income inequality, with 31 of these being statistically significant. On the other hand, 68 of the estimates recorded negative partial correlations, with 13 of these being statistically significant. For the trade policy variables, we were able to extract 94 estimates from 19 studies. Of these, 68 recorded positive partial correlations between a trade policy variable and income inequality, with 19 of these being statistically significant. On the other hand, 26 of the estimates recorded negative partial correlations, with 15 of these being statistically significant.

Table 8: Description of the samples

	Government spending	Taxation	Trade policy
Number of studies	87	15	19
Number of estimates	987	128	94
Distribution of results			
Positive	521	60	68
Of which statistically significant	*243*	*31*	*19*
Negative	466	68	26
Of which statistically significant	*180*	*13*	*15*
Total	987	128	94

Notes: A list of the included studies is provided in Appendix 4.

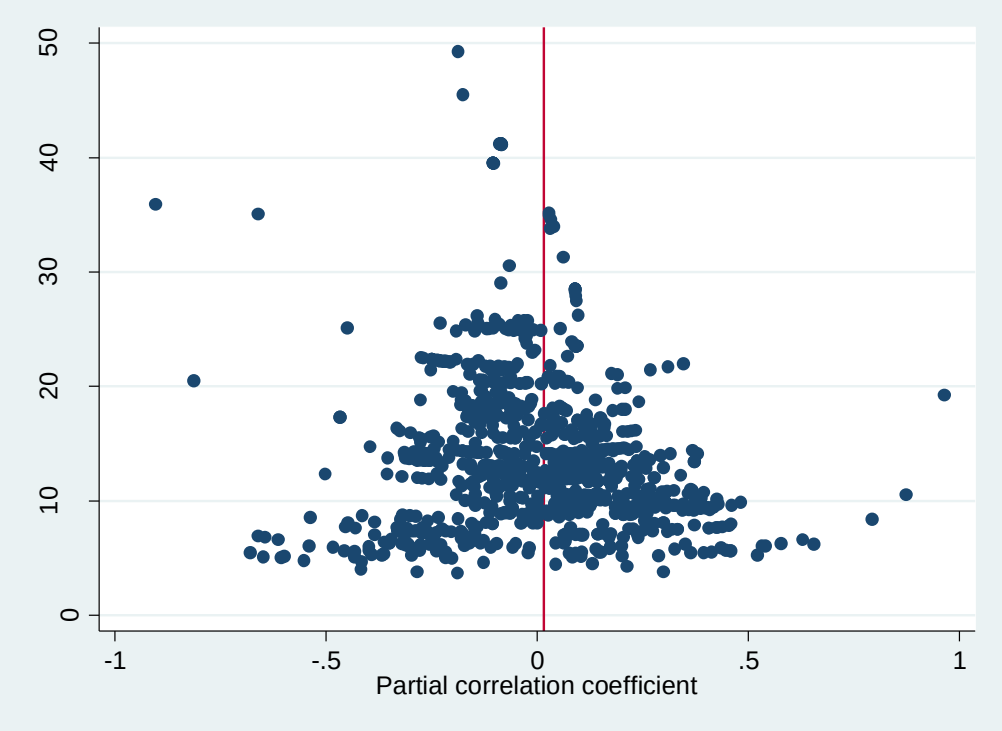
In Appendix 5.1 we show the frequency distributions of the partial correlation coefficient. For the government spending variables, the estimates approximate a normal distribution with the majority of the estimates having a coefficient that is close to 0.

PUBLICATION BIAS

Publication bias is a serious issue, in particular in the context of systematic reviews, as it can introduce serious biases in the meta-analytical results. It is argued that studies reporting statistically significant findings are more likely to be published in peer-reviewed journals than studies reporting statistically non-significant findings. This bias in the literature will then also be reflected in the meta-analysis as published studies are more likely to be included in a meta-analysis (Borenstein et al. 2009).

The funnel plot is one of the most common methods to illustrate the presence of publication bias (see, for example, Egger et al. 1997). Figure 4 illustrates a funnel plot which plots the effect size on the x-axis, here the partial correlation coefficient between measures of government spending and income inequality, and precision (or the inverse of the standard error of the partial correlation coefficient) on the y axis. At the bottom of the graph we find the estimates with less precision (i.e. with the larger standard errors), while the estimates with more precision (i.e. smaller standard errors) are more towards the top of the funnel plot. There is no publication bias present when the studies are distributed symmetrically. In this case a visual inspection of the funnel plot suggests symmetry as both positive and negative estimates are evenly distributed around the mean value of the partial correlation coefficient (0.016), indicated by the solid vertical line.

Figure 4: Funnel plot, partial correlations of government spending and income inequality (n=987)



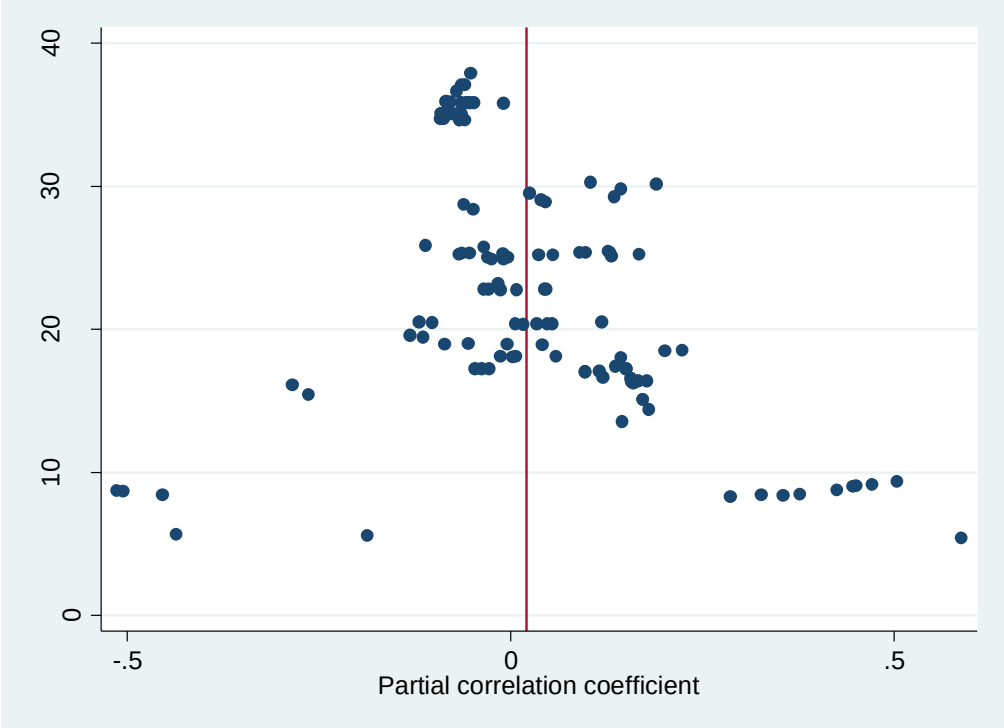
Note: Precision is calculated as 1/standard error of the partial correlation coefficient. The weighted mean of the partial correlation coefficient is marked with a red line with the value of 0.016 (SE = 0.005).

Note that Figure 4 indicates the presence of outliers, particularly in the upper left corner. These estimates were double-checked to ensure they had been correctly reported and coded. In Appendix 5.2 we present the funnel plot removing the main outliers, but we cannot observe any substantial differences between the funnel plots with and without outliers.

Figures 5 and 6 present equivalent funnel plots for the partial correlations between measures of taxation and inequality, and between trade policy and inequality. In these cases, the number of observations is smaller, and it is hard to detect any asymmetry in the funnel plots suggesting the presence of publication bias.[17] However, visual inspections of this nature can be subjective (Borenstein et al. 2009, Abdullah et al. 2015) and thus Stanley (2005, 2008) suggests the use of the FAT-PET (funnel-asymmetry precision-effect) regression as an empirical test to check more reliably for any publication bias. We carry out this test as part of our meta-regression analysis.

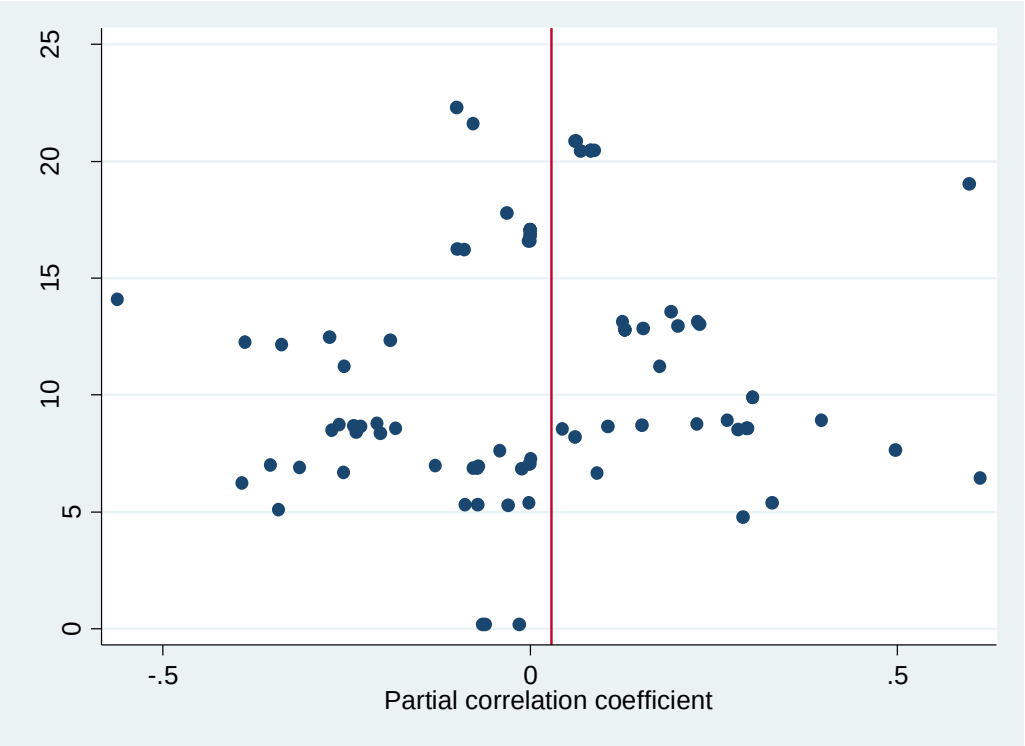
[17] Due to the small number of observations we did not present additional results after excluding the outliers in these cases, as we did with the results for government spending.

Figure 5: Funnel plot, partial correlations of taxation and income inequality (n=128)



Note: Precision is calculated as 1/standard error of the partial correlation coefficient.
The weighted mean of the partial correlation coefficient is marked with a red line with the value of 0.02

Figure 6: Funnel plot, partial correlations of trade policy and income inequality (n=94)



Note: Precision is calculated as 1/standard error of the partial correlation coefficient.

The weighted mean of the partial correlation coefficient is marked with a red line with the value of 0.030 (SE=0.017).

MODELLING HETEROGENEITY

We mentioned above that we suspect that a certain degree of heterogeneity remains in the studies we included in this synthesis. This view is confirmed by the funnel plot we presented in Figure 4 as the reported estimates are rather spread out. To better understand what drives this heterogeneity we follow Abdullah et al. (2015) and adopt the following meta-regression model to explore heterogeneity in the reported estimates:

$$r_{ij} = \beta_1 + \sum \beta_k Z_{ki} + \beta_0 SE_{ij} + \varepsilon_{ij}$$

where *r* is the partial correlation coefficient expressing the link between one of our policy variables (a measure of government spending, tax, or trade policy) and income inequality, of the *i*th estimate from the *j*th study. Z is a vector of variables that capture differences in the effect between the policy variable and income inequality. *SE* is the standard error of the partial correlation coefficient and ε_{ij} is the error term. The standard error of the partial correlation coefficient is calculated as follows:

18

$$SE = \frac{r}{t}$$

The following variables are included in the Z vector (see Table 9):

Measures of the dependent variable: Our variable of interest is income inequality. We included any study that used a recognised measure of income inequality. While most of the estimates use the Gini coefficient, a substantial proportion use income shares or other measures of inequality.

Measures of government spending, tax, or trade policy: Government spending was coded into 10 different categories: total, health, education, health and education, social safety net, military, housing, social general, consumption spending, and others (see Table 9).[19] The tax variables were codified into four categories, and the trade policy variables were coded into three categories (see Table 9). Our meta-regression model aims to test the differential impact of these different types of government spending, tax, and trade policy measures on the reported results.

Country composition: The main geographical areas covered include sub-Saharan Africa, Latin America, and South Asia. Although our main focus is on low- and middle-income countries, over half of the estimates include data from developed countries.

Data: Most estimates use a non-OLS method, for example dynamic panel estimators such as generalised method of moments (GMM), more traditional panel data analysis using random and/or fixed effects, other econometric approaches such as instrumental variables, 2- or 3-stage least squares, propensity score matching, differences-in-differences, or similar. The average year of the data used was included to account for different time periods and spans, as the relationship between government spending or taxation and income inequality may vary over time. The variable was calculated by subtracting the average year of data used across all studies (1987) from the average year of data used in study.

Other explanatory variables: Most studies include a range of explanatory variables in their regressions. In our MRA we include variables corresponding to whether any of the six broad control variables were included: trade, tax, inflation, governance, education, and population. The variables are coded 1 if they are included in the regressions as explanatory variables and 0 if otherwise. These specific variables were chosen after carefully reviewing all included studies and

[18] Note that the standard error of the partial correlation coefficient is different from the standard error of the individual regression coefficients.

[19] Social safety net includes components such as pensions, social security, social protection, and welfare spending. Coefficients fall into the social general category when disaggregation into subgroups (such as health, education, housing) was not provided in the study. Total government spending is used if the study refers to 'total government spending', or just 'government spending' or 'government size'. Government consumption is used if the study refers specifically to government 'consumption' expenditure. If the study refers to categories of government spending not covered by the other codes (e.g. government investment spending or wage bill) it was coded under government spending others.

counting the variables that appear more frequently. The trade category incorporates all variables that were considered valid policy variables such as import tariffs, export duties, non-tariff barriers, and trade policy indices. Tax includes all tax-related variables, in particular tax revenue, direct and indirect taxes, and other measures of tax progressivity. The governance category was included to reflect all democracy and institutional aspects as proxied by voice and accountability, corruption, and so on. Education variables include years of education and schooling-related variables such as educational attainment, enrolment rates, or human capital. Population and inflation appeared frequently in the specifications and it was decided to include them as well.

Publication: The standard error of the partial correlation coefficient is included to account for publication bias. We also account for the differences between published and unpublished studies.

More detailed descriptive information for each variable, including their mean values and standard deviations, is reported in Appendix 5.3.

We should note that we decided to run our meta-regression analysis with and without the outliers. The results including outliers are presented in the main text, while the findings excluding outliers are presented in Appendix 5.4. It is not clear which of these findings should be preferred, as comparing the results of the regressions with and without outliers we typically only observe very small differences. We comment further below on the few cases in which they do differ.

Estimations are carried out using a regression procedure with a weighted least squares (WLS) routine that Stanley and Doucouliagos (2013, 2015) advocate in a recent set of papers. They demonstrate how an unrestricted WLS-MRA is likely to be as good as and often better than both random-effects and fixed-effects meta-analysis and meta-regression analysis in practical applications (using the command metareg in STATA). The majority of the studies we included reported more than one result that could be used to calculate the partial correlation coefficient, but none of the studies specified a preferred result, and thus we were faced with multiple dependent estimates per study. This needs to be dealt with appropriately to avoid bias due to data dependency (Lipsey and Wilson 2001, pp. 105, 125). The literature suggests a number of approaches to dealing with multiple estimates per study (see, for example, Lipsey and Wilson 2001, Borenstein et al. 2009, p. 230) and there is no consensus on the preferred approach. Thus, in Appendix 5.5, we explore different approaches to dealing with multiple dependent estimates per study as robustness checks. We find that irrespective of the approaches we adopted our findings hold. Following Abdullah et al. (2015), our preferred approach to accounting for multiple estimates per study is to use precision squared (inverse variance or 1/standard error squared) as weights with study-level clustered standard errors.

Finally, the data used for the meta-regression analysis as well as the corresponding STATA do-files are available from the authors on request.

Table 9: Meta-regression variable definitions

Variable name	Variable description
Inequality measure	
Gini	BD=1: Gini coefficient (used as the base)
Income share bottom	BD=1: Income share of the bottom quintile
Income share top	BD=1: Income share of the top quintile
Income share other	BD=1: Income share other (e.g. income ratios, average income of the poorest quintile)
Income inequality other	BD=1: Other inequality measures (e.g. Theil, Atkinson)
Government spending measure	
Total spending	BD=1: Total government spending (used as the base)
Health spending	BD=1: Health government spending
Education spending	BD=1: Education government spending
Health and education spending	BD=1: Health and education government spending
Social net government spending	BD=1: Social net government spending
Military government spending	BD=1: Military government spending
Housing government spending	BD=1: Housing government spending
General social spending	BD=1: Social government spending
Consumption spending	BD=1: Government spending (consumption)
Other types of spending	BD=1: Government spending (any/not specified/other)
Tax policy measure	
Total tax revenue	BD=1: Total tax revenue
Direct taxes	BD=1: Direct taxes (e.g. income taxes, corporate taxes)
Indirect taxes	BD=1: Indirect taxes (e.g. VAT, excises, customs)
Tax progressivity measures	BD=1: Tax progressivity measures (used as the base)
Trade policy measure	
Import tariffs	BD=1: Import tariffs (used as the base)
Trade policy indices	BD=1: Trade policy indices (e.g. Sachs-Warner index)
Other trade measures	BD=1: Other measures (e.g. non-tariff barriers, export duties)
Country composition	
Sub-Saharan Africa (SSA)	BD=1: Countries in sub-Saharan Africa included in samples
Latin America (LAC)	BD=1: Countries in Latin America included in samples
South Asia (SA)	BD=1: Countries in South Asia included in samples
Developed	BD=1: Developed countries included in samples
Data	
OLS	BD=1: OLS estimator used
Year data	Average year of data used in each study minus the average year of data across all studies
Other explanatory variables	
Tax	BD=1: Tax included as explanatory variable
Trade	BD=1: Trade included as explanatory variable
Education	BD=1: Education included as explanatory variable
Inflation	BD=1: Inflation included as explanatory variable
Population	BD=1: Population included as explanatory variable
Governance	BD=1: Governance included as explanatory variable
Publication	
Standard error	Standard error of the partial correlation coefficient
Unpublished	BD=1: Study is unpublished

Notes: * BD means binary dummy with a value of 1 if condition is fulfilled and zero otherwise.

4.2 SYNTHESIS AND META-ANALYSIS: GOVERNMENT SPENDING

In this section we use a meta-regression approach to establish whether there is a relationship between government spending and income inequality, and to explore the reasons for heterogeneity in either the size or direction of this relationship.

META-REGRESSION RESULTS

Our main results are reported in Table 10. Regression 1 reports the FAT-PET results where the standard error of the partial correlation coefficient is regressed on the partial correlation coefficient. Recall that the FAT-PET regression is an empirical check to explore publication bias. The results indicate that there is some publication bias as the coefficient for the standard error is statistically significant. This finding holds across all estimations presented in Table 10. This implies that the visual inspection of the funnel plot might have been misleading as it indicated no publication bias. In addition, the coefficient for the standard error is positive, indicating that the estimated partial correlation coefficients are skewed towards positive values; negative effects are being under-reported in the literature.

To get an indication of the magnitude of this bias, recall from Figure 4 that the average reported correlation coefficient across all 987 observations is slightly positive, at 0.016. The constant in regression 1 quantifies the overall or average relationship between government spending and income inequality, after correcting for publication bias. This takes the value of -0.134, which is statistically significant at a 1 percent significance level, implying a statistically significant negative relationship between government spending and income inequality. Adjusting for publication bias therefore turns a small positive relationship into a larger negative (and statistically significant) relationship.[20]

In regression 2 additional dummy variables are added representing different income inequality measures, to explore whether the relationship differs depending on the income inequality measure that has been adopted. Only the values for *income share other* and *income inequality other* are positive and statistically significant at 5 percent and 1 percent respectively. A positive effect for a moderator variable means that the variable results in a larger positive (or smaller negative) relationship between government spending and income inequality. Thus the results in regression 2 indicate that in the case of using the *Gini* coefficient there is an average relationship of -0.13 (significant at 1 percent) between government spending and income inequality, after

[20] Another way to control for publication bias is to focus on the 10 percent of the most precise reported estimates (Stanley and Doucouliagos 2012, p. 56). The average correlation coefficient among the 10 percent most precise estimates in our sample is -0.051 (SE=0.008). Thus again controlling for publication bias turns a small positive relationship into a negative relationship.

correcting for publication bias. On the other hand, the relationship is slightly positive when *income share other* (0.021) and *income inequality other* (0.064) are used.[21]

Regression 3 is our main model as it includes all potentially relevant explanatory variables described above. Of the moderator variables for the control variables (tax, trade, governance, inflation, education, and population), *governance* is positive and statistically significant at the 5 percent level. This implies that studies which control for governance indicators report a smaller negative (or larger positive) relationship between government spending and income inequality, other things being equal. By contrast, the coefficient for inflation is negative and statistically significant (at the 5 percent level), indicating that studies that control for inflation report a larger negative (or smaller positive) relationship between government spending and income inequality. In terms of the variables for sample coverage, the variable for SA is positive and significant at the 10 percent level, indicating that studies including South Asian countries in the sample find a smaller negative (or larger positive) relationship. However, none of the other moderator variables for control variables or sample coverage are statistically significant.

