Working Paper Series

#2023-046

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Published 20 December 2023

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UNU-MERIT Working Papers
ISSN 1871-9872

Maastricht Economic and social Research Institute on Innovation and Technology
UNU-MERIT | Maastricht University

Structural change and income inequality: a meta-analysis*

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December 2023

Abstract

This paper performs a meta-analysis of the literature on the relation between structural change and within-country income inequality. Structure is understood here as the sectoral composition of an economy. The meta-analysis is performed on 686 individual regressions coming from 44 papers. Results indicate no evidence of publication bias but also no evidence for an overall effect of structural change on inequality. However, results also indicate that significant changes in the effect size and sign come from different decisions taken in the empirical setup. Particularly, the decision of measuring structure as the size of agriculture or as the size of industry drives results to opposite directions in similar magnitudes. It is possible that these cancel each other out in the overall picture, leading to the observation of the lack of an overall effect. Other decisions that cause significant changes in the effect size include the data source for inequality, the functional form, the use of an econometric technique robust to endogeneity, the use of heteroskedasticity-robust standard errors, and the inclusion of covariates related to structure, inequality, demography, development level, and labour markets.

Keywords: structural change, income inequality, meta-analysis

JEL Codes: L16, D31, J21, J31

*This paper profited from significant contributions provided by Tommaso Ciarli and Bart Verspagen, and by participants of the MAER-Net Colloquium 2023, the UNU-MERIT Internal Conference 2023, and the School of Economics Seminar at the University of Cape Town. I most likely have stubbornly kept mistakes throughout the paper despite their best efforts to get rid of them. I also thank Art Alderson, Bruno Martorano, Emanuele Pugliese, Matías Ciaschi, Rosario Crinò, and their co-authors for providing feedback on specific questions regarding data used in their papers.

1 Introduction

The relation between structural change and income inequality has been a topic of discussion for many decades (Lewis, 1954; Kuznets, 1955), with inconclusive results found in the empirical literature (Kanbur, 2000; Förster and Tóth, 2015). Förster & Tóth (2015) suggest that conceptual and methodological heterogeneity might be part of the explanation for this inconclusiveness. Indeed, papers that relate structure to inequality show a high degree of heterogeneity in many aspects, such as in the definitions and measurements of both structure and inequality, in the covariates included in the model specifications, and in the techniques for estimating the model parameters. By defining structure and inequality differently, papers might be exploring slightly different phenomena. By including different covariates, papers might be differently exposed to omitted variable biases, and capturing the effect of different mechanisms in the estimated parameter related to structure. By using different estimation techniques, papers might be differently exposed to biases due to endogeneity – which could be especially important given that the use of techniques that do not account for reverse causality seems to be a prevalent practice.

In this paper, we assess whether there are significant patterns between empirical setups and results in the literature on structural change and income inequality. We do so by performing a meta-analysis of the relevant literature. The meta-analysis is a quantitative systematic literature review that draws general conclusions out of results gathered across different regressions found in the literature. When primary studies use different data, models, and estimation techniques, it provides tools to investigate whether these heterogeneous empirical approaches cause systematic changes in the estimated effects. It additionally provides tools for calculating an overall effect size, when it exists; and to investigate the presence of biases in the selection of papers for publication (Stanley and Doucouliagos, 2012).

It is important to note that structure is a polysemic word in the economic development literature (Silva and Teixeira, 2008). In this review, we focus on structure as meaning the sectoral composition of the economy, i.e. the relative sizes of sectors, mainly defined by their outputs. Inequality can be approached in many different ways as well (McGregor et al., 2019). In this paper, we will focus on within-country income inequality, with no restriction to how it is measured. These choices were made in order to select regressions that are not too dissimilar in nature and do not imply that other processes of structural change (e.g., technological change) and its effects on other expressions of inequality (e.g., between-country inequality) are of any lesser importance.

We do not find evidence of an effect size that is distinguishable from zero in the overall literature. However, we do find that certain decisions of the empirical setup cause significant change in this effect size. Perhaps most importantly, and as expected, we find that the decision of measuring structure as the size of agriculture or as the size of manufacturing drive the effect size in similar magnitudes but opposite directions. Since these cancel each other out in the big picture, they might be part of the explanation for the lack of an effect size in the overall analysis. They might also suggest that different types of structural change (e.g. the Kuznetsian transition vs. the diversification into sectors with higher complexity) have different impacts on inequality.

We also find evidence that the effect size is influenced by adopting a technique that accounts for endogeneity; by the use of heteroskedasticity-robust standard errors; by the choice of data source for inequality; by the functional form of the model; and by the inclusion of covariates related to demography, structure, inequality, development level, and the labour market. Particularly, the results related to covariates and to the econometric technique seem to suggest that researchers on structural change and inequality should be wary of biases due omitted variables and, potentially, reverse causality.

This paper is divided in seven sections. After this introduction, Section 2 covers the theoretical frameworks that relate the sectoral composition of an economy with its level of income inequality in the reviewed literature. Section 3 details the data and method, including the selection of primary studies and the meta-regression models. Sections 4 and 5 present and discuss results related, respectively, to publication bias and to heterogeneity. Section 6 explores robustness checks and Section 7 concludes the paper.

2 Frameworks and mechanisms for the relation between sectoral composition and income inequality

Papers in the reviewed literature frame the causal relation between structural change and income inequality in different ways. In this section, we overview the streams of literature to which the primary studies connect. These include: the Kuznets curve; deindustrialisation; skill- and routine-biased technological change (SBTC, RBTC); financialisation; dual economy models; neoclassical international trade theories; and economic complexity. We pay particular attention to the mechanisms that come into play in these research streams, noting that they show significant overlap. We finish with a note about reverse causality.

Perhaps the most well-known model for this relation is the one proposed by Kuznets (1955), which focuses on what happens with income inequality during the coupled processes of urbanisation and industrialisation. Since incomes in the industrial sector are both higher and more dispersed, inequality rises in the early stages of this sectoral transformation. In later stages, Kuznets (1955, p. 18) argues, inequality should fall, given that urban workers have more opportunities to organise themselves, and because urban native population cohorts will have better knowledge about how to profit from the economic opportunities of cities. This means that inequality should draw an inverted-U shape if plotted against structural change or economic growth – indeed, it was the observation of such a shape in empirical data that prompted the formulation of the model. Despite the large popularisation of the Kuznets curve, it is important to mention that Kuznets (1955, p. 26) himself makes a very clear caveat about the model being based on very little data and being extremely speculative. Indeed, the literature that attempted to verify empirically the phenomenon has reached inconclusive results (Kanbur, 2000).

Two mechanisms can be identified in the process behind the Kuznets curve. First, there is the role of *sectoral dissimilarity*. Incomes received by individuals which are economically active in a given sector have a distribution with a certain mean and a certain variance. If different sectors have different means and variances for these distributions, changes in the relative sizes of sectors should lead, without mediation, to changes in the overall profile of inequality, at least when measured by gross income. This is the mechanism for the upwards part of the inverted-U curve. Second, there is the role of *social norms and institutions*. Different economic structures might be more or less conducive to the development of institutions that redistribute market income. Institutions may act, then, as a mediator between structure and inequality. This is the mechanism for the downwards part of the inverted-U curve. As we will see, most research streams explain the connection between sectoral transformations and income inequality with reference to these two mechanisms.

Kuznets (1955) is a central reference in the reviewed literature, with many papers framing the discussion along its lines (Rossi, 1981; Nielsen, 1994; Nielsen and Alderson, 1995; Nielsen and Alderson, 1997; Lee, 2008; Adams and Klobodu, 2019; Baymul and Sen, 2019; Sulemana et al., 2019; Le et al., 2020; Behera and Pozhamkandath Karthiayani, 2022; Ali, 2023). Some variations of the mechanisms are also explored. The dissimilarity of sectors might be framed in relation to the factors they employ (e.g., relying more or less heavily on skilled labour). Here, changes in the sectoral composition imply changes in the relative demand of these factors, with consequences for inequality between owners of these different factors (Ciaschi et al., 2021). Other papers discuss the Kuznets process but applied to shifts to and from other sectors. For instance, instead of the classical transition from agriculture to industry, papers might consider shifts from low-paying services towards knowledge-based sectors (Rohrbach, 2009; Kwon, 2014, 2016), or from industry towards services (Moller et al., 2009; Kollmeyer, 2018).

The latter connects to another research stream, the one on deindustrialisation (Silver and Bures, 1997; Mehic, 2018; Pariboni and Tridico, 2019; Liu et al., 2022). Despite being a different research stream than the one on the Kuznets curve, the mechanisms are very much the same. The transition to services has consequences related to sectoral dissimilarity because the latter generates incomes with a lot more variance than industry. Deindustrialisation also affects social norms and institutions, e.g., by shifting labour opportunities from well-paid unionised industrial jobs to low-paid non-unionised service jobs or by lowering the bargaining power of the remaining unions overall.

One research stream tangentially related to deindustrialisation and covered by reviewed papers is the one on SBTC/RBTC and job polarisation (Mollick, 2012; Martorano and Sanfilippo, 2015; Martorano et al., 2016; Mehic, 2018; Chongvilaivan and Hur, 2019; Ghosh et al., 2023). In short, this literature suggests that recent technological change has substituted jobs of non-skilled workers and complemented jobs of skilled workers, thus raising the productivity and eventually the wages of the latter. In newer versions, it is rather routine tasks that are substituted and cognitive tasks that are complemented, with the corresponding consequences. This stream is not extensively covered in this meta-analysis because it looks at structure mainly as technology rather than as sectoral composition. However, there are sectoral mechanisms embedded in these processes. Sectoral dissimilarity comes to play in the extent that, for instance, substituted industrial workers are absorbed by other sectors, such as services, similarly to the discussion on deindustrialisation. This literature however, opens the space for a new mechanism, one of *changes in the within-sector distribution*, as it is possible that different jobs within the same sector are substituted or complemented. It is important to note, though, that these changes in the distribution would be a consequence of technological change, and not of changes in the sectoral composition.

Papers might also focus on particular sectors rather than on the Kuznetsian shifts. Examples include knowledge-based business sectors (Antonelli and Tubiana, 2020), tourism (Nguyen et al., 2021), and sectors with dutch-disease dynamics (Richards, 1994). This way of framing the discussion also connects to an additional research stream, that on financialisation. Again, the same mechanisms apply again in this case. Particularly, the growth of the financial sector might affect inequality through its effect on institutions. A more financialised economy may be associated with stronger political support for labour market reforms and more focus on delivering shareholder value, leading to higher shares of profit and other capital incomes (Pariboni and Tridico, 2019).

