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Can trade explain the rising trends in income inequality? Insights from 40 years of empirical studies☆

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ABSTRACT

There have been concerns that international trade is responsible for rising inequality. However, existing empirical studies provide no consensus on this matter. This article studies the effect of trade on income inequality by applying meta-regression analysis on 40 years of empirical studies. We discover that the disagreement in the literature can be explained by differences in the development levels of the countries chosen by the studies and whether the endogeneity of trade is controlled for. When endogeneity is addressed, we find that trade can *reduce* income inequality in middle- and high-income countries, but has no statistically significant effects in low-income countries. Therefore, concerns that trade leads to more inequality could be overstated. Our work sheds light on how certain features in an empirical model of inequality on trade could influence the analysis itself, which provides some guidance on empirical design for future research.

1. Introduction

Over the past 25 years, trade flows have nearly quadrupled. At the same time, within-country income inequality, especially in developed countries, has been worsening rapidly (Ravallion, 2014; Nolan et al., 2019). This has led to renewed concerns that international trade is responsible for rising inequality, leading to scepticisms towards globalization. In theory, how trade affects income inequality is ambiguous and depends on factors such as how developed the country in question is.[1] Empirically, the observed effects of trade on inequality have turned out to be ambiguous as well (see reviews from Feenstra and Hanson, 2003; Goldberg and Pavcnik, 2007; Harrison et al., 2011), which reinforce that notion that how trade actually affects inequality is far from clear.

In this paper, we survey 494 primary studies (Fig. 1) spanning over 40 years to investigate why there is little reconciliation among empirical studies on how trade affects inequality. For instance, among the studies we have surveyed, we find that only 143 have reported positive and statistically significant effects (at the 10% level) of trade on inequality. By contrast, the remaining 351 have reported negative or insignificant effects (see Appendix A for more details). To understand why the literature disagrees and to determine the true effect of trade on income inequality, we employ a meta-regression analysis (MRA) on these 494 empirical studies. The MRA first involves constructing a common unit-free effect size (i.e. the partial correlation coefficient) from the primary estimates as the meta-dependent variable. Then, the meta-dependent variable is regressed against various characteristics of the primary

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[1] The standard Heckscher-Ohlin model predicts that trade reduces inequality in developing countries but increases it in developed countries. There are also numerous theoretical models predicting that trade could raise inequality in both developed and developing countries (e.g., Feenstra and Hanson, 1996; Helpman et al., 2010). In contrast, the modernization theory (Kuznets, 1955) and the models that account for learning and skill upgrading (e.g., Aghion and Howitt, 1998: 262) predict that trade increases inequality in countries during the early stages of development but later reduces it through promoting their economic growth and democratization. See Harrison et al. (2011) for a detailed review.

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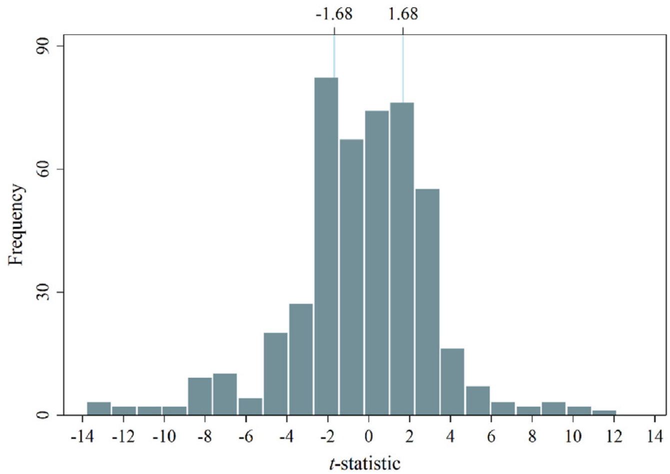

Fig. 1. Estimated effects of trade on income inequality among 494 studies. *Note*: This figure presents the *t*-statistic of the estimated coefficient of trade on inequality for 494 primary studies. The estimated effect is defined as as significant and positive (negative) if the *t*-statistic is larger (smaller) than 1.68 (i.e., at the 10% significance level), and as statistically insignificant if the absolute value of the *t*-statistic is smaller than 1.68.

studies that may explain the studies' disagreement of the trade-inequality relationship.[2]

Employing MRA, we find that the disagreement among primary studies can be explained by their differences in the measures of trade and inequality employed, the economic development level of the sample countries considered, and their efforts in addressing endogeneity. Importantly, our analysis reveals that studies based on high-income countries tend to observe clear negative effects of trade on inequality compared with studies based on low-income countries. Similarly, studies that address the concern that trade is potentially endogenous tend to find stronger negative effects than studies that do not. As such, if endogeneity is addressed, we should find that trade may *reduce* inequality in middle- and high-income countries but has no effect on the inequality in low-income countries. Therefore, while the upward trends suggest that trade has increased income inequality, the evidence actually does not support this hypothesis.

Rising income inequality has been a core social concern for both rich and poor countries, and is often blamed for countries' poor economic, social, and political conditions (Keefer and Stephen, 2002; Berg et al., 2012; Jaumotte et al., 2013). Despite being the focus of a large body of empirical studies, the question of which forces are driving an increasing wedge between the rich and poor, notably with respect to the roles of globalization and technological changes, remains hotly debated (Agnello et al., 2012; Nolan et al., 2019; Aghion et al., 2019; Islam et al., 2020; Chu et al., 2021; Vu, 2021). Using MRA to synthesize the 494 existing empirical studies on the effect of trade on income inequality, we find no evidence supporting that trade is a cause of the increasing inequality over the past decades.

The finding of our MRA does not support the prediction of the standard Heckscher-Ohlin model that trade reduces inequality in developing countries but increases it in developed countries. Our finding also does not support theoretical models predicting that trade increases inequality in both developed and developing countries (e.g., Feenstra and Hanson, 1996; Helpman et al., 2010). However, the finding that trade reduces inequality in countries with high development levels is consistent with the prediction of modernization theory (Kuznets, 1955) and theoretical models that account for learning and skill upgrading (e.g., Aghion and Howitt, 1998: 262).

The current article contributes to the body of literature reviews on the effect of globalization on income inequality (e.g., Goldberg and Pavcnik, 2004; Feenstra and Hanson, 2003; Goldberg and Pavcnik, 2007; Harrison et al., 2011; Yang and Mallick, 2014). These reviews generally find mixed effects of various dimensions of globalization (including trade, FDI, financial liberalization, and immigration) on inequality and suggest that differences in the measures of globalization and inequality are important causes of the mixed findings. Two characteristics distinguish the current article from the existing reviews. First, the current article is the first to focus specifically on the effect of trade flows on inequality; previous reviews usually included multiple dimensions of globalization.[3] By focusing on trade flows, our survey focuses on a more comparable set of literature and avoids mixing different dimensions of globalization, which enables us to draw sharper conclusions. Second, while previous reviews are mainly narrative, our review depends on MRA to provide quantitative evidence. To the best of our knowledge, the current article is the first meta-analysis that studies the effect of trade flows on income inequality.

The following of the article is as follows. Section briefly reviews the theoretical models, Section 3 summarizes the 494 primary studies included in this meta-analysis, Sections 4 and 5 introduce the meta-regression approach and report the estimation results, respectively. The last section is concluding remarks.

2. A brief summary of the theoretical literature

The theoretical literature suggests that the effect of trade on inequality could be positive (i.e., inequality-exacerbating) or negative (i.e., inequality-narrowing), and may depend on how developed the country being studied is. The standard Heckscher-Ohlin (HO) model predicts that trade reduces inequality in developing countries but increases inequality in developed countries. The rational is that trade increases the real return to the factor that is relatively abundant in each country (the Stolper-Samuelson theorem), and developed and developing countries are abundant in skilled and unskilled labours respectively. However, since 1990, many studies have found that inequality has actually increased in developing countries with major trade reforms (e.g., Harrison and Hanson, 1999; Goldberg and Pavcnik, 2007).

The emergence of stylized facts at odds with the HO model has led economists to search for new theoretical explanation for the concurrent rising trends of trade (and other aspects of globalization) and inequality in countries at all income levels (Harrison et al., 2011). For example, Feenstra and Hanson (1996) develop a trade model with trade in tasks and predict that trade in tasks will raise income inequality in both developed and developing countries; Dinopoulos and Paul, (1999) provide a two-country growth model in which firms compete through research and development (R&D), and predict that if R&D is skilled-labor intensive relative to manufacturing, then trade liberalization will increase income inequality all around the world; Anderson (2011) studies a model in which workers must choose which sector to acquire skills, and predicts that opening trade increases income inequality by increasing income differentials across industries; and Helpman et al. (2010) incorporate heterogeneous-firms monopolistic competition (and a number of other elements) into a model of international trade and predict that trade liberalization unambiguously increases wage inequality.