We are particularly interested in the results for the disaggregated measures of government spending. Surprisingly, the coefficients for *health and education spending* and *social spending* are both positive and statistically significant. This indicates that studies that use these measures of government spending find on average a larger positive (or smaller negative) relationship between spending and inequality than studies using *total government spending*. This is surprising since, of all the categories of spending, we would most expect health, education, and social spending (which includes cash transfers) to have a negative relationship with income inequality, other things being equal. However, it is consistent with evidence from studies such as Tanzi (1974), Alesina (1998), and Davoodi et al. (2003), suggesting that much of the benefits of government spending on health and education in developing countries are received by middle-income groups in urban areas, which can end up raising inequality.

We are also particularly interested in the results for the *OLS* variable, which captures the different analytical approaches used. The coefficient for this variable is negative and statistically significant at the 5 percent level. This implies that studies using *OLS* as an estimation method report, on average, larger negative (or smaller positive) correlations between government spending and income inequality, holding all other MRA variables constant.

In regression 4 we follow Leonard et al. (2014) and employ a general-to-specific modelling strategy, removing the variable that has the largest p-value until all p-values are <0.05. The rationale for employing a general-to-specific approach can be found in Stanley and Doucouliagos (2012), who argue that they prefer a more specific model as it makes the underlying associations clearer. In the specific model (regression 4) we observe that *income inequality other*, *OLS*, *governance*, *health and education government spending*, and *government spending others* are

[21] This value was calculated by adding the coefficient on *income share other* to the constant, i.e. -0.13 +0.151. The same logic applies for the calculations we present for the other income inequality measures presented in regressions 3–5.

statistically significant and positive (except *OLS*) as already seen in regression 3. In addition, *tax* is negative and significant at 5 percent.

Finally, in regression 5 we report the estimates from a robust regression, which strengthen our findings further. We should note that six of the variables in regression 3, being *income share other*, *OLS*, *governance*, *health and education government spending*, *social net government spending*, and *government spending others*, are statistically significant across most of the estimations we present in Table 10. In addition, when we compare the results in Table 10 with those in Appendix 5 (which exclude outliers), we typically only observe very small differences. The main differences can be found on the variables *LAC* and *Trade* that appear consistently significant throughout the regressions with no outliers, which is not the case in Table 10. The standard error is not significant in one out of five regressions with no outliers. However, removing the outliers does not seem to change our main results: *income inequality other*, *OLS*, *governance*, *inflation*, and the three measures of government spending (*health and education*, *social net*, *and government others*) are statistically significant in both tables.

Table 10: MRA of the effects of government spending on income inequality (dependent variable=partial correlation)

	(1) FAT-PET WLS	(2) WLS	(3) WLS general	(4) WLS specific	(5) Robust
Standard error	1.778***	1.532***	1.496**	1.748***	0.433**
	(0.530)	(0.482)	(0.724)	(0.482)	(0.169)
Income share bottom		0.055	0.057		-0.015
		(0.072)	(0.081)		(0.018)
Income share top		-0.068	-0.055		-0.124***
		(0.061)	(0.055)		(0.024)
Income share other		0.151**	0.153*		0.162***
		(0.059)	(0.089)		(0.022)
Income inequality other		0.194***	0.208**	0.205***	0.087
		(0.031)	(0.087)	(0.030)	(0.062)
Developed			0.004		0.001
			(0.038)		(0.014)
Unpublished			-0.029		-0.062***
			(0.033)		(0.013)
Year data			0.001		-0.002**
			(0.004)		(0.001)
OLS			-0.097**	-0.124***	-0.101***
			(0.041)	(0.033)	(0.016)
LAC			-0.071		-0.106***
			(0.048)		(0.022)
SSA			-0.084		0.026
			(0.080)		(0.034)
SA			0.139*		0.024
			(0.077)		(0.031)
TAX			-0.059	-0.079**	-0.032
			(0.053)	(0.038)	(0.027)
TRADE			0.049		0.195***
			(0.105)		(0.020)

	(1) FAT-PET WLS	(2) WLS	(3) WLS general	(4) WLS specific	(5) Robust
Governance			0.077**	0.088**	0.084***
			(0.036)	(0.034)	(0.012)
Inflation			-0.067**		-0.052***
			(0.028)		(0.012)
Population			-0.006		-0.034**
			(0.039)		(0.015)
Education			-0.041		0.007
			(0.035)		(0.013)
Health government spending			-0.052		0.070**
			(0.100)		(0.034)
Education government spending			0.075		0.074***
			(0.054)		(0.020)
Health and education government spending			0.276***	0.119***	0.259***
			(0.066)	(0.033)	(0.067)
Social net government spending			0.145***	0.079**	0.102***
			(0.046)	(0.032)	(0.021)
Military government spending			0.000		0.101**
			(0.080)		(0.046)
Housing government spending			-0.012		0.090
			(0.066)		(0.068)
Social general government spending			0.001		-0.054
			(0.093)		(0.046)
Government spending (consumption)			0.053		0.063***
			(0.048)		(0.016)
Government spending others			0.238***	0.137***	0.171***
			(0.051)	(0.038)	(0.041)
Constant	-0.134***	-0.130***	-0.119	-0.158***	-0.016
	(0.034)	(0.033)	(0.080)	(0.033)	(0.026)
N	987	987	974	987	974
R^2	0.076	0.125	0.298	0.198	0.399

Notes: Columns report estimates variants of regression 2. Regressions 1, 2, and 4 use 987 estimates from 87 studies, while regressions 3 and 5 use 974 estimates from 85 studies. Standard errors are reported in parentheses. All regressions use cluster standard errors to adjust for data dependence, i.e. multiple estimates per study. All columns use weighted least squares, except for regression 5, which uses robust regression. In regression 4 we employed a general-to-specific modelling strategy, removing the variable that had the largest p-value until all p-values are <0.05. For definitions of variables see Table 9. Total government spending is used as the base category for the government spending variable. Gini is used as a base in the inequality variable. In order to test for multicollinearity we use the variance inflation factor (VIF) for both the general (3) and specific (4) regressions; the mean VIF is 2.65 and 1.36, respectively, which is not a case for concern. According to Hosmer and Lemeshow (2000), values of VIF exceeding 10 are often regarded as indicating multicollinearity and should be investigated.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To check the robustness of our findings we conduct a range of subgroup analyses. In Table 11 we explore how our findings differ by the different categories of government spending, focusing on the four measures of government spending where we had sufficient number of observations. For the remaining government spending variables, most of the variables of interest were omitted due to the low number of observations, and hence we felt there is not much value in presenting them here. Regression 1 shows the results for total government spending, regression 2 the results for

government education spending, regression 3 the results for government social spending, and regression 4 the results for government consumption spending, respectively.

The findings in Table 11 show some interesting differences between the results for different spending measures. In regression 3, for example, the coefficients of the *income share top* and *income share bottom* are both negative and statistically significant. This indicates that, compared with studies that use the *Gini* coefficient, studies that use these measures find a larger negative (or smaller positive) relationship between social spending and inequality, all else being equal. On the other hand, the coefficients on *income share bottom* and *income share other* in regression 4 are positive and statistically significant. This suggests that, compared with the *Gini* coefficient, studies that use either of these measures report a larger positive (or smaller negative) relationship between consumption spending and income inequality, all else being equal.

The results for the *average year of the data* used also differ somewhat between spending measures. This variable is included in order to explore whether the relationship between government spending and inequality varies with time. The coefficient is positive and statistically significant in regressions 2 and 4, in other words when education government spending and consumption government spending are used. This suggests that, holding all else constant, studies that use more recent data find a smaller negative (or larger positive) relationship between education spending and income inequality, and between consumption spending and income inequality, in comparison with studies using older data.

Particularly interesting are the results for the regional variables. For example, consider the results for Latin America (*LAC*). The negative coefficient for this variable in regressions 1 and 2 (statistically significant at the 1 percent level and 5 percent respectively) suggests that studies including Latin American countries in the sample find a larger negative (or smaller positive) relationship between total government spending and income inequality. By contrast, the positive coefficient for sub-Saharan Africa (*SSA*) in the same regressions indicates that studies including countries from this region find a less negative (or larger positive) relationship between total spending and income inequality. One possible explanation for these results is that government spending has a greater effect in reducing income inequality in Latin America than in sub-Saharan Africa. Note, however, that the results for consumption government spending in regression 4 show a larger negative (smaller positive) relationship with income inequality for studies including countries from sub-Saharan Africa.

As in Table 10, the subgroup analysis in Table 11 suggests that the inclusion of different control variables in econometric models (*trade*, *governance*, *inflation*, *population*, *and education*) influences the partial correlation between government spending and income inequality. Studies that control for *trade* find a smaller negative (or larger positive) relationship between total government spending and inequality. The same applies for studies that control for *governance*, which find a smaller negative (or larger positive) relationship between total government spending and income inequality, and social spending and income inequality. The *education* variable is statistically significant across three out of four subgroups, implying that the inclusion of this control variable in studies of the relationship between government spending and income inequality is particularly appropriate.

In Table 12 we conduct a series of other subgroup analyses with the objective to explore additional aspects of the data and to check the robustness of our main findings (note also that Appendix 5 presents further robustness checks). We were particularly interested to explore how our findings might differ by region (Latin America, sub-Saharan Africa) and check whether the inclusion of developed countries in the sample makes a difference.

The results from Table 12 strengthen our previous discussion and results. In regressions 3–5 (i.e. studies including developed countries, Latin America, and sub-Saharan African countries in the sample) two measures of government spending are consistently positive and statistically significant (*social net* and *government spending others*) indicating that these categories of government spending report smaller negative (or larger positive) partial correlations with income inequality. By contrast, in regression 5 (studies including the sub-Saharan Africa region in the sample) *health spending* has a negative and statistically significant value, meaning that the relationship with inequality is on average more negative (less positive) for this measure of spending. *Health and education government spending* is highly significant and positive in samples that include Latin America countries. These results are consistent with the hypothesis that the relationship between government spending and income inequality varies significantly across regions. However, we are unable to test this hypothesis directly, since most of the studies included in our review focus on more than one region, and only rarely provide separate results for different regions.

Table 11: Subgroup analysis for government spending variables (dependent variable=partial correlation)

	(1) Total government spending	(2) Education government spending	(3) Social net government spending	(4) Government spending (consumption)
Standard error	0.114	-2.216	-0.456	4.899**
	(0.806)	(1.345)	(1.389)	(2.168)
Income share bottom	0.004	-0.007	-0.385**	0.220**
	(0.108)	(0.077)	(0.138)	(0.104)
Income share top	-0.108***	-0.042	-0.406**	0.076
	(0.029)	(0.185)	(0.159)	(0.092)
Income share other	-0.127***	-0.153*	.	0.342***
	(0.041)	(0.075)		(0.067)
Income inequality other	-0.059	.	.	.
	(0.068)			
Developed	-0.072	-0.100**	0.142	0.127*
	(0.047)	(0.046)	(0.156)	(0.068)
Unpublished	-0.048	0.018	-0.078	-0.066
	(0.031)	(0.091)	(0.080)	(0.070)
Year data	-0.002	0.015**	-0.008	0.016***
	(0.004)	(0.007)	(0.006)	(0.005)
OLS	-0.086**	-0.023	0.000	-0.161
	(0.037)	(0.044)	(0.071)	(0.101)
LAC	-0.278***	-0.638**	-0.045	0.091
	(0.060)	(0.250)	(0.207)	(0.069)
SSA	0.597***	0.612*	0.015	-0.313***
	(0.086)	(0.303)	(0.266)	(0.094)
SA	-0.408***	-0.298	-0.264	0.490***
	(0.060)	(0.188)	(0.272)	(0.113)
TAX	.	-0.123	-0.084	.
		(0.135)	(0.082)	
TRADE	0.196***	.	0.040	-0.045
	(0.052)		(0.104)	(0.134)
Governance	0.093***	0.005	0.050	0.064
	(0.031)	(0.042)	(0.081)	(0.063)
Inflation	-0.135***	-0.061	0.009	0.001
	(0.035)	(0.076)	(0.044)	(0.099)
Population	0.046	-0.024	-0.021	-0.247**
	(0.049)	(0.091)	(0.063)	(0.106)
Education	0.134***	-0.214**	-0.190*	-0.159*
	(0.045)	(0.093)	(0.106)	(0.088)
Constant	0.079	0.820**	0.391	-0.648**
	(0.079)	(0.295)	(0.257)	(0.303)
N	277	96	110	392
R^2	0.411	0.735	0.699	0.514

Notes: Standard errors are reported in parentheses. All regressions use cluster standard errors to adjust for data dependence, i.e. multiple estimates per study. All columns use weighted least squares.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Subgroup analysis for selected non-government variables (dependent variable=partial correlation)

	(1) FAT-PET WLS	(2) WLS general	(3) Developed, yes=1	(4) Latin America, n=1	(5) Sub-Saharan Africa, yes=1
Standard error	1.778***	1.496**	2.146**	1.678**	2.343**
	(0.530)	(0.724)	(0.933)	(0.812)	(0.884)
Income share bottom		0.057	0.106	0.077	0.075
		(0.081)	(0.088)	(0.090)	(0.088)
Income share top		-0.055	-0.073	-0.059	-0.077
		(0.055)	(0.060)	(0.060)	(0.059)
Income share other		0.153*	0.141	0.162*	0.162*
		(0.089)	(0.105)	(0.091)	(0.092)
Income inequality other		0.208**	0.281	0.219	0.197
		(0.087)	(0.180)	(0.146)	(0.152)
Developed		0.004		0.008	0.009
		(0.038)		(0.039)	(0.036)
Unpublished		-0.029	-0.036	-0.038	-0.076
		(0.033)	(0.052)	(0.039)	(0.047)
Year data		0.001	-0.002	-0.000	-0.002
		(0.004)	(0.005)	(0.004)	(0.004)
OLS		-0.097**	-0.178***	-0.102**	-0.207***
		(0.041)	(0.064)	(0.046)	(0.062)
LAC		-0.071	-0.321		-0.161
		(0.048)	(0.212)		(0.101)
SSA		-0.084	0.174	0.028	
		(0.080)	(0.199)	(0.057)	
SA		0.139*	0.074	.	.
		(0.077)	(0.072)		
TAX		-0.059	-0.088	-0.055	-0.041
		(0.053)	(0.079)	(0.055)	(0.070)
TRADE		0.049	-0.034	0.045	0.047
		(0.105)	(0.147)	(0.117)	(0.127)
Governance		0.077**	0.036	0.061	0.059
		(0.036)	(0.046)	(0.038)	(0.043)
Inflation		-0.067**	-0.059	-0.070*	-0.062
		(0.028)	(0.046)	(0.035)	(0.039)
Population		-0.006	0.000	0.001	-0.011
		(0.039)	(0.059)	(0.041)	(0.039)
Education		-0.041	-0.042	-0.043	-0.024
		(0.035)	(0.043)	(0.036)	(0.040)
Health government spending		-0.052	-0.132	-0.079	-0.235**
		(0.100)	(0.124)	(0.123)	(0.102)
Education government spending		0.075	0.072	0.077	0.053
		(0.054)	(0.066)	(0.061)	(0.068)
Health and education government spending		0.276***	.	0.284***	.
		(0.066)		(0.074)	
Social net government spending		0.145***	0.146**	0.161***	0.117*
		(0.046)	(0.070)	(0.049)	(0.063)
Military government spending		0.000	0.119	0.090	0.083
		(0.080)	(0.094)	(0.084)	(0.093)

	(1) FAT-PET WLS	(2) WLS general	(3) Developed, yes=1	(4) Latin America, n=1	(5) Sub-Saharan Africa, yes=1
Housing government spending		-0.012	0.003	-0.059	-0.095
		(0.066)	(0.079)	(0.078)	(0.073)
Social general government spending		0.001	0.044	0.010	-0.146
		(0.093)	(0.122)	(0.100)	(0.145)
Government spending (consumption)		0.053	0.034	0.047	0.011
		(0.048)	(0.074)	(0.056)	(0.064)
Government spending others		0.238***	0.254***	0.244***	0.228***
		(0.051)	(0.070)	(0.055)	(0.062)
Constant	-0.134***	-0.119	-0.070	-0.174**	0.002
	(0.034)	(0.080)	(0.100)	(0.073)	(0.108)
N	987	974	569	810	736
R^2	0.076	0.298	0.312	0.284	0.345

Notes: Standard errors are reported in parentheses. All regressions use cluster standard errors to adjust for data dependence, i.e. multiple estimates per study. All columns use weighted least squares.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Finally, there are some similarities between the regressions presented in Table 10 and some of the subgroup analyses presented in Table 12. For example, using *income share other* yields significant results on the effect of government spending on income inequality (however, the results are only significant at the 10 percent level). *Government spending others* is positive and significant across all subgroup analyses and thus confirms the trends outlined in Table 10. This implies that this more varied category of spending will tend to yield a smaller negative (or larger positive) relationship with inequality, as might be expected. The same applies to *social net spending* that is consistently positive and significant. Similarly, *OLS* is consistently negative and significant, implying that studies using this estimation method are, on average, reporting a larger negative (or smaller positive) relationship between government spending and income inequality. This finding is quite important because, according to the risk of bias tool developed previously, most of the studies identified as having a medium risk of bias use *OLS* as an estimation method.

As mentioned above, we also conducted further robustness checks using different approaches to address multiple dependent estimates per study. Appendix 5.5 presents the results of the different weighting schemes that are often used to deal with biases due to data dependence.

IS THERE AN ASSOCIATION BETWEEN GOVERNMENT SPENDING AND INCOME INEQUALITY?

The results in Tables 10–12 show that both the size and direction of the estimated relationship between government spending and income inequality are affected by a range of factors, including the country composition of the sample, the control variables included in the analysis, the analytical approach used, and the measure of government spending used. This makes it difficult to answer the question of whether or not there is – on average – a strong association between government spending and income inequality. However, we are able to make some progress

towards this question by calculating the average (or predicted) relationship between government spending and income inequality implied by the results in Tables 10–12 for a certain set of values of the moderator variables. This is done in Table 13.

Panel A of Table 13 shows the average relationship between government spending and the different measures of inequality predicted by the results in regression 3 of Table 10. Here we consider a study that is published, uses a more robust non-*OLS* analytical approach, includes all developing country regions but not developed countries in the sample, uses a period of time centred on 1987, includes all six control variables in the analysis (e.g. governance, inflation, education), and focuses on total government spending. At least for this case, the average relationship when including outliers is negative for the *Gini* coefficient (-0.182), the *income share of the poorest* (-0.125), and the *income share of the richest* (-0.238). However, only the results for the income share of the richest quintile is statistically significant (at the 10 percent level); in addition, the results when excluding outliers are all statistically insignificant. For the other measures of inequality, the average relationship is slightly positive, but not statistically significant.

Panel B of Table 13 then shows the average relationship between each measure of government spending and income inequality, predicted by the results in regressions 1–4 of Table 11.[22] We again consider a study that is published, uses a more robust non-*OLS* analytical approach, includes all developing country regions but not developed countries in the analysis, uses a sample coverage centred on 1987, and includes all six control variables in the analysis (e.g. governance, inflation, education). For this case, the predicted relationship is positive for total government spending, across all inequality measures; the results are statistically significant at the 10 percent level or below. For *education spending*, the results are mixed and not statistically significant. In contrast, the predicted relationship is negative for both government *social spending* and *consumption spending*, across all inequality measures except the *Gini* coefficient for social spending.

These results in Table 13 refer of course only to one particular set of moderator variables. Nevertheless, the results do show at least some evidence of a statistically significant negative relationship between government social spending and inequality, and between government consumption spending and income inequality. In terms of the strength of association, it has been suggested that a partial correlation coefficient of less than 0.07 in absolute terms can be considered small, with 0.17 or above considered to be moderate, and 0.33 or above large (Doucouliagos 2011, Abdullah et al. 2015). This is in line with what Cohen (1988, p. 115) suggests, who argues that, for partial correlation coefficients, the effects are considered to be small when r = 0.1, medium when r = 0.3 and large when r = 0.5. Judging by these guidelines, therefore, the results in Table 13 imply a potentially large negative relationship between government social spending and government consumption spending and income inequality.

[22] The results of the pooled analysis in Table 10 are based on a strong assumption – that the effect of each moderator variable on the partial correlation between government spending and income inequality is the same across all measures of spending. The fact that the results of the subgroup analysis in Tables 11–12 differ from the pooled analysis suggests that this assumption is not well supported by the data, which in turn implies that the subgroup results are more reliable, despite the smaller sample size.

Table 13: Predicted (average) relationship between government spending and income inequality

	Gini	Income share bottom	Income share top	Other income share measure	Other inequality measure
A. Pooled analysis ~					
Including outliers	-0.182	-0.125	-0.238*	-0.030	0.025
Excluding outliers	-0.049	-0.019	-0.105	0.089	0.074
B. Subgroup analysis, including outliers[#]					
Total spending	0.324***	0.328**	0.216**	0.197*	0.265***
Education spending	0.079	0.072	0.037	-0.074	.
Social spending	-0.099	-0.484*	-0.506*	.	.
Consumption spending	-0.765***	-0.545**	-0.688***	-0.423*	.
C. Subgroup analysis, excluding outliers[$]					
Total spending	0.330***	0.335**	0.223**	0.202*	0.270***
Consumption spending	-0.496***	-0.340***	-0.430***	-0.120	.

Notes: ~Based on regression 3 from Table 10; [#] based on regressions 1–4 in Table 11; [$]underlying regressions not reported but available from authors on request. The following values of each moderator variable are assumed: standard error=0; developed=0; unpublished=0; year=0; OLS=0; LAC, SSA, SA all equal to 1; Tax, Trade, Governance, Inflation, Population, Education all equal to 1. In Panel A, the spending measures are all equal to 0, so the results refer to total government spending (base case). In Panel B, for total spending the variables for SA and Tax are set to 0 by default; the same applies to the variable Trade for Education spending and Tax for social spending. Only the results for total and consumption spending are shown excluding outliers because the results for education and social spending are identical in this case.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

SUMMARY

Overall, the results in this section show that both the size and direction of the relationship between government spending and income inequality are affected by a range of factors.