It is important to note that although the Kuznets curve is widely considered a seminal paper in this discussion, it is preceded by the Lewis (1954) model of a dual economy, with which it has certain similarities. In the Lewis model, an economy is starting its transition from a technologically-stagnant rural/agricultural sector, to a dynamic urban/industrial one. Its agricultural sector is characterised by having an "unlimited" supply of labour: since these productive units are typically of subsistence agriculture, there are more workers than the strictly necessary and the marginal productivity of labour is zero. Higher industrial wages attract labour to cities, but since this implies no loss of product in agriculture, wages are kept stagnant in both agriculture and industry. This goes on until a tipping point is reached in which the marginal productivity in agriculture is no longer zero, and wages start growing in both sectors. Although the Lewis model is arguably less focused on inequality then the one behind the Kuznets curve, the consequences of the Lewis process for inequality are evident. Indeed, some reviewed papers frame the discussion in terms of dual economy models (Cook and Uchida, 2008; Mollick, 2012; Sulemana et al., 2019). Once more, however, the main mechanism at work here is one of sectoral dissimilarity, although the process is also related to changes in within-sector distributions. Particularly, and differently from the SBTC/RBTC literature, these changes are endogenous to the sectoral shift, and not triggered by changes in technology.

We have mentioned how sectoral dissimilarity may be framed in reference to changes in the relative demand for particular factors. These changes might be triggered by international trade (Le et al., 2020; Lee et al., 2022), an idea which is in line with neoclassical models. In the Hecksher-Ohlin framework, countries export goods intensive on their abundant factor, this factor gaining from trade, while the scarce factor loses. In the Stolper-Samuelson theorem, if skilled labour is abundant, trade widens the skill premium for a given increase in the relative price of skilled-intensive goods (and vice-versa), bringing an additional framing for the discussion on job polarisation. This tradition, with some variations, is adopted by part of the reviewed papers (Crinò and Epifani, 2014; Martorano et al., 2016; Mallick et al., 2020; Topuz and Dağdemir, 2020; Hinojosa, 2021). In terms of the mechanisms at play here, changes in inequality might occur by the dissimilarity of sectors that gain or lose in the process but also by changes in the within-sector distributions.

A final research stream connected to some of the primary studies is the one on economic complexity (Hausmann et al., 2013), which argues that several benefits in terms of growth and development are associated with having a more complex knowledge base in the economy. In this literature, this definition of complexity is typically proxied in reference to export baskets, which represent the specialisation patterns of countries. More complexity would be associated with baskets that are more diversified and more composed of products rarely found in the export baskets of other countries. Lower complexity in this sense tends to be associated with a structure that has a small competitive high-income sector that is however not capable of employing many workers; while with higher complexity there is a higher diversity of skills and knowledge, and a flatter labour occupational structure (Sbardella et al., 2017; Chu and Hoang, 2020; Lee and Vu, 2020; Zhu et al., 2020; Bandeira Morais et al., 2021; Lee and Wang, 2021).

Complexity would lead to lower inequality not only through this mechanism of sectoral dissimilarity but also through institutions. Higher complexity economies tend to be associated with more inclusive institutions and are more likely to have sectors that are more conducive to unionisation (Hartmann et al., 2017; Chu and Hoang, 2020; Lee and Vu, 2020; Zhu et al., 2020; Bandeira Morais et al., 2021; Ghosh et al., 2023). Additionally, higher complexity might also promote processes of SBTC/RBTC (Chu and Hoang, 2020) or of more accelerated creative destruction, rendering older worker cohorts obsolete (Lee and Vu, 2020). Trade flows might also be behind the propagation of inequalities through economies (Fawaz and Rahnama-Moghadamm, 2019).

It is worth noting that in all these frameworks the causality is assumed or analysed as going from structure to inequality. The regressions we will cover in this meta-analysis indeed have inequality as the dependent variable, and structure as one of the dependent variables, because this is how the literature largely addresses the issue. There is however reason to believe that changes in income inequality might also induce changes in the economic structure. People in different income brackets have different sectoral patterns of consumption. This means that changes in inequality might induce differential growth across sectors through changes in the sectoral composition of demand (Patriarca and Vona, 2013; Liu, 2017; Desdoigts and Jaramillo, 2020). The details of this mechanism might be related to reaching the saturation point of Engel curves — that is, with the idea that the consumption of products rises with income, but reaches a maximum point where it stagnates (Moneta and Chai, 2014; Saviotti and Pyka, 2017). Also, the behaviour of high-income consumers might stimulate the growth of sectors that exhibit winner-take-all characteristics, reinforcing inequality (Wilmers, 2017). Finally, demand might also shape the evolution of technology: if people in different income brackets prioritise different product characteristics, such as price vs. quality, changing inequality might induce firms to focus on different types of innovation, influencing technological trajectories (Ciarli and Valente, 2015). Although papers that discuss this direction of the causality are not covered in this meta-analysis, they bring an important contribution. If causality goes both ways, regressions of inequality on structure should adopt econometric techniques that account for the subsequent endogeneity.

Summing up, there are two main mechanisms that connect the sectoral composition to income inequality in the reviewed papers. The first is an immediate one, related to sectoral dissimilarity in the level and variation of incomes. The second is a mediated one, in which different structures are more or less conducive to the development of distributive institutions. Most research streams related to the primary studies of this meta-analysis cover both mechanisms (Kuznets curve, deindustrialisation, SBTC/RBTC, financialisation, economic complexity), while some are more focused on the former (dual economy models, neoclassical international trade theories). A third mechanisms is noticeable, that of changes in the within-sector distribution of income, be it triggered by extenal factors, such as technological change or international trade (as in the SBTC/RBTC or neoclassical trade literature) or endogenously during process of the sectoral shift (as in the dual economy literature). These mechanisms will help to inform which elements of empirical approaches might affect the estimated effect of structure on inequality in the reviewed literature.

3 Data and method

This section details the data and method used in the meta-analysis. It covers the mapping and screening of the literature; the choice of measure for the effect size; a description of the moderator variables used to model heterogeneity; and the specification of the metaregression models. We follow closely the guidelines proposed by the Meta-Analysis in Economics Research Network (MAER-Net) (Stanley et al., 2013; Havránek et al., 2020; Irsova et al., 2023).

3.1 Mapping and screening the literature

To map the literature, we combined a search for keywords with a citation analysis or snowballing process. The query used in the search was refined in a series of steps represented in Figure 1 and detailed below.

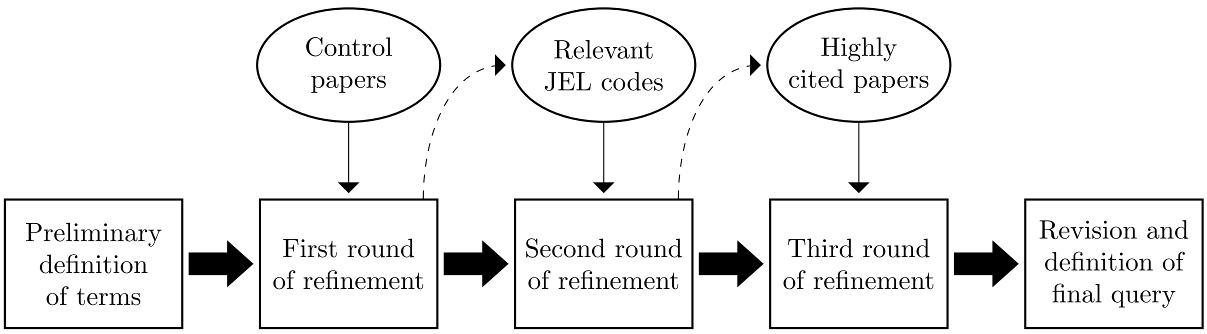

Figure 1: Steps for refining keywords in search query

We begin defining the query with a preliminary set of terms. To define these, we framed the research question in the PICOS elements[1]. Two sets of keywords were chosen for this preliminary query: one

[1]PICO stands for Population, Intervention, Comparison, and Outcome. It is a tool originally used in systematic reviews in the life sciences as a way to detail the scope of papers covered in the review (Thomas et al., 2023). In the original tool,

related to structural change (Intervention) and another to income inequality (Outcome). No other restrictions were imposed at this stage[2].

For a first round of refinement, the query was tested for accuracy by checking whether it could find, on Scopus, six control papers (Gustafsson and Johansson, 1999; Alderson and Nielsen, 2002; Rohrbach, 2009; Kwon, 2016; Kollmeyer, 2018; Hope and Martelli, 2019). These were defined in reference to the qualitative literature review of Förster & Tóth (2015). Only one such paper was retrieved by the preliminary query, and it was augmented with relevant terms found on the title and abstract of the remaining five papers.

For a second round of refinement, the updated query was used on EconLit. The most frequent JEL codes among the results were filtered for those related either to sectoral transformation or to income inequality. Then, EconLit was searched again, for papers that combined any of the structural JEL codes and any of the inequality ones, finding 5691 results. A frequency analysis of terms in the titles and abstracts of these results excluding stopwords was performed with the help of Voyant Tools. This frequency analysis identified new terms related to inequality and to sectoral composition to be added to the query.

For an third round of refinement, we analysed the full text of highly cited papers, as follows. The resulting query after the second round of refinement was used on Scopus, finding 857 results. The 36 most cited papers among these were selected and filtered for topic, narrowing them down to six papers. The full texts of these six papers were screened for the terms already present in the query, to analyse whether they appeared in different new phrasings which could be useful. Finally, for a last round, the terms of the query were revised and slightly adjusted[3].

The final query was then used on Scopus in November 2022 to retrieve 1487 papers. The search query looked for any of the structure-related expressions together with any of the inequality-related expressions on title, abstract, or keywords: *(TITLE-ABS-KEY ("structural change" OR "sectoral transformation" OR "sectoral diversification" OR (("shift*") W/1 ("employment" OR "sector*" OR "lab* force")) OR "sectoral composition" OR "economic complexity" OR (("structure*") W/1 ("econom*" OR "product*" OR "occupation*" OR "industr*" OR "export*")) OR "dual econom*" OR "dual structure*" OR (("duali*") W/1 ("sector*" OR employment)) OR (("employment" OR "lab* force") W/1 ("share" OR "percent*" OR "proportion" OR "fraction")))) AND (TITLE-ABS-KEY ((("income*" OR "wage*" OR "earnings") W/1 ("inequalit*" OR "distribution" OR "gap" OR "disparity" OR "differen*" OR "premium" OR "heterogeneity" OR "dispersion" OR "share")) OR "lab* share")).*

These 1487 papers were screened according to the inclusion criteria detailed immediately below[4], narrowing the sample down to 156. These 156 papers were then used for a citation analysis. The 3184 documents that cited them and 5603 documents that they referenced were retrieved through Scopus. The 10% most cited among each of these groups were screened (first on title and abstract, then on full text), filtering them down to 15 additional papers.

The screened papers, then, finally amounted to 171: 6 literature reviews, 29 purely theoretical papers, and 136 empirical papers. Among the latter, 51 papers ran regressions of inequality on structure and other covariates and were the initial sample for this meta-analysis. During the process of coding and

each element refers, respectively: to the population groups covered in the primary studies; to the (medical) intervention performed therein; to control groups used for comparison, when available; and to the measured outcome. PICO is later expanded to PICOS to also cover Study Type, as a way to narrow the scope down to particular methods (Methley et al., 2014).

