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Structure of income inequality and household leverage: Cross-country causal evidence☆

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ABSTRACT

How does income inequality and its structure affect credit? Based on various strands of the literature, we hypothesize that rising income inequality should lead to higher household credit at the aggregate level, and that a substantial part of this effect should be driven by the impoverishment of the middle class relative to top-income households. These intuitions are empirically confirmed by a study based on a country-level dataset over the period 1970–2017. To identify exogenous variations in inequality, we develop an instrumental variable approach based on two types of country-level instruments: the total number of ratified ILO conventions and factor endowments. Our results show exogenous variations in inequality have a positive impact on household credit: a one-standard-deviation increase in the Gini index generates a 5- to 8- percentage-point expansion in the ratio of household credit to GDP. In addition, the impact is 1.5–1.8 times stronger when the increase in inequality is driven by the income of top earners relative to the middle class rather than by the increase in top earners' incomes at the expense of the lowest percentiles of the distribution. Those results are robust to various sets of instruments, databases, controls, and variable definitions. They also consistently disappear in countries where financial markets are insufficiently developed.

1. Introduction

The 2007-2008 financial crisis led to a renewed interest by the academic literature in the potential causes of financial instability. A consensus emerged on the role of private credit as the main driving factor of banking and financial crises (Schularick and Taylor, 2012; Jordà et al., 2011; Jordá et al., 2013; 2015b; 2015a). Among private credit, household leverage appears to be the main driver of financial vulnerability (Buyukkarabacak and Valev, 2010; Jordà et al., 2016). Mian and Sufi (2010) confirm the importance of household debt in the specific context of the 2007-2008 crisis in the US. They

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conclude that "a focus on household finance may help elucidate the sources of macroeconomic fluctuations" (Mian and Sufi, 2010, p 74). Beyond the sole financial effects, Mian and Sufi (2018) argue the "credit-driven household demand channel" has been an important driver of business cycles and helps explain the Great Recession in the US, as well as many economic cycles in other countries over the past 40 years. Martin and Philippon (2017) also show the role of household leverage in the Great Recession in the eurozone.

We focus here on a specific dynamic that might affect this household credit channel, namely, the role of income inequality. Rajan (2010) and Galbraith (2012) argued that rising income inequality forced low- and middle-income households to increase their indebtedness in order to maintain their consumption level, leading to the subprime crisis. Kumhof et al. (2015) provide a theoretical framework where top earners will save most of their additional income to increase their financial wealth through loans to bottom earners. This increasing leverage of bottom-income households allows them to support their consumption level. Note that different frameworks, such as the relative income and consumption approaches (e.g., Duesenberry, 1949; Frank et al., 2014; Bertrand and Morse, 2016), also deliver conclusions in favor of a positive impact on household leverage of an unequal distribution of income. These approaches suggest changes at the top of the income distribution will affect the consumption of the group just below, and then lead to an expenditure cascade, also influencing the consumption of low- and middle-income households.

Drawing from these various contributions, our paper provides an empirical investigation of the existence and the characteristics of a causal relationship between income inequality and the expansion of household credit. Our goal is to assess how income inequality might affect this household credit channel not only in the US but also in a large panel of developed countries. We identify several sources of exogenous variations in inequality and estimate their effect on household credit to GDP, while controlling for many other time-varying, country-level determinants of credit. In particular, we test different instrumentation strategies to ensure our estimates are stable, regardless of the source of the exogenous shock to inequality. In addition, beyond the establishment of a clear causal link between income inequality and credit dynamics, we investigate how this relationship may quantitatively vary along the income distribution. More specifically, we note that the impact on credit dynamics is stronger when middle incomes decrease relative to top incomes. Here, again, simple intuitions stemming from Kumhof et al. (2015) or relative income frameworks suggest that, for a comparable income loss relative to top incomes, the middle class should contribute more to borrowing at the aggregate level. The possibility that the middle class suffering the same income loss as the bottom earners would contribute more to the dissaving at the aggregate level seems natural, because the middle class has a higher marginal propensity to save. In addition, middle-class households are, by definition, higher in the income distribution, such that they have higher past levels of income and consumption and their reference group is closer to top incomes. In other words, the middle class is expected to have a higher level of consumption to support, requiring higher borrowing than bottom-income households.

The related empirical literature has been rather scarce, to some extent inconclusive, and in any case provides no evidence of a potential heterogenous effect depending on where the inequality shock would hit. Based on quarterly US data from 1980 to 2003, Christen and Morgan (2005) find evidence consistent with a positive correlation between inequality and household indebtedness, in relation to an increase in credit demand from individuals. Based on data on individual mortgage applications, still from the US, Coibion et al. (2016) find low-income households in high-inequality regions borrowed relatively less than similar households in low-inequality regions. However, they find a significant impact of the level of income inequality in debt accumulation in both regions. From a cross-country perspective, Bordo and Meissner (2012) rely on a panel of 14 mainly advanced countries from 1920 to 2008 to study the determinants of total bank credit growth, using macroeconomic variables and the level of inequality measured by the 1% top-income share. They find no significant relationship between inequality and credit growth. However, based on a close sample of 18 OECD countries over the period 1970–2007, Perugini et al. (2016) find very different results, namely, a positive impact of income inequality on credit.[1] El Herradi and Leroy (2020) also find a positive relation between top incomes and credit growth, using a panel of 12 advanced countries between 1948 and 2015. These various, diverging outcomes emphasize the difficulties inherent to the identification of a causal relationship between inequality and finance, due to obvious endogeneity issues. Indeed, both variables are likely to be simultaneously determined by common shocks, and reverse causality from finance to inequality is also very likely.[2]

Therefore, our empirical analysis relies on a country-level yearly dataset for 30 developed countries over the period 1970–2017, based on two building blocks. We mainly rely on income-inequality data coming from the World Income Inequality Database (WIID), but we also check at each key step that our results are not altered when we use other databases, such as the Standardized World Income Inequality Database (SWIID) and the World Inequality Database (WID). Credit data come mainly from the Bank of International Settlements, completed by carefully checked and harmonized data from central banks. Our contribution is twofold. First, we estimate a causal relationship between several measures of income inequality and household leverage. Our second and main contribution is to investigate the structure of inequality. Indeed, the existing literature tends to focus almost exclusively on the role of top incomes, as opposed to a "bottom category", which actually mixes low and middle incomes. Therefore, in addition to the investigation of the existence of a causal relationship between income inequality and the expansion of credit, we also propose investigating the effects of income shocks affecting low- and middle-income groups, respectively.

[1] Based on