First, we find some evidence that studies using measures of inequality other than the *Gini* coefficient, the income share of the poorest 20 percent, or the income share of the richest 20 percent find a smaller negative (or larger positive) relationship between government spending and income inequality. This is an important finding because, although the majority of studies do use the *Gini* coefficient or income shares (see Section 3), these are not necessarily the best measures of inequality, nor are they necessarily the most relevant from a policy perspective. It would be

better in future if each econometric study reported the results for a wider range of inequality measures.

Second, we find little evidence that the inclusion of developed countries in the samples used for estimation affects the results. This is an important finding because many econometric studies do include both developed and developing countries in the sample, in the interests of increasing sample size. Our results on the whole provide no indication that this generates bias.

With regard to the regional coverage of the samples used for estimation, the results are more mixed with fewer consistent patterns. The majority of studies include countries from all regions, and there are very few studies focusing on one region only. This means that direct comparisons of the relationship between government spending and inequality in different regions are not generally possible. There is some evidence that *not* including Latin American countries in the sample leads to a smaller negative (or larger positive) relationship between government spending and inequality, a possible explanation being that government spending has a more redistributive effect in Latin America than in other regions. But in the absence of more direct comparisons across regions, this conclusion remains tentative.

Third, we find little evidence that the period of time covered by the sample makes a big difference to the results. This is important because econometric studies typically use the largest possible time period, again in the interests of increasing sample size; our results on the whole provide no indication that this affects the results substantially. The only exceptions are for education spending and consumption spending, where the use of more recent data leads to a less negative (more positive) relationship with inequality. One possible explanation is a general tendency for expenditure in these areas to become less progressive (or more regressive) over time, thus lowering their negative (raising their positive) impact on inequality. Another possible explanation is diminishing returns to government spending; in other words, the higher the overall level of spending, the lower the effect of additional spending on the reduction of income inequality.

Fourth, we find fairly consistent evidence that studies using ordinary least squares (*OLS*) as an estimation method find a larger negative (or smaller positive) relationship between government spending and income inequality. This is an important finding because, according to the risk of bias tool developed previously, most of the studies identified as having a medium risk of bias use *OLS* as an estimation method. It indicates the importance of using more robust analytical approaches with lower risk of bias, such as panel data methods and instrumental variables (IV) estimation. Studies relying on *OLS* appear to have had a tendency to overestimate the contribution of government spending to the reduction of inequality, compared with more robust analytical approaches.

Fifth, we also find consistent evidence that the control variables used in the analysis make a difference to the results. This is an important finding because researchers tend to differ in terms of precisely which control variables they include in their analysis. Our results show that these choices affect the estimated results, sometimes quite substantially – and therefore highlight the importance of very careful consideration by researchers of the control variables included in their analysis. Although a large set of control variables is not always possible (due to lack of data), our

results suggest that failing to control for measures of governance and inflation could lead to biased estimates of the relationship between government spending and inequality.

Sixth, we find consistent evidence of publication bias. This is an important finding because, unless corrected for in some way, publication bias can lead to significant errors in attempts to summarise empirical knowledge on a given issue. Stanley and Doucouliagos (2012), for instance, discuss how the existence of publication bias in the estimated effect of minimum wages on employment can lead researchers to overestimate the negative effect of minimum wages on employment by a factor of 5 or more. Publication bias is common in the literature, and has been observed in a number of different contexts. Stanley and Doucouliagos (2012, Table 4.1), for instance, provide evidence of publication bias in estimates of the employment effect of minimum wages; in this case, positive estimates appear to be under-reported. Similarly, Doucouliagos and Paldam (2008, 2009, 2014) have found evidence of publication bias in estimates of the relationship between foreign aid and economic growth, with negative estimates being under-reported.

In our case, it appears that negative estimates are being under-reported in the literature. It is, however, difficult to say precisely what might be driving this. One possible explanation is that it is due to 'polishing' – in other words, the tendency for researchers and editors to report and publish results that are statistically significant (Doucouliagos and Paldam 2008). Another possible explanation is that researchers themselves are reluctant to report negative relationships, perhaps because of ideological persuasion (e.g. a belief in limited government involvement in the economy), or because they work for or are funded by institutions which are predisposed towards this view.[23] We are not able to disentangle these different possible explanations for publication bias. Nevertheless, we can at least correct for publication bias when determining whether or not there really is a relationship between government spending and inequality.

Finally, in terms of our central question – is there a strong association between measures of government spending and income inequality – we find that the answer depends very much on the type of spending being considered. When considering *total* government spending, we find evidence of a moderate positive relationship with income inequality. However, when considering more disaggregated types of spending, we find evidence of a moderate negative relationship between government *social* spending and income inequality, and between government *consumption* spending and inequality. It is important to recognise, however, that both the size and direction of the estimated relationship between government spending and income inequality is affected by a range of factors. For example, we have seen that studies using measures of inequality other than the *Gini* coefficient or income shares tend to find a smaller negative relationship between government spending and income inequality. This makes it difficult to say whether or not there is on average a strong association between any particular type of

[23] This is in fact the explanation favoured by Doucouliagos and Paldam (2008, 2009) to explain the evidence of publication bias in the aid effectiveness literature. They argue that the research community is reluctant to publish negative estimates of this relationship, partly because of researchers' desire to be seen as supporting the 'do-good' activity of foreign aid, and partly because research is often funded by aid organisations.

government spending and income inequality.

4.3 SYNTHESIS AND META-ANALYSIS: TAXATION

Similar to Section 4.2, in this section we use a meta-regression approach to establish whether there is a relationship between taxation and income inequality, and to explore the reasons for heterogeneity in the size or direction of this relationship.

META-REGRESSION FINDINGS

Our main results are shown in Table 14. Similar to the previous section, regression 1 reports the FAT-PET results where the standard error of the partial correlation coefficient is regressed on the partial correlation coefficient. In regression 2 additional dummy variables are added representing different income inequality measures, to explore whether the relationship differs depending on the income inequality measure that has been adopted. Regression 3 is our main model as it includes all potentially relevant explanatory variables described in Section 4.1.4 above. In regression 4 we follow Leonard et al. (2014) and employ a general-to-specific modelling strategy, removing the variable that has the largest p-value until all p-values are <0.05. Finally, in regression 5 we report the estimates from a robust regression.

The results in Table 14 again indicate the existence of publication bias, in that the coefficient for the standard error is statistically significant in regressions 1, 2, and 5. In addition, the coefficient is positive, suggesting that negative effects are being under-reported in the literature. To get an idea of the magnitude of bias, the weighted mean partial correlation coefficient between taxation and income inequality across all 128 observations, without adjusting for publication bias, is slightly positive and not statistically significant (0.020, SE 0.013). The results in regression 1, by contrast, imply that the average coefficient, after correcting for publication bias, is negative and statistically significant (-0.134, SE 0.054).

With regard to the measures of inequality, the majority of studies use the *Gini* coefficient; we have only six observations in this case for any other measures of inequality. There is some evidence that using the *Gini* coefficient leads to a more negative (less positive) relationship between taxation and inequality, in comparison with these other measures, but this difference is statistically significant only in regression 5.

With regard to the sample coverage, the coefficient for *developed* is negative and statistically significant at the 1 percent level in all regressions. This indicates that studies including developed countries find, on average, larger negative (smaller positive) associations between measures of taxation and income inequality. The regional composition of the sample also makes a difference, as suggested by the statistical significance of the coefficients for *LAC*, *SSA*, and *SA* in all five regressions. The negative coefficients for *LAC* and *SA* imply that studies including countries from these regions find a larger negative (or smaller positive) correlation between measures of taxation and income inequality, in comparison with studies that do not. Conversely, the positive coefficient for *SSA* implies that studies including countries from this region find a smaller negative (or larger

positive) correlation between taxation and income inequality, in comparison with studies that do not.

With regard to analytical approach, the coefficient for *OLS* is positive and statistically significant in regressions 3 and 4. This implies that studies using *OLS* as an estimation method report, on average, larger positive (or smaller negative) correlations between taxation and income inequality. There is no evidence, however, that the time period covered by the sample makes a difference to the results, nor whether or not government spending is included in the regression as a control variable.

Finally, with regard to the different measures of taxation, the coefficients for direct taxes, indirect taxes, and total tax revenue (as a share of GDP) are all positive and statistically significant in the majority of cases. This indicates that measures of tax revenues (as a share of GDP) show a smaller negative (or larger positive) relationship with inequality, in comparison with measures of tax progressivity (the base case). This is particularly the case with *indirect taxes*, which have the largest coefficients in Table 14. This is not surprising, given the evidence that indirect taxes are often found to be regressive in nature (see Section 5).

Ideally we would investigate these results further by carrying out subgroup analysis for each type of tax measure, as we did for the different measures of government spending in Section 4.2.1, but we are limited in our ability to do this by the smaller number of observations.

Table 14: MRA of the effects of taxation on income inequality (dependent variable=partial correlation)

	(1) FAT-PET WLS	(2) WLS	(3) WLS general	(4) WLS specific	(5) Robust
Standard error	3.189**	3.094**	2.903		1.187***
	(1.332)	(1.376)	(2.023)		(0.204)
Gini		-0.044	-0.020		-0.049**
		(0.029)	(0.031)		(0.020)
Total tax revenue			0.115		0.364***
			(0.118)		(0.024)
Direct taxes			0.071***	0.087***	0.070***
			(0.014)	(0.013)	(0.016)
Indirect taxes			0.139***	0.148***	0.199***
			(0.032)	(0.028)	(0.018)
LAC			-0.636***	-0.414***	-0.845***
			(0.203)	(0.013)	(0.043)
SSA			0.735***	0.440***	0.849***
			(0.119)	(0.009)	(0.033)
SA			-0.622***	-0.372***	-0.801***
			(0.116)	(0.012)	(0.042)
Developed			-0.187***	-0.237***	-0.243***
			(0.054)	(0.043)	(0.023)
OLS			0.030**	0.030***	0.026
			(0.010)	(0.009)	(0.021)
Year data			-0.007		-0.000
			(0.009)		(0.002)

	(1) FAT-PET WLS	(2) WLS	(3) WLS general	(4) WLS specific	(5) Robust
GOV			0.033		-0.018
			(0.085)		(0.022)
Unpublished			0.092		0.051***
			(0.116)		(0.019)
Constant	-0.134**	-0.088	0.481	0.520***	0.933***
	(0.054)	(0.076)	(0.353)	(0.043)	(0.077)
N	128	128	128	128	128
R^2	0.214	0.218	0.613	0.482	0.929

Notes: Columns report variants of regression 2. Standard errors are reported in parentheses. All regressions use cluster standard errors to adjust for data dependence, i.e. multiple estimates per study. All columns use weighted least squares, except for regression 5, which uses robust regression. The tax progressivity measures variable is used as the base category for the dummy variable type of trade. In regression 4 we employed a general-to-specific modelling strategy, removing the variable that had the largest p-value until all p-values are <0.05. For definitions of variables see Table 9. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

IS THERE A RELATIONSHIP BETWEEN TAXATION AND INCOME INEQUALITY?

To answer the question of whether there is a strong association between measures of taxation and income inequality, we follow a similar approach as in Section 4.2. We calculate the average (or predicted) relationship between each type of tax measure and income inequality, implied by the results in regression 3 of Table 14, for a certain set of values of the moderator variables. We consider a study that is published, uses a more robust non-*OLS* analytical approach, includes all developing country regions in the sample, uses a period of time centred on 1994, uses the *Gini* coefficient as the inequality measure, and controls for government spending. In addition, we show the results separately for samples excluding and including developed countries, since this emerged as a key factor from Table 14.

The results are shown in Table 15. Panel A shows that the average relationship between the three measures of tax revenue (as a % of GDP) and income inequality is in fact positive for samples excluding developed countries. However, the size of each relationship is relatively small and not statistically significant. The relationship between measures of tax progressivity and income inequality is negative, as expected, but the magnitude of association is again small and not statistically significant. The results in Panel B correspond to what is arguably the expected pattern, in which measures of tax revenue, particularly direct tax revenue, and tax progressivity, are all negatively associated with income inequality. Nevertheless, the size of the relationships remain small to moderate in size, and are statistically significant only for tax progressivity measures.

Table 15: Predicted (or average) effects of tax measures on income inequality

	Total tax revenue	Direct taxes	Indirect taxes	Tax progressivity
A. Samples excluding developed countries				
Gini	0.086	0.042	0.110	-0.030
B. Samples including developed countries				
Gini	-0.101	-0.145	-0.077	-0.216**

Notes: Predictions based on the results from regression 3, where the following values of each moderator variable are used: standard error=0; Gini=1; developed=0 (Panel A), developed=1 (Panel B); unpublished=0; year=0; OLS=0; LAC, SSA, SA all equal to 1; GOV=1.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

SUMMARY

In this section we have explored the relationship between measures of taxation and income inequality using meta-regression analysis. Our main findings can be summarised as follows.

First, we find little evidence that the measure of inequality used makes a difference to the results, but this is mainly because the vast majority of studies rely on the *Gini* coefficient. It would be better in future if econometric studies of the relationship between taxation and income inequality reported the results for a wider range of inequality measures, to allow more testing of the extent to which results are sensitive to this choice.

Second, we find consistent evidence that the analytical approach used makes a difference to the results – although by a smaller magnitude than was the case with government spending measures. Studies relying on *OLS* appear to have had a tendency to underestimate the contribution of taxation to the reduction of inequality, compared with more robust analytical approaches. This is an important finding that again indicates the importance of using more robust analytical approaches with lower risk of bias, such as panel data methods and instrumental variables (IV) estimation.

Third, we again find consistent evidence of publication bias. As was the case for government spending measures, it appears that negative estimates are being under-reported in the literature. It is again difficult to say precisely what might be driving this bias, but we are at least able to correct for publication bias when determining whether or not there really is a relationship between taxation and inequality.

Finally, in terms of our central question – is there a strong association between measures of taxation and income inequality – we find that the answer depends very much on the sample of countries included in the analysis. Only when including developed countries do we see evidence of

a negative relationship between taxation – in particular, the progressivity of taxation – and income inequality. For samples excluding developed countries, however, there is no evidence of such a relationship. This finding is consistent with evidence from ex-ante simulation studies, discussed further in Section 5, which suggests that the redistributive effect of taxation in developing countries has been limited, at least until now (e.g. Tanzi 1974, Bird and Zolt 2005, Chu et al. 2000, Martinez-Vazquez 2004, Goni et al. 2011, Mahon 2012, Claus et al. 2012). It is an important finding as it suggests that there may be 'structural' factors which to some extent limit the amount of redistribution that can be achieved via taxation in the short to medium term, particularly in low-income countries.

4.4 SYNTHESIS AND META-ANALYSIS: TRADE POLICY

Similar to Sections 4.2 and 4.3, in this section we use a meta-regression approach to establish whether there is a relationship between measures of trade policy and income inequality, and to explore the reasons for any heterogeneity in the size or direction of this relationship.

META-REGRESSION RESULTS

Our main results are shown in Table 16. Similar to the previous sections, regression 1 reports the FAT-PET results where the standard error of the partial correlation coefficient is regressed on the partial correlation coefficient. In regression 2 additional dummy variables are added representing different income inequality measures; regression 3 is our main model as it includes all potentially relevant explanatory variables; regression 4 employs a general-to-specific modelling strategy; and regression 5 reports the estimates from a robust regression. Recall that our measures of trade policy are defined in such a way that higher values of each measure imply higher trade barriers. Thus, a positive relationship between a trade policy measure and income inequality implies that trade protectionism raises inequality, while trade liberalisation (i.e. a reduction in trade barriers) reduces inequality. By contrast, a negative relationship implies that trade protectionism reduces inequality, while trade liberalisation raises inequality.

In contrast to the results for government spending and tax, Table 16 shows no evidence of publication bias: the coefficient for the standard error is not statistically significant in any of the regressions. With regard to the measures of inequality, there is some evidence that using the *Gini* coefficient leads to a more negative (less positive) relationship between trade policy and inequality, as indicated by the negative and statistically significant coefficients on the *Gini* variable in regressions 2 and 5.

With regard to the sample coverage, the coefficient for *developed* is positive and statistically significant at the 1 percent level or below in regressions 3–5. This indicates that studies including developed countries find on average a smaller negative (larger positive) relationship between measures of trade policy and income inequality. The regional composition of the sample also makes a difference: the negative and statistically significant coefficients for the *SSA–SA* variables in regressions 3–5 imply that studies including countries from these regions find a larger negative (or smaller positive) correlation between trade policy and income inequality.

With regard to analytical approach, the coefficient for *OLS* is relatively small in size and statistically significant only in regression 5. There is no evidence in this case therefore that the estimation method makes a difference to the results. The coefficients for *year data* are also small in size, indicating that the time period covered by the sample also makes little difference to the estimated relationship between trade policy and inequality.

Finally, with regard to the different measures of trade policy, there is no evidence from regression 3 of any statistically significant differences between the three types of measures considered here, namely import tariffs (the base case), trade policy measures, and other measures of trade policy. The only exception is in regression 5, which suggests that studies using trade policy indices (e.g. Sachs-Warner index) find a larger negative (smaller positive) relationship between trade policy and income inequality, while other measures of trade policy (e.g. non-tariff barriers, export duties) find a smaller negative (or larger positive) relationship.

Table 16: MRA of the effects of trade policy on income inequality (dependent variable=partial correlation)

	(1) FAT-PET WLS	(2) WLS	(3) WLS general	(4) WLS specific	(5) Robust
Standard error	-0.289	-0.648	0.203		0.003
	(0.743)	(0.789)	(0.642)		(0.008)
Gini		-0.115*	-0.088		-0.089***
		(0.061)	(0.055)		(0.018)
Trade policy indices			-0.030		-0.271***
			(0.258)		(0.030)
Other trade measures			0.106	0.184***	0.128***
			(0.101)	(0.058)	(0.025)
SA and SSA			-0.331***	-0.348***	-0.384***
			(0.079)	(0.068)	(0.025)
LAC			0.044		0.085***
			(0.076)		(0.031)
Developed			0.279***	0.314***	0.307***
			(0.089)	(0.034)	(0.024)
OLS			-0.059		0.029
			(0.127)		(0.017)
Year data			0.003		-0.009***
			(0.013)		(0.002)
GOV			0.025		-0.022
			(0.095)		(0.018)
Unpublished			-0.024		-0.127***
			(0.115)		(0.019)
Constant	0.046	0.165**	0.120	0.057	0.260***
	(0.052)	(0.076)	(0.158)	(0.064)	(0.032)
N	94	94	87	94	87
R^2	0.074	0.145	0.543	0.167	0.912

Notes: Columns report estimates variants of regression 2. Regressions 1, 2, and 3 use 94 estimates from 19 studies, while regressions 3 and 4 use 87 estimates from 18 studies. Standard errors are reported in parentheses. All regressions use cluster standard errors to adjust for data dependence, i.e. multiple estimates per study. All columns use weighted least squares, except for regression 5, which uses robust

regression. In regression 4 we employed a general-to-specific modelling strategy, removing the variable that had the largest p-value until all p-values are <0.05. For definitions of variables see Table 9. Import tariffs are used as a base in the dummy for the trade variable. In order to test for multicollinearity we use the variance inflation factor (VIF) for both the general (3) and specific (4) regressions; the mean VIF is 3.27 and 2.87, respectively, which is not a case for concern. According to Hosmer and Lemeshow (2000), values of VIF exceeding 10 are often regarded as indicating multicollinearity and should be investigated.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

IS THERE AN ASSOCIATION BETWEEN TRADE POLICY AND INCOME INEQUALITY?

To answer the question of whether there is a strong association between trade policy and income inequality, we follow a similar approach as in Sections 4.2 and 4.3. We calculate the average (or predicted) relationship between trade policy and income inequality, implied by the results in regression 3 of Table 16, for a certain set of values of the moderator variables. We consider a study that is published, uses a more robust non-*OLS* analytical approach, includes all developing country regions in the sample, uses a period of time centred on 1987, uses the *Gini* coefficient as the inequality measure, and controls for government spending. We again show the results separately for samples excluding and including developed countries, since this emerged as a key factor from Table 16.

The results are shown in Table 17. Panel A shows that, for samples excluding developed countries, the average relationship between the three measures of trade policy and income inequality is negative and moderate in size, but not statistically significant. For samples including developed countries, the average relationship is positive, but again not statistically significant (Panel B). Overall, therefore, we find no evidence here of any association, positive or negative, between measures of trade policy and income inequality.

Table 17: Predicted (or average) effects of trade policy measures on income inequality

	Import tariffs	Trade policy indices	Other trade measures
A. Samples excluding developed countries			
Gini	-0.230	-0.259	-0.124
B. Samples including developed countries			
Gini	0.050	0.019	0.155

Notes: Predictions based on the results from regression 3 with the following values of each moderator variable are used: standard error=0; Gini=1; developed=0 (Panel A), developed=1 (Panel B); unpublished=0; year=0; OLS=0; LAC, SSA, SA all equal to 1; GOV=1.

SUMMARY

In this section we have explored the relationship between measures of trade policy and income inequality using meta-regression analysis. Our measures of trade policy are defined in such a way that higher values of each measure imply higher trade barriers. Overall, our results in this section are more mixed, with fewer consistent patterns than was the case for government spending or taxation in Sections 4.2 and 4.3. In our sample of studies we see a range of estimates of the relationship between trade policy and income inequality, including both positive and negative estimates, but the average relationship is not statistically different from zero. In addition, our meta-regression model has limited success in terms of explaining the variation in the size and direction of the estimated relationship. We find some evidence that the average relationship is more negative in samples including just developing countries, but even in this case the results are not statistically significant. Overall, therefore, we find very little evidence of an association between trade policy and income inequality.