There are also theoretical models predicting that trade first increases and then reduces inequality as a country develops (as opposed to the prediction of the HO model). According to the modernization theory

[2] To ensure that the primary empirical studies included in our analysis are comparable, we focus on only trade in goods and only on its effect on country-level income inequality; thus, studies on foreign direct investment (FDI) or inequality across countries are outside of the scope of this MRA. The interested reader on the effect of FDI on inequality is referred to Huang et al. (2020) and on the evolution of global inequality and its drivers is referred to Bourguignon (2015), Milanovic (2016) and Ravallion (2018).

[3] The only exception is Huang et al. (2020), which focuses on the effect of FDI on inequality.

(Kuznets, 1955) and "Kuznets' inverted-U curve" hypothesis, during the early stages of development, rising globalization would increase the share of population involved in the narrow but modern high-income sector of the economy, which therefore increases the economy's overall income inequality. In the later stages, however, with further economic growth, accompanied by a more likelihood of democratization, would lead to a more equal society and thereby reverse the trade-inequality relationship. The economic models account for learning and skill upgrading (e.g., Aghion and Howitt, 1998: 262) also predict similar effects: globalization first leads to a higher skill premium within domestic firms and thus increases income inequality; however, with the increase of the supply of the required skills, firms transition to adopt new and better technologies, which results in inequality to decline over time.

3. Summary statistics of primary studies

In this section, we introduce the search strategy for identifying the existing econometric studies that will be included in our meta-analysis. We then move on to introduce the effect-size that we will use as the dependent variable of our meta-analysis. Finally, we provide summary statistics for the key characteristics of the collected primary studies that we will use as the independent variable of our meta-analysis.

3.1. How to identify primary studies

Our search strategy closely follows the meta-analysis guidelines of MAER-NET (Stanley et al., 2013). To avoid coding errors, the coding of all variables was checked independently by three authors of this article. We conducted the search of primary studies from October 2019 through to February 2020. For studies that have been published in journals and books, we looked up the Web of Science, JSTOR, Scopus, Google Scholar, EconLit, major publishers' websites, and several other possible sources. For unpublished papers (mainly dissertations and working papers), we sequentially searched SSRN, Google Scholar, and websites of renowned institutes. Finally, we also investigated all references cited in the collected prior articles. The keywords that we have used for search were income/wage (inequality/distribution/share/ratio), Gini, Theil index, Atkinson index, trade, import, export, openness, liberalization, globalization, tariff, and combinations of these words.

Eventually, our search covered more than 1500 relevant primary articles, of which we selected 69 (listed in Appendix A) according to the following six criteria:

1. *Econometric-based study*: the papers included have to contain econometric estimates on the effect of trade on inequality. Primary studies that were only theoretical or contained only descriptive statistics were excluded, and studies examining the inequality's effect on trade were also excluded.
2. *Common effect size*: the primary studies must include sufficient information for us to directly obtain or indirectly construct the *t*-statistic and degrees of freedom pertaining to the regression coefficient of trade.[4]
3. *Income inequality measures*: the analysis is restricted to studies measuring income inequality by the standard indicators, including the Gini coefficient, the income share of the top decile or quintile(s), the income ratio of the top to bottom decile or quintile(s), and other measures as detailed in Table 2.
4. *National income inequality*: we only include primary studies that focused on income inequality at the national level. We exclude studies on cross-country income inequality, within-country subgroup income inequality (such as the inequality between skilled and unskilled workers, and that between ethnic groups), and poverty.[5] Studies on other aspects of inequality, such as gender inequality and education inequality, were also excluded.
5. *Trade measures*: we include studies that measure trade by the value of imports, exports, or the sum of them (as a percentage of GDP/GNP). We exclude studies on the effect of trade liberalization considering the difficulty of measuring trade liberalization; as stressed by Goldberg and Pavcnik (2004), trade protection has increasingly taken the form of non-tariff barriers that are inherently hard to measure.[6] We also exclude studies on the effect of other dimensions of globalization (e.g., FDI, financial globalization) to ensure the estimates collected are comparable.
6. *English language*: for practical considerations, we only include studies written in English.

From the 69 primary articles, we constructed a dataset with 494 primary estimates. These are the total number of regression estimates from these articles that satisfy all the above six criteria. The sample size of 494 exceeds the number of primary articles, because each primary article usually includes more than one studies and thus reports multiple estimates (for this reason, *studies* and *estimates* are used interchangeably in the current paper). Specifically, the number of estimates derived from each article ranges from 1 to 25, with a mean of 7.2 and a standard deviation of 9.1. Fig. 2 presents the distribution of the publication year of these articles. Among these 69 articles, the first was published in 1976 by Rubinson (1976) and the last was published in 2020 by Le et al. (2020). About 70% of these articles are published after 2010, indicating an increasing interest on the trade-inequality issue.

As presented in Panel A of Table 1, among the 494 primary studies, 357 are multi-country studies and only 127 are single-country studies. We find that most multi-country studies focused either on developing or on developed countries, so we can classify them according to the development level of their sample country.[7] As presented in Panel B, 170 primary studies are based on high-income countries, 125 on middle-income countries, 127 on low-income countries, and the remaining 72 on countries with mixed development levels.

It is difficult to classify the development level of the *sample* of a primary study not only because the development level of a country changes over time but also because the definition of development level changes over time. The classification we use is based on the definition of the World Bank. In 2020, the World Bank defines an economy as low-income, lower middle-income, higher middle-income, or high-income based on its GNP per capita in 2018 and the threshold values $1,025, $3,995, $12,375. Because primary studies usually combine data from low- and lower middle-income countries,[8] we classify studies based on both low and lower middle-income countries into the "low-income group". Therefore, we only use the threshold values of $3995 and $12,375 to classify primary studies into low-, middle-, and high-income groups. To do so, we derive GNP per capita (transformed into constant 2018 USD) from the World Bank for the sample countries and sample years of each

[4] The *t-statistic* can be calculated from the P-value, Z-value, significance level, and estimated coefficient and standard error.

[5] Therefore, our meta-analysis is not able to explore the inconsistency pertaining to differences in within-country subgroup income inequality and poverty. Future meta-analyses examining the differential effect of trade on poverty or on skilled and unskilled labor would be interesting, but these are out of the scope of this paper.

[6] Note that we only exclude studies that used the trade liberalization measures (such as the trade openness index, trade liberalization index, and trade reform dummy) that do not always capture trade. In the robustness check in Table 6, we include these studies and find comparable results.

[7] Since only a small share of the primary studies used regional samples, we are not able to examine whether the inconsistent findings can be explained by regional differences in our meta-analysis.

[8] Our survey only identified 9 primary studies based specifically on low-income countries, while there are 96 studies based on data from both low- and lower middle-income countries.

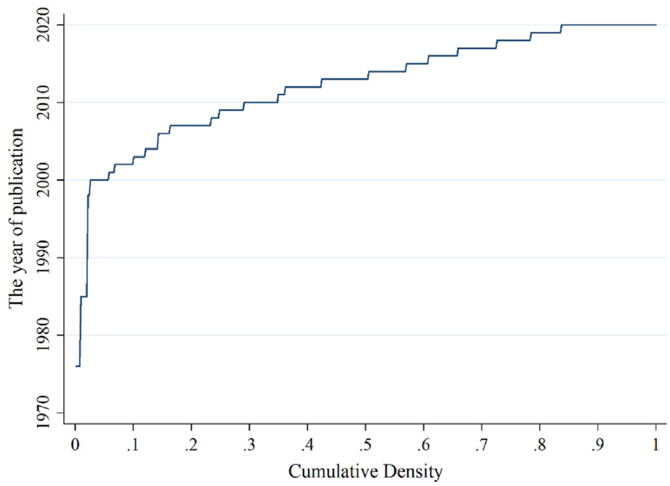

Fig. 2. Cumulative distribution of the publication year of primary studies.

Table 1
Distribution of primary studies across regions and income levels.

A. Regional distribution			
Country	Number	Country	Number
Multiple country studies	357	Germany	12
Mexico	30	India	11
China	20	Russia	7
United States	18	Pakistan	6
Italy	16	Indonesia	2
Brazil	15		
Total			*494*
B. Development levels			
Classification of sample country			Number
High income			170
Middle income			125
Low income			127
Mixed			72
Total			*494*

Note: The classification in Panel B is based on the definition of the World Bank.

primary study. We then compare the mean GNP per capita of each primary study with the threshold values to determine which income group it belongs to. For example, if the mean GNP per capita (in constant 2018 USD) of the sample of a primary study belongs to the interval from $3995 to $12,375, we define the study as using data from the middle-income country. Finally, if a primary study has more than one-third of its sample country-years with GNP per capita belonging to two of the three income groups, we classify it as mixed.[9] Note that this classification does not take into account the changes in the definition of development levels. Fortunately, as presented in a robustness check in column 3 of Table B1, using the development level definition of the World Bank in 2000 leads to compare results.