[2]Since our scope is not focused on particular country types and we do not necessarily look at comparisons between different groups, we do not have keywords related to Population or Comparison. Our scope does however narrow papers down to those that run regressions. Despite this, we did not include keywords related to the type of study during mapping, rather deciding to account for that during the screening process. This way, we were able to also build a larger parallel database of papers that investigate the topic using different methods, which may be useful in the future.

[3]The queries for each intermediate step can be found in the Appendix.

[4]As explained above, at this moment, we had not yet adopted the criterion that papers should run a regression.

preliminary data analysis, seven of these 51 papers were dropped, along with their 32 regressions. Five of these dropped papers reported only the significance levels of estimated parameters, and not the standard error nor the t-statistic[5]. One paper was dropped for poor reporting of the empirical setup, and another was dropped both for issues in reporting the empirical setup and for having outlying values[6]. Among the remaining papers, 13 regressions could not be used because they reported a standard error of 0.0 for the structure-related estimated parameter.

The inclusion criteria adopted during the screening phases were:

1. *Definition of structure*: We included papers that defined structure at least partially as the sectoral composition of the economy and excluded otherwise (e.g., papers that only discussed structure as technology or as institutions).

2. *Definition of inequality*: We excluded papers that covered exclusively horizontal income inequality or exclusively inequalities other than income, such horizontal income inequality (e.g., gender or racial gaps) or exclusively inequalities other than income (e.g., health inequality or education inequality).

3. *Journals*: We included only papers published in journals, excluding books and papers presented in conferences.

4. *Language*: Papers not in English, Portuguese, Spanish, or French were excluded.

5. *Method*: We included papers that ran regressions of inequality on structure.

No restrictions were adopted for measurements of income inequality (e.g. Gini, income ratios, labour share etc.) nor for measurements of structure (e.g. share of GDP, share of employment, share of exports etc.). The regressions found in the literature were then generically of the type $INEQ_{it} = \alpha_0 + \alpha_1 * STR_{it} + \alpha * X_{it} + \epsilon_{it}$, where $INEQ_{it}$ is some measure of inequality in country i in time t; STR_{it} is some measure of structure; X_{it} is a vector of other covariates that might also affect inequality, such as those related to macroeconomics, institutions, education, demography, among others; ϵ_{it} is the error term, and country-specific and time-specific dummies might be added; and the α are the estimated parameters. It is also possible that studies focus on a time series of a single country, or perform cross-sectional analyses. It is worth mentioning that these equations are in the same spirit of the specification that Förster & Tóth (2015) call the "grand inequality regression equation" (GIRE), although with the particular focus on independent variables related to structure.

The meta-analysis was then performed on 686 individual regressions coming from 44 papers. The screening process is synthesised in the PRISMA flowchart (Page et al., 2021) of Figure 2. In Figure 2, exclusions related to the first four criteria are presented first (narrowing papers down from "articles assessed for eligibility" to "articles on topic"), before moving to exclusions related to not running regressions, or having issues related to reporting or outlying values (narrowing down to "included articles").

3.2 Effect size

We are interested in the size of the effect of structural change on income inequality. The analysis focuses, then, on the estimated parameter for structure in the regressions – the α in the generic regression presented in the previous section. Since papers vary significantly on how they measure both inequality and structure,

[5]Significance levels could be used to reverse engineer upper and lower bounds for the t-statistics. But since this would add uncertainty and these regressions were not numerous, they were dropped.

[6]See the Appendix for a short discussion on outliers.

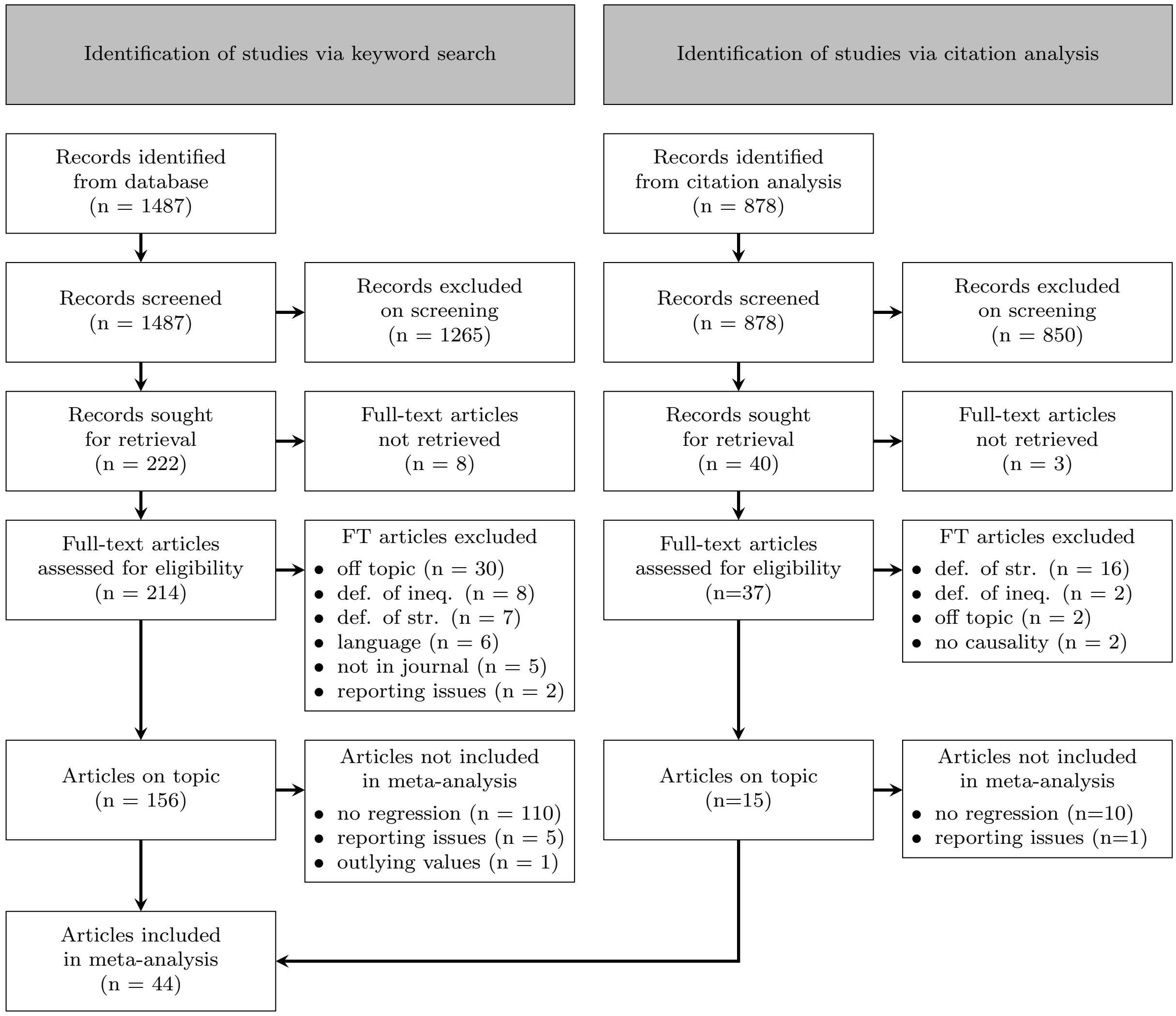

Figure 2: PRISMA flowchart

it is hard to compare the values of the coefficients themselves. In these cases, the meta-analysis literature (e.g., Stanley and Doucouliagos, 2012) suggests to use instead the partial correlation coefficient (PCC)[7].

The equations for the PCC and for its standard error are, respectively:

$$r_i = \frac{t_i}{\sqrt{t_i^2 + df_i}} \quad \text{and} \quad se_i = \frac{r_i}{t_i} = \sqrt{\frac{(1 - r_i^2)}{df_i}},$$

where r_i is the PCC related to structure in regression i; se_i is the standard error of the PCC; t_i is the t-statistic of the estimated parameter related to structure in the regression; and df_i are the degrees of freedom of that regression.

[7]The partial correlation coefficient differs from the simple (Pearson) correlation coefficient in that it represents the correlation between two variables while holding other variables constant. In that sense, it has a *ceteris paribus* property similar to coefficients in a multiple regression (Stanley and Doucouliagos, 2012). We might be interested, for instance, in understanding whether a correlation between income and education is due only to the fact that both tend to rise as individuals become older. The PCC would then allow to "purge" the effect of age in the correlation (Greene, 2018). In this example, the PCC would be calculated as the correlation between residuals of two regressions: that of income on a constant and age; and that of education on a constant and age (Greene, 2018). The calculation is made easier after running a multiple regression of income on both education and age: as in the equation in this section, we can use the t-statistic for testing the significance of the coefficient related to education, and the degrees of freedom of the regression (Greene, 2018).

3.3 Moderator variables: coding and descriptive statistics

As discussed in the introduction, the main motivation for performing this meta-analysis is to investigate to which extent variation in the effect size is driven by the heterogeneity of empirical approaches. For that purpose, alongside the PCC and its standard error, a series of variables were coded for each regression. These variables, referred to as metavariables or moderator variables, can be fitted in two groups: those related to data and measurement; and those related to the model or econometric technique.

The first variable related to data and measurement is (i) the *measure of inequality*, a binary dummy for whether the paper uses the Gini or Theil index or coefficient to measure inequality. Papers that don't use them use top income shares, bottom income shares, income ratios, labour/wage share, and geographical or skill income gaps. We expect that structural change has different implications for people in different income brackets, meaning that we could be looking at different phenomena by measuring inequality differently. Second, we have (ii) the *data source for inequality*, a binary dummy equal to 1 when the paper uses any version of the World Income Inequality Databases (WIID) (UNU-WIDER, 2022) or of the Standardised WIID (SWIID) (Solt, 2020), and 0 otherwise. The third variable is (iii) the *type of income*, a categorical variable for whether inequality is measured for gross income, income net of taxes or net of transfers, income net of both taxes and transfers, or whether the original data has a mix of gross and net incomes (transformed into dummy variables with gross income as the reference). The economic structure should affect directly, even mechanistically, gross/market income inequality. Net income, however, would be decoupled from structure by redistribution. If distribution is independent from the structure, we would expect there to be a difference in the effect size depending on which type of income is being measured. The fourth variable in this group is (iv) the *focal sector for measuring structure*, a categorical variable that can be equal to agriculture; industry; manufacturing; other; or none, due to the use of an aggregate measure or of sectoral data (transformed into dummy variables with the latter as the reference). This is relevant because, for instance, the impact on inequality due to a movement of agriculture towards manufacturing will be opposite whether structure is being measured as the share of agriculture or the share of manufacturing. The fifth variable is (v) the *regional scope*: a dummy variable for whether the analysis is done at country level rather than for sub-national regions. And the final variable of this group is (vi) the *time span*, the number of years covered by the data set in the regression.