5. RESULTS (III): SYNTHESIS, EX-ANTE SIMULATION STUDIES

5.1 INTRODUCTION

As discussed in Section 3, we identified 177 studies using ex-ante simulation methods which meet our inclusion criteria. For the purposes of synthesis we restrict the focus to studies reporting estimates of the effect of fiscal or trade policy simulations on income inequality at the national level. A total of 148 studies meet these more restricted criteria. In this section we report the results of our synthesis and meta-analysis for these studies.

For each of the policy simulations contained in each study, we calculate the percentage change in inequality before and after the policy change thus:

$$d = \frac{I(y_1) - I(y_0)}{I(y_0)} * 100$$

where *y* is a measure of income, *I* is some summary measure of inequality (e.g. the Gini coefficient, Theil index), and subscripts 0 and 1 indicate the time period before and after the policy change. A positive value of *d* implies that the policy change increases inequality, while a negative value implies that it reduces inequality.

The use of proportional as opposed to absolute changes in inequality is used to control for differences in inequality measures used. Although most studies use the *Gini* coefficient, other measures are also used (e.g. the Theil index, income shares), which are not directly comparable with the *Gini*. By measuring impacts in proportional terms, the results are more comparable across the different inequality measures. Note that for studies measuring inequality by income shares, we use the results for shares of income received by the richest households, rather than the poorest households. In this way, a higher value of each inequality measure always corresponds to higher income inequality.

Given the fact that some studies carry out more than one policy simulation and use more than one measure of inequality, and that some studies focus on more than one country (and year), we have a total sample of 1,106 observations. Each one of these observations corresponds to the impact of a particular fiscal or trade policy simulation on a particular measure of income inequality at the national level, in a particular country and year. In some cases we were unable to calculate the value of *d* due to missing information, in particular when studies report the absolute change in inequality in response to a policy simulation, but not the baseline level of inequality. A total of 31 potential observations from 10 studies were lost in this way.

Table 18 shows some basic descriptive statistics for our sample of observations, both in total and separately for fiscal and trade policy simulations. The range of impacts is shown to be relatively large, especially for the fiscal policy simulations. The largest negative effect on income inequality is 89 percent; at the other extreme, the largest positive effect is 27 percent. These overall figures do not, however, reflect the diversity among the estimated impacts, in terms of the analytical approach used (e.g. standard incidence analysis vs CGE models), and the more specific type of

fiscal or trade policy simulation carried out (e.g. tax reforms vs cash transfer programmes). In the next two subsections we disaggregate further between the results in each of the two main policy areas with a view to explaining some of the diversity shown in Table 18.

Table 18: Effects of fiscal and trade policy simulations on income inequality: descriptive statistics and frequency distribution

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Fiscal policy simulations	-8.7	-2.4	16.1	-89.4	26.7	107	943
Trade policy simulations	0.3	0.0	1.7	-7.0	14.0	35	163
TOTAL	-7.4	-1.8	15.2	-89.4	26.7	138	1106
B. Frequency distribution							
	Reductions in inequality[#]				**Increases in inequality[#]**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Fiscal policy simulations	144	203	440	49	88	15	4
Trade policy simulations	0	2	68	10	82	1	0
TOTAL	144	205	508	59	170	16	4

Notes: The numbers in this table refer to the percentage change in a given measure of income inequality from a given base year; [#]small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent. The underlying figures were extracted from the 177 studies that use Study Design B to estimate the effects of trade or fiscal policy on a measure of income inequality at the national level. A list of the included studies is provided in Appendix 4.

A final point worth noting relates to risk of bias. The ex-ante simulation studies reviewed in this section encompass three main analytical approaches. The first and simplest approach, which we refer to as standard incidence analysis, assumes there are no behavioural responses to fiscal or trade policy changes. This approach is typically justified as being relatively straightforward to implement, and providing a reasonable first approximation of the true effect of a policy change (Sahn and Younger 2003, Martinez-Vazquez 2004). Nevertheless, by not taking into account behavioural responses to policy changes it has an inherent 'risk of bias'. This risk is greater when considering the medium-term effects of policy changes – since in the immediate short term individuals may be limited in their ability to adjust their behaviour in response to policy changes – and also when considering larger, more substantial changes in policy, since small policy changes at the margin may be argued to have less effect on behaviour.

The two other approaches are behavioural incidence analysis and applied computable general equilibrium (CGE) analysis. Behavioural incidence analysis is a general term used here to refer to studies which take into account some behavioural responses considered important, although within a partial equilibrium framework. Applied CGE analysis, by contrast, estimates the impact of fiscal and trade policy interventions in the context of a model of the whole economy, allowing for a range of behavioural responses, linkages, and spill-overs between different sectors of the economy. In theory, applied general equilibrium analysis should provide more accurate estimates of the effects of fiscal and trade policy interventions than standard incidence analysis. However, they often only take some behavioural responses into account, and they rely on the accuracy of the equations and parameters which are used to construct them. In addition, CGE models are computationally complex and the results are often sensitive to modelling choices.

In summary, therefore, standard fiscal incidence analysis is relatively straightforward to implement, but has an inherent risk of bias since it does not take into account behavioural responses to policy changes. However, behavioural incidence analysis and applied general equilibrium analysis are also subject to bias, depending on the range of behavioural responses taken into account and the reliability of the estimates of relevant elasticities included in the analysis. This in turn means that it is difficult to simply rank the three approaches in terms of 'high', 'medium', and 'low' risk of bias. Instead, we investigate the extent to which the results differ across these three approaches, exploring the extent to which conclusions drawn from one approach are backed up by results from other approaches.

5.2 SYNTHESIS: FISCAL POLICY

In this section we review the evidence on the effects of fiscal policy interventions on income inequality. We distinguish between the results from average and marginal fiscal incidence analysis (Martinez-Vazquez 2004). Average fiscal incidence analysis compares the existing level of inequality with a counterfactual situation in which one or more taxes or spending programmes are set at zero. For example, we might compare the level of inequality under the prevailing rate of VAT (say 15 percent) with a counterfactual scenario in which VAT is removed (i.e. set at 0 percent). This gives the average or 'total' effect of VAT on income inequality. Marginal incidence analysis, by contrast, looks at smaller changes in tax rates or levels of spending. For example, we might compare the distribution of income under a VAT of 15 percent with a counterfactual scenario in which VAT is increased to 16 percent. This gives the effect of a change in VAT (in this case a 1 percent increase) on the distribution of income.

Of these two approaches, average incidence analysis is more common: we have 754 estimates in this case, derived from 85 studies, compared with 189 estimates from 25 studies for marginal incidence analysis. Nevertheless, the results from marginal incidence analysis are often considered to have more relevance to policy-makers, since they correspond more closely to the sorts of fiscal policy reforms typically implemented in practice (Bourguignon et al. 2006, de Ferranti et al. 2004). They are also considered to be less demanding in terms of data requirements, and less subject to bias, since small policy changes at the margin may be argued to have less effect on behaviour.

5.2.1 AVERAGE INCIDENCE ANALYSIS

Table 19 shows the estimated effects of fiscal policy interventions on income inequality obtained from average incidence analysis. These results give an indication as to which types of fiscal policy interventions – for example, direct taxes and transfers, indirect taxes and subsidies, in-kind transfers – have the largest overall impacts on income inequality.

Table 19: Effects of fiscal policy simulations on income inequality: average incidence analysis

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers	-16.2	-4.8	21.8	-89.4	0.8	52	358
Income taxes	-3.7	-2.3	5.5	-28.5	14.0	33	128
Indirect taxes	2.3	0.0	5.9	-1.8	26.7	9	37
In-kind transfers	-6.4	-3.9	5.6	-25.5	-0.1	16	76
Other fiscal policies	-4.1	-3.3	2.5	-7.6	-1.3	1	6
Combined fiscal~	-9.6	-6.8	12.2	-53.2	20.5	27	149
B. Frequency distribution							
	Reductions in inequality#			No change	**Increases in inequality#**		
	Large	Moderate	Small		Small	Moderate	Large
Cash transfers	108	68	164	9	9	-	-
Income taxes	4	30	75	14	3	2	-
Indirect taxes	-	-	12	7	14	2	2
In-kind transfers	2	29	45	-	-	-	-
Other fiscal policies	-	2	4	-	-	-	-
Combined fiscal~	30	60	31	4	19	4	1

Notes: The numbers in this table refer to the percentage change in a given measure of income inequality from a given base year; #small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent; ~combined fiscal policy simulations involving changes in taxes and spending.

Table 19 shows that the largest impacts are observed for cash transfer programmes: the average effect of cash transfers across all estimates is to reduce inequality by 16.2 percent (358

observations, 52 studies). Direct income taxes are also found to lower inequality, although in this case the effects are generally smaller: the average effect is a reduction of 3.7 percent (128 estimates, 33 studies). Similarly, in-kind transfers lower inequality on average, although again with a smaller effect than cash transfer programmes: a reduction of 6.4 percent (76 estimates, 16 studies). Indirect taxes, by contrast, are shown to raise inequality, although the effects are again generally small: the average is an increase in inequality by 2.3 percent (37 estimates, 9 studies).

The results in Table 19 also highlight the large diversity in the estimated impacts of fiscal policy interventions on income inequality. Considering cash transfers, for example, the largest negative impact is minus 89 percent; this is the estimated impact of social transfers on the coefficient of variation of log income in Bulgaria in 2010, from the study by Tsanov et al. (2013). But despite some very large negative effects, for 164 out of 358 observations the size of the negative impact is relatively small (less than 5 percent), and 9 observations show a positive effect of cash transfers on inequality, even though the size of the impact is again small.[24] For direct income taxes, the largest negative impact is minus 28.5 percent; this is the estimated impact on the Gini coefficient in Slovakia in 2006, from the study by Zaidi (2009). But for 19 observations, direct taxes have either a zero or a small to moderate positive impact on inequality. For indirect taxes, although the average effect is to raise income inequality, in more than half of observations the estimated effect of indirect taxes is either zero or a small reduction in inequality.

In Appendix 6 we provide a more detailed statistical analysis which helps explain the diversity of the estimated total impacts of fiscal policy interventions on income inequality. Three main findings stand out.

First, there are some significant differences by region. For Latin America, the effects of direct taxes are negative but small on average, and the negative effect of in-kind transfers on inequality significantly exceeds that of direct cash transfers. This mirrors the findings of other recent studies on the effect of fiscal policy on inequality in this region (e.g. Goni et al. 2011, Lustig et al. 2012). In Eastern Europe and Central Asia, however, fiscal policy has a much larger negative effect on income inequality, particularly via direct taxes and cash transfers. Larger negative effects of direct taxes and cash transfers on inequality are also observed in East Asia and Pacific, and also in sub-Saharan Africa – although in this case the results are driven primarily by those for South Africa, which accounts for the majority of observations. In East Asia and Pacific, we also see evidence that indirect taxes raise inequality by much more on average than they do in Latin America.

Second, there are significant differences by level of average income, as measured by GNP per capita. The negative impacts of cash and in-kind transfers on inequality tend to become larger (in absolute terms) as a country's GNP per capita rises. In middle-income countries, for example, cash transfers reduce income inequality by 17.4 percent on average, but in low-income countries the

[24] Of these nine observations, one is the impact of public transfers on the Gini coefficient in Nepal in 1995, from Kang (2004); another five refer to the impact of public transfers on different measures of inequality in Vietnam in 2004 and 2006, from Van den Berg and Cuong (2011); and the last is the impact of a targeted social pension on the Gini coefficient in Argentina, from Acosta et al. (2011). In all other cases, the effect of cash transfers on inequality is negative.

average effect is a reduction of 2.8 percent. Similarly, in-kind transfers reduce inequality by 6.6 percent on average in middle-income countries, but by 1.9 percent in low-income countries.[25] More surprisingly, however, the negative impact of direct taxes on inequality is similar between low- and middle-income countries: 3.9 percent compared with 2.3 percent respectively. More broadly, we find no tendency for the negative impact of direct taxes on inequality to become larger as GNP per capita rises.

Third, there are some significant differences in the results between the different analytical approaches. For example, the average effect of cash transfers on income inequality estimated by standard fiscal incidence analysis (minus 18.2 percent) is over twice the size of the average impact estimated by CGE models (minus 7.6 percent). Although both of these approaches are subject to bias, and the types of policy simulations carried out differ, this could reflect a tendency for studies relying on standard fiscal incidence analysis to overestimate the impacts of cash transfers on inequality.

This finding should be treated with caution, however, since we are only able to provide a direct comparison of results between analytical approaches in three cases: the study by Debowicz and Golan (2014) on the effects of the Oportunidades cash transfer programme in Mexico, the study by Tiberti et al. (2013) on the effects of child support grants in South Africa, and the study by Salanauskaite and Verbist (2009) on the effects of reforms to the targeting of child allowances in Lithuania. These studies are the only ones that report and compare estimates derived from different analytical approaches. Two of these studies (Debowicz and Golan 2014, Tiberti et al. 2013) in fact find only small differences between the estimated impacts derived from partial equilibrium approaches (standard or behavioural) and those obtained from a CGE model. Furthermore, Salanauskaite and Verbist (2009) show that estimated impacts on inequality are significantly larger when using behavioural incidence analysis than standard incidence analysis.

MARGINAL INCIDENCE ANALYSIS

Table 20 shows the impacts of fiscal policy interventions on income inequality derived from marginal incidence analysis.

For **cash transfers** (27 observations, 7 studies), the majority of estimates refer to the estimated impacts of expansions in cash transfer programmes, either in terms of the size of the transfer or in terms of coverage and eligibility. Four of these focus on Brazil in the early 2000s (Azzoni and Guilhoto 2007, Cury et al. 2010, Tavares et al. 2009, Fochezatto and Bagolin 2012). The estimated effects are all negative, but are typically small in size – a reduction of inequality of at most 3 percent. Two similar studies, one on the effects of expanding the Oportunidades cash transfer

[25] Note, however, that the relationship between GNP per capita and the redistributive impact of cash and in-kind transfers applies within low- and middle income countries, and not just to the comparison between low- and middle-income countries. Once we control for the level of GNP per capita, the boundary between low- and middle income countries has no statistically significant impact on the redistributive impact of cash or in-kind transfers (see Appendix 6).

programme in Mexico (Debowicz and Golan 2014) and another on the effects of increases in the value of and/or eligibility for child support grants in South Africa (Tiberti et al. 2013), report estimates of similar direction and size: a reduction of inequality of up to 3 percent depending on the modelling assumptions made.

For **direct taxes** (29 observations, 5 studies), the majority of estimates refer to the impact of shifts towards more simplified, 'flat' structures of income tax. Two studies using standard incidence analysis find that flat tax reforms raise income inequality – by between 3 and 20 percent in Estonia and Hungary in 2005 (Paulus et al. 2009b), depending on the measure of inequality used, although by less than 3 percent in Chile in 1996 (Engel et al. 1999), and with next to no effect in Brazil in 2002 (Fochezatto and Bagolin 2012). The one study using a CGE model is Amir et al. (2013), who analyse the impact of a more simplified income tax structure in Indonesia. Their study shows that this reform has only a minor impact on inequality: a 1 percent increase in the Gini coefficient. The one study using a behavioural approach is Duncan (2014), who analyses the impact of the flat tax reform in Russia in 2001. He finds that the overall effect of this reform was to *reduce* net income inequality, by between 7 and 9 percent depending on the precise measure of inequality used.

Table 20: Effects of fiscal policy simulations on income inequality: marginal incidence analysis

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers	-1.9	-0.7	2.7	-11.1	0.2	7	27
Income taxes	3.4	1.1	6.5	-9.4	20.4	5	29
Indirect taxes	0.3	-0.02	4.4	-4.2	20.0	5	26
Other fiscal policies	-0.2	0.0	1.5	-8.4	1.2	8	31
Combined fiscal~	-1.7	-0.5	2.7	-10.4	1.2	6	76
B. Frequency distribution							
	Reductions in inequality#				**Increases in inequality#**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Cash transfers	0	2	20	4	1	0	0
Income taxes	0	2	6	0	14	6	1
Indirect taxes	0	0	13	1	11	1	0
Other fiscal policies	0	1	11	7	12	0	0
Combined fiscal~	0	9	59	3	5	0	0

Notes: The numbers in this table refer to the percentage changes in a given measure of income inequality from a given base year; #small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent; ~combined fiscal policy simulations involving changes in taxes and spending.

For **indirect taxes** (26 observations, 5 studies), 1 study looks at the effects of rises in indirect taxes. Using standard incidence analysis, Engel et al. (1999) find that a rise in VAT from 18 to 25 percent, or doubling excise tax on gasoline, would raise inequality in Chile in 1996, but by no more than 3 percent, depending on the measure of inequality used. A further 2 studies look at the effects of reductions in indirect taxes in Brazil in the early 2000s by Ferreira Filho et al. (2010) and Fochezatto and Bagolin (2012), using multisector models, but in each case they find next to no effect on the Gini coefficient or, in the latter case, on the ratio of the share of the richest 20 percent of households to that of the poorest 20 percent. A further 2 studies look at the effects of simplifying the structure of VAT. Ramirez et al. (2006) find that a shift to a two-tier VAT system would reduce income inequality in Colombia in 2002, by between 1 and 4 percent. By contrast, Karl (2004) finds that unifying VAT rates at 10 percent would raise the Gini coefficient in Colombia in 2000 by 20 percent.

SUMMARY

In summary, the evidence reviewed in this section indicates four main findings.

First, fiscal policy interventions often have a large redistributive effect in developing countries, tending to reduce income inequality, but clearly not in all cases. There is a general tendency for the redistributive effect of fiscal policy to be greater, the higher the level of GNP per capita, particularly on the spending side. This is most likely due to higher overall levels of government spending on cash and in-kind transfers as GNP per capita rises. It in turn suggests that there are certain structural constraints which to some extent limit the amount of redistribution that can be achieved by fiscal policy in low-income countries, at least in the short term. Nevertheless, there are countries in which the redistributive effect of fiscal policy is substantial, even among lower-income countries. Such countries offer useful examples for other developing countries seeking to use fiscal policy to reduce income inequality.

Second, the redistributive impact of fiscal policy is greater for government spending than it is for taxation. This supports the conclusions of other researchers who have highlighted the limited impact of taxation on inequality in developing countries (Tanzi 1974, Bird and Zolt 2005, Chu et al. 2000, Martinez-Vazquez 2004, Goni et al. 2011, Mahon 2012, Claus et al. 2012).[26] It in turn provides the basis for a widespread view, namely that the capacity to reduce income inequality via the revenue side of the budget is limited. Nevertheless, our results also highlight important exceptions to this broad conclusion. We find a significant number of cases in which direct taxes

[26] According to Tanzi (1974, p. 73), for example, 'much of the empirical evidence available … supports the conclusion that the tax systems of Latin America have done little to modify the existing income distribution' (cited by Alesina 1998, p. 304). In Western Europe, by contrast, direct taxes do appear to make a significant contribution to lower inequality: an average reduction of 5 percentage points in the Gini coefficient (Goni et al. 2011, Table 1).

reduce inequality by more than 5 percent. Many of these are countries in Eastern Europe, where strongly redistributive taxation systems are typically a historical legacy of former socialist rule, and are something of a special case. Nevertheless, there are also examples from outside the region, including South Africa, South Korea, Indonesia, and Pakistan. These cases highlight the potential contribution that direct taxes can make to the reduction of income inequality, at least in middle-income countries. We also observe some cases in which indirect taxes increase inequality by more than 20 percent. This indicates the importance of designing indirect tax systems in such a way that avoids aggravating inequality.

Third, with regard to the spending side of the budget, in many cases the largest contribution to inequality reduction comes from in-kind transfers (e.g. subsidised health, education, housing), particularly in Latin America where such spending is estimated to reduce inequality in final income by around four percentage points, much larger than the effects of cash transfers (Goni et al. 2011). This is an important finding in and of itself, since it has often been argued that even supposedly 'pro-poor' government expenditures have little impact on income inequality; the groups that benefit the most are in the middle classes, particularly in urban areas (e.g. Alesina 1998, Davoodi et al. 2003).

Nevertheless, when considering the results for all regions, the largest reductions in inequality appear to be the result of cash transfer programmes rather than in-kind transfers. This finding is partly due to the large effects observed in Eastern Europe. Nevertheless, we also observe large effects of cash transfers on income inequality in other regions. This is a useful counterpoint to the argument sometimes made, that cash transfer programmes in developing countries are too small to have an impact on income inequality at the national level.

The final point relates to the distinction between average and marginal fiscal incidence analysis referred to at the start of this section. The results from marginal policy show much smaller impacts of fiscal policy interventions on income inequality. In many ways this is not surprising, since the simulations in this case refer to smaller changes in tax rates and/or government spending 'at the margin'. But it is an important finding nonetheless, since the sorts of policy simulations considered by marginal incidence analysis are typically much closer to the sorts of fiscal policy reforms actually considered and implemented by governments in practice. It suggests that any one single policy change is unlikely to have a large impact on income inequality; instead, substantial reductions in inequality are more likely to result from a succession of gradual and incremental changes to taxes and spending implemented over a period of time.

5.3 SYNTHESIS: TRADE POLICY

We now turn to a discussion of the results from the trade policy simulations. In this case we have 163 estimates from 35 studies (Table 21).