3.2. *Effect size*

The "effect size" obtained from the primary studies is the dependent variable of a meta-regression analysis. Because the primary studies may use different metrics, in order to harmonize their results, we follow the common practice of the literature (e.g., Stanley and Doucouliagos, 2012) to use the partial correlation coefficient (*PCC*) as the effect size, which is constructed as

[9] We have also tried to use 20% and 40% of sample country-years as the cut-offs and found quite similar results.

Table 2
Definition and summary statistics of variables.

Name	Description	Summarize	
		1	0
Measures of trade and inequality			
Total trade value	BD = 1: Total trade value (as a percentage of GDP/GNP)	368	126
Export value	BD = 1: Export value (as a percentage of GDP/GNP)	50	444
Import value	BD = 1: Export value (as a percentage of GDP/GNP)	76	418
Gini	BD = 1: Gini coefficient	396	98
Income Share Top	BD = 1: Income share of the top decile or quintile(s)	21	473
Income Share Ratio	BD = 1: Income share ratios of the top to bottom decile or quintile(s)	29	465
Other inequality measures	BD = 1: E.g., Atkinson index, Theil index, and EHII Gini coefficient (used as the base)	48	446
Measure of the development development			
High income	BD = 1: High-income countries	170	324
Middle income	BD = 1: Middle-income countries	125	369
Low income	BD = 1: Low-income countries	127	367
Mixed	BD = 1: Countries with mixed income levels	72	422
Measures of addressing endogeneity			
Estimation methods	BD = 1: Using estimation methods to deal with endogeneity (e.g., IV, 2SLS, GMM, Heckman two-stage)	126	368
Panel fixed effect	BD = 1: Panel model with country fixed effects (instead of random effect panel model, cross-sectional model, or time-series model)	403	91
Other moderating variables			
GDP control	BD = 1: Control for GDP (e.g., growth rate, log per capita/total GDP)	276	218
Education control	BD = 1: Control for education levels	183	311
Government control	BD = 1: Control for government's efforts in reducing inequality	140	354
Demographic control	BD = 1: Control for demographic factors (e.g., population size, birth rate, dependency ratio)	107	387
Openness control	BD = 1: Control for other measures of openness of a country (e.g., FDI, Capital account openness, terms of trade)	146	348
If published	BD = 1: Published in journal or book	441	53
Publication year	BD = 1: Published after 2010	322	172
Standard error of the PCC	Continuous variable	Mean = 0.08	

Notes: BD denotes a dummy.

$$r_i = \frac{t_i}{\sqrt{t_i^2 + df_i}}, \tag{1}$$

in which t_i and df_i are, respectively, t-statistic and degrees of freedom associated with the regression coefficient on trade in study i. The *PCC* is an indicate of the strength of the association between trade and inequality, holding the same of other factors. It suggests that the effect of trade is small if the absolute value of *PCC* is below 0.10, large if it is greater than 0.4, and moderate if it is between them (Doucouliagos and Ulubaşoğlu, 2008). The *PCC* is unit-free, and therefore, is comparable across studies. The variation due to sampling error (i.e., the standard error of the *PCC*) is given by

$$se_i = \frac{r_i}{t_i} = \sqrt{\frac{(1 - r_i^2)}{df_i}}. \tag{2}$$

Fig. 3 presents a funnel graph that shows the association between *PCC* and the inverse of its standard error of it for the 494 primary studies. In line with Fig. 1, it shows substantial inconsistency in both the magnitude and direction of the estimated effects. The *PCC* is widely distributed, suggesting that the true effect could be significantly negative or positive. The average of *PCC* (indicated by the blue line) is negative but close to zero (−0.016), which suggests that the average effect of trade on inequality is negative but very weak.

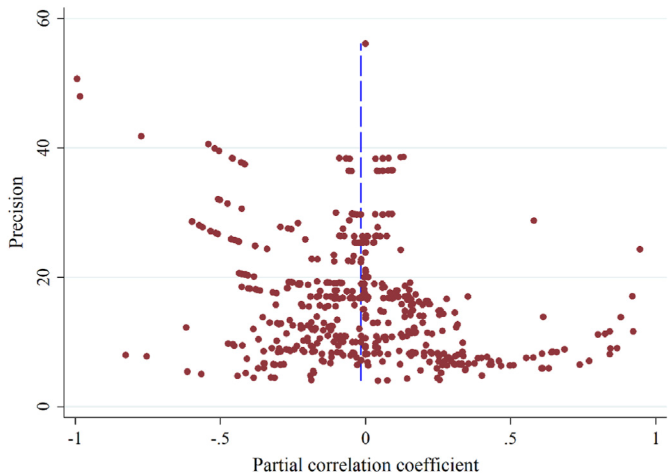

Fig. 3. The distribution of the trade-inequality correlation (funnel plot). *Note*: This figure plots the association between *PCC* and the inverse of its standard error for 494 primary studies.

3.3. Moderating variables

Here we introduce the moderating variables that may have explanatory power on the inconsistency across the primary studies. We classify these variables into five categories: 1) measures of trade, 2) measures of income inequality, 3) indicators of the economic development level of the study country, 4) indicators of addressing endogeneity of primary estimation, and 5) other characteristics of primary studies. We define the moderating variables in Table 2 and summarize them in Tables 2 and 3.

3.3.1. Trade measures

As presented in Table 2, among the 494 studies, the most frequently used measure of trade is the value of total trade (used by 368 studies), followed by the value of imports (76 studies) and the value of exports (50 studies). Panel A of Table 3 presents the percentage of primary studies reported positive, insignificant, or negative effect of trade on inequality (at 10% significance level) separately for studies that employed the total trade measure (Column 2) and other trade measures (Column 3). It shows that studies that employed the total trade measure tend to find effects that are negative, while studies that employed other trade measures tend to find effects that are positive or insignificant. As presented in the last column of Table 3, 18.3% of studies that did not measure trade by its total value had found the effects of trade to be negative. By contrast, the negative effect of trade was observed by 40% of studies that measured trade by its total value. Therefore, compared with studies that did not use the total value of trade, for studies that did, 21.7% more of them found the effects of trade to be negative (i.e. 40%–18.3%), 10.5% less of them found the effects of be insignificant effects, and 11.2% less of them found the effects to be positive.

Table 3
The explanatory power of moderating variables.

Significance Level	(1) Yes (%)	(2) No (%)	Difference (%) (1)–(2)
A. Measure trade by total trade value			
Negative	40.0	18.3	**21.7**
Insignificant	33.9	44.4	−10.5
Positive	26.1	37.3	−11.2
B. Measure inequality by the Gini coefficient			
Negative	36.6	25.5	11.1
Insignificant	39.1	26.5	12.6
Positive	24.2	48.0	**−23.8**
C. Based on high-income countries			
Negative	52.5	35.9	**16.6**
Insignificant	28.7	40.4	−11.7
Positive	18.8	23.7	−4.9
D. Addressed endogeneity			
Negative	31.4	19.4	12.0
Insignificant	49.0	20.9	**28.1**
Positive	19.6	59.7	**−40.1**

Note: This table reports the percentage of primary studies reported positive, insignificant, or negative effect of trade on inequality separately for studies that with a give characteristic (Column 2) or not (Column 3). For example, Panel A presents the percentage of primary studies reported positive, insignificant, or negative effect of trade on inequality separately for studies that employed the total trade measure (Column 2) and other trade measures (Column 3). We define the estimated effect as significant (at the 10% level) and positive (negative) if the t-statistic is larger (smaller) than 1.68, and statistically insignificant if the absolute value of the t-statistic is smaller than 1.68.

3.3.2. Inequality measures

The most widely used measure of inequality is the Gini coefficient (used by 396 studies), and 21 studies measured inequality by income share of the top decile or quintile(s), 29 studies by income share ratios of the top to bottom decile or quintile(s), and 48 studies by other inequality measures.[10] As presented in Panel B of Table 3, compared with other studies, those that measured inequality by the Gini coefficient reported 11.1% more negative effects, 12.6% more insignificant effects, and 23.8% less positive effects. Note that the main analysis of this article classifies inequality based on the measuring methods, instead of based on the type of inequality. This is mainly because, as detailed in subsection 3.1, the meta-analysis includes only primary studies focused on national income inequality and thus excludes studies on other types of (subgroup) inequality, such as the inequality between skilled and unskilled labor and the inequality between genders. However, in Appendix B, we also examined the difference between wage and (net) income inequality, as long as they are measured at the national level.