The second group of metavariables are related to the model or econometric technique. We have coded (vii) the *functional form*, a categorical variable for whether the econometric model, in relation to inequality and structure, is log-log, log-linear, or linear-linear (transformed into dummy variables with the latter as the reference). Another variable in this group is (viii) *endogeneity*: a dummy variable for whether the econometric technique accounts for endogeneity (IV, 2SLS, GMM etc.). We have reason to believe that the relation between economic structure and income inequality has a two-way causality, leading to endogeneity issues when running regressions of the latter on the former. Techniques that do not recognise this might generate biased estimators. We also code the use of (ix) *robust standard errors*, a dummy variable for whether the estimation is done using some sort of heteroskedasticity-robust standard errors. A different standard error for the same point estimate will affect the values of the PCC and of its standard error. Finally, we have (x) the *categories of covariates included*, a list of nine dummy variables for whether there are covariates in structure; inequality; development level; macroeconomics; demography; education; institutions; labour market; or taxes and transfers. There is no reference value because the dummies are not mutually exclusive. These loosely follow the categories of drivers of inequality as discussed in Förster & Tóth (2015). Table 1 shows, for each of the coded variables, their mean, standard deviation, and count of observations equal to one (or the total number of observations for continuous variables).

Table 1: Descriptive statistics

Categorical variable	Variable description	Mean	SD	N
Effect	Partial correlation coefficient (PCC)	-0.049	0.316	686
Publication bias	Standard error of the PCC	0.070	0.045	686
Measure of inequality	DV = 1: Uses Gini or Theil (= 0 otherwise)	0.784	0.412	538
Data source for inequality	DV = 1: Uses WIID or SWIID (= 0 otherwise)	0.517	0.500	355
Type of income	*(Reference: uses gross income)*			
	DV = 1: Uses income after taxes and transfers	0.289	0.453	198
	DV = 1: Uses income after taxes or transfers	0.092	0.289	63
	DV = 1: Uses a mix of gross and net incomes	0.102	0.303	70
Focal sector	*(Reference: no focal sector)*			
	DV = 1: Focal sector is agriculture	0.160	0.367	110
	DV = 1: Focal sector is industry	0.347	0.476	238
	DV = 1: Focal sector is manufacturing	0.076	0.265	52
	DV = 1: Focal sector is other	0.073	0.260	50
Time span	DV = 1: Number of years covered in data	26.242	20.257	686
Regional scope	DV = 1: Analysis is done at country level	0.888	0.316	609
Functional form	*(Reference: functional form is linlin)*			
	DV = 1: Functional form is loglin	0.236	0.425	162
	DV = 1: Functional form is loglog	0.382	0.486	262
Endogeneity	DV = 1: Technique accounts for endogeneity	0.206	0.404	141
Robust standard errors	DV = 1: Uses robust standard errors	0.609	0.488	418
Categories of covariates				
	DV = 1: Has cov. for structure	0.579	0.494	397
	DV = 1: Has cov. for inequality	0.117	0.321	80
	DV = 1: Has cov. for development level	0.736	0.441	505
	DV = 1: Has cov. for macroeconomics	0.538	0.499	369
	DV = 1: Has cov. for demography	0.452	0.498	310
	DV = 1: Has cov. for education	0.306	0.461	210
	DV = 1: Has cov. for institutions	0.276	0.447	189
	DV = 1: Has cov. for labour market	0.179	0.384	123
	DV = 1: Has cov. for taxes and transfers	0.044	0.205	30

It is worth mentioning that many of the categorical variables above could assume a longer list of values. In general, we would like to have coded each categorical variable in as much detail as possible, but some values occurred too rarely, prompting us to do some recoding. This occurred most notably for the focal sector used to measure structure, in which we felt the need to group together rather dissimilar sectors, such as services and the knowledge sector, under "other". This is an important limitation, because it groups together rather dissimilar sectors, whose growth should have different consequences for inequality. Recoding was also done for the measure of inequality, which ended up becoming a binary variable; and the type of income, which ended up grouping together income net of only taxes and income net of only transfers. One other variable (whether macro- or micro-level data was used) that showed too little variation was dropped. This recoding process is detailed in the Appendix.

Additionally, the list of metavariables described above and on Table 1 was also narrowed down from an originally longer list of 40 metavariables. This was done because there was significant correlation between the original coded metavariables, which could lead to multicollinearity issues on our regressions. For this process, we took the initial list of categorical variables, turned them into binary dummies and, before eliminating baseline reference values, analysed the pairwise Pearson correlations for all pairs of binary dummy variables. The original categorical metavariables dropped in this process were: (i) the measure of structure (whether it was the GDP share, the employment share, the difference between these shares, the Economic Complexity Index (ECI) and similar measures, or the share of exports); (ii) the type of data used (whether time series, cross-sectional, or panel); and (iii) whether the regression adopted a covariate related to trade and globalisation. Out of these, the measure of structure might seem like a particularly critical metavariable, and too critical to be dropped. It is worth mentioning though that it overlaps significantly with the metavariable of the focal sector used to measure structure, particularly because regressions that measure structure with the ECI necessarily do not have a focal sector.

This was just another intermediate step in preparing the data and would only be briefly mentioned. However, it indirectly highlights some patterns and helps to describe the literature, so we will present it with some level of detail. The thirteen pairs with a correlation above 0.60 in absolute value are shown in Table 2, with the abandoned variables marked in italic. Pairs marked in 2, 12, and 13 suggest there some tendency of approaching the problem at hand by measuring the share of industry in GDP, using data on inequality from WIID/SWIID, and including a covariate related to trade and globalisation. Also interestingly, pairs 3 and 5 suggest that parameters estimated through panel data tend to be very precise, while those estimated through time series tend to be very imprecise. This could be explained by the number of observations typically seen when these types of data are used[8].

Table 2: Highly correlated pairs of (original) moderator variables

	Variable 1	Variable 2	Corr.
1	*Str. measure: ECI*	Sector: Aggregate measure	0.82
2	*Str. measure: GDP share*	Sector: Industry	0.74
3	PCC standard error	*Data type: Panel*	-0.72
4	*Data type: Panel*	*Data type: Time series*	-0.71
5	PCC standard error	*Data type: Time series*	0.70
6	*Cov.: Trade and glob.*	Func. form: LinLin	-0.67
7	Income: Gross	Income: Net (tax and tr.)	-0.66
8	*Str. measure: Sector dualism*	Sector: Agriculture	0.66
9	Time span	*Data type: Time series*	0.64
10	Ineq. measure: Gini/Theil	*Str. measure: Emp. share*	-0.63
11	Func. form: LinLin	Func. form: LogLog	-0.62
12	Ineq. source: (S)WIID	*Str. measure: GDP share*	0.62
13	*Cov.: Trade and glob.*	Ineq. source: (S)WIID	0.61

The remaining metavariables, those described on Table 1, have a variance inflation factor (VIF) ranging from 1.73 to 4.44 and there are only three pairs with a pairwise correlation above 0.50, suggesting that this process has adequately addressed worries related to multicollinearity.

Two final tasks performed to organise the coding of metadata are worth mentioning. The first, a minor one, was the inversion of the signal of parameters when inequality is being measured as the labour share, the wage share, or the share of the bottom 70%. This is because, naturally, a higher measure of these variables means *lower* inequality. The second task, a more extensive one, was related to the type of income metavariable, and motivated by the fact that 23 out of the 44 primary studies omit whether they calculate inequality based on gross or net incomes. In these cases, we verified the documentation of the sources used by these papers. When this was ambiguous, we contacted authors. After these efforts, the uncertainty was narrowed down to four papers, covering 29 regressions. For these, we adopted our best guess given

[8]Pairs 6 (covariate on trade and globalisation and linear-linear functional form) and 10 (inequality measured by Gini/Theil and structure measured by employment share) seem harder to interpret and could be due to sheer coincidence, or driven by a large number of observations coming from the same primary study. Other pairs are more obvious and give less interesting information, such as pairs 1 (by definition, the ECI has no focal sector) and 9 (time series tend to have longer time spans). Pair 8 is also expected, as it refers to a specific literature that measures structure as the difference between the GDP share and the employment share of agriculture, referring to this variable as "sector dualism" (Nielsen, 1994; Nielsen and Alderson, 1995; Nielsen and Alderson, 1997; Kwon, 2016; Topuz and Dağdemir, 2020). Finally, for pairs 4, 7 and 11, both dummies come from the same original categorical variable and would be dropped anyway when removing reference values.

the information in the documentation of the original data sources. It is additionally important to mention that this metavariable has an inherent level of uncertainty: for instance, there might be differences across countries and time in the inclusion or exclusion of certain income components, such as taxable transfers, from the calculation of pre-tax incomes (Bartels and Waldenström, 2021). We thus understand that we narrowed down this uncertainty to levels adequate enough to proceed with our analysis.

3.4 Model specifications for publication bias and heterogeneity

We start by estimating a simple regression of the PCC of the structure-related parameter in regression i (r_i) on its standard error (se_i):

$$r_i = \beta_0 + \beta_1 se_i + \epsilon_i, \tag{1}$$

where ϵ_i is the error term and the β are the estimated parameters. This specification is useful for testing publication bias in the so-called Funnel Asymmetry Test (FAT): $H_0 : \beta_1 = 0$, in which rejecting H_0 is evidence for publication bias (Egger et al., 1997; Stanley and Doucouliagos, 2012). The test is named in reference to the funnel plot, shown in the next section. This specification also allows for the Presence of Effect Test (PET): $H_0 : \beta_0 = 0$, in which rejecting H_0 is evidence for presence of effect.

To model heterogeneity, we add the moderator variables of the previous section as covariates, allowing us to investigate their influence on the effect size. The model becomes:

$$r_i = \beta_0 + \beta_1 se_i + \sum_{k=1}^{p} (\beta_k Z_{ki}) + \epsilon_i, \tag{2}$$

where r_i is the PCC related to structure in regression i; se_i is the standard error of the PCC; Z_k are the p moderator variables; ϵ_i is the error term; and the β are the estimated parameters.

We run four different specifications of the model with heterogeneity, varying the set of moderator variables included. First, in the full model, all variables listed in Table 1 are used. Second, profiting from having removed highly correlated variables (Irsova et al., 2023), we adopt a general-to-specific procedure that simplifies the model by iteratively removing moderator variables whose estimated parameters are not statistically significant and re-estimating the model until only significant metavariables are left (Stanley and Doucouliagos, 2012). We then consider two reduced versions of the model, including the two groupings of metavariables one at a time: first, only metavariables related to data and measurement; and then only metavariables related to model and econometric technique.

Estimating such models by ordinary least squares is inadequate because there is heteroskedasticity by construction: higher values of the PCC standard error will be associated with values of the PCC that are more spread out around the true effect size, meaning a higher variance of the error term. Because of that, we follow the meta-analysis literature (Stanley, 2017; Stanley and Doucouliagos, 2017) in using weighted least squares (WLS) with the inverse of the variance of the PCC as the weights. Regressions coming from the same paper are also subject to having some level of dependence. For that matter, we cluster standard errors at the level of the papers. In the robustness tests, we will also account for dependence by adopting an alternative set of weights. The next two sections present our results and discussion. We first cover publication bias and presence of effect, and then move to our main analysis, of heterogeneity.