The majority of observations (134 estimates, from 32 studies) refer to the effects of trade liberalisation, namely reductions in, or elimination of, artificial barriers or inducements to trade, such as import tariffs, import quotas, export taxes, and export subsidies. The estimated effects range from a reduction in inequality by 7 percent to an increase in inequality by 4 percent; the

former is the effect of full trade liberalisation on the Theil index in Mozambique in 2003, from the study by Arndt and Thurlow (2010), while the latter is the effect of full import tariff liberalisation on the Gini coefficient in Sri Lanka in 1995, from the study by Naranpanawa et al. (2011). The average effect is close to zero, however.

A further 13 observations from 4 studies refer to the effects of trade protectionism, or increases in artificial barriers to trade. In these cases, the estimated effects are all very small in size (less than 1 percent), except in one case – the effect of increasing export taxes on the Theil index in Côte d'Ivoire in 1980, from the study by Lambert et al. (1991). This is a clear outlier, however. Finally, a further 16 observations from 2 studies focus on the effects of changes in the structure of import tariffs on income inequality, but the effects are again very small in size (less than 2 percent).

Table 21: Effects of trade policy simulations on income inequality

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Trade liberalisation	0.2	0.03	1.4	-7	3.8	32	134
Trade protection	0.9	-0.07	3.9	-0.6	13.9	4	13
Other trade reforms	0.4	0.3	1	-1.7	1.9	2	16

B. Frequency distribution							
	Reductions in inequality[#]				**Increases in inequality[#]**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Trade liberalisation	0	2	54	10	68	0	0
Trade protection	0	0	10	13	2	1	0
Other trade reforms	0	0	4	0	12	0	0

Notes: The numbers in this table refer to the percentage changes in a given measure of income inequality from a given base year; [#]small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent.

In Appendix 6 we provide a more detailed statistical analysis which aims to explain some of the variation in the estimated impacts of trade liberalisation on income inequality. In particular we investigate the effects of:

- the region, income status, and average income of the country undergoing liberalisation;
- the year in which the trade liberalisation takes place;
- the extent of liberalisation, either 'full', meaning the complete removal of all import tariffs, or 'partial', meaning only a reduction in import tariffs;
- the measure of inequality used, such as the Gini coefficient or the Theil index.

The results of this analysis provide some indication of differences across regions – for example, the average effect is -1 percent in sub-Saharan Africa, compared with 1 percent in South Asia, and the Middle East and North Africa. The average effect is also more negative when using the Theil index than the Gini coefficient. The largest difference in the average effects is in fact by the year of liberalisation: the average effect of trade liberalisation in the 1990s is positive, raising income inequality, whereas in the 2000s it is negative, tending to reduce inequality. These results are all statistically significant at the 10 percent level or below.

Despite these findings, however, the evidence from ex-ante simulation studies suggests more broadly that trade policy reforms typically do not have large impacts on income inequality. Only in a very small number of cases – 3 estimates out of a total of 163 – do we observe more than a small change in income inequality. This is even more surprising given that many of the trade policy simulations covered by these reviews are substantial, involving the liberalisation of all import tariffs in the majority of cases. In practice, trade policy reforms are much more partial and typically involve only the gradual reduction of tariffs and other trade barriers in some sectors.

6. RESULTS (IV): SYNTHESIS, QUANTITATIVE CASE STUDIES USING DECOMPOSITION ANALYSIS

6.1 INTRODUCTION

In this section we report the results of our synthesis for the 41 studies using Study Design C which meet our inclusion criteria and which in addition focus on income inequality at the national level. We present descriptive statistics for the relative contribution (in percentage terms) of taxes or transfers to overall income inequality, given by:

$$D_i = \frac{S_i R_i G_i}{G_y}$$

where G_i is the Gini index calculated with respect to income source i, S_i is the share of total income obtained from source i, R_i is the Gini correlation between income from source i and total income, and G_y is the overall Gini index.

This measure gives the estimated contribution to overall income inequality of income from taxes or transfers, in percentage terms. A positive contribution implies that the income source contributes to (i.e. increases) overall inequality, while a negative contribution implies that the income source reduces overall inequality. Another way of interpreting this statistic is the difference (in percentage terms) between the level of inequality in income with and without income from transfers or taxes. This makes it comparable with the effect size measure used for the ex-ante simulation studies analysed in Section 5.

6.2 SYNTHESIS: FISCAL POLICY

We are able to calculate the value of our chosen effect size measure for a total of 143 observations. Of this total, 92 refer to the contribution of cash transfer programmes (social assistance), 34 refer to the contribution of social insurance programmes (e.g. unemployment assistance, contributory pensions), 16 refer to the contribution of direct taxes and social security payments, and one refers to the contribution of in-kind transfers.

Table 22 shows that the contribution of cash transfers to inequality varies quite widely, from -32 percent to 13 percent. The largest contributions to the reduction of inequality are observed for the Czech Republic and Slovakia in 1993, from the study by Garner and Terrell (1998). Outside Eastern Europe, however, contributions to inequality reduction are much smaller, the largest in absolute terms being Chile in 2009, where contribution is 1 percent, from the study by Contreras and Ffrench-Davis (2012). By way of comparison, studies by Soares et al. (2007) and de Souza (2012) both put the contribution of Bolsa Familia to the reduction of overall inequality in Brazil, in 2004 and 2009, at around 0.5 percent; a similar figure is reported for cash transfer programmes in Mexico in 2004 by Soares et al. (2007). Surprisingly, the estimated contribution of social transfers to the reduction of inequality in South Africa is also very small (e.g. Bhorat and Jacobs 2009, Leibbrandt et al. 2001, 2013).

Some insights into the reasons for these results can be gained from examining the results for the so-called Gini income elasticities (GIEs). These reflect the pure redistributive effect of the transfer programmes, and unlike the proportional contribution, they are not affected by the size of the programmes (see Appendix A2.3). The GIEs for cash transfer programmes in Brazil and Mexico reported by Soares et al. (2007) and de Souza (2012) are around -0.9, indicating a fairly strong redistributive element. By way of comparison, the GIEs for the Czech Republic and Slovakia reported by Garner and Terrell (1998) are not that much larger, between -1.0 and -1.4. The main reason therefore for the much smaller overall negative contribution of cash transfers to overall inequality in Brazil and Mexico as compared with the Czech Republic and Slovakia is the much smaller size of these programmes. In particular, their share of total national income remains less than 1 percent, compared with close to 20 percent in the latter cases.

Table 22 also shows that the contribution of social insurance programmes (e.g. unemployment benefits, official contributory pensions) to income inequality is often positive, implying that these programmes raise inequality. For example, Hoffman (2011) reports positive contributions of official pensions to inequality in Brazil, rising from 15 percent in 1998 to 18 percent in 2007; a similar magnitude is reported by Litchfield et al. (2006) for social insurance in Brazil more generally, in 2004. Official pensions are also estimated to contribute more than 10 percent to overall inequality in Hungary in 2002 and Romania in 2004, in studies by Molnar and Galla (2009) and Stanculescu and Pop (2009).

Table 22: Contribution of taxes, transfers and social insurance to income inequality: results from decomposition analysis

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers (social assistance)	-0.7	-0.2	6.3	-32.0	13.2	28	92
Social assistance	5.9	4.1	7.4	-5.3	19.0	9	34
Direct taxes and social security contributions	-25.0	-26.5	9.3	-37.0	-5.1	6	16

B. Frequency distribution							
	Reductions in inequality[#]				**Increases in inequality[#]**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Cash transfers (social assistance)	3	6	50	1	25	7	0
Social assistance		1	10	3	4	16	0
Direct taxes and social security contributions	13	3	0	0	0	0	0

Notes: The numbers in this table refer to the percentage contribution of an income source to income inequality in a given year; [#]small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent.

For **income taxes** we have 16 observations, all but 2 of which are for countries in Eastern Europe. The contributions are all negative, indicating that direct income taxes reduce inequality. The largest contribution to the reduction of inequality is 37 percent, which is observed for Hungary in 1991 in the study by Kattuman and Redmond (2001). The contribution also reached 34 percent in the Czech Republic in 2002, according to Vecernik (2006). Outside Eastern Europe, there is just one estimate for Brazil in 2009, from the study by Medeiros and Souza (2013), where direct taxes also reduce inequality, by 14 percent, and one estimate for Greece in 1998, from the study by Papatheodorou (1998), where the contribution to inequality reduction is 5 percent.

We have only one estimate of the contribution of **in-kind transfers** to overall inequality using this approach – an estimate of the contribution of in-kind income and subsidies to inequality in China in 1998, from the study by Khan et al. (1993). According to this study, in-kind income and subsidies raise inequality by a substantial amount: 32 percent. There are, however, no other studies with which this finding can be compared.

In Appendix 7 we comment on a selection of the studies using decomposition analysis in more detail, focusing on the more technical aspects of results from Wodon et al. (2003) for Mexico, Garner and Terrell (1998) for the Czech and Slovak Republics, Leibbrandt et al. (2001) on South Africa, Khan et al. (1993) on China, Kattuman and Redmond (2001) on Hungary, Kakwani (1999) on Ukraine, Papatheodorou (1998) on Greece, Scorzafave and Carvalho de Lima (2010) on Brazil, and Contreras and Ffrench-Davis (2012) on Chile.

6.3 SUMMARY

The results of studies using decomposition analysis analysed in this section provide a series of additional insights into the effects of government fiscal policies on income inequality. Similar to the ex-ante simulation studies analysed in Section 5, they show the large redistributive potential of cash transfers, as evidenced in particular for countries in Eastern Europe. The redistributive effect of such programmes in other regions of the world, however, particularly in Latin America, has been limited by their relatively smaller scale as a proportion of total national income.

The results in this section also highlight the importance of distinguishing between social assistance and social insurance. Social insurance programmes (e.g. official pensions, unemployment benefits) are typically found to make a positive contribution to income inequality, often quite substantially: by 10 percent or more. Reducing the size of this contribution could therefore make a substantial contribution to the reduction of income inequality.

7. SUMMARY AND CONCLUSIONS

The issue of inequality has been a key issue in development for several decades now. Since the 1970s, a large literature has emerged which documents the many adverse effects of inequality. Moreover, income inequality remains high in a large number of developing countries. Over 50 low- and middle-income countries have Gini coefficients of income inequality exceeding 40, above which the potential to undermine progress in key development outcomes, or to conflict with basic notions of equity and fairness, is considered significantly greater. For this reason, there is a clear demand from policy-makers in national governments and international organisations for accurate, reliable, and up-to-date evidence about which policies and interventions can be used to reduce income inequality, and also which policies and interventions may (in the absence of complementary, offsetting measures) raise income inequality.

The overall aim of this systematic review is to identify and synthesise the evidence on the relationship between government policies and interventions and income inequality. The objectives of our review are fourfold:

1) to map the available evidence that seeks to evaluate or better understand the effects of government policies and interventions on income inequality, in low- and middle-income countries;

2) to establish whether any particular types of policies or interventions tend to reduce or increase income inequality *on average*: in other words, whether there are any consistent and generalisable findings or results across contexts and methods;

3) to explain heterogeneity in the estimated effect of such policies or interventions, across countries, regions, or over time ('structural' heterogeneity), or research methods used ('method' heterogeneity);

4) to understand better the processes and mechanisms through which government policies and interventions affect income inequality.

With regard to the first objective, we have identified a total of 407 studies providing evidence about the effects of one or more government policy interventions on income inequality, in one or more low- or middle-income countries. Although there have been other reviews of the literature on how government policies affect income inequality, we believe that the body of evidence we have identified and documented is by far the largest and most comprehensive assembled to date.

Our mapping of the 407 studies included in the review has generated some important findings. Research has tended to focus predominantly on the effects of government fiscal policies on income inequality: for example, the role of taxes and transfers, and government spending more generally. To a lesser extent, trade policy interventions, including the liberalisation (i.e. reduction) of tariff and non-tariff barriers to trade, are also reasonably well covered. But there is much less evidence on the impact of other sorts of government policies, such as labour market reforms, pension reform, privatisation, and land reform. Research has also tended to focus predominantly

on middle-income countries, particularly for the single-country studies. This may simply be the result of data availability, but it is problematic from the point of view of drawing policy implications in low-income countries.

In addition, there has been a clear reliance in recent research on the Gini coefficient as a measure of inequality. This is a potential problem because the Gini coefficient is by no means the 'best' measure of inequality, nor is it necessarily the most relevant from a policy perspective. The vast majority of econometric studies also adopt a basic linear framework, and do not test for the possibility of diminishing returns to the effect of government spending or taxation on income inequality, nor do they allow for the possibility of lagged effects (e.g. when government spending reduces inequality but only after a period of time). Since these relative gaps in the literature suggest priorities for future research, our mapping exercise provides an important resource for both researchers and policy-makers involved in the commissioning of research.

With regard to the second and third objectives, we focused the synthesis on studies which look at the effects of fiscal policy or trade policy interventions on income inequality. There is a sufficiently large body of comparable studies on these policy areas which can be subjected to meaningful synthesis and meta-regression analysis. By contrast, for most other intervention types (e.g. finance or labour market reforms), the number of studies is too small to allow meta-analysis. We also restricted the synthesis to studies focusing on income inequality at the national level, to further reduce the heterogeneity of studies. Our main findings from the synthesis of these studies are set out in detail in Sections 4, 5, and 6; here we highlight four key overall findings.

First, our synthesis of the *ex-post observational studies* shows that both the size and direction of the estimated relationship between measures of fiscal and trade policy and income inequality are affected by a range of factors. For example, we find fairly consistent evidence that studies relying on ordinary least squares (OLS) as an estimation method have had a tendency to overestimate the contribution of government spending to the reduction of inequality, compared with other more robust approaches with lower risk of bias, such as panel data methods and instrumental variables (IV).

We also find consistent evidence of publication bias, in that negative estimates of the relationship between government spending and income inequality are under-reported in the literature. One possible explanation for this is 'polishing' – in other words, the tendency for researchers and editors to report and publish results that are statistically significant. Another possible explanation is that researchers are reluctant to report negative relationships, perhaps because of ideological persuasion (e.g. a belief in limited government involvement in the economy). We are not able to disentangle these different possible explanations for publication bias, but we can at least correct for publication bias.

In terms of the central question of the review, we find evidence of a moderate negative relationship between government spending and income inequality, but only when considering certain sectors of spending – in particular, social welfare spending and consumption spending. However, we find little evidence of an association between measures of taxation and income inequality, nor between measures of trade policy and income inequality. Overall, therefore, ex-

post observational studies provide evidence that government spending in certain areas is associated with lower income inequality, but little evidence of a relationship between taxation and income inequality, or between trade policy and income inequality. This supports the conclusions of other researchers who have highlighted the limited impact of taxation on inequality in developing countries, and the mixed impacts of trade reforms.

Second, evidence from *ex-ante simulation studies* shows that the redistributive impact of fiscal policy is greater for government spending than it is for taxation. Nevertheless, it also highlights important exceptions. We find a significant number of cases in which direct taxes reduce inequality substantially, and some cases in which indirect taxes increase inequality substantially. This highlights the potential (if as yet unrealised) contribution of direct taxes to inequality reduction, and the importance of designing indirect tax systems in such a way that avoids aggravating inequality.

With regard to the spending side of the budget, we find that in many cases the largest contribution to inequality reduction comes from in-kind transfers (e.g. subsidised health, education, housing). This is an important finding, since it has often been argued that even supposedly 'pro-poor' government expenditures have little impact on income inequality, since the groups that benefit the most are in the middle classes, particularly in urban areas. Nevertheless, when considering the results for all regions, the largest reductions in inequality are the result of cash transfer programmes rather than in-kind transfers. This finding is partly due to the large effects observed in Eastern Europe, where large-scale welfare programmes are typically a historical legacy of former socialist rule, and which is as a result a special case. Nevertheless, we also observe large effects of cash transfers on income inequality in other regions. This is a useful counterpoint to the argument sometimes made, that cash transfer programmes in developing countries are too small to have impact on income inequality at the national level.

Third, the synthesis of the *studies using decomposition analysis* also support this finding, showing the large redistributive potential of cash transfers, as evidenced in particular for countries in Eastern Europe. However, the redistributive effect of such programmes in other regions of the world, particularly in Latin America, has been limited by their relatively smaller scale as a proportion of total national income. The results in this case also highlight the importance of distinguishing between social assistance on the one hand, and social insurance on the other. Social insurance programmes (e.g. official pensions, unemployment benefits) are typically found to make a positive contribution to income inequality, often quite substantially. Reforms to social insurance programmes could therefore make a substantial contribution to the reduction of income inequality.

A key issue when assessing the evidence on the effects of fiscal and trade policy is the distinction between 'first-round' and 'second-round' effects. For example, the immediate or first-round effect of cash transfers to households in the bottom income quintile is to reduce inequality in household *disposable* income. Beyond the immediate short run, however, households adjust their behaviour in response to taxes and transfers. For example, transfers to households in the bottom quintile may raise their productivity and earnings potential (e.g. by improving human capital), tending to

raise incomes. These second-round effects mean that taxes and transfers also affect households' *market* income, making the overall effect on income inequality harder to predict.

So-called standard fiscal incidence analysis, and also decomposition analysis, assumes there are no behavioural responses to government policy changes, and therefore measures first-round effects only. This is not particularly realistic, but is often justified as providing a 'reasonable first approximation' to the real results that would be obtained if behavioural responses were included. By contrast, computable general equilibrium (CGE) models take into account at least some of the likely behavioural responses to policy changes, and should therefore provide more accurate estimates of impact, depending on the accuracy of the underlying estimates used to predict these responses. Our findings do indicate significant differences between the results of these different approaches. For example, the average reduction in inequality resulting from cash transfers estimated by standard fiscal incidence analysis is over twice the size of the average figure from CGE models: 18.2 percent compared with 7.6 percent (see Section 5.2). This suggests a tendency for studies relying on standard fiscal incidence analysis to overestimate the impacts of cash transfers on inequality.

With regard to the fourth and final objective, we have made less progress. In Section 1 (and also Appendix 1) we set out a basic overall framework which illustrates how fiscal and trade policy interventions can affect inequality in four different concepts of income, namely market income, disposable income, real income, and final income. Nevertheless, because the review necessarily focuses on such a wide range of government policies and interventions, and a very large body of literature (over 400 studies), it has not been possible to provide more detail about the underlying processes and mechanisms by which each policy affects income inequality, particularly in market income. We believe that greater understanding of these mechanisms requires focusing in more detail on the specific government policy interventions. The underlying processes and mechanisms through which a cash transfer programme affects income inequality are likely to be quite different, for example, from a change in indirect taxes, or in trade policy, or in minimum wage legislation. It also requires examining other intermediary variables through which such policy interventions are likely to affect inequality – for example, we might examine how minimum wage legislation affects inequality in wages, which in turn contributes to income inequality.

We end with a discussion of certain other limitations of the review, and some potential areas for future review work.

First, the review has only included studies using a quantitative approach. Although we did seek to include studies using a qualitative design in the review, and we did identify several studies using a qualitative case study approach which discuss income inequality, we found on further scrutiny that they did not address in sufficient depth or detail the relationship between government policies and income inequality to be included in the review. We believe that qualitative study designs can play an important role in understanding the mechanisms by which policies affect income inequality, although as discussed this requires focusing in more detail on one or two specific policy interventions and examining impacts on intermediary variables.

Second, while the synthesis carried out in this review has also focused only on fiscal and trade policy interventions, further work could involve synthesising the results for other types of interventions, using narrative approaches (since meta-regression analysis is unlikely to be feasible).

Third, while this review has focused on income inequality, any measure of inequality is a summary statistic which can at best only summarise the often complex changes in income distribution which result from a government policy or intervention. For this reason, it may be worth complementing the current review with a further review which looks more broadly at the distributional impacts of a government policy or intervention, and not just the impacts on inequality. Information as to precisely who benefits and who loses from a policy is often hidden from the trend in a summary measure of inequality such as the Gini coefficient.

Fourth, while the review focuses on inequality across households or individuals, there are many studies which look at income inequality across groups: for example, between spatial units (e.g. states, provinces, regions) within countries, between urban and rural areas, and between demographic groups, as defined, for example, by gender or ethnicity. These 'between-group' inequalities often account for a large proportion of the overall amount of inequality observed across households and individuals, but are typically analysed separately. There is therefore a case for further review work looking at the effects of government policies on between-group inequalities; for example, a study on the effect of anti-discrimination legislation on income gaps between ethnic groups.

Finally, income inequality is just one type of inequality alongside many other important dimensions. This again suggests possible future directions for further synthesis work in this area, which could explore the effects of government policies on inequalities in other important dimensions, such as health or education.

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9. APPENDICES

A1 CONCEPTUAL FRAMEWORK

In the Protocol to this review, we set out a broad conceptual framework which shows the range of government policies and interventions that can affect income inequality, and the main transmission mechanisms involved. Here we provide a shorter and more condensed discussion, focusing on the two broad policy areas that form the basis of the synthesis stage of our review, namely fiscal policy and trade policy.