3.3.3. Economic development levels

As detailed before, we classify primary studies into four groups based on the development level of their sample countries. We find 170 studies in the high-income group, 125 in the middle-income group, 127 in the low-income group, and 72 used mixed samples. Panel C of Table 3 compares the findings between studies based on high-income countries and those based on middle- or low-income countries. We find that studies based on high-income countries reported 16.6% more negative effects, 11.7% less insignificant effects, and 4.9% less positive effects.

3.3.4. Control for endogeneity

We use the dummy "*estimation methods*" to indicate if a primary study has attempted to address endogeneity by using certain estimation methods such as 2SLS regression, IV regression, Heckman two-stage, or GMM.[11] In addition, we use the dummy "*panel fixed effect*" to indicate if a primary study has controlled for confounding factors by using fixed effects in a panel model setting. We find 126 primary studies have adopted estimation methods to address endogeneity and 403 have included fixed effects in the estimation. Panel D of Table 3 shows that studies that did not address endogeneity by these methods found 40.1% more positive effects, while those adopted both of these two methods found 12.0% more negative effects and 28.1% more insignificant effects.

[10] All these measures of inequality have a common feature: a larger value represents more inequality. When collecting primary studies, we have also identified 22 studies measuring inequality by the *income share of the bottom decile or quintile*. We exclude these estimates from our main analysis because they are not comparable to other measures of inequality: a larger value of the income share of the bottom decile or quintile means less inequality. But we include them in a robustness check in Column (1) of Table 6.

[11] The GMM estimation refers to GMM estimations where external IVs and/or internal IVs (lagged endogenous variables) are used.

3.3.5. Other moderating variables

This category includes 5 dummy variables that indicate whether each of the following 5 commonly used control variables is included in the primary study: GDP (measured in growth rate, per capita, or natural log), education levels, government's effort in reducing inequality, demographic factors (e.g., population size and dependency ratio), and other measures of openness of a country (e.g., FDI and terms of trade). This category also includes the dummy "*published*" to indicate if the primary study was published (in a journal or book) and the dummy "*publication year*" to indicate if the study was published after 2010.[12]

4. Models of meta-regression analysis (MRA)

Meta-regression analysis (MRA) is a literature review with the target of integrating and explaining the literature on specific important parameters (Stanley, 2001). In the MRA, we regress the *PCC* (i.e, the standardized effect size) collected from each primary study against factors that could explain the inconsistency among the primary studies. The coefficients on these factors reflect their contributions to the disagreement among the primary studies.

The MRA model is

$$r_{ij} = \beta_1 + \sum \beta_k Z_{ki} + \beta_o se_{ij} + \varepsilon_{ij} \quad (3)$$

in which r_{ij} is the *PCC* of the study *i* from the paper *j* (there are 494 studies from 69 papers), se_{ij} denotes the standard error of r_{ij}; Z_{ki} denotes a vector of *k* characteristics of primary studies (detailed in Table 2); β's denote coefficients; and ε_{ij} denotes standard error.

Publication bias (or publication selection and reporting bias) is a common concern in empirical studies as results conforming to prevailing views are more likely to be published (Rosenthal and Rosnow, 1991; Card and Krueger, 1995). If $\sum \beta_k Z_{ki}$ is omitted from Eq. (3), the resulting model will become the basis for the funnel-asymmetry and precision-effect (FAT-PET) test for detecting publication bias and verifying if a genuine effect exists beyond bias (Stanley, 2008):

$$r_{ij} = \beta_1 + \beta_o se_{ij} + \varepsilon_{ij}. \quad (4)$$

In the FAT-PET test, which has been frequently used in the literature (e.g., Stanley, 2005), the FAT (funnel-asymmetry test) tests for $\beta_o = 0$ while the PET (precision-effect test) tests for $\beta_1 = 0$. It indicates publication bias if $|\beta_o| \geq 1$ and a statistically significant β_1 suggests the existence of a true effect after controlling for publication bias (Stanley et al., 2013).

The MRA models, represented by Eqs. (3) and (4), is estimated by using weighted least squares (WLS) regression, which is preferred better than OLS regression because the latter is subjected to obvious heteroskedasticity arising from the heterogeneity of the primary studies (Stanley et al., 2018). We obtain a WLS version of the MRA models by weighing the squared errors using the inverse of each estimate's variance $\frac{1}{se_{ij}^2}$.

As discussed above, almost all collected papers report several estimates and there is the concern that estimates within the same paper may be correlated (Nelson and Kennedy, 2009). To verify whether independence of studies matters for our estimation results, we provide robustness checks that corrected for the bias from within-study dependence by reporting standard errors robust to clustering and by estimation hierarchical linear model (Doucouliagos and Laroche, 2009).

[12] These last two dummies provide tentative evidence for publication bias (Card and Krueger, 1995) in the sense that if publication bias is absent, the publication status (published or not and publication year) of a primary article should be uncorrelated with the effect of trade on inequality.

5. Results

We first present the baseline estimates of Eq. (3) and the PET-FAT test based on Eq. (4). We then examine the robustness of the baseline estimates to various estimation methods, different measures of trade and inequality, and subsamples. Finally, we predict the conditional effect of trade on inequality.

5.1. Baseline results

Column (1) of Table 4 presents the PET-FAT test. The test suggests a significant publication bias as indicated by the large coefficient (i.e., 1.92) on the standard error of the *PCC*; recall that publication bias is deemed as strong when the coefficient is larger than 1. The intercept of the PET-FAT regression suggests that after controlling for the publication bias, the primary studies together suggest a negative and statistically significant effect (PCC = −0.17) of trade on income inequality. However, since the PET-FAT regression does not control for other study characteristics, the overall effect indicated by the intercept has covered up the

Table 4
Sources of inconsistency.

Dependent variable: *PCC*	(1) FAT-PET test	(2) Full model	(3) "Specific" model
Measures of trade (base: import)			
Total trade (1 = Yes)		−0.18***	−0.15***
		(0.04)	(0.03)
Export (1 = Yes)		−0.06	
		(0.05)	
Measures of inequality (base: other measures)			
Gini (1 = Yes)		−0.14***	−0.09***
		(0.05)	(0.03)
Income Share Top (1 = Yes)		−0.08	
		(0.08)	
Income Share Ratio (1 = Yes)		−0.08	
		(0.07)	
Development levels (base: middle income)			
High income (1 = Yes)		−0.09**	−0.08**
		(0.04)	(0.04)
Low income (1 = Yes)		0.08**	0.10***
		(0.04)	(0.04)
Mixed (1 = Yes)		−0.08*	−0.07*
		(0.04)	(0.04)
Controlling for endogeneity			
Estimation methods (1 = Yes)		−0.09***	−0.09***
		(0.03)	(0.03)
Panel fixed effect (1 = Yes)		−0.12**	−0.16***
		(0.05)	(0.04)
Other moderating variables			
GDP control (1 = Yes)		−0.06**	−0.06**
		(0.03)	(0.03)
Education control (1 = Yes)		−0.09***	−0.07**
		(0.03)	(0.03)
Government effort control (1 = Yes)		0.03	
		(0.03)	
Demographic control (1 = Yes)		−0.13***	−0.15***
		(0.03)	(0.03)
Openness control (1 = Yes)		0.03	
		(0.03)	
If published (1 = Yes)		−0.02	
		(0.04)	
Publication year (1 = after 2010)		0.10***	0.10***
		(0.03)	(0.03)
Standard error of the PCC	1.92***	0.57	
	(0.33)	(0.41)	
Constant	−0.17***	0.36***	0.35***
	(0.03)	(0.10)	(0.05)
Observations	494	494	494
Adjusted R^2	7.39%	27.82%	27.49%

Note: All models are estimated by weighted least square. The standard errors reported in parentheses are heteroskedastic-consistent. Significance levels are denoted by ***p < 0.01, **p < 0.05, *p < 0.1.

heterogeneity among studies due to factors beyond the publication bias.