4 Results: publication bias and presence of effect

It is possible that papers or specific regressions are selected out of publication by authors, reviewers, and publishers, because they lack statistical significance or because their results are too dissimilar from theoretically expected values (Stanley and Doucouliagos, 2012). A tool used in the meta-analysis literature to visually investigate this possibility is the funnel plot: a scatter plot of the estimated effect sizes on the x-axis and their precisions (i.e., typically the inverse of their standard errors) on the y-axis. Considering that one "true" effect size exists, points at the bottom of the plot are less precise and are expected to be more scattered around the two sides of the true effect size than the points at the top, which are expected to lie closer to the true effect. In the absence of publication selection bias, the plot is then expected to have the shape of an inverted funnel which is symmetrical and centred in the true effect size. A lack of symmetry suggests that the literature is biased and omits the reporting of certain regression estimates.[9]

Figure 3 presents the funnel plot for the regressions in the sample. The red dotted line is drawn at the centre of the funnel (-0.087), calculated as the weighted average of the PCCs, with the inverse of their variances as the weights (Stanley and Doucouliagos, 2012). A visual inspection suggests the absence of publication bias, given that the funnel looks significantly symmetrical. It also suggests that the true effect, at the centre of the funnel is slightly negative, though very close to zero.

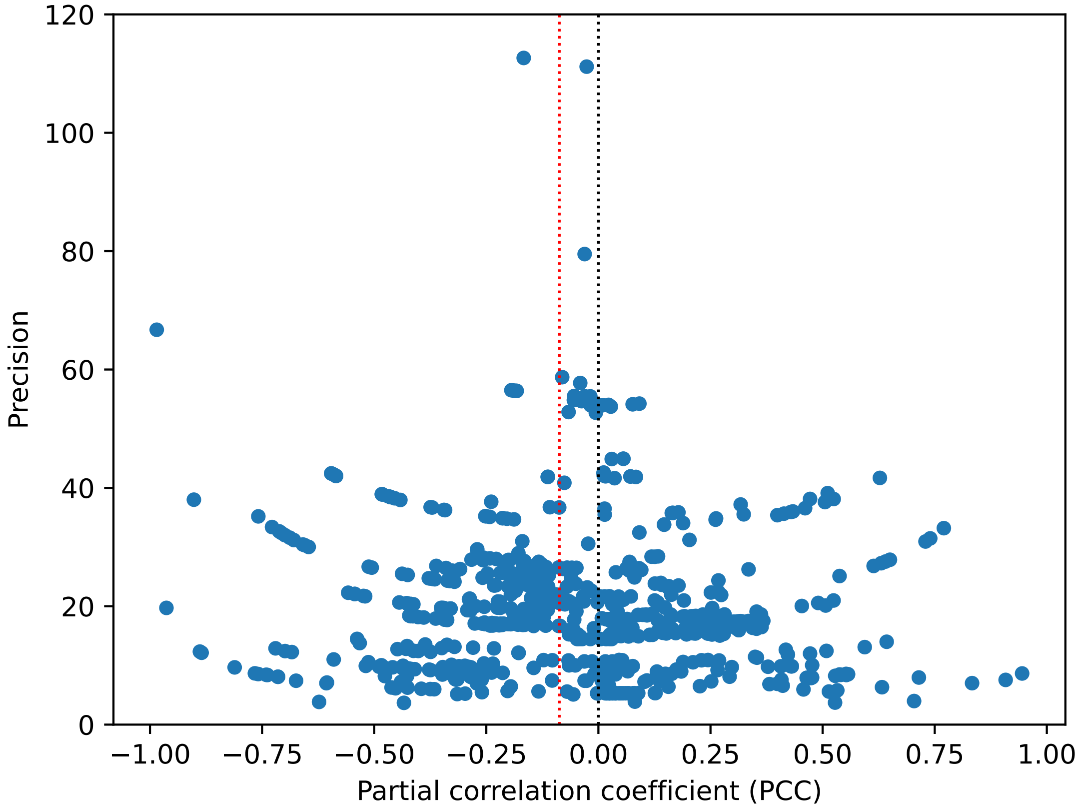

Figure 3: Funnel plot

Although visual inspections may be misleading, the FAT-PET tests confirm the conclusions. In the results of the simple model (Equation 1), the p-value for β_1 is 0.151, meaning that we do not reject $H_0 : \beta_1 = 0$ and have evidence for the lack of publication bias. This result is also robust across the specifications that investigate heterogeneity[10]. As for the PET, we have a a p-value 0.190 for β_0, meaning that we also

[9] The funnel plot also helps understanding the motivations for the FAT and for the use of WLS. The scatterplot for the models specified in the previous section are similar to what the funnel plot would look like when turned 90 degrees to the left. In the absence of publication bias, there should be an even distribution of points above and below the centre of the turned funnel, and a fitted line should have no slope, motivating the FAT. Data is also clearly heteroskedastic, with the variation of the error term depending on the value of the PCC, motivating the adoption of WLS.

[10] Table 1 shows the estimated parameters for the simple model in the first column and for the other models in the subsequent columns.

reject $H_0 : \beta_1 = 0$. This is evidence for the lack of an overall effect, confirming the visual observation that the funnel is centred too close to zero. As we will see in the next section, this result is slightly less robust, as it changes in the some of the other specifications.

It is worth recalling the mechanisms discussed in Section 2. Incomes generated in different sectors have different means and variances, and changes in the relative sizes of sectors should change the overall profile of inequality immediately. The evidence for lack of an overall effect can be then quite surprising, given this rather mechanical relation. On the other hand, it is true that this lack of effect would be in line, for instance, with Piketty's (2014) argument against the Kuznets curve: that inequality is more closely related to social norms and/or major socioeconomic shocks than to the economic structure.

However, it is also possible that the lack of an overall effect is being driven here by empirical setups rather than by economic mechanisms. Different characteristics of the empirical approaches found in the literature might be driving results systematically in opposite directions, cancelling each other out when looking at the overall picture. The next section shows some indication that this is indeed at least part of the explanation.

5 Results: heterogeneity

This section presents the results for all model specifications, estimated via WLS with the inverse of the variance of the PCC as the weights, and with standard errors clustered at the level of primary studies. It is important to keep in mind what the estimated parameters mean. The parameter of a given moderator variable represents the change in the effect size, measured by the PCC, when that particular variable is changed from its reference value, while all other variables remains equal, and at their own reference values. A positive (negative) value in an estimated parameter means, then, that switching the empirical approach to what is captured by that metavariable makes the relation between structure and inequality stronger (weaker) if initially positive, or weaker (stronger) if initially negative.

It is worth then to recall that the baseline reference values correspond to a setup with the following characteristics: inequality is *not* measured by Gini nor Theil; gross income is used to measure inequality; structure is measured without a focal sector (via an aggregate measure or with sectoral data); the time span is (impossibly) of zero years; the analysis is *not* done at country level; the model has a linear-linear functional form; the technique does not account for endogeneity; robust standard errors are not used; and no covariates are added to the model.

One drawback of the PCC, perhaps the main one, is that it is not readily interpretable. Particularly, it is not intuitive to understand whether an effect size (or change thereof) is small or large. In reference to values found in the literature of meta-analysis in economics, Doucouliagos (2011) proposes the following thresholds: an absolute value of the PCC under 0.07 would be a small effect; between 0.07 and 0.33, a medium effect; and above 0.33, a large effect. The author makes several caveats related to these thresholds but they will be adopted here as rough guides for understanding the estimated parameters.

Table 3 reports the results of the five models described in the previous section: the simple model according to Equation 1, and the models according to Equation 2 with different metavariables as covariates: the full model with all metavariables listed in Table 1; the specific model, derived from the general-to-specific approach; the reduced model with only metavariables related to data and measurement; and the reduced model with only metavariables related to model and econometric technique.

Table 3 shows wide evidence for heterogeneity driving changes in the estimated effect size, with several medium- or high-sized, and statistically significant parameters. A general trend, however, is that the

Table 3: Main results

Variable	Simple	Full	Spec.	Data	Techn.
Intercept	-0.136	-0.595***	-0.144**	-0.020	0.022
	(0.104)	(0.220)	(0.059)	(0.128)	(0.225)
PCC standard error	1.385	-0.483	0.760	1.497	-0.572
	(0.964)	(1.302)	(1.235)	(1.246)	(1.337)
Ineq. measure: Gini/Theil		0.104		-0.125	
		(0.099)		(0.143)	
Ineq. source: (S)WIID		-0.233**	-0.226***	0.019	
		(0.106)	(0.080)	(0.181)	
Income: Net (tax and tr.)		0.065		0.143	
		(0.065)		(0.174)	
Income: Net (tax or tr.)		0.064		0.104	
		(0.120)		(0.067)	
Income: Mixed		0.220*		0.123	
		(0.133)		(0.106)	
Sector: Agriculture		0.238*		0.202**	
		(0.125)		(0.091)	
Sector: Industry		-0.101		-0.077	
		(0.113)		(0.153)	
Sector: Manufacturing		-0.231**		-0.168*	
		(0.098)		(0.090)	
Sector: Other		-0.043		0.056	
		(0.248)		(0.197)	
Time span		0.006		0.002	
		(0.004)		(0.003)	
Scope: Country level		-0.089		-0.152	
		(0.236)		(0.213)	
Func. form: LogLin		0.128			-0.098
		(0.176)			(0.146)
Func. form: LogLog		0.425**	0.251***		0.164
		(0.192)	(0.087)		(0.138)
Endogeneity		-0.114**			-0.052
		(0.053)			(0.106)
Robust SE		0.343**			0.103
		(0.134)			(0.085)
Cov.: Structure		0.137***			0.063
		(0.049)			(0.114)
Cov.: Inequality		0.121*			0.177
		(0.063)			(0.117)
Cov.: Development		-0.095			-0.203**
		(0.101)			(0.103)
Cov.: Macroeconomics		-0.054			-0.137
		(0.078)			(0.127)
Cov.: Demography		0.220***	0.194**		0.139
		(0.074)	(0.098)		(0.110)
Cov.: Education		-0.039			-0.053
		(0.074)			(0.079)
Cov.: Institutions		0.079			-0.004
		(0.068)			(0.112)
Cov.: Labour market		0.167*			0.056
		(0.091)			(0.095)
Cov.: Taxes and transfers		-0.042			-0.203
		(0.139)			(0.164)
R-squared	0.010	0.551	0.301	0.197	0.346
R-squared Adj.	0.009	0.534	0.296	0.183	0.332
F-statistic	2.07	42.36	4.88	3.46	2.16
Number of observations	686	686	686	686	686

size and significance of these parameters appears to be concentrated in the full model, not necessarily appearing in the specific and reduced specifications.

Only one pair of variables have robust results in the sense that they are significant and have important magnitude in both the full and reduced models (although they are removed from the specific model). They are both related to the same original metavariable: the focal sector used to measure structure. In relation to the reference value of having no focal sector, measuring structure as the size of agriculture drives up the effect size with an important magnitude; while measuring structure as the size of manufacturing does the same thing, but driving down.