FISCAL POLICY

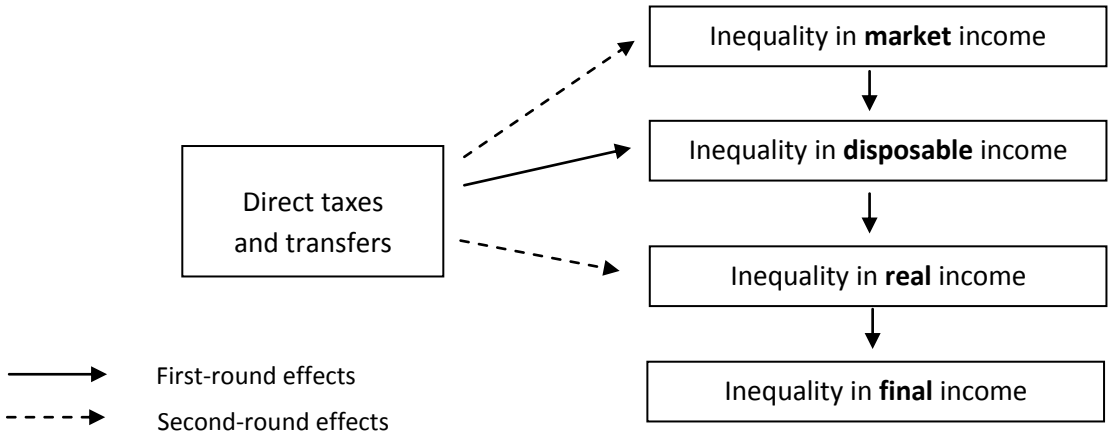
Perhaps most obviously, governments influence the level of income inequality through fiscal policy: that is, through choices about the level and structure of taxes on the one hand, and the level and composition of government spending on the other. Three main types of fiscal policy interventions are considered here: direct taxes and transfers, indirect taxes and subsidies, and in-kind transfers.

Direct taxes and transfers

Direct taxes and transfers represent one key area of fiscal policy for tackling income inequality. They have direct 'first-round' effects on inequality in households' disposable income. To the extent that households adjust their behaviour in response to taxes and transfers, with potential knock-on effects throughout the economy, they may also have 'second-round' effects on households' market income, and also on households' real income (via their effects on the prices of goods and services). This is shown in Figure A1.

For example, consider a direct tax-transfer scheme that taxes households in the top income quintile and uses the revenues to provide cash transfers to households in the bottom income quintile. The direct effect of the scheme is to reduce inequality in disposable income. Beyond the immediate short run, however, households may adjust their behaviour in response to the scheme, which can have knock-on effects. For example, taxes on households in the richest quintile may reduce incentives for investment, which may in turn lower employment opportunities for households in the poorest quintile. On the other hand, transfers to households in the bottom quintile may raise their productivity and earnings potential (e.g. by improving human capital), tending to raise incomes. These indirect effects mean that the tax-transfer scheme also affects households' market income. The overall effect of the scheme, taking account of the direct and indirect effects, is ambiguous.

Figure A1: Effects of direct taxes and transfers

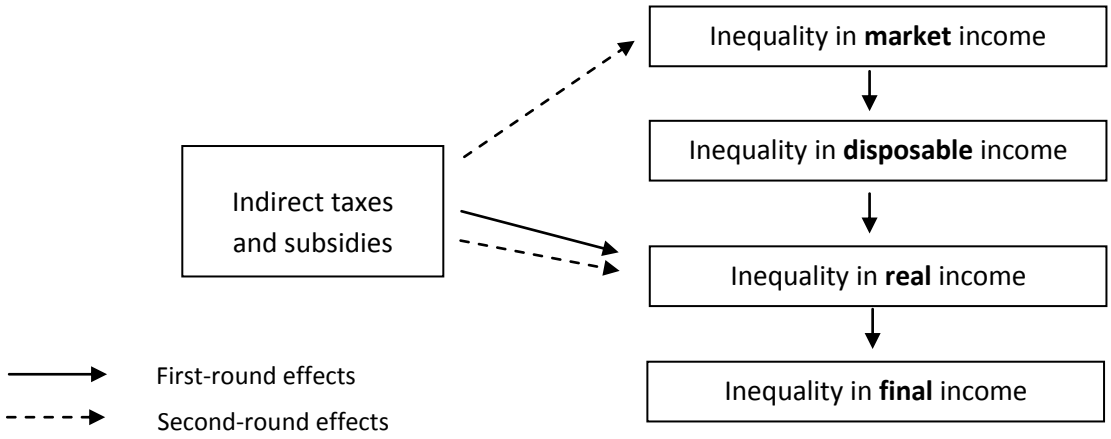


Indirect taxes and subsidies

In many developing countries, a larger share of tax revenue comes from indirect taxes (e.g. value-added tax (VAT), excise duties) rather than direct taxes. In Latin America, for example, around 60 percent of tax revenue comes from VAT, in comparison with 40 percent in OECD countries (Goni et al. 2011, Table 3). In addition, a significant share of government expenditure is made up of spending on indirect subsidies (e.g. fuel subsidies). In Indonesia, for example, fuel and electricity subsidies in 2011 amounted to 3.4 percent of GDP, more than government spending on infrastructure (Rhee et al. 2014). Policies on indirect taxes and subsidies can therefore have large potential impacts on income inequality. In theory, indirect taxes and subsidies have direct, first-round effects on inequality in households' *real income*. To the extent that households adjust their behaviour in response to indirect taxes and subsidies, they may also have second-round effects on households' market income, or on the prices of other goods and services in the economy, which also affects households' real income. This is shown in Figure A2.

For example, consider an indirect tax-subsidy scheme that taxes a luxury item (e.g. foreign cars) consumed by households in the top income quintile and uses the revenue to subsidise a basic product (e.g. kerosene) consumed by households in the bottom income quintile. The tax on the luxury item means that households in the top quintile spend more on the same basket of goods and services; their real income falls. By contrast, the subsidy on the basic item means that households in the bottom quintile pay less for the same basket of goods and services; their real income rises. The direct effect of the scheme is therefore a reduction in inequality in real income. Beyond the immediate short run, however, households may adjust their behaviour in response to the scheme. For example, households in the top income quintile may instead spend more on domestic goods and services, raising the prices of these goods for households in the bottom quintile. This in turn lowers real income for households in the bottom quintile. The overall effect of the scheme on inequality in real income could therefore be positive or negative.

Figure A2: Effects of indirect taxes and subsidies

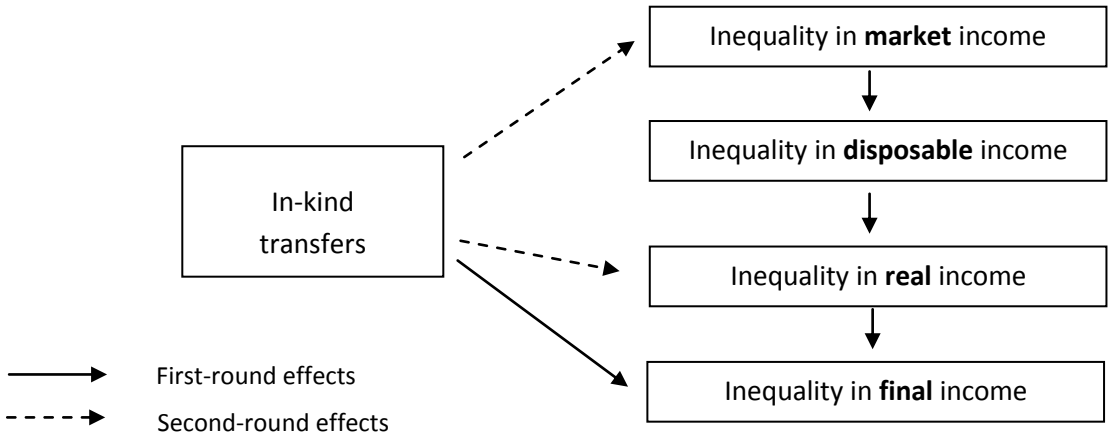


In-kind transfers

Despite the increasing role of cash transfer programmes in many developing countries, in-kind transfers are often more significant in terms of their share of total government expenditure and their potential impact on income inequality (Bearce et al. 2000). Once again, there is a distinction between first-round and second-round effects. In-kind transfers have direct, first-round effects on inequality in households' *final* income. They may also have second-round effects on households' market income, or on households' real income via their effects on the prices of goods and services in the economy. This is shown in Figure A3.

For example, consider a tax-transfer scheme which taxes households in the top income quintile and uses the revenues to provide a public service which is used equally by all households in the population. The direct effect of the scheme is to reduce inequality in disposable income, but only because of the tax side of the scheme; the public service has no direct impact on disposable income. However, the in-kind transfer reduces inequality in final income.

Figure A3: Effects of in-kind transfers

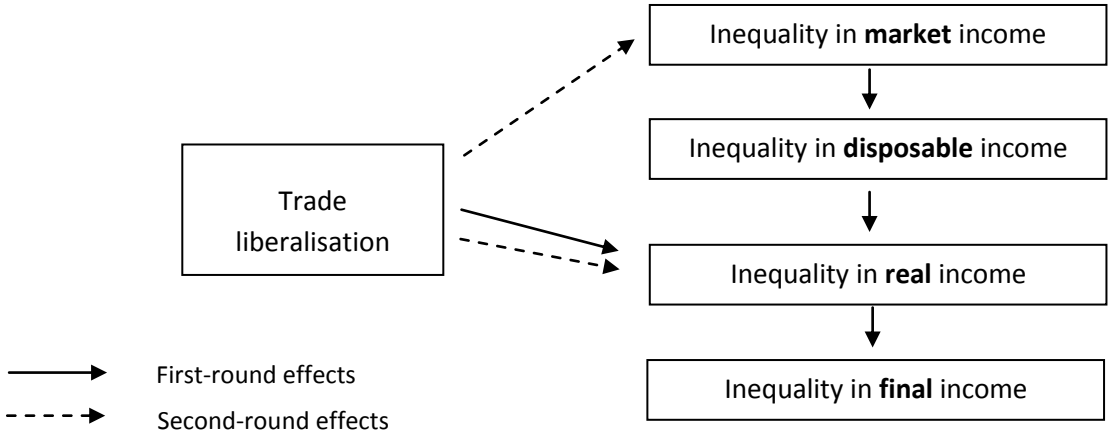


TRADE POLICY

In recent decades, a large number of developing countries have embarked on trade policy reforms aimed at reducing artificial barriers to international trade. This has involved the gradual liberalisation of import tariffs and quotas, as well as various other non-tariff barriers (e.g. Martin 2003, Sally 2008). It is widely accepted that trade policy reforms of this nature can have a large impact on income inequality.

Once again, a basic distinction can be made between direct and indirect effects (see Figure A4). Import tariffs and quotas tend to raise the domestic prices of imported goods and services. The direct or immediate effect of trade liberalisation in the form of tariff or quota reduction is therefore to reduce the domestic prices of imported goods. This affects inequality in household real income. Beyond the immediate short run, however, the reduction in tariffs is likely to have knock-on effects. Increasing competition from imports is, for example, likely to lead to some contraction of import-competing industries, with indirect effects on market incomes. The overall effects of trade liberalisation on inequality are therefore difficult to predict.

Figure A4: Effects of trade liberalisation



One important argument, based on Hecksher-Ohlin trade theory, is that trade liberalisation tends to reduce inequality in market income in low-income countries, by favouring the expansion of labour-intensive industries which boost the demand and therefore wages of low-skilled labour. Evidence in support of this argument from the experience of East Asia in the 1960s and 1970s is provided by Wood (1997). In middle- and high-income countries, by contrast, liberalisation can have the opposite effect. For example, the process of trade liberalisation in many Latin American countries during the 1980s and 1990s (e.g. Mexico, Chile, Colombia) is considered to have raised the demand, and hence the wages, of skilled relative to unskilled labour (Wood 1997, Robertson 2000, Gindling and Robbins 2001, Anderson 2005). This in turn contributed to rising levels of income inequality.

A2 STUDY DESIGNS

EX-POST OBSERVATIONAL STUDIES (STUDY DESIGN A)

Ex-post observational studies use econometric analysis to estimate the effects of government policies on income inequality. They involve estimating a regression in which a measure of income inequality is the dependent variable and the explanatory variables include one or more measure of government policy ('policy variables'). This can be written in the general form as:

$$I_{it} = \beta_0 + \beta_j X_{itj} + \beta_k Z_{itk} + \varepsilon_{it} \qquad (1)$$

where *I* is a measure of income inequality, *X* is a vector of policy variables, Z is a vector of other explanatory variables, and ε is the error term, with subscripts *i* and *t* indicating country (or region within a country) and year respectively.

A policy variable is an explanatory variable that is clearly and closely influenced by government policy. For this review, we adopt a relatively narrow definition of what constitutes a policy variable, to include:

- any measure of government spending or taxation;
- any direct measure of government trade policy, including measures of import tariffs and/or quotas, non-tariff barriers, and export taxes or quotas, but excluding measures of trade openness (defined as imports and/or exports as a share of GDP);
- any other direct measure of government policy, including measures of user fees for public services, price controls (e.g. interest rate ceilings, minimum wages), quantitative restrictions (e.g. limits on foreign direct investment, capital controls);
- any policy index which is calculated on the basis of one or more of the above indicators.

In addition, while we include all studies containing one or more policy variables (as defined above) in the mapping exercise, we further limit the synthesis and meta-analysis to fiscal and trade policy variables (the first two categories above).

As discussed in Section 3, the dependent variable may be measured using either a global or partial measure of inequality (e.g. the Gini coefficient, or the share of the poorest quintile in national income), measured across households or individuals, using either total income or total consumption expenditure as a proxy for income. One exception occurs in the case of an approach first used by Dollar and Kraay (2002), and adopted since then by other researchers. This involves a regression of the form:

$$y_{it}^{P} = \alpha_0 + \alpha_1 y_{it} + \alpha_j X_{itj} + \alpha_k Z_{itk} + \upsilon_{it} \qquad (2)$$

where y^P is the logarithm of the average income of the poorest quintile (or quintiles), and y is the logarithm of average per capita income for the population as a whole. Although the dependent variable in this case is not a valid measure of inequality, this regression can be rewritten as:

$$\ln\left(\frac{q_{it}^P}{0.2}\right) = \alpha_0 + (\alpha_1 - 1)y_{it} + \alpha_j X_{itj} + \alpha_k Z_{itk} + \upsilon_{it} \qquad (2a)$$

where q^P is the share of the poorest quintile (or quintiles) in national income.[27] It is clear therefore that the coefficients α_j estimated from a regression of the form indicated by equation (2) are identical to those obtained from equation (2a), where the dependent variable is a valid measure of inequality. Studies using this 'Dollar and Kraay' type approach are therefore included in the review.

Finally, the econometric studies included in this review must include low- and/or middle-income countries in the analysis; studies which focus only on high-income countries are excluded. It is worth noting, however, that many studies include countries from all income groups in the analysis, in the interests of increasing sample size. We include such studies on the grounds that they typically contain a significant proportion of low- and/or middle-income countries.[28] However, we exclude studies that focus on groups of countries which consist mainly of high-income countries: in particular, 'developed countries', 'OECD countries', 'advanced industrial countries', 'Western Europe', 'North America', or the European Union. Although some of these groups do sometimes contain one or two countries that are (or have at times been) middle income – for example, Mexico has been an OECD member since 1994 – they are overwhelmingly made up of high-income countries.

EX-ANTE SIMULATION STUDIES (STUDY DESIGN B)

Ex-ante simulation studies analyse the impact of government policies on income inequality using an economic model applied to recent empirical data for a particular country or region. The model contains a set of assumptions about how households and firms respond to government policy interventions, and can be used to compare the distribution of income under an initial or 'given' set of government policies with a counterfactual scenario in which one or more policy is changed from its initial level. The comparison can be written as follows:

$$D = I(y_1) - I(y_0)$$

[27] This follows from the fact that the average income of the poorest quintile (or quintiles) is equal to the share of the poorest quintile(s) in national income, multiplied by average per capita income, and divided by 0.2.

[28] During the data extraction stage we assess what proportion of the countries included in a cross-country study are low or middle income, and include this as a variable in the meta-regression analysis.

where *y* is a measure of individual or household income, *I* is some summary measure of inequality (e.g. the Gini coefficient, Theil index), and subscripts 0 and 1 indicate the time period before and after the policy change. A positive value of *D* implies that a policy change increases inequality, while a negative value implies that it reduces inequality.

We refer to a change in a variable that is directly controlled by the government as a 'policy simulation'. We group these simulations into three main headings: fiscal policy, trade policy, and other:

- **fiscal policy simulations** include changes in domestic tax and subsidy rates (e.g. income tax, VAT, fuel subsidies), transfer payments (e.g. social pensions, cash transfers), the supply of publicly provided goods and services (e.g. roads, education, health), and the prices or user fees charged for public goods and services (e.g. school tuition fees);
- **trade policy simulations** include changes in import tariffs, import quotas, export taxes, export subsidies, and non-tariff-barriers;
- **other policy simulations**, which include changes in official price floors or ceilings (e.g. minimum wages, interest rate controls), and in government restrictions or prohibitions (e.g. anti-discrimination legislation, or a ban on child labour).

Each study must include at least one policy simulation; we exclude studies in which the simulations refer only to the effects of external or internal shocks; examples include a change in a country's terms of trade, or an increase in productivity.

The general term given to research that tries to understand or assess how government fiscal policies affect the distribution of income is **fiscal incidence analysis** (see Martinez-Vazquez 2004). This includes tax incidence analysis – analysis of who ultimately bears the burden of government taxes, and by how much – and expenditure (or benefit) incidence analysis – analysis of who benefits from government spending, and by how much. The burden (also called economic incidence) of a tax refers to who finally experiences a decrease in real income as a result of the tax, not necessarily who is required by law to pay the tax (referred to as statutory incidence).

The ex-ante simulation studies included in this review encompass a variety of methods, from the simple to the more complex, depending mainly on how the likely behavioural responses of economic agents are dealt with. The simplest approach, which includes so-called standard fiscal incidence analysis, assumes there are no behavioural responses to government policy changes. Households and individuals are assumed to have perfectly inelastic supplies of the factors of production that they own (e.g. labour, human capital), and consumers are assumed to have perfectly inelastic demand for commodities (Martinez-Vazquez 2004, van de Walle 1998). This approach is clearly not particularly realistic, but is often justified as being relatively straightforward to implement, and providing a reasonable 'first approximation' to the real results that would be obtained if behavioural responses were included.

The more complex approach is applied general equilibrium analysis, which studies the incidence of taxes or spending in the context of a computable general equilibrium (CGE) model of the whole economy, allowing for some (not necessarily all) behavioural responses. In theory, this type of

analysis should provide more accurate estimates of incidence. However, CGE models rely on the accuracy of the equations and parameters which are used to construct them; they are also computationally complex and the results are often sensitive to modelling choices. More importantly, estimates from CGE models do not always differ that much from estimates from standard fiscal incidence analysis – as is the case where elasticities are low, even if not zero.

QUANTITATIVE CASE STUDIES USING DECOMPOSITION ANALYSIS (STUDY DESIGN C)

These study designs analyse the contribution of government taxes or transfers to income inequality using decomposition analysis. Decomposition analysis encompasses a wide variety of methods (Cowell 2000). The two main types are:

- **decomposition by subgroup**: this involves decomposing a measure of inequality in a population of households or individuals to a part representing inequality between subgroups of the population, and another part representing inequality within each subgroup;
- **decomposition by income source**: this involves decomposing a measure of inequality in total income to components relating to inequality of wages and salaries (income from work), rents (income from property), government transfers, and so on.

Decomposition analysis is typically used when seeking to explain or 'account for' observed levels of inequality at a point in time or trends in income inequality over time. For example, Lustig et al. (2013b) show that one of the major sources of the decline in income inequality in many Latin American countries during the 2000s has been a fall in the inequality of labour incomes. This is helpful in that it allows further research to focus on the factors that may have contributed to the decline in labour inequality. In the words of Azevedo et al. (2013 p. 3): '[D]ecompositions … are a useful tool to identify empirical regularities and, as an accounting tool, can be useful to focus attention on the elements which are quantitatively more important in describing distributional changes.'

However, decomposition analysis differs from econometric analysis, in that it is only able to identify the proximate sources of inequality, not the factors that determine inequality – such studies 'do not allow for the identification of causal effects' (ibid.). Thus, the fact that much of the fall in inequality in Latin America can be explained by a fall in inequality in labour earnings does not tell us whether the latter was driven by government policy interventions or not. This limits the relevance of decomposition analysis to this review.

Some forms of decomposition analysis are relevant to this review, however. In particular, decomposition analysis by income source, in which income from government transfers as a separate source of income, is relevant. This type of analysis can provide an estimate of the effect of a government transfer programme on income inequality, which is comparable with estimates derived from ex-ante simulation studies. It can also provide an estimate of the effect of taxes on inequality in disposable income – since taxes can be treated as negative income. Therefore, studies which use decomposition analysis by income source and which report government transfers or taxes as separate categories of income are included in this review. However, studies

using decomposition analysis by income source but which do not treat transfers or taxes as a separate income source are excluded.

To see how decomposition by income source works from a technical point of view, define G_y as the Gini index of inequality calculated on the overall income distribution *y*. Following the seminal contribution of Lerman and Yitzhaki (1985) and the useful review by Wodon and Yitzhaki (2002), G_y can be seen as the sum of different components which relate to the different income sources. Letting the subscript *i*=1, 2, …, *k* denote *k* different income sources, the decomposition formula for the Gini coefficient reads as follows:

$$G_y = \sum_{i=1}^{k} S_i R_i G_i \ ,$$

where G_i is the Gini index calculated with respect to income source *i*, S_i is the share of total income obtained from source *i*, and R_i is the Gini correlation between income from source *i* and total income. The latter is a ratio of covariances Cov[y_i ,$F(y)$] and Cov[(y_i, $F(y_i)$], where $F(y)$ and $F(y_i)$ are the cumulative distribution functions of per capita overall income and income from source *i*, respectively. Note that tax payments are treated in decomposition analyses as negative income. Transfers may take the form of cash or in-kind payments, with an imputed value used for the latter.

This approach represents an advancement from the earlier work of Shorrocks (1982), which proposed a two-way decomposition of the Gini index based of the product of R_i and G_i – the so-called concentration index or 'pseudo-Gini'.