Column (2) of Table 4 present the full MRA model that includes all the moderating variables listed in Table 2. Consistent with our hypothesis and summary statistics, we find statistically significant effects of moderating variables from each of the five categories on the inconsistency among primary studies (measured by differences in the *PCC*). As shown in the first category, compared with studies that measured trade by imports (which is chosen as the base for the three collinear dummy measures of trade), studies that measured trade by the total trade value estimated a 0.18 smaller *PCC* on average, and this difference is statistically significant at the 1% level. This finding could reflect that the effect of total trade on income inequality is smaller than the effect of imports; however, we cannot exclude the possibility that studies using the total trade measure have some unobservable characteristics that are correlated with lower inequality. Similarly, as presented in the second category, studies that measured inequality by the Gini coefficient estimated a 0.14 smaller *PCC* than studies that used "other measures" of inequality (the base). Recall that most of primary studies had measured trade by its total trade value and measured inequality by the Gini coefficient, we can conclude that studies using the mainstream measures of trade and inequality tend to find negative effects (i.e., inequality-reducing effects) of trade on inequality than studies using other minor measures.

More interestingly, as presented in the third category, compared with studies based on middle-income countries (the base), those based on high-income countries tend to find that trade has significantly *reduced* inequality while those based on low-income countries tend to find that trade has significantly *increased* inequality. These findings contradict the HO model, which predicts that trade *reduces* inequality in developing countries but *increases* inequality in developed countries. They are also inconsistent with the predictions of models that predict an inequality-increasing effect of trade for countries in all income levels (e.g., Feenstra and Hanson, 1996; Dinopoulos and Paul, (1999); Anderson, 2011). But these findings do support the modernization theory (Kuznets, 1955) and models with learning and skill upgrading (e.g., Aghion and Howitt, 1998: 262) in predicting that trade will first increase and then reduce inequality as a country develops.

The fourth category shows the importance of addressing the endogeneity bias in primary studies. We find that, other things been equal, studies that adopted estimation methods (e.g., IV, 2SLS, GMM) to address endogeneity reported a 0.09 smaller *PCC* than those that did not. Similarly, studies that addressed endogeneity bias by including fixed effects in a panel model tend to find a 0.12 smaller *PCC* than those that did not. In other words, these two estimates suggest that if endogeneity has been addressed by these two methods, a study is more likely to find that trade reduces inequality.

As presented in the last category, the estimated coefficients of the control variables confirm our finding that once endogeneity has been addressed, studies would tend to observe negative effects of trade on inequality. Specifically, we find that studies controlling for GDP, education levels, and demographic factors are more likely to find an inequality-reducing effect of trade than studies that have omitted these potential confounding factors from the model.

In the last category, the coefficient of "publication year" suggests the existence of publication bias, consistent with the FAT-PET test. We find that studies published after 2010 are more likely to reported a positive effect of trade on inequality, potentially because the increasing trends of trade and inequality make studies that support these trends more publishable.[13] Finally, the intercept in this full model is 0.36 and statistically significant, which means that when *setting all the moderating variables to zero*, the estimated effect of trade on inequality will be 0.36 (measured by *PCC*).[14] However, as it is unrealistic to set all other modelling variables to zero, the value of the intercept on itself is meaningless; at the end of this section, the intercept will be combined with the coefficients of moderating variables to predict the overall effect.

Note that some of the moderating variables are not relevant. For this reason, we now try to obtain the "best" meta-regression model by removing variables that are obviously not relevant. In Column (3) of Table 4, we follow the method of Stanley and Doucouliagos (2012) to adopt a general-to-specific modelling strategy, which removes the variable that with the largest *p*-value until no p-values are larger than 0.1. The reason for employing this approach is that it removes moderating variables that have only weak influences on the trade-inequality estimates. After paring down the meta-regression, we show that the estimated coefficients for the remaining variables in the "specific" model reported in Column (3) are comparable to those reported in the full model reported in Column (2). In the following section, we will employ the "specific" model as the baseline model and provides various robustness checks for it.

5.2. Robustness checks

We conduct robustness checks to examine the sensitivity of our baseline results to alternative estimation methods, different measures of trade and inequality, and subsamples. All robustness checks use the model setting of the specific model (Column 3 of Table 4), except for the one specified in in each check. Five additional robustness checks are presented in Appendix B.

5.2.1. Sensitivity to alternative estimation methods

A potential concern of the above finding is that the multiple effect sizes reported by the same primary study may be correlated, which could bias the standard error of our meta-regression (Nelson and Kennedy, 2009). To addresses this concern, previous meta-regressions usually cluster observations within an article or adopt hierarchical linear models. The clustering implicitly corrects for the statistical dependence of the standard errors, while the hierarchical linear models explicitly model within-study dependence (Stephen and Anthony, 2002). Column (1) of Table 5 reports the standard error clustered at the article level. Column (2) adopts the method of Doucouliagos and Laroche (2009) to estimate a hierarchical linear model. Specifically, the estimation augments Eq. (3) with a random effects term to nest the estimates within articles.[15] It shows that the estimates are very similar to the baseline estimates, indicating that our baseline results are note sensitive to the dependence of the primary studies.

In Table 5, we examine if the baseline results are sensitive to alternative estimation approaches. Specifically, we estimate Eq. (3) using a OLS model (Column (3)) or robust regression following Verardi and Croux (2009) (Column (4)). The OLS regression excludes all weightings, while the robust regression approach drops the most influential data points and down weights observations with large residuals in order to addresses the outliers and unobserved heterogeneity. The estimated effect size, direction, and significance level of the moderating variables are comparable to those from the baseline estimation.

5.2.2. Sensitivity to measures of trade and inequality

Table 6 checks the sensitivity of our findings to the measures of trade and inequality. Columns (1) and (2) include additional primary studies that have been excluded from our main MRAs because of comparability.

[13] The coefficient of "standard error of the PCC", which indicates the existence of publication bias in the PET-FAT tests, becomes statistically insignificant in the full model. This result is not surprising as we now include 17 moderating variables that have the potential to account for the effect of publication bias, especially the "publication year".

[14] The constant term in a regression represents the mean of the dependent variable when setting all of the independent variables to zero (Nelson and Kennedy, 2009, p. 249–250).

[15] The hierarchical linear model shows that the between-study variance is moderate (0.062) but statistically significant.

Table 5
Robust to estimation methods.

Dependent variable: *PCC*	(1) Adjust for clustered standard error	(2) Hierarchical linear model	(3) OLS estimates	(4) Robust regression
Measures of trade				
Total trade (1 = Yes)	−0.15***	−0.15***	−0.14***	−0.12***
	(0.04)	(0.03)	(0.03)	(0.03)
Measures of inequality				
Gini (1 = Yes)	−0.09**	−0.08**	−0.08**	−0.08***
	(0.04)	(0.03)	(0.03)	(0.03)
Development levels				
High income (1 = Yes)	−0.08***	−0.07**	−0.07*	−0.08**
	(0.03)	(0.04)	(0.04)	(0.03)
Low income (1 = Yes)	0.10***	0.10***	0.10***	0.08**
	(0.03)	(0.04)	(0.04)	(0.03)
Mixed (1 = Yes)	−0.07	−0.07	−0.07	−0.11***
	(0.06)	(0.04)	(0.04)	(0.04)
Controlling for endogeneity				
Estimation methods (1 = Yes)	−0.09**	−0.09***	−0.09**	−0.11***
	(0.04)	(0.03)	(0.03)	(0.03)
Panel fixed effect (1 = Yes)	−0.16***	−0.15***	−0.15***	−0.17***
	(0.06)	(0.04)	(0.05)	(0.03)
Other moderating variables				
GDP control (1 = Yes)	−0.06**	−0.05*	−0.05*	−0.05**
	(0.03)	(0.03)	(0.03)	(0.03)
Education control (1 = Yes)	−0.07***	−0.08**	−0.08***	−0.09***
	(0.02)	(0.03)	(0.02)	(0.03)
Demographic control (1 = Yes)	−0.15***	−0.14***	−0.14***	−0.16***
	(0.04)	(0.03)	(0.03)	(0.03)
Publication year (1 = after 2010)	0.10***	0.10***	0.10***	0.06**
	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.35***	0.27***	0.33***	0.40***
	(0.09)	(0.05)	(0.05)	(0.04)
Observations	494	494	494	494
Adjusted R^2	27.49%	26.86%	24.51%	19.78%

Note: Column (1) clusters the standard error of the regression by articles, Column (2) provides the hierarchical linear model estimates, Column (3) provides the OLS estimates, and Column (4) provides robust estimates. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

Table 6
Robust to measures of trade and inequality.