This is perhaps the most important result in Table 3, as it might help to explain the general inconclusiveness of the literature, alluded to in qualitative systematic reviews and confirmed in the visual and econometric tests of the funnel plot. It is also a very logical one. The typical process of structural change, historically analysed by the literature, is that of a transition from agriculture to manufacturing. Naturally, measuring structure as the size of agriculture or as the size of manufacturing should lead to diametrically opposed results. These results suggest that at least part of the inconclusiveness of the literature might be driven by positive and negative values associated to different ways of measuring structure cancelling each other out in the general picture.

There is room for some questioning of the robustness of this evidence. These two metavariables were dropped in the specific model, and their parameters in the full and reduced models are only significant at 5% and 10% levels. We would also expect the parameters for industry to follow closely those for manufacturing; and indeed they have the same sign and similar magnitude but they are not significant. In any case, we believe this is an important result because of its consequence for the overall appraisal of the literature.

For three of the metavariables which are significant in the full model, the result is robust in the sense that they are the only ones remaining in the specific model after the step-wise approach. The strongest result overall occurs for choosing a log-log specification, which drives up the effect size very significantly in relation to choosing a linear-linear model. It is also driven up by the inclusion of a covariate related to demography, such as population size, urbanisation rate, share of certain population groups etc. On the other hand, there is evidence for a significant reduction in the effect size when the WIID/SWIID data sets are used rather than any other source of data for inequality. Again, as mentioned, both magnitude and significance fade for these metavariables in the reduced forms of the model.

Other metavariables have parameters with important magnitudes, although they have been dropped out of the specific model and show fading size and significance across the full and reduced models. The effect size is driven down importantly up when an econometric technique that accounts for endogeneity is adopted, which might be evidence for the idea that the causal relation between structure and inequality is two-way. One the other hand, it is driven very importantly down when the standard errors in the primary study are corrected for heteroskedasticity; and mildly so when inequality is measured from a mix of gross and net income instead of using exclusively gross income. On the latter, it is however somewhat surprising that using only net incomes does not change significantly the effect size. A final set of variables which are significant in either the full or reduced models are covariates related to structure, inequality, development level, and the labour market.

Finally, it is worth briefly mentioning the estimated parameter for the time span. It is not significant and has a very low value. But it is important to remember that this relates to the change in the effect size expected when an incremental year is added to the analysis. The change in the effect size would surpass the threshold for a medium-sized change for an increment in the time span of 12 years in the full model and 35 in the reduced model. The next section investigates some alternatives to assess the robustness of these results.

6 Robustness checks

In this section we test how robust the previous results are to three alternative decisions in our method. First, we run the same tests as before but changing the vector of weights used for WLS. Then, we run the FAT-PET tests on subsets of the original data. Finally, we include an additional metavariable: the estimated share of high-income countries in the observations of each regression.

6.1 Alternative weighting for WLS

As already discussed, papers normally run more than one regression of inequality on structure, either as robustness checks or to explore different mechanisms. With 686 regressions coming from 44 papers, the number of regressions per paper ranges from 1 to 112, with a mean of 15.6, a median of 11, and a standard deviation of 19.51. It can be argued, and it is perhaps expected, that regressions coming from the same paper will show some degree of dependence. Even if authors change some decisions in alternative empirical settings, many of the other decisions tend to remain constant.

In our main analysis, we dealt with this risk of dependence by running the metaregression with standard errors clustered at the level of primary studies. We turn to an alternative option of weighting the effect sizes also by the inverse of the number of regressions in the primary study to which they belong. We then effectively run WLS on the same models as the main analysis, but using a composite weight: the inverse of the variance of the PCC times the inverse of the number of regressions in the paper.

Table 4 shows the results for this alternative set of weights. Results for the simple model change and the FAT-PET tests now reach the opposite conclusion, with the rejection of both hypotheses, suggesting presence of effect but also publication bias. This rejection is not very consistent across models, particularly for the latter, which changes sign and magnitude. In any case, this does challenge to some extent the main results.

In relation to heterogeneity, the number of metavariables with significant parameters drops dramatically compared to the main results, with no metavariable having significant parameters in more than one model. Note the absence of the specific model in Table 4, as all metavariables were eventually dropped in the general-to-specific procedure. The magnitude of the point estimates are also a lot closer to zero. Measuring structure as the size of agriculture has the higher significance but only in one of the models, although its signs and also those of having manufacturing as the focal sector remain coherent. The inclusion of a covariate on taxes and transfers appears as the metavariable with the highest change in effect size overall, although it is also significant in one of the models. Other metavariables that do maintain some significance in this robustness check include whether the paper employs a technique that accounts for endogeneity, and covariates related to structure, inequality, and demography. Overall, the findings under this alternative set of weights are less strong but the profile of heterogeneity does not change dramatically, with many of the same metavariables continuing to be the ones that drive changes in the effect size.

6.2 Subsampling

As mentioned in the main analysis, perhaps the most important finding is that measuring structure as the size of agriculture or of manufacturing lead to significant changes in the effect size when compared to

Table 4: Results for alternative set of weights

Variable	Simple	Full	Data	Techn.
Intercept	-0.070**	-0.354**	-0.105	-0.068
	(0.031)	(0.144)	(0.072)	(0.162)
PCC standard error	1.363*	-0.207	0.037	0.356
	(0.701)	(0.960)	(0.993)	(0.759)
Ineq. measure: Gini/Theil		-0.058	-0.043	
		(0.078)	(0.080)	
Ineq. source: (S)WIID		-0.127	-0.055	
		(0.115)	(0.102)	
Income: Net (tax and tr.)		0.128	0.120	
		(0.083)	(0.102)	
Income: Net (tax or tr.)		0.052	0.073	
		(0.093)	(0.080)	
Income: Mixed		0.087	0.005	
		(0.141)	(0.069)	
Sector: Agriculture		0.122	0.192***	
		(0.096)	(0.067)	
Sector: Industry		0.003	0.004	
		(0.122)	(0.164)	
Sector: Manufacturing		-0.155	-0.040	
		(0.095)	(0.087)	
Sector: Other		-0.002	0.070	
		(0.196)	(0.186)	
Time span		0.003	0.000	
		(0.004)	(0.002)	
Scope: Country level		0.072	0.088	
		(0.243)	(0.121)	
Func. form: LogLin		0.086		0.008
		(0.183)		(0.114)
Func. form: LogLog		0.183		0.111
		(0.203)		(0.093)
Endogeneity		-0.140*		-0.153
		(0.074)		(0.104)
Robust SE		0.067		0.047
		(0.082)		(0.075)
Cov.: Structure		0.133**		0.081
		(0.060)		(0.082)
Cov.: Inequality		0.116		0.120*
		(0.071)		(0.070)
Cov.: Development		-0.066		-0.113
		(0.096)		(0.072)
Cov.: Macroeconomics		-0.004		-0.001
		(0.081)		(0.096)
Cov.: Demography		0.156**		0.056
		(0.065)		(0.078)
Cov.: Education		0.057		0.018
		(0.073)		(0.063)
Cov.: Institutions		0.075		0.043
		(0.098)		(0.097)
Cov.: Labour market		0.102		0.015
		(0.086)		(0.066)
Cov.: Taxes and transfers		-0.115		-0.249**
		(0.086)		(0.124)
R-squared	0.028	0.369	0.159	0.221
R-squared Adj.	0.027	0.345	0.144	0.205
F-statistic	3.78	36.94	6.86	34.38
Number of observations	686	686	686	686

having no sector. These changes are of similar magnitude, and work in opposite directions – the latter being particularly important, as it could be a relevant explanation for why qualitative literature reviews indicate inconclusive results.

To further explore this aspect, we perform analyses on two subsamples of the full data: the first only with regressions that have agriculture as the focal sector, with 110 observations; and the second only with regressions that have manufacturing as the focal sector, with 58 observations. Since the number of observations per paper in each subsample changed in relation to the full dataset, we recalculate the alternative set of weights within each subsample.

Figure 4 shows the funnel plots for each of the subsamples. The vertical axis is kept at the same scale as the one on Figure 3 for comparison; and once more the red lines represent the weighted averages of the PCCs (respectively, 0.169 and -0.114). Since we are looking at a subset of the full data, the funnels are naturally a lot less populated. There is also a concentration of imprecise estimates, with a few exceptions of high precision, particularly in the case of manufacturing. There is a risk that results are particularly driven by these precise estimates, and the conclusions of this analysis should be taken with caution. In any case, as expected, the funnels do seem to have effect sizes different from zero and with comparable magnitudes, but in opposite directions.

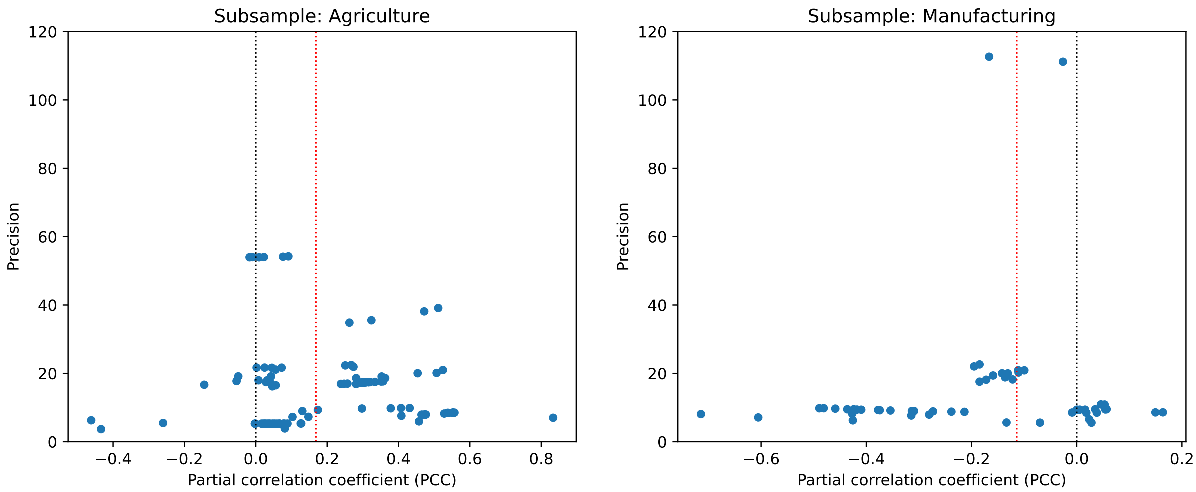

Figure 4: Funnel plot for subsamples

We turn to the quantitative appraisal. We estimate the simple model (according to Equation 1, with no metavariables) via WLS with standard errors clustered at the original study level. We do the estimation twice, for the two sets of weights: the inverse of the variance (W1) and the inverse of the variance times the inverse of the number of regressions in the primary study (W2). Recall that the tests look at the p-values of the β_1 and β_0: rejecting the null hypothesis for β_1 provides evidence for lack of publication bias (FAT); and rejecting the null hypothesis for β_0 provides evidence for lack of effect (PET).