The absolute magnitude of the contribution of source of income *i* to overall income inequality is given by $S_iR_iG_i$, while the relative magnitude (or proportional contribution) is obtained by dividing by G_y, namely $S_iR_iG_i/G_y$. The marginal impact of a percentage change in income from source *i* on the overall income inequality coefficient is $Z_i = \left(S_i R_i G_i / G_y\right) - S_i = S_i\left(\eta_i - 1\right)$, where $\eta_i = R_i G_i / G_y$ is the Gini income elasticity (GIE) for income source *i*.

In general, a positive value of η_i means that changes in income source *i* are correlated with existing income inequality and therefore these changes benefit the upper part of the income distribution; the opposite holds for negative values of η_i . For example, consider a 10 percent increase in incomes from a particular source (e.g. government cash transfers). If the marginal impact of this income source is -0.2, then the Gini coefficient would change in proportionate terms by 0.10*-0.2 = -0.02, in other words a reduction in the Gini by 2 percent. Furthermore, if this source accounts for 5 percent of total income, and the GIE is 0, then the Gini would fall by 0.5 percent.

It can be seen that for $\eta_i = 1$, changes in income source *i* are distributed in a way which mirrors the existing income distribution, so that overall inequality is not affected; for example, transfers are distributed in a way which reproduces existing inequality and in this way it leaves total inequality unchanged. Values of η_i between zero and one can be considered 'mildly' redistributive

in the sense that although they favour richer individuals, they do not do so as much as the original distribution. The lower the value of η_i is, the stronger the redistributive character of the transfer.

A3 DETAILS OF ELECTRONIC SEARCHES

Table A3.1: Electronic databases

EXPORT DATE	# HITS	DATABASES	SEARCH STRING
28/04/2014	1941	EBSCO EJS	AB ((polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*) AND ((income* OR expenditure*) W1 (*equal* OR *distribut* OR disparit* OR differen* OR gap* OR *equit* OR share* OR ratio* OR gini)))
28/04/2014	638	Science Direct	title-abstr-key((polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*) AND ((income* OR expenditure*) W/1 (*equal* OR *distribut* OR disparit* OR differen* OR gap* OR *equit* OR share* OR ratio* OR gini)))
28/04/2014	6560	Scopus	TITLE-ABS-KEY ((polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*) AND ((income* OR expenditure*) W/1 (*equal* OR *distribut* OR disparit* OR differen* OR gap* OR *equit* OR share* OR ratio* OR gini))) AND SUBJAREA(mult OR arts OR busi OR deci OR econ OR
28/04/2014	271	JSTOR	(ab:("income inequality" OR "income *distribut*") AND ab:(polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*))
28/04/2014	1201	Web of Knowledge (Social Science Citation Index)	TOPIC: ("income inequality" and polic*)
28/04/2013	2032	IBSS (International Bibliography of the Social Sciences)	AB((polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*) AND ((income* OR expenditure*) NEAR/1 (*equal* OR *distribut* OR disparit* OR differen* OR gap* OR *equit* OR share* OR ratio* OR gini)))
28/04/2013	434	ASSIA (Applied Social Sciences Index and Abstract)	AB((polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*) AND ((income* OR expenditure*) NEAR/1 (*equal* OR *distribut* OR disparit* OR differen* OR gap* OR *equit* OR share* OR ratio* OR gini)))

EXPORT DATE	# HITS	DATABASES	SEARCH STRING
28/04/2014	2992	Econlit (Ebsco)	AB ((polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*) AND ((income* OR expenditure*) W1 (*equal* OR *distribut* OR disparit* OR differen* OR gap* OR *equit* OR share* OR ratio* OR gini)))
28/04/2014	29	Research for Development (R4R)-DFID	"income inequality"
28/04/2014	45	Scielo	("income inequality" OR "income *distribut*") AND (polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*)
28/04/2014	2558	PROQUEST Dissertations and thesis	AB((polic* OR intervention* OR program* OR instrument* OR tool* OR reform* OR legislation* OR govern*) AND ((income* OR expenditure*) NEAR/1 (*equal* OR *distribut* OR disparit* OR differen* OR gap* OR *equit* OR share* OR ratio* OR gini)))
01/05/201	983	Google Scholar	allintitle: ("INCOME INEQUALITY" OR "INCOME DISTRIBUTION" OR "INCOME REDISTRIBUTION") AND (policy OR intervention OR program OR instrument OR tool OR reform OR legislation OR government)

Note: Due to the fact that some search engines only allow a limited number of operators, a long and a short version were used as previously described in the initial draft of the protocol.

Table A3.2: Other online databases

SCREENING BY	# HITS	DATABASES	SEARCH STRING
23/10/2014	1288	IDEAS	"income inequality" policy
22/10/2014	56	NBER	income inequality
13/09/2014	1966	SSRN	"income inequality"
07/10/2014	15	3IE Impact Evaluation Database	income inequality
15/10/2014	255	Eldis	"income inequality"
26/09/2014	892	World Bank Open Knowledge repository	"income inequality"
09/10/2014	144	OECD iLibrary	income inequality and policy
09/10/2014	90	International Labour Organization	income inequality and policy
08/10/2014	69	Chronic Poverty Research Center	*(assets and inequality area)*
17/10/2014	201	Overseas Development Institute	"income inequality"
07/10/2014	7	Center for Global Development	inequality
08/10/2014	31	International Policy Center for Inclusive	inequality
18/10/2014	493	JOLIS (IMF and World Bank databases)	Keywords anywhere "income inequality" AND keywords anywhere "policy"

SCREENING BY	# HITS	DATABASES	SEARCH STRING
08/10/2014	61	Millennium Challenge Corporation	income inequality
07/10/2014	11	USAID Development Experience	"income inequality"
29/10/2014	565	African Development Bank Evaluation	*does not allow to write a search string. All results were screened*
07/10/2014	15	Asian Development Bank Evaluation	inequality
08/10/2014	9	Inter-American Development Bank	inequality
30/10/2014	453	CLASE (Citas Latioamericana en Ciencias	desigualdad*
31/10/2014	202	e-revistas-Plataforma Open Access de	desigualdad *
31/10/2014	122	REDALyC	"income inequality"

Notes: *'desigualdad' is the Spanish word for inequality.

A4 LIST OF STUDIES INCLUDED IN THE REVIEW

EX-POST OBSERVATIONAL STUDIES

Synthesis, government spending (102)

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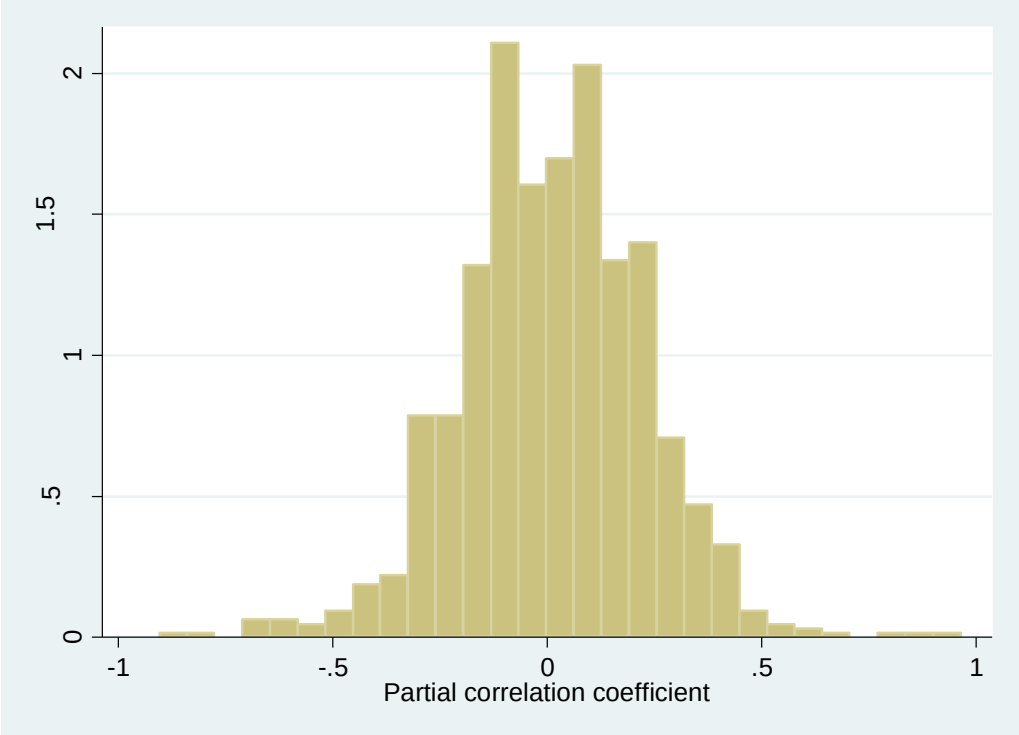
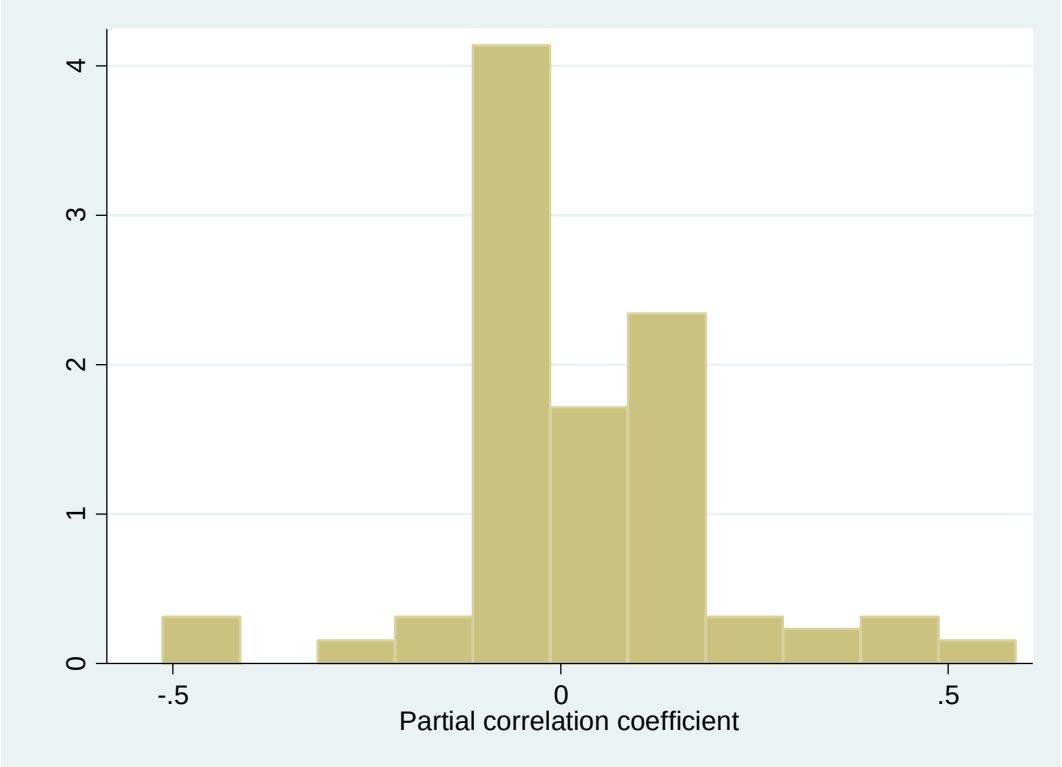
A5 ADDITIONAL RESULTS, EX-POST OBSERVATIONAL STUDIES

FREQUENCY DISTRIBUTIONS

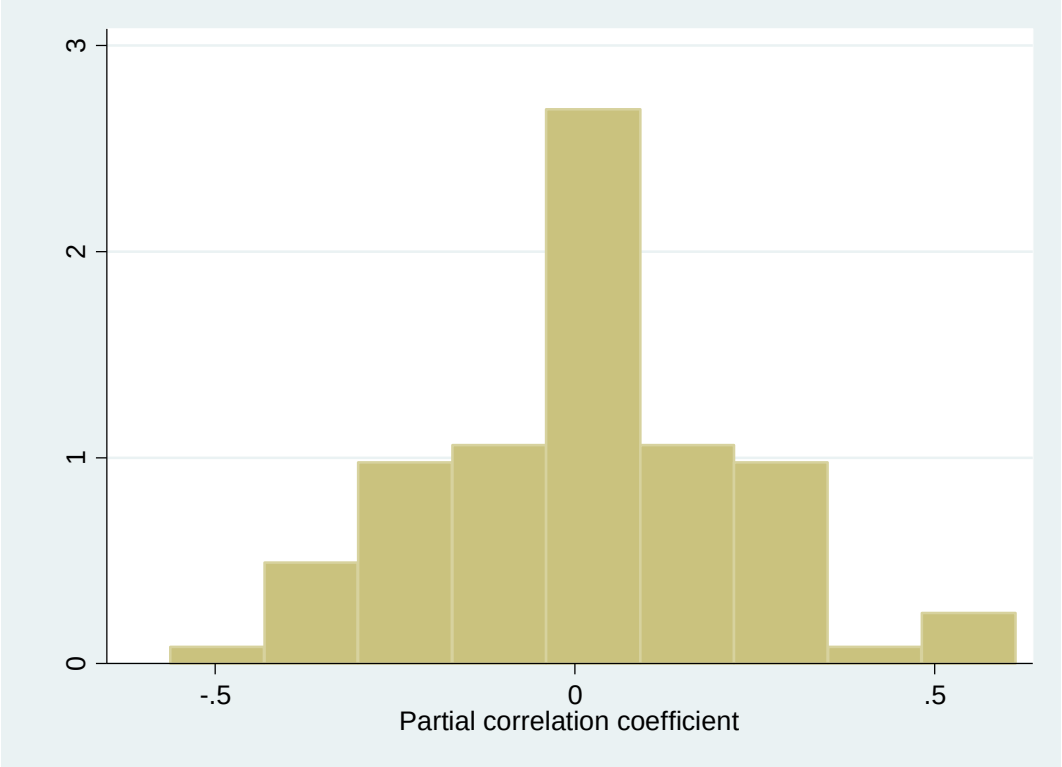
Partial correlations of government spending and income inequality (n=987)



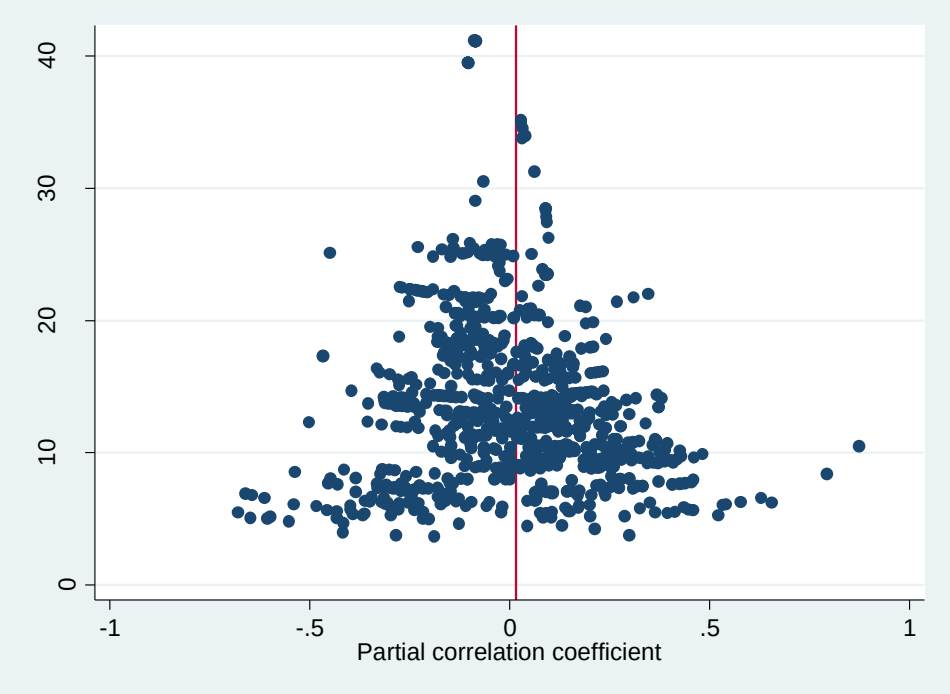
Partial correlations of taxation and income inequality (n=128)



Partial correlations of trade policy and income inequality (n=94)



Partial correlations of government spending and income inequality (n=987) after removing the main outliers



Note: Precision is calculated as 1/standard error of the partial correlation coefficient.

The weighted mean of the partial correlation coefficient is marked with a red line with the value of 0.016

MRA, government spending

Variable name	Variable description	Mean (n=987)	Standard deviation (n=987)
Partial correlation	Partial correlation of the effect of government spending variables on income inequality. This is the dependent variable in the meta-regression.	0.011[29]	0.215
Inequality measure			
Gini	BD=1: Gini coefficient (used as the base)	0.741	0.439
Income share bottom	BD=1: Income share of the bottom quintile	0.137	0.344
Income share top	BD=1: Income share of the top quintile	0.045	0.206
Income share other	BD=1: Income share other (ratio, growth of the poor, etc.)	0.068	0.252
Income inequality other	BD=1: Other income inequality measure (Theil, Atkinson, EHHI)	0.010	0.100
Government spending measure			
Total government spending	BD=1: Total government spending included as explanatory variables (used as the base)	0.281	0.450
Health government spending	BD=1: Health government spending included as explanatory variables	0.025	0.157

[29] The weighted mean of the partial correlation coefficient by study is 0.016.

Education government spending	BD=1: Education government spending included as explanatory variables	0.109	0.312
Health and education government spending	BD=1: Health and education government spending included as explanatory variables	0.006	0.078
Social net government spending	BD=1: Social net government spending included as explanatory variables	0.112	0.316
Military government spending	BD=1: Military government spending included as explanatory variables	0.019	0.137
Housing government spending	BD=1: Housing government spending included as explanatory variables	0.006	0.078
Social general government spending	BD=1: Social government spending included as explanatory variables	0.015	0.122
Government spending consumption	BD=1: Government spending (consumption) included as explanatory variables	0.397	0.490
Government spending others	BD=1: Government spending (any/not specified/other) included as explanatory variables	0.028	0.166
Country composition			
Sub-Saharan Africa (SSA)	BD=1: Countries in sub-Saharan Africa region included in samples	0.746	0.436
Latin America (LAC)	BD=1: Countries in Latin America region included in samples	0.821	0.384
South Asia (SA)	BD=1: Countries in South Asia region included in samples	0.799	0.401

Developed*	BD=1: Developed countries included in samples	0.584	0.493
Data			
OLS	BD=1: OLS estimator used	0.196	0.397
Year data	Average year of data used in each study minus the average year of data across all studies (Yr=Avg-1987)	-0.283	7.610
Other specification variables			
Tax	BD=1: Tax included as explanatory variable	0.102	0.303
Trade	BD=1: Trade included as explanatory variable	0.124	0.329
Education	BD=1: Education variables included as explanatory variable	0.475	0.500
Inflation	BD=1: Inflation included as explanatory variable	0.503	0.500
Population	BD=1: Population included as explanatory variable	0.188	0.391
Governance	BD=1: Governance variables included as explanatory variable	0.435	0.496
Publication			
Standard error	Standard error of the partial correlation coefficient. Used to correct for publication bias.	0.088	0.041
Unpublished	BD=1: Study is unpublished	0.479	0.500

Notes: *This variable has n=974. BD means binary dummy with a value of 1 if condition is fulfilled and zero otherwise.

MRA, taxation

Variable name	Variable description	Mean	n=128 Standard deviation
Partial correlation	Partial correlation of the effect of tax variables on income inequality. This is the dependent variable in the meta-regression.	0.025	0.175
Inequality measure			
Gini	BD=1: Gini coefficient	0.953	0.212
Tax variables			
Total tax revenue	BD=1: Total tax revenue	0.070	0.257
Direct taxes	BD=1: Direct taxes (e.g. income taxes, corporate taxes, social security taxes)	0.203	0.404
Indirect taxes	BD=1: Indirect taxes (e.g. VAT, excises, customs)	0.188	0.392
Tax progressivity measures	BD=1: Tax progressivity measures (used as the base)	0.539	0.500
Country composition			
Latin America (LAC)	BD=1: Countries in Latin America region included in samples	0.898	0.303
Sub-Saharan Africa (SSA)	BD=1: Countries in sub-Saharan Africa region included in samples	0.656	0.477
South Asia (SA)	BD=1: Countries in South Asia region included in samples	0.781	0.415
Developed	BD=1: Developed countries included in samples	0.836	0.372

Data			
OLS	BD=1: OLS estimator used	0.094	0.293
Year data	Average year of data used in the study minus the average year of data across all studies (Yr=Avg-1994)	0.313	5.567
Other specification variables			
GOV	BD=1: Government variables included as explanatory variable	0.609	0.490
Publication			
Standard error	Standard error of the partial correlation coefficient. Used to correct or publication bias.	0.053	0.031
Unpublished	BD=1: Study is unpublished	0.797	0.404

Notes: All studies use Gini as the inequality outcome. BD means binary dummy with a value of 1 if condition is fulfilled and zero otherwise.