Dependent variable: *PCC*	(1) Adding Income Share Bottom studies	(2) Adding openness studies	(3) Include only total trade studies	(4) Include only Gini studies
Measures of trade				
Total trade (1 = Yes)	−0.16***	−0.14***	−0.15***	
	(0.03)	(0.03)	(0.03)	
Openness (1 = Yes)		−0.13***		
		(0.04)		
Measures of inequality				
Gini (1 = Yes)	−0.08**	−0.08***		−0.10**
	(0.03)	(0.03)		(0.04)
Lower quantile groups (1 = Yes)	−0.05			
	(0.07)			
Development levels				
High income (1 = Yes)	−0.10***	−0.06*	−0.12***	−0.13***
	(0.03)	(0.03)	(0.04)	(0.05)
Low income (1 = Yes)	0.11***	0.06*	0.07*	0.10**
	(0.03)	(0.03)	(0.04)	(0.04)
Mixed (1 = Yes)	−0.07	−0.04	−0.09*	−0.10*
	(0.04)	(0.04)	(0.05)	(0.05)
Controlling for endogeneity				
Estimation methods (1 = Yes)	−0.11***	−0.09***	−0.12***	−0.08**
	(0.03)	(0.03)	(0.03)	(0.04)
Panel fixed effect (1 = Yes)	−0.13***	−0.15***	−0.09**	−0.12**
	(0.04)	(0.03)	(0.04)	(0.05)
Other moderating variables				
GDP control (1 = Yes)	−0.08***	−0.04*	−0.05	−0.07*
	(0.03)	(0.03)	(0.03)	(0.04)
Education control (1 = Yes)	−0.09***	−0.06**	−0.10***	−0.05
	(0.03)	(0.03)	(0.03)	(0.04)
Demographic control (1 = Yes)	−0.15***	−0.17***	−0.16***	−0.21***
	(0.03)	(0.03)	(0.04)	(0.05)
Publication year (1 = after 2010)	0.11***	0.09***	0.09***	0.13***
	(0.03)	(0.03)	(0.03)	(0.04)
Constant	0.35***	0.33***	0.26***	0.19***
	(0.05)	(0.04)	(0.05)	(0.06)
Observations	516	575	396	368
Adjusted R^2	24.75%	25.41%	27.49%	22.21%

Note: All models are estimated by weighted least square. The base chosen for the collinear dummy measures in each group of independent variables are those omitted from the meta regression but listed in Table 2; for example, for measures of trade in column 2, the base are the trade measures of export value and import value. Standard errors reported in parentheses are heteroskedastic-consistent. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

Column (1) includes 22 additional primary studies that measured inequality by the *income share of the bottom decile or quintile(s)*. These studies had earlier been excluded from our main MRAs because, different from all the other measures, a higher income share at the bottom corresponds to lower inequality. To include them, we reverse the signs of their *PCC* so that a higher PCC indicates more inequality, which is consistent with the rest of the primary studies. We find that including these 22 additional studies does not significantly change the coefficient of other moderating variables. Additionally, the effect of the new moderating variable "*lower quantile groups*" is statistically insignificant.

Column (2) includes 81 additional primary studies that measured trade by measures of trade openness (liberalization), such as the trade openness index, trade liberalization index, and trade reform dummy. We have earlier excluded these studies from our main MRAs as the openness indices do not always capture trade. For example, the openness index is not necessarily proportional to real trade flows and therefore not proportional to the true effect of trade on inequality. As stressed by Goldberg and Pavcnik (2004), trade protection has increasingly taken the form of non-tariff barriers that are inherently hard to measure. Despite including studies that focused on trade openness, Column (2) shows that the effect of the moderating variable "openness" has virtually no difference from that of total trade (i.e., −0.13 versus −0.14). Additionally, whether we measure trade by trade flows or openness, our results still show that trade significantly reduces (increases) inequality in high-income (low-income) countries relative to middle-income countries.

Columns (3) and (4) focused only on primary studies that measured trade by the value of total trade and inequality by the Gini coefficient, respectively. Most of primary studies adopted these two measures, so it would be interesting to check if the effects of other moderating variables are still important when we focus on these more homogeneous subsets of primary studies. It turns out that the effects of most moderating variables are similar to that found in the baseline model; the major differences are that the effect of "*education control*" is no longer statistically significant in Column (3) and the effect of "*GDP control*" is no longer significantly significant in Column (4).

5.2.3. Sensitivity to subsamples

Table 7 checks that if the identified effects of moderating variables are mainly driven by primary studies from a specific income group. As presented in Columns (1) to (4), we do this by focusing on the subsamples that exclude studies based on high-income countries, middle-income countries, low-income countries, and mixed-income countries, respectively. We still find that moderating variables in each of the four categories have significant effects on the estimated trade-inequality correlation, and the effect directions are the same as that found in the baseline model. An exception is that, in Column (1), when excluding studies from the high-income countries, we find no significant effect of

Table 7
Robust to subsamples.

Dependent variable: *PCC*	(1) Exclude high-income countries	(2) Exclude middle-income countries	(3) Exclude low-income countries	(4) Exclude mixed countries
Measures of trade				
Total trade (1 = Yes)	−0.07*	−0.15***	−0.17***	−0.13***
	(0.04)	(0.04)	(0.03)	(0.03)
Measures of inequality				
Gini (1 = Yes)	−0.08**	−0.12***	−0.07*	−0.07**
	(0.04)	(0.04)	(0.04)	(0.03)
Development levels				
High income (1 = Yes)		0.00	−0.10***	−0.08**
		(0.05)	(0.04)	(0.04)
Low income (1 = Yes)	0.09***	0.18***		0.11***
	(0.03)	(0.04)		(0.04)
Mixed (1 = Yes)	−0.06		−0.09**	
	(0.04)		(0.04)	
Controlling for endogeneity				
Estimation methods (1 = Yes)	−0.03	−0.10**	−0.15***	−0.06*
	(0.03)	(0.04)	(0.04)	(0.03)
Panel fixed effect (1 = Yes)	−0.10**	−0.20***	−0.14***	−0.19***
	(0.04)	(0.04)	(0.04)	(0.04)
Other moderating variables				
GDP control (1 = Yes)	−0.01	−0.07**	−0.06*	−0.10***
	(0.04)	(0.03)	(0.03)	(0.03)
Education control (1 = Yes)	0.01	−0.09***	−0.08**	−0.09***
	(0.04)	(0.03)	(0.04)	(0.03)
Demographic control (1 = Yes)	−0.22***	−0.13***	−0.17***	−0.12***
	(0.04)	(0.04)	(0.04)	(0.04)
Publication year (1 = after 2010)	0.07**	0.08**	0.10***	0.11***
	(0.03)	(0.04)	(0.04)	(0.03)
Constant	0.19***	0.36***	0.37***	0.37***
	(0.06)	(0.06)	(0.05)	(0.05)
Observations	324	369	367	422
Adjusted R^2	33.04%	30.94%	28.20%	27.49%

Note: All models are estimated by weighted least square. The heteroskedastic-consistent standard errors are reported in parentheses. Significance levels are $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

"*estimation methods*", "*GDP control*", and "*education control*", indicating that these three moderating variables have no significant explanatory power on the inconsistency observed among primary studies outside the high-income countries.[16]

5.3. Synthesizing the effect of trade on income inequality

We calculate the conditional effect of trade on inequality based on the estimated "specific" model reported in Column (3) of Table 4. We calculate the effects with respect to each of the three income groups: high income, low income, and middle income. As presented in Row (1) of Table 8, for each income group, we predict the effect of trade on inequality under the "best practice" of estimation: dealing with endogeneity by estimation methods and including the important controls of GDP, education levels, and demographic factors.[17] Specifically, when predicting the effect of trade on inequality in each income group, we combine the *intercept* of the regression with the coefficients of the corresponding income-level measure, the two moderating variables under the category of "controlling for endogeneity", and the three control variables (i.e., "*GDP control*", "*education control*", and "*demographic control*").[18] We find that under this "best practice", trade significantly reduces income inequality in high- and middle-income countries, but has no statistically significant effect on low-income countries.

Table 8
MRA predictions: The conditional effect of trade on inequality.

Model setting	(1) High income	(2) Middle income	(3) Low income
(1) Deal with Endogeneity = Yes	−0.25***	−0.18**	−0.08
Include control variables = Yes	(0.07)	(0.06)	(0.06)
(2) Deal with Endogeneity = Yes	0.03	0.11**	0.21***
Include control variables = No	(0.05)	(0.05)	(0.05)
(3) Deal with Endogeneity = No	−0.01	0.07	0.17**
Include control variables = Yes	(0.07)	(0.06)	(0.06)

Note: The prediction is based on the estimates of the specific model. Significance levels are $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

To show the extent to which endogeneity bias may lead to misleading results, Rows (2) and (3) of Table 8 predict the effects when the three control variables are not included and when the estimation methods employed by the primary studies do not address the issue of endogeneity, respectively. As presented in Row (2), if a study does not include the three important control variables, endogeneity bias (caused by omitted variables) could mislead us to conclude that trade is statistically insignificant for inequality in high-income countries, but significantly increases inequality in middle- and low-income countries. Similarly, as presented in Row (3), if a study does not employ estimation methods (e.g., IV approaches and panel fixed effect) that deal with endogeneity, it may observe that trade is statistically insignificant for inequality in high- and middle-income countries, but significantly increases inequality in low-income countries. Therefore, when endogeneity bias is not addressed, the estimated effects of trade on inequality are potentially misleading.