Table 5 presents the p-values for both tests in reference to each subsample using each of the weights. We do confirm the lack of evidence for publication bias, except for manufacturing with the second set of weights, and at the 10% level. We are only able to provide evidence for presence of effect for manufacturing, though. It is possible that lack of evidence for an effect in the agriculture subsample is due to the few estimates with higher precision and which are very close to zero, as can be seen in Figure 4. Although this does weaken slightly our findings, we are still left with effect sizes of relevant magnitudes and the expected signs. Additionally, the caveats made earlier about the size and distribution of these subsamples must be kept in mind.

Table 5: p-values of FAT-PET tests for subsamples

	FAT	PET
Agric. (W1)	0.304	0.376
Agric. (W2)	0.416	0.364
Manuf. (W1)	0.291	0.000
Manuf. (W2)	0.093	0.000

With the limited number of observations in each subsample, multicollinearity becomes again a very important issue. To illustrate, we removed from each subsample metavariables that ended up having only zeroes or only ones after subsampling, and calculate the VIF for the remaining ones. For the agriculture subsample, there are two metavariables with VIF equal to infinity, while the manufacturing subsample has eleven metavariables in this condition. We then decided to not run the heterogeneity analysis for the subsamples.

6.3 Inclusion of the share of high-income countries in observations

As we have discussed, there is strong reason to believe that that the relation between structural change and income inequality varies in different phases of the transformation in the economic structure. Since structural change is also intrinsically connected with economic growth, it could be expected that richer and poorer countries show different patterns for the relation between structural change and income inequality. This could be reinforced by the fact that most richer countries have stronger social protection, decoupling inequality (when measured as net income) from structure.

The inequality literature also tends to over-represent richer countries. This is particularly problematic for the discussion on structural change: some of the most unequal countries in the world, such as South Africa and Brazil, still need to transform their economic structures in their pathway to higher prosperity, and understanding how this impacts and is impacted by income inequality is a pressing matter.

For these reasons, we believe it would be very important to include a metavariable that expresses to which extent high-income countries are represented in the observations of each regression covered in the meta-analysis. This could be done by coding the share of observations that are related to countries classified as high-income by the World Bank. We could check, for each pair of country-year, if that country in that year is classified as a high-income country.

However, implementing such a variable proved itself to be a very difficult task. As in the case of gross vs. net income, omission is prevalent. The full list of countries is not stated in 267 regressions (i.e., over a third of our data set), coming from 10 of the 44 papers. It is true that, for some of these, it is possible to indirectly deduce the share of high-income countries in observations, such as when papers run robustness analysis on subsamples of countries divided by income level. In other cases, subsampling makes things worse: some primary studies state the full list of countries in the main analysis but do not explicit which countries are included in subsample analyses. When subsampling is not done along the lines of the income level of countries, it may become impossible to know the share of high-income countries for their analyses. Another difficulty appears for primary studies that do provide the full country list but employ unbalanced panels, making it impossible to know exactly how many of the missing observations correspond to high-income countries. Additionally, some primary studies do provide the full list of countries and do employ balanced panels, but have a long-run scope, while the historical classification provided by the World Bank only goes back to 1987.

In face of all these difficulties, we decided to not include this metavariable in our main analysis. However, given its importance, we include an approximation of it as a robustness check. The calculation will be done in the following manner: for papers that disclose their full country list, we pick the first and last years covered by the data used in each regression. We then count how many pairs of country and year are high-income in the historical World Bank classification (World Bank, 2023), and divide that by the product of the number of countries times the number of years. In essence, our approximation implicitly assumes that all papers using panel data have balanced panels. For papers that do not state the full list of countries, we analysed the paper for any hints of how many observations correspond to high-income countries. We could do this for six of the ten papers – for the others, we dropped the regressions. Our number of observations was then reduced to 629. The share of high-income countries estimated through this process has a mean of 0.401, and a standard deviation of 0.390.

We run our model for heterogeneity (Equation 2), including the share of high-income countries as a metavariable. We run the full, specific, and reduced (for data variables) versions of the model, for both sets of weights, with the alternative set of weights being recalculated after observations were dropped. It is worth recalling that one of the main mechanisms at stake here is the presence of distributive institutions decoupling (net) income from structural determinants. This implicitly assumes that gross incomes come directly, mechanically, from the sectoral composition. The metavariable on the type of income (gross or net) used to calculate inequality already partially addressed the mechanism. Because both act through the main mechanism, we remove the metavariables related to the type of income, but keep all remaining metavariables in Table 1.

As can be seen on Table 6, results for the share of high-income countries are similar in both sets of weights. The variable is significant for the reduced model with data-related metavariables and suggest that a medium-sized change to the effect size occurs when the share of high-income countries in the sample goes from zero to one. This finding is not so strong, though, as the variable does not remain in the specific model, and its parameter is not significant in the full model.

The estimate we have is importantly imprecise. Since we used the same share for all regressions within each paper, we ignored subsample analysis and the like. The estimation could still be improved. We could, like we did for papers with no information on the list of countries, look for hints on the primary studies that allow us to take educated guesses at the shares, and use the approximation only on those for which we don't find such hints.

In any case, we believe there is only so much reduction one could do for the uncertainty here. We finish with a plea about the importance of disclosing, even if in supplementary material, at least the full list of countries covered, and ideally the correspondence of observations per country and per year. Omitting this information might make it impossible for literature reviews to capture the impact on results of focusing on specific sets of countries. This is crucial information for all phenomena that may work through different mechanisms for different types of countries, such as for structural change and income inequality but also on topics such as climate change and many others.

7 Conclusion

This paper has performed a meta-analysis on the causal relation between the sectoral composition of an economy and its level of income inequality. While we do not find evidence of an overall effect of structure on inequality, we do find that the size of the estimated effect is influenced by decisions related to the empirical setup. These include the focal sector for measuring structure; the functional form of the model; the choice of data set; the use of a technique that accounts for endogeneity; the use of robust standard

Table 6: Results with the estimated share of high-income countries

Variable	Full (W1)	Spec. (W1)	Data (W1)	Full (W2)	Spec. (W2)	Data (W2)
Intercept	-0.443***	-0.225***	0.043	-0.340**	-0.167***	0.013
	(0.168)	(0.066)	(0.085)	(0.144)	(0.026)	(0.073)
PCC standard error	-1.205	-0.978		-0.051	0.037	
	(0.878)	(1.177)		(0.875)	(0.670)	
Ineq. measure: Gini/Theil	0.034		0.024	-0.085		-0.022
	(0.067)		(0.064)	(0.068)		(0.059)
Ineq. source: (S)WIID	-0.325***	-0.277***	-0.078	-0.161*		-0.030
	(0.078)	(0.030)	(0.065)	(0.090)		(0.065)
Sector: Agriculture	0.138*		0.167***	0.128		0.188***
	(0.071)		(0.056)	(0.085)		(0.056)
Sector: Industry	-0.020		-0.142	0.075		0.016
	(0.081)		(0.167)	(0.091)		(0.166)
Sector: Manufacturing	-0.121*		-0.052	-0.090		0.030
	(0.062)		(0.056)	(0.074)		(0.060)
Sector: Other	0.075		0.078	0.090		0.103
	(0.221)		(0.180)	(0.167)		(0.157)
Time span	0.001		0.000	0.002		0.000
	(0.002)		(0.001)	(0.002)		(0.002)
Scope: Country level	0.373***	0.377***	0.022	0.253**	0.136***	0.056
	(0.116)	(0.070)	(0.076)	(0.119)	(0.052)	(0.078)
Func. form: LogLin	-0.216***	-0.185***		-0.172		
	(0.079)	(0.038)		(0.113)		
Func. form: LogLog	-0.002			-0.053		
	(0.071)			(0.091)		
Endogeneity	-0.081**			-0.092*		
	(0.035)			(0.053)		
Robust SE	0.220***	0.151***		0.072		
	(0.064)	(0.051)		(0.062)		
Cov.: Structure	0.053*			0.052		
	(0.029)			(0.035)		
Cov.: Inequality	0.005			-0.002		
	(0.065)			(0.065)		
Cov.: Development	0.138*			0.139*		
	(0.072)			(0.079)		
Cov.: Macroeconomics	-0.136***	-0.205***		-0.052		
	(0.051)	(0.074)		(0.060)		
Cov.: Demography	0.220***	0.253***		0.150***	0.156***	
	(0.058)	(0.054)		(0.057)	(0.046)	
Cov.: Education	-0.007			0.087		
	(0.054)			(0.056)		
Cov.: Institutions	0.197***	0.127***		0.207***		
	(0.067)	(0.049)		(0.078)		
Cov.: Labour market	0.118*			0.073		
	(0.070)			(0.065)		
Cov.: Taxes and transfers	0.023			-0.055		
	(0.117)			(0.092)		
Share of HI countries	-0.063		-0.150**	-0.074		-0.136**
	(0.080)		(0.066)	(0.090)		(0.057)
R-squared	0.658	0.606	0.207	0.479	0.190	0.184
R-squared Adj.	0.645	0.601	0.196	0.459	0.186	0.172
F-statistic	70.35	41.93	4.99	100.92	5.75	8.68
Number of observations	629	629	629	629	629	629

errors; and the inclusion of covariates on demography, structure, inequality, development level, and the labour market.

Importantly, measuring structure as the size of agriculture versus manufacturing drives the results in opposite directions and similar magnitudes in relation to the reference value of having no focal sector. It is possible that these cancel each other out, leading to the impression of nonexistence of an overall effect. If the findings on agriculture and industry are indeed only mirror images of the same phenomenon, the results seem to suggest that different types of structural change – such as the Kuznetsian transition away from agriculture versus the more general diversification implied by the economic complexity literature – have different impacts on inequality.

The sensitivity of the effect size to the inclusion of certain groups of covariates suggests that researchers on structural change and within-country income inequality should be wary of omitted variable biases. The significant impact of adopting techniques that account for endogeneity might also suggest that the often overlooked role of reverse causality is important, and that researchers should also be cautious about changes in inequality feeding back into changes in the structure when designing their empirical approaches.

The meta-analysis faced some limitations during coding. One is related to the metavariable on the focal sector: it differentiates between agriculture, manufacturing, and industry, but groups together rather heterogeneous sectors under "other", such as the knowledge sector, low technology sectors, and services as a whole. This was done because of each of these sectors occurred too rarely in the literature as the focal point for measuring structure. All other limitations on coding are related to papers omitting certain details about empirical approaches and results. One such detail is the degrees of freedom in each regression, which is used together with the t-value of the parameter related to structure to calculate the effect size (PCC). Papers in the literature do not tend to report them explicitly, and it is not necessarily straightforward to deduce them. We rely on the observation from Stanley & Doucouliagos (2012, p. 156) that results tend to be rather robust to imprecise values of the degrees of freedom. We also faced difficulties in coding the type of income (gross vs. net) and calculating the share of high-income countries in the observations of each regression, which also tend to be omitted by the primary studies. In the case of the type of income, we mitigated the issue by investigating the documentation of data sources of primary studies and by contacting authors. In the case of the share of high-income countries, we included that metavariable only as a robustness check, and performed approximations to compare with the coded values. We argue that is very important that authors disclose these details, at least in supplementary online materials. Particularly for the latter, many phenomena occur through different mechanisms in different types of countries. Not knowing precisely which are being covered by the each regression makes it harder to account for this source of heterogeneity in results.