MRA, trade policy

Variable name	Variable description	n=94# Mean	Standard deviation
Partial correlation	Partial correlation of the effect of trade variables on income inequality. This is the dependent variable in the meta-regression.	0.014	0.213
Inequality measure			
Gini	BD=1: Gini coefficient	0.734	0.444
Trade measure			
Import tariffs	BD=1: Import tariffs included as explanatory variables (used as the base)	0.457	0.501
Trade policy indices	BD=1: Trade policy indices (e.g. EFI4, Sachs-Warner index) included as explanatory variables	0.436	0.499
Other trade measures	BD=1: Other trade measures (e.g. non-tariff barriers, export duties)	0.106	0.310
Country composition			
Sub-Saharan Africa (SSA) and South Asia (SA)	BD=1: Countries in sub-Saharan Africa or South Asia region included in samples[ᵗ]	0.723	0.450
Latin America (LAC)	BD=1: Countries in Latin America region included in samples	0.926	0.264
Developed*	BD=1: Developed countries included in samples	0.628	0.486

Data			
OLS	BD=1: OLS estimator used	0.234	0.426
Year data#	Average year of data used in each study minus the average year of data across all studies (Yr=Avg-1987)	0.391	5.937
Other specification variables			
GOV	BD=1: Government variables included as explanatory variable	0.340	0.476
Publication			
Standard error	Standard error of the partial correlation coefficient. Used to correct or publication bias.	0.500	0.503
Unpublished	BD=1: Study is unpublished	0.269	0.935

Notes: BD means binary dummy with a value of 1 if condition is fulfilled and zero otherwise. ᵗ Initially two different dummies were created for SSA and SA, however there was a multicollinearity issue since the studies that include SSA also include SA, and therefore it was decided to create a unique variable that merges those two regions. # All variables have 94 observations except for year data, which has 87, since there was a study that did not report the year of intervention.

RESULTS EXCLUDING OUTLIERS

MRA of the effects of government spending on income inequality after removing the outliers (dependent variable=partial correlation)

	(1) FAT-PET WLS	(2) WLS	(3) WLS General	(4) WLS specific	(5) Robust
Standard error	1.395***	1.109**	0.690	0.877**	0.412**
	(0.509)	(0.457)	(0.416)	(0.424)	(0.169)
Income share bottom		0.074	0.030		-0.015
		(0.061)	(0.052)		(0.018)
Income share top		-0.070	-0.056		-0.126***
		(0.061)	(0.053)		(0.024)
Income share other		0.152**	0.138	0.139**	0.161***
		(0.059)	(0.086)	(0.068)	(0.022)

Income inequality other		0.186***	0.123**	0.159**	0.087
		(0.035)	(0.049)	(0.071)	(0.061)
Developed			-0.011		-0.000
			(0.031)		(0.014)
Unpublished			-0.024		-0.062***
			(0.026)		(0.013)
Year data			-0.001		-0.002**
			(0.003)		(0.001)
OLS			-0.081**	-0.068**	-0.100***
			(0.037)	(0.034)	(0.016)
LAC			-0.100**	-0.066**	-0.110***
			(0.042)	(0.032)	(0.021)
SSA			-0.001		0.031
			(0.067)		(0.034)
SA			0.067	0.059**	0.021
			(0.068)	(0.027)	(0.031)
TAX			-0.053	-0.071**	-0.030
			(0.038)	(0.028)	(0.027)
TRADE			0.133***	0.107**	0.196***
			(0.047)	(0.043)	(0.020)
Governance			0.055*	0.075**	0.084***
			(0.028)	(0.029)	(0.012)
Inflation			-0.074***		-0.055***
			(0.028)		(0.012)
Population			-0.006		-0.036**
			(0.034)		(0.015)
Education			-0.028		0.007
			(0.033)		(0.013)
Health government spending			0.069		0.070**
			(0.047)		(0.035)
Education government spending			0.100**		0.075***
			(0.050)		(0.020)
Health and education			0.282***	0.147***	0.262***
government spending			(0.054)	(0.035)	(0.067)
Social net government spending			0.131***	0.075**	0.101***
			(0.042)	(0.029)	(0.021)
Military government spending			0.078		0.099**
			(0.050)		(0.046)
Housing government spending			-0.005		0.093
			(0.076)		(0.068)
Social general government			0.041		-0.045
spending			(0.069)		(0.046)
Government spending			0.074*		0.063***
(consumption)			(0.041)		(0.016)
Government spending others			0.211***	0.123***	0.170***
			(0.046)	(0.029)	(0.040)
Constant	-0.102***	-0.098***	-0.042	-0.104***	-0.010
	(0.031)	(0.030)	(0.060)	(0.033)	(0.026)
N	981	981	968	981	968
R^2	0.063	0.136	0.329	0.268	0.403

Notes: This table reports the same regressions as Table 7 after removing 6 outliers that were identified through visual inspection of the funnel plot. Columns report estimates variants of regression 2. Regressions 1, 2, and 3 use 981 estimates from 87 studies, while regressions 3 and 5 use 968 estimates from 85 studies. Standard errors are reported in parentheses. All regressions use

cluster standard errors to adjust for data dependence, i.e. multiple estimates per study. All columns use weighted least squares, except for regression 5, which uses robust regression. For definitions of variables see Table 5. Total government spending is used as the base category for the government spending variable. Gini is used as a base in the inequality variable.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

ROBUSTNESS CHECKS

Robustness checks using different weights (dependent variable=partial correlation) for government spending variables

	(1) FAT-PET WLS	(2) WLS general	(3) WLS weights1	(4) WLS weights2	(5) WLS weights3	(6) WLS weights4
Standard error	1.778***	1.496**	0.659*	-2.220*#	0.401	-1.033#
	(0.530)	(0.724)	(0.383)	(1.208)	(0.323)	(0.788)
Income share bottom		0.057	0.056	0.061	0.001	0.013
		(0.081)	(0.065)	(0.064)	(0.062)	(0.024)
Income share top		-0.055	-0.033	-0.021	0.016	0.001
		(0.055)	(0.067)	(0.068)	(0.101)	(0.019)
Income share other		0.153*	0.212**	0.222**	0.175***	0.006
		(0.089)	(0.104)	(0.101)	(0.066)	(0.023)
Income inequality other		0.208**	0.139**	0.141*	0.130*	0.130***
		(0.087)	(0.068)	(0.082)	(0.067)	(0.038)
Developed		0.004	-0.010	-0.025	0.014	-0.009
		(0.038)	(0.024)	(0.021)	(0.031)	(0.012)
Unpublished		-0.029	-0.025	-0.011	-0.003	0.030
		(0.033)	(0.031)	(0.031)	(0.036)	(0.021)
Year data		0.001	-0.003	-0.005	-0.002	-0.004***
		(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
OLS		-0.097**	-0.071*	-0.029	-0.117**	-0.006
		(0.041)	(0.041)	(0.035)	(0.046)	(0.016)
LAC		-0.071	-0.151**	-0.201***	-0.074	-0.108***
		(0.048)	(0.058)	(0.056)	(0.053)	(0.037)
SSA		-0.084	-0.060	-0.046	-0.148	-0.049
		(0.080)	(0.100)	(0.102)	(0.101)	(0.060)
SA		0.139*	0.146*	0.146	0.198**	0.088
		(0.077)	(0.088)	(0.093)	(0.088)	(0.056)
TAX		-0.059	-0.062	-0.073**	-0.080	-0.061**
		(0.053)	(0.038)	(0.034)	(0.064)	(0.029)
TRADE		0.049	0.140**	0.141**	0.142**	0.053
		(0.105)	(0.057)	(0.057)	(0.059)	(0.033)
Governance		0.077**	0.090**	0.097***	0.114***	0.035**
		(0.036)	(0.035)	(0.034)	(0.032)	(0.017)
Inflation		-0.067**	-0.080***	-0.094***	-0.048	-0.045**
		(0.028)	(0.029)	(0.030)	(0.039)	(0.021)
Population		-0.006	-0.029	-0.024	-0.062*	-0.046*
		(0.039)	(0.034)	(0.033)	(0.035)	(0.026)
Education		-0.041	-0.068	-0.087*	-0.046	-0.035*
		(0.035)	(0.043)	(0.044)	(0.045)	(0.018)
Health government spending		-0.052	0.101	0.121*	0.087	0.049

	(1) FAT-PET WLS	(2) WLS general	(3) WLS weights1	(4) WLS weights2	(5) WLS weights3	(6) WLS weights4
		(0.100)	(0.070)	(0.068)	(0.058)	(0.033)
Education government		0.075	0.123**	0.159***	0.074	0.088***
spending		(0.054)	(0.051)	(0.053)	(0.061)	(0.025)
Health and education		0.276***	0.351***	0.385***	0.284***	0.173***
government spending		(0.066)	(0.060)	(0.063)	(0.060)	(0.038)
Social net government		0.145***	0.183***	0.211***	0.141**	0.101***
spending		(0.046)	(0.061)	(0.062)	(0.069)	(0.025)
Military government		0.000	0.063	0.104	0.126*	-0.005
spending		(0.080)	(0.081)	(0.091)	(0.068)	(0.038)
Housing government		-0.012	0.082	0.112*	0.076	0.009
spending		(0.066)	(0.062)	(0.061)	(0.071)	(0.044)
Social general government		0.001	0.082	0.166***	0.006	0.143***
spending		(0.093)	(0.066)	(0.052)	(0.083)	(0.026)
Government spending		0.053	0.104**	0.139***	0.091*	0.062**
(consumption)		(0.048)	(0.045)	(0.046)	(0.049)	(0.025)
Government spending others		0.238***	0.283***	0.309***	0.225***	0.105***
		(0.051)	(0.064)	(0.064)	(0.070)	(0.026)
Constant	-0.134***	-0.119	-0.025	0.071	-0.077	0.053
	(0.034)	(0.080)	(0.069)	(0.064)	(0.065)	(0.039)
N	987	974	974	974	974	974
R^2	0.076	0.298	0.498	0.497	0.462	0.506

Notes: Standard errors are reported in parentheses. All regressions use cluster standard errors to adjust for data dependence, i.e. multiple estimates per study. All columns use weighted least squares. # The standard error of the mean of the partial correlation coefficient is reported instead of the standard error of the partial correlation coefficient. Regression 3 uses the sum of precision squared (or inverse variance) for each study as weights. Regression 4 uses the sum of precision squared (or inverse variance) for each study as weights and the standard error of the mean of the partial correlation coefficient instead of the standard error of the partial correlation coefficient. Regression 5 uses 1/n as weights where n is number of estimates per study. Regression 6 uses the weighted mean of the partial correlation, the sum of precision squared (or inverse variance) for each study as weights and the standard error of the mean of the partial correlation.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A6 ADDITIONAL RESULTS, EX-ANTE SIMULATION STUDIES

Table A6.1: Effects of fiscal policy simulations on income inequality: average incidence analysis – Latin America

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers	-2.6	-1.7	3.0	-21.0	0.8	23	148
Income taxes	-1.5	-1.9	3.1	-5.8	14.0	16	60
Indirect taxes	0.5	0.0	1.4	-1.8	4.1	7	29
In-kind transfers	-6.6	-4.0	5.6	-25.5	-0.01	12	69
Combined fiscal~	-6.3	-5.9	8.1	-40.0	10.2	17	98

B. Frequency distribution							
	Reductions in inequality#				**Increases in inequality#**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Cash transfers	1	20	118	8	1	-	-
Income taxes	-	2	50	6	2	-	-
Indirect taxes	-	-	10	7	12	-	-
In-kind transfers	2	28	39	-	-	-	-
Combined fiscal~	6	47	24	4	16	1	-

Notes: The numbers in this table refer to the percentage changes in a given measure of income inequality from a given base year; #small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent; ~combined fiscal policy simulations involving changes in taxes and spending.

Table A6.2: Effects of fiscal policy simulations on income inequality: average incidence analysis – Eastern Europe and Central Asia

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers	-33.3	-27.3	23.7	-89.4	-0.1	18	150
Income taxes	-6.3	-3.8	7.9	-28.5	4.5	7	36
In-kind transfers	-6.6	-4.0	7.6	-15.1	-0.7	2	3
Combined fiscal~	-24.7	-24.0	14.0	-53.2	20.5	5	28

B. Frequency distribution							
	Reductions in inequality#				**Increases in inequality#**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Cash transfers	101	34	15	-	-	-	-
Income taxes	4	13	12	4	3	-	-
In-kind transfers	-	1	2	-	-	-	-
Combined fiscal~	21	6	-	-	-	-	1

Notes: The numbers in this table refer to the percentage changes in a given measure of income inequality from a given base year; #small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent; ~combined fiscal policy simulations involving changes in taxes and spending.

Table A6.3: Effects of fiscal policy simulations on income inequality: average incidence analysis – East Asia and Pacific

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers	-5.3	-1.2	9.4	-31.1	0.4	4	24
Income taxes	-2.8	-1.4	4.6	-17.4	0.0	4	14
Indirect taxes	10.2	8.9	10.5	-0.2	26.7	1	7
Combined fiscal~	-2.6	-1.6	10.3	-26.9	12.3	5	17

B. Frequency distribution							
	Reductions in inequality#				**Increases in inequality#**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Cash transfers	3	3	10	1	7	-	-
Income taxes	-	3	7	4	-	-	-
Indirect taxes	-	-	1	-	2	2	2
Combined fiscal~	2	2	7	-	3	3	-

Notes: The numbers in this table refer to the percentage changes in a given measure of income inequality from a given base year; #small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent; ~combined fiscal policy simulations involving changes in taxes and spending.

Table A6.4: Effects of fiscal policy simulations on income inequality: average incidence analysis – sub-Saharan Africa

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers	-7.8	-3.3	13.0	-71.3	-0.5		35
Income taxes	-5.9	-6.2	3.3	-10.7	-0.7		14
In-kind transfers	-2.3	-3.1	1.4	-3.3	-0.7		3
Combined fiscal~	-16.1	-9.1	13.1	-31.3	-8.0		

B. Frequency distribution							
	Reductions in inequality#				**Increases in inequality#**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Cash transfers	3	11	21	-	-	-	-
Income taxes	-	9	5	-	-	-	-
Indirect taxes	-	-	3	-	-	-	-
Combined fiscal~	1	2	-	-	-	-	-

Notes: The numbers in this table refer to the percentage changes in a given measure of income inequality from a given base year; #small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent; ~combined fiscal policy simulations involving changes in taxes and spending.

Table A6.5: Effects of fiscal policy simulations on income inequality: average incidence analysis – low-income countries

A. Descriptive statistics							
	Mean	**Median**	**Standard deviation**	**Min. value**	**Max. value**	**Studies**	**Observations**
Cash transfers	-2.8	-1.7	3.0	-11.2	0.6	5	31
Income taxes	-3.8	-2.2	4.3	-12.1	0.0	2	8
In-kind transfers	-1.9	-1.9	1.5	-3.3	-0.5	2	4
Combined fiscal~	-2.2	-5.9	11.0	-10.9	10.2	1	3

B. Frequency distribution							
	Reductions in inequality#				**Increases in inequality#**		
	Large	Moderate	Small	No change	Small	Moderate	Large
Cash transfers	-	6	24	-	1	-	-
Income taxes	-	3	4	1	-	-	-
In-kind transfers	-	-	4	-	-	-	-
Combined fiscal~	-	2	-	-	-	1	-

Notes: The numbers in this table refer to the percentage changes in a given measure of income inequality from a given base year; #small: less than 5 percent; moderate: between 5 and 20 percent; large: more than 20 percent; ~combined fiscal policy simulations involving changes in taxes and spending.

Table A6.6: Fiscal policy and income inequality: explaining differences in estimated effects

	Cash transfers	*In-kind transfers*	*Direct taxes*	*Combined fiscal policy*
Year of simulation	0.065	-0.338***	-0.082	0.530***
	0.07	0.10	0.08	0.18
GNI per capita	-3.588***	-3.235**	-0.822	-4.855***
	0.89	1.61	1.36	1.78
Low-income country	2.125	-	2.161	0.171
	2.01		2.45	4.63
East Asia and Pacific	-8.454***	-	-2.354*	-1.841
	1.36		1.21	3.30
Sub-Saharan Africa	-9.731***	-5.030	-5.180***	-7.667*
	1.38	4.90	1.22	4.38
South Asia	-	2.368	-9.010***	-
		5.31	2.68	
Middle East and North Africa	-		-	-3.188
				4.385
Number of obs.	190	72	88	104
R-squared	0.29	0.254	0.27	0.19

Notes: The dependent variable in each column is the percentage change in inequality stemming from each type of fiscal policy simulation; results all derived from average incidence analysis. Results for the Eastern Europe and Central Asia region include a number of outliers and are therefore excluded from the sample.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A7 ADDITIONAL RESULTS, STUDY DESIGN C

In this Appendix we comment on a selection of the studies using decomposition analysis in more detail, focusing on the more technical aspects of results.

Wodon et al. (2003) analyse the impact of a number of transfer programmes in Mexico. They find values of η_i which are consistently lower than one. This confirms the redistributive character, at least to some extent, of these programmes. However, there is significant heterogeneity in the extent to which these programmes foster redistribution. This can be seen looking at two well-known programmes, PROGRESA (Programa de Educación, Salud y Alimentación) and PROCAMPO (Programa de Apoyos Directos al Campo). PROGRESA involves cash transfers conditional on the recipient engaging in a set of behaviours designed to improve health and nutrition. PROCAMPO offers agricultural producers a fixed payment per hectare of land cultivated. While for PROGRESA $\eta_i < 0$, in the case of PROCAMPO η_i is between zero and one. The difference is explained by the fact that while PROGRESA targets specifically poor households, PROCAMPO also addresses non-poor individuals; in particular, thanks to PROCAMPO, well-off producers owning a large amount of land receive a substantial amount of benefits. Other programmes are also found to have a negative η_i – these are Desarrollo Integral de la Familia (both its food support and school breakfasts components) and Nino de Solidaridad. For other programmes (Liconsa and Tortilla), η_i is either negative or slightly positive depending on alternative versions of the Gini index used.

Garner and Terrell (1998) evaluate the distributional changes that occurred during 1989 to 1993 in the Czech and Slovak Republics during the transition to market economies. Their interest is to explore the marginal impact of a percentage change in income from source on the overall income inequality (i.e. coefficient Z_i), and in particular to understand whether this is larger in the case of government transfers or taxes; in other words, they estimate which of the two government interventions has the greatest effect on income inequality. They find that, as expected, in both the cases of government transfers and taxes Z_i is negative, which indicates a negative effect on income inequality. In terms of magnitude, they find a consistent pattern of government transfers having an almost fourfold impact on inequality compared with taxes. This holds for both the first and the last year of transition in their analysis (1989 and 1993) and for both the Czech Republic and the Slovak Republic. Depending on the country and the year, a marginal increase in transfers is found to decrease inequality by 0.065 to 0.096 points, while a marginal increase in taxes is found to increase inequality by 0.013 to 0.026 points (see Table 6). Among the different government transfers, the largest role is played by pensions, followed by child benefits.

The potential for state transfers to bring about a reduction in inequality is also highlighted by Leibbrandt et al. (2001) in an inequality decomposition study on South Africa. They conclude that 'a 1 percent increase in state transfers will reduce the Gini by 0.03, a more significant impact than is achieved by changing any other component' (p. 76). Within the category of state transfers they include social pensions, disability grants, poor relief, unemployment

insurance, and child benefits. The results by Leibbrandt et al. (2001), which refer to data from 1994, are substantially confirmed by Bhorat et al. (2009) for year 1995. However, Bhorat et al. show that in 2005 this equalising effect of government transfers vanished and their effect on the Gini was null; this is attributed to the failure in 2005 of government transfers to reach households at the bottom of the income distribution.

A different picture emerges from the work on income inequality sources in post-reform China by Khan et al. (1993). They analyse the overall income distribution in 1988 as well as the rural and urban subsamples separately. The authors find evidence that the Chinese subsidies system has a strongly disequalising effect on the income distribution. This holds for a range of government measures such as ration coupon subsidies, housing subsidies, and other subsidies. The authors recommend the adoption of a subsidies system which is negatively correlated to the existing income distribution, which, as we saw above, would reach the bottom end of the income distribution.

The paper by Kattuman and Redmond (2001) shows that the inequality trend in post-transition Hungary is at least partially explained by the changes in the redistributive impact of taxes and government transfers. They show that inequality increased only moderately between 1987 and 1991, to then increase sharply until 1993, continuing to grow until 1996, although more moderately. They analyse how the contribution of different income sources to overall inequality varied, taking into account examination years 1987, 1989, 1991, 1993, and 1996. According to their calculations, the negative contribution to inequality given by taxation (i.e. its redistributive effect) decreased by a third in 1993, and in 1996 was still considerably lower than in 1989 and 1991. This coupled with the change in state transfers from progressive to regressive: their contribution to overall inequality was initially negative but turned positive in 1993 and 1996. Looking at state transfers in detail, they are able to show that this pattern can be ascribed to pensions, while transfers other than pensions preserved their inequality-reducing character.

A similar change in the role of government transfers emerges from Kakwani's (1999) study of the determinants of income inequality in transition Ukraine. He focuses his analysis on the concentration index, which is similar to the R_i term seen above and captures the extent to which the distribution of a certain income source follows the pattern of the overall distribution or is in contrast to it. He finds that government transfers have an equalising role in 1989 and 1991, although their role in decreasing inequality is modest (around -0.4 percent). In 1992 the role of government transfers changed and they became regressive, accounting for around 6 percent of total inequality.

Papatheodorou (1998) also looks at the contribution to inequality of different income sources in Greece in 1988. He does not disentangle the different aspects of government intervention on the income distribution and looks jointly at the effect of taxes and social security contributions, including pensions and a number of benefits and allowances. He finds that taxes and social security contributions have an inequality-decreasing effect; however, this effect is rather low due to the problem of tax evasion, which strongly affects incomes from entrepreneurial activities.

Evidence from South America of an inequality-reducing effect of government programmes and transfers is provided by Scorzafave and Carvalho de Lima (2010) and Contreras and Ffrench-Davis (2012). By analysing the contribution of different income components to overall inequality in Brazil in 2004, Scorzafave and Carvalho de Lima show that all of the programmes named Bolsa Família, Benefício de Prestação Continuada, and Erradicação do Trabalho Infantil contribute negatively to it. Similar results are found by Contreras and Ffrench-Davis (2012) for Chile during the years 1990–2009; government transfers are found to have an equalising effect, and this effect has been increasing throughout the years thanks to the introduction of programmes such as Chile Solidario.