The findings that trade reduces inequality in middle- and high-income countries whereas it has a statistically insignificant effect on low-income countries can be explained by the theoretical models discussed in Section 2. Specifically, modernization theory (Kuznets, 1955) and the economic models accounting for learning and skill upgrading (e.g., Aghion and Howitt, 1998: 262) predict that inequality tends to decline with income growth. As trade generally improves economic growth (Singh, 2010; Van den Berg and Lewer, 2015), this suggests that trade, by enhancing economic growth, may reduce inequality, and therefore, an underlying tendency for trade to reduce inequality for higher income countries. If this inequality-narrowing effect of trade (through improving economic growth) overcomes its inequality-exacerbating effect predicted by the economic theories discussed in Section 2 (e.g., Harrison et al., 2011; Helpman et al., 2010), we should observe a net negative effect of trade on inequality among high income countries, consistent with the results in Columns (1) and (2) of Table 8. By contrast, for less developed countries

[16] The coefficient of "*high income*" in Column (2) of Table 7 is statistically insignificant and close to zero because when the middle-income countries are excluded, we choose to use the "*mixed*" countries as the base of the regression (instead of the "*middle-income*" countries in the baseline regression). Because the effects in the "*mixed*" and "*high income*" countries are similar (see Column (3) of Table 4), their relative effect should be close to zero.

[17] Dealing with endogeneity by the estimation methods and by including the three important controls are complementary when identifying an unbiased estimate. For example, although a model that includes country-fixed effects can address the endogeneity due to *time-unvarying* factors, it cannot address the endogeneity caused by omitting these three *time-varying* control variables. Similarly, the identification of an IV approach is likely conditional on controlling for these three important variables.

[18] The prediction does not use the coefficient of trade or inequality measure because we have no clear evidence that one of these measures is better than others; we have also tried to focus on the most frequently used trade and inequality measures (e.g., total trade and the Gini coefficient) and found much more negative effects of trade on inequality in all development levels. The prediction also excludes the coefficient of "publication year"; by setting the dummy of "publication year" to zero we eliminate the publication bias.

that benefited less from international trade, the inequality-exacerbating effect of trade may offset its inequality-narrowing effect, thus causing the net effect of trade to be close to zero as observed in Column (3) of Table 8.

6. Concluding remarks

One of the few well-accepted insights of trade theory is that changes in a country's exposure to international trade may affect the within-country income distribution. The concurrent increasing trends in trade and inequality for many countries have led to the belief that trade may drive a wedge between the rich and poor. However, even after decades of empirical studies, the evidence on how trade affects inequality remains highly mixed. The lack of consensus in the literature motivates us to employ a meta-analysis to understand why the literature disagrees on the effects of trade on inequality and to uncover the genuine effect of trade on inequality.

Surprisingly, by combining 494 estimates from comparable primary studies, we find that the overall effect of trade on income inequality is negative or close to zero. Our meta-analysis also reveals that the substantial disagreements among the primary studies are partly due to the differences in the development level of their sample countries and their efforts in addressing the bias of endogeneity. By focusing on studies where attempts to address the issue of endogeneity has been made, we find strong evidence that trade leads to *reduced* income inequality in middle- and high-income countries but is statistically insignificant for inequality in low-income countries. Therefore, from our meta-analysis, there is no evidence that trade leads to increasing within-country income inequality.

Finally, we would like to stress two limitations of our meta-analysis. Firstly, in order to ensure that our primary studies are comparable, we employ the sample of studies that only focus on the effect of trade but not other measures of globalization. As such, it is important to emphasize that our work does not speak directly to the literature, theoretical or empirical, that focuses on the relationship between globalization and inequality. Secondly, our meta-analysis is based on the national income inequality measures that are standard in the literature. Therefore, we should be cautious about interpreting our results in the context of inequality for within-country subgroups, such as inequality between skilled and unskilled labours.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. List of Primary Studies

Table A1
An Overview of Primary Articles

Citation	Article code	T-value	PCC	Trade measure	Inequality Measure	Development level	Addressing endogeneity	Panel fixed effect
(Matano and Naticchioni, 2010)	1	1.2	0	Import	Gini	High	No	Yes
(Hesse, 2015)	2	2.4	0.2	Export	Others	High	No	Yes
(Mahesh, 2016)	3	2.8	0.2	Import	Gini	Middle	Yes	Yes
(Daumal, 2013)	4	0.8	0	Total	Gini	Middle	No	No
(Thewissen et al., 2018)	5	−0.2	0	Import	Gini	High	No	Yes
(Rudra, 2004)	6	1.2	0.2	Total	Gini	High	No	Yes
(Reuveny and Li, 2003)	7	−1.8	−0.2	Total	Gini	Low	No	Yes
(Munir et al., 2013)	8	3	0.4	Total	Gini	Low	No	Yes
Meschi and Vivarelli 2009	9	−0.2	0	Import	Gini	Low	No	Yes
(Mamoon and Murshed, 2013)	10	12.6	0.6	Total	Others	Low	Yes	No
(Lin and Fu, 2016)	11	3.6	0.2	Total	Gini	Low	No	Yes
(Lee, 2006)	12	0	0	Total	Gini	High	No	Yes
(Kumo et al., 2018)	13	−1.2	0	Total	Ratio	Middle	Yes	Yes
(Kai and Hamori, 2009)	14	2.2	0.2	Total	Others	Low	No	Yes
(Barusman and Barusman, 2017)	15	5.4	0.6	Import	Gini	High	No	No
Ali et al. 2015	16	1.4	0.2	Total	Gini	Low	No	No
(Asteriou et al., 2014)	17	−1.6	−0.2	Total	Gini	High	Yes	Yes
Jaumotte et al. (2013)	18	−1.4	0	Import	Gini	Mixed	No	Yes
(Elmawazini et al., 2013)	19	5.6	0.4	Total	Gini	High	No	Yes
(Chaudhry and Imran, 2013)	20	1.4	0.2	Total	Gini	Low	No	No
(Bogliaccini, 2013)	21	1.8	0.2	Total	Gini	Middle	No	Yes
(Jalil, 2012)	22	2.4	0.4	Total	Gini	Middle	No	No
(Ha, 2012)	23	2	0.2	Total	Gini	Low	No	Yes
(Adams, 2008)	24	0	0	Total	Gini	Low	No	Yes
(Benar, 2007)	25	12	1	Total	Gini	Low	No	Yes
(Avalos and Savvides, 2006)	26	−2	0	Total	Ratio	Middle	No	Yes
(Calderon and Chong, 2001)	27	−2.4	−0.2	Total	Gini	Mixed	No	Yes
(Chakrabarti, 2000)	28	−2.8	−0.4	Total	Gini	Mixed	Yes	No
Kollmeyer 2018	29	1.4	0.2	Import	Gini	High	No	No
(Zakaria et al., 2016)	30	3	0.2	Total	Gini	Middle	No	Yes
(Mirajul et al., 2016)	31	3	0.2	Total	Gini	Low	Yes	Yes
(Anyanwu et al., 2016)	32	2.8	0.4	Total	Gini	Low	Yes	Yes
(Oldenski, 2014)	33	0	0	Export	Ratio	High	Yes	Yes
(Dizaji and Badri, 2014)	34	−2.8	−0.2	Total	Gini	Low	No	Yes
(Aradhyula et al., 2007)	35	1.2	0	Total	Gini	Low	Yes	Yes

(*continued on next page*)

Table A1 (*continued*)