A future related meta-analysis could focus on the literature that has tried to estimate the Kuznets curve. To our knowledge, only qualitative literature reviews of this literature have been done. We do not cover this literature entirely because most of its primary studies proxy structural change by the level of the GDP per capita, while we chose to focus on studies that had looked at structure directly, as sectoral sizes. A meta-analysis on the Kuznets curve would be interesting not only because of the importance of this literature but it would also add what we believe to be a novelty in the meta-analytical literature. Primary studies that estimate the Kuznets curve normally use a quadratic model specification. We would be looking, then, at two parameters of interest for the meta-analysis, instead of the usual one.

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8 Appendix: details on methods

This appendix shows more details on certain methodological decisions presented in the paper. We show the intermediate queries resulting from the refinement steps of keywords, and give more information on the removal of outliers and recoding of metavariables with low variation.

8.1 Intermediate keyword queries

In this section, we detail the intermediate keyword queries resulting from each refinement step. As detailed in Section 3, we started with a preliminary set of words defined in relation to the PICOS (Population, Intervention, Comparison, Outcome, Study type) framework, which are shown in Table 7. We only have keywords related to Intervention and Outcome. As mentioned in Section 3, our research design does not impose restrictions related to Population nor Comparison. We do have a criterion related to the type of study, but decided to account for that only during screening. It is also important to note that, although we defined structural change as the intervention and income inequality as the outcome, the search terms are agnostic as to which causes which. However, as discussed in the paper, the literature largely frames the causality as going from structure to inequality, although there is reason to believe it works both ways.

PICOS item	Scope of the review	Preliminary free text terms
Population	Countries in general	(None)
Intervention	Structural change	"structural change" OR "structural transformation" OR "sectoral composition" OR "economic structure" OR "productive structure"
Comparison	(None)	(None)
Outcome	Changes in income inequality	"income inequality" OR "income distribution"
Study type	Quantitative	(None)

Table 7: Preliminary terms defined in the PICOS framework

These keywords could only find one of our six control papers in Scopus. We then screened them for title and abstract, and included three new groups of keywords: (i) "share(s)" or "percentage(s)" when close to "employment" or "labo(u)r force"; (ii) "sector dualism", "dual economy", and "dual structure"; and (iii) "employment shift(s)", "sectoral change", or "sectoral shift(s)".

We then used the refined search terms on EconLit, counted the most frequent JEL codes among the results, and filtered the codes that are in fact on the relevant topics by checking their descriptions. We then searched EconLit for a combination of all codes related to structural change with all codes related to income inequality, retrieving 5691 results. We performed a frequency analysis of their titles and abstracts using Voyant Tools, and included new keywords in both sets.

After this second round of refinement, we were left with the following terms for structural change: "structural change"; "structural transformation"; "sectoral composition"; "economic structure"; "productive structure"; "sector dualism"; "dual economy"; "dual structure"; "employment shift*"; "sectoral change"; "sectoral shift*"; "share" or "percentage" close to "employment" or "lab* force"; "sectoral diversification". For income inequality, the terms after the second round are the following: "income inequality"; "income distribution"; "income gap"; "income disparity"; "wage inequality"; "wage distribution"; "wage gap"; "wage differential"; "wage premium"; "wage disparity".

We ran these search terms on Scopus, retrieving 857 results. The 36 most cited papers among these accounted for roughly half of the total count of citations in the sample. We analysed the titles and abstracts of these 36 papers to filter those that were on topic, reaching a list of six papers. The full text of these were analysed one by one to understand their usage of relevant terms.

This analysis led to a large restructuring of the list of search terms. For organisation, it helps to separate the terms related to the sectoral composition from those related to income inequality. Starting by the latter, the terms related to income inequality so far were binomes in which one word relates to income or similar terms (income, wage), and the other related to inequality or similar terms (inequality, distribution, gap, disparity, differential, premium). The analysis suggests adding "earnings" to the first group and "heterogeneity", "dispersion", "share", and "stratification" to the second. It also suggests substituting some binomes with proximity operators – we will extend this idea by breaking down all binomes and interacting both groups (income etc., and inequality etc.) mediated by proximity operators. Finally, also in line with the use of proximity operators, the analysis suggests adopting truncations in "income*", "wage*", "inequalit*", and "differen*". The search terms for income inequality would, then, be: "income*", "wage*" or "earnings"; when close to "inequalit*", "distribution", "gap", "disparity", "differen*", "premium", "heterogeneity", "dispersion" or "share".

Grouping the terms related to the sectoral composition is less straightforward. One separation can be done between "static" terms, that refer to the sectoral composition itself, and "dynamic" ones, that refer to changes thereof. In the static search terms, the binome "sectoral composition" is joined by the new binome "economic complexity". In the previous list, two binomes used "structure" to refer to the sectoral composition ("economic structure" and "productive structure"), and three new similar ones are suggested by the refinement ("occupational structure", "industrial structure", and "export structure"). Moreover, in line, with the search terms related to income inequality, we suggest breaking down these binomes with proximity operators, and adding truncation – this group of search terms is then converted to: "structure*" when close to "economic", "productive", "occupation*", "industrial", or "export*". Another group of static search terms is related to when the sectoral composition has a dual structure. Here, we add truncation to the binomes "dual econom*" and "dual structure*", and break the binome "sectoral dualism", substituting for "duali*" when close to "sector*". Finally, there is a group of static terms that uses proximity operators to refer to the shares of each sector: on one side, "employment" and "labo[u]r share", which we keep; and on the other, "share" and "percent*"(substituted from "percentage"), to which we add "proportion", and "fraction". We then turn to the dynamic search terms related to the sectoral composition. We keep the binomes "structural change", "sectoral transformation", and "sectoral diversification" as they were. For the terms related to shifts in employment ("employment shift" and "sectoral shift"), we add the reference to the labour force and again break the binomes by using proximity operators, and add truncations, resulting in: "shift*" when near "employment", "sector*", or "labo[u]r force".

After all these changes, we performed a final step of revision and minor adjustments to the keywords, reaching the final query presented in Section 3.

8.2 Removing outliers

As stated in Section 3, the initial coded sample had 51 papers, out of which five were dropped for only reporting significance levels, and one was dropped for poor reporting of the method. A final paper was dropped for issues in reporting the method, for having standard errors reported as 0.0 in two of its regressions, and for having outlier values in the three remaining regressions. Figure 5 shows the funnel plots of the PCC against its precision (on the left) and the point estimate against its precision (on the right) to illustrate the magnitude of the outlying behaviour. All three remaining regressions from this paper were finally dropped.

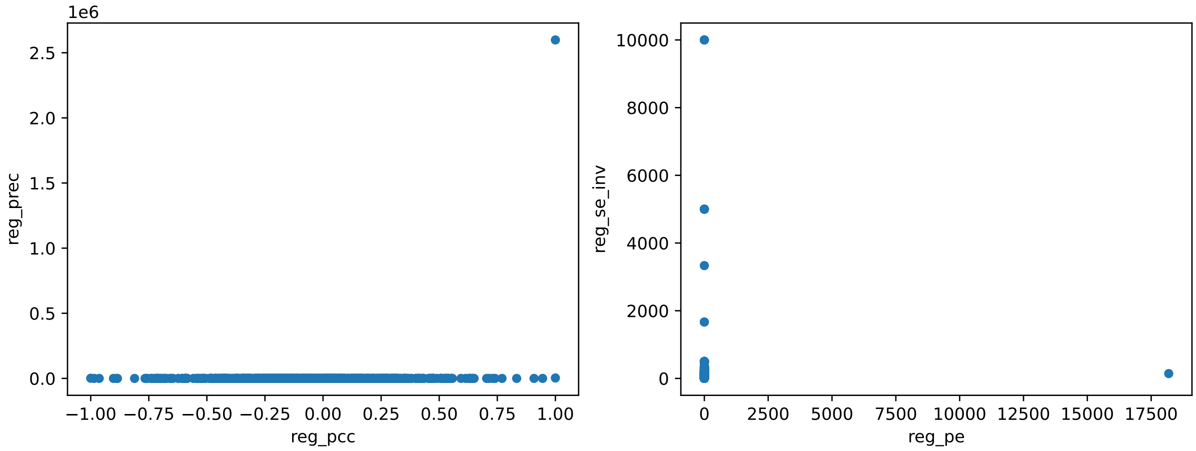

Figure 5: Funnel plots before removing outliers

8.3 Dealing with low variation

The original coding of some of the categorical metavariables had a larger variation of values. For instance, the metavariable for measure of inequality was used in the paper as a binary variable equal to one when inequality is measured as the Gini or Thiel indices, and zero otherwise. The original coding, however, differentiated between Gini and Thiel, and also between top income shares, low income shares, work share, labour share, and rural gap.

The original coding was done with more granularity because we would like the coded data to be as detailed as possible and because it was also not possible to know in advance the distribution of values of each categorical variable. This meant, however, that some of the coded values occurred too rarely. We then checked the distribution of each metavariable to investigate to which extent this occurred. Before the process of removal of highly correlated metavariables, as detailed in Section 3, we one-hot encoded all metavariables and checked their distributions.

Table 8: Distribution of values for metavariables with low variation

	0	1
Cov.: taxes and tr.	656	30
Data level: micro	666	20
Ineq. measure: income ratio	658	28
Ineq. measure: low income share	685	1
Ineq. measure: rural gap	684	2
Ineq. measure: top income share	652	34
Ineq. measure: work or labour share	654	32
Income: net of transfers	677	9
Str. measure: share of exports	679	7
Focal sector: business services	671	15
Focal sector: knowledge sector	675	11
Focal sector: low technology sectors	682	4
Focal sector: none (sectoral data)	647	39
Focal sector: services	666	20

Some recoding decisions were taken upon analysing these results. Almost all variables related to the measurement of inequality were listed, prompting, as mentioned above, its recoding into a binary variable equal to one when the Gini or Thiel index was used, and 0 otherwise. The metavariable on the type of income was originally coded separately for income net of taxes and for income net of transfers – since the latter occurred too rarely, both values were merged together as net of taxes or transfers. We did not group that, though, with the coding for net of both taxes *and* transfers.

For the focal sector, having no such sector due to the use sectoral data was grouped together with not having it due to the use of an aggregate measure. The other cases within this metavariable were more tricky because they should drive the relation in different directions: business services and the knowledge sector are high-income services; while low technology sectors are typically lower-income; and services as a whole are heterogeneous, combining both high- and low-income jobs. We nonetheless grouped them together because of the low variation, conscious however that this imposes an important limitation in our data.

Since the share of exports was the only one on its category, we kept it at this point, but it is worth remembering that this whole metavariable was dropped later on when highly correlated metavariables were removed. The covariate on taxes and transfers was also kept both because it was the only one in its group (although those in that group are independent metavariables, and not connected to the same original one), and because its variation was not as low as for other variables. The binary variable on the data level being micro-level data was dropped.

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