Citation	Article code	T-value	PCC	Trade measure	Inequality Measure	Development level	Addressing endogeneity	Panel fixed effect
(Barro, 2000)	36	3.4	0.2	Total	Gini	Mixed	No	Yes
(Li et al., 1998)	37	−0.8	0	Import	Gini	Mixed	No	Yes
Le et al. (2020)	38	−9.4	−0.4	Total	Gini	Middle	No	Yes
(Kavya and Santhakumar, 2020)	39	−7.8	−0.2	Total	Gini	Middle	Yes	Yes
(Franco and Elisa, 2013)	40	0.2	0	Total	Gini	Low	Yes	Yes
(Wang et al., 2008)	41	−5.8	−0.8	Total	Gini	Middle	No	No
(Batuo and Asongu, 2015)	42	−0.6	0	Import	Gini	Low	Yes	Yes
(Thewissen et al., 2013)	43	0	0	Import	Gini	High	No	Yes
(Mah, 2013)	44	3.6	0.6	Total	Ratio	Middle	No	No
(Oloufade, 2012)	45	−0.8	0	Total	Others	Low	Yes	Yes
(Shahbaz and Islam, 2011)	46	2	0.4	Total	Gini	Low	No	No
(Çelik and Basdas, 2010)	47	−12	−1	Total	Gini	High	No	Yes
(Borraz and Lopez-Cordova, 2007)	48	−1.4	−0.2	Import	Gini	Middle	Yes	Yes
(Kratou and Goaied, 2016)	49	0.4	0	Total	Highest	Low	No	Yes
(John et al., 2016)	50	2.8	0.4	Total	Gini	Low	Yes	Yes
(Tanja, 2014)	51	2.2	0.2	Total	Ratio	High	No	Yes
(Marta et al., 2012)	52	−0.4	0	Export	Highest	Middle	No	Yes
(Horácio and Carim, 2011)	53	−2.8	−0.2	Total	Gini	High	Yes	Yes
(Hussain et al., 2009)	54	−2	−0.4	Total	Gini	Low	No	No
(Easterly, 2004)	55	−4.2	−0.2	Total	Gini	Mixed	No	Yes
(D'Elia and De Santis, 2018)	56	−1.8	−0.2	Total	Gini	High	No	Yes
Chao et al. 2019	57	−0.4	0	Total	Gini	Mixed	Yes	Yes
(Khusaini et al., 2018)	58	−1.6	−0.2	Total	Gini	Low	No	No
(Bakker, 2018)	59	2.4	0.2	Total	Gini	High	No	Yes
Barua and Chakraborty 2010	60	3.2	0.4	Total	Others	Low	No	No
(Auguste, 2018)	61	2.6	0.2	Total	Gini	High	No	Yes
(Wei and Wu, 2002)	62	0	0	Total	Highest	Middle	Yes	No
(Tai, 2020)	63	−1.8	0	Import	Gini	Middle	No	No
(Gozgor and Ranjan, 2017)	64	0	0	Total	Gini	Low	No	Yes
Muhammad and Bashir Ahmed 2016	65	2.2	0.4	Total	Gini	Middle	No	Yes
(Odedokun and Round, 2004)	66	−0.4	0	Total	Gini	Low	No	Yes
(Minnich, 2003)	67	−3	−0.4	Total	Gini	High	No	Yes
(Prechel, 1985)	68	3.2	0.4	Import	Gini	Mixed	No	No
(Rubinson, 1976)	69	1.8	0.2	Import	Gini	Mixed	No	No

Note: The code is corresponding to that in the dataset, which is available for download from our website. The *t-value* and *PCC* are within article means. For other variables the enters are the most frequently observed one in each article.

Appendix B. Additional Robustness Checks

Column 1 of Table B1 tests the robustness of the meta-regression results to focusing on estimates from the preferred model specifications of primary studies. In primary studies, the author(s) typically start with the simplest models and add more complexities to investigate various factors (inclusion of additional controls, heterogeneity, interactions, etc.). In each, the author(s) typically have one or two preferred model specifications. Here we replicate the baseline MRA presented in column 3 of Table 4 by using only estimates from the "preferred" model specification of each primary study. The preferred model specification is chosen based on the following three criteria: first, for the primary study that used data from the same countries for all regressions, we choose the estimates from the preferred model specification(s) of the study; second, for the primary study that did not explicitly state the preferred model specification, we choose the estimates from the model with the most complete control variables and the best treatment of endogeneity issues; third, for the primary study that used data from different countries in different regressions, we choose one preferred estimate from each regression that were based on different study samples according to the two criteria listed above. These criteria lead to 232 estimates, which is still much larger than the number of papers (69) because most papers have multiple preferred model specifications (when using different trade and inequality measures and when estimating the preferred model for different sample countries). The estimates presented in column 1 of Table B1 is only slightly different from the baseline estimates presented in column 3 of Table 4, suggesting that focusing on the preferred model specifications does not alter the main findings of our meta-analysis.

Column 2 of Table B1 tests the robustness of the MRA results to three additional differences in the model setting of primary studies: adopting static versus dynamic models, using short-versus long-run estimating models, or using interactions as the key explanatory variable. Out meta-analysis did not use dummy independent variables to capture these differences because only a small number of primary studies used dynamic models (5 studies), examined long-run effects (12 studies), or used the interactions of trade measures with other variables as the key explanatory variable (15 studies). A too small number of primary studies means that we cannot meaningfully capture the effect of these differences in a meta-regression analysis. As presented in column 2 of Table B1, excluding primary studies with these features leads to virtually identical estimates as those from the baseline model, suggesting that the MRA results are robust to these omitted features.

Column 3 of Table B1 tests the robustness to alternative classification of the development level of the *sample* of primary studies. Recall that the classification of the main analysis is based on the development level definition of the World Bank in 2020. However, the development level definition changes over time. To investigate whether the changes in the definition of development level have a significant effect on the MRA results, column 3 adopts the development level definition of the World Bank in 2000, which classifies countries into low-, middle-, and high-income groups use the threshold values of $2995 and $9265 (in constant 2000 USD). All other procedures of classifying the sample of primary studies are the same as detailed in Section 3.1. The estimates presented in column 3 are very similar to those from the baseline model.

Table B1
Additional Robustness Checks

Dependent variable: *PCC*	(1) Preferred model specification	(2) Excluding studies with the three features	(3) Alternative definition of the development level	(4) FGLS estimates	(5) Wage or income inequality
Measures of trade					
Total trade (1 = Yes)	−0.13***	−0.15***	−0.15***	−0.15***	−0.15***
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Measures of inequality					
Gini (1 = Yes)	−0.10***	−0.08***	−0.09**	−0.08***	−0.08***
	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)
Wage inequality (1 = Yes)					0.01
					(0.03)
Development levels					
High income (1 = Yes)	−0.09***	−0.08***	−0.10***	−0.07**	−0.07**
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
Low income (1 = Yes)	0.11***	0.10***	0.10***	0.10***	0.10***
	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
Mixed (1 = Yes)	−0.06	−0.07*	−0.08*	−0.07	−0.07
	(0.06)	(0.04)	(0.04)	(0.05)	(0.05)
Controlling for endogeneity					
Estimation methods (1 = Yes)	−0.10***	−0.09***	−0.09***	−0.09**	−0.09**
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Panel fixed effect (1 = Yes)	−0.13***	−0.15***	−0.16***	−0.15***	−0.15***
	(0.05)	(0.03)	(0.04)	(0.05)	(0.05)
Other moderating variables					
GDP control (1 = Yes)	−0.04	−0.06**	−0.06	−0.05*	−0.05*
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
Education control (1 = Yes)	−0.07**	−0.07**	−0.07**	−0.08***	−0.08***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Demographic control (1 = Yes)	−0.12***	−0.15***	−0.15***	−0.14***	−0.14***
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Publication year (1 = after 2010)	0.15***	0.10***	0.10***	0.10***	0.10***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Constant	0.45***	0.38***	0.39***	0.33***	0.33***
	(0.09)	(0.06)	(0.05)	(0.07)	(0.07)
Observations	232	462	494	494	494
Adjusted R^2	32.15%	27.66%	28.10%	24.51%	24.54%

Note: Significance levels are ***p < 0.01, **p < 0.05, *p < 0.1.

Column 4 shows that the estimates are robust to estimating model (3) by Feasible Generalized Least Square (FGLS) instead of the Weighted Least Squares (WLS). The main analysis follows the practice of most meta-regression analyses to estimate model (3) by the WLS that weighs the squared errors by the inverse of each estimates' variance $\frac{1}{se_{ij}^2}$. Instead of using the assumed structure of heteroskedasticity, column 4 presents the estimates from the FGLS estimation that estimates the structure of heteroskedasticity from OLS. However, the resulting estimates for model (3) are very similar to those from the baseline estimation.

Finally, column 5 examines the difference between primary studies that focused on wage inequality and (net) income inequality. There are 85 primary studies used inequality measures (such as Gini coefficient and income share of the top decile) constructed based on wages. We create a dummy that equals 1 for studies focused on wage inequality and 0 for others. The estimated coefficient of this dummy is small and statistically insignificant, suggesting no substantial difference between the findings of primary studies focused on wage inequality and income inequality. This is not surprising because wage is an important component of income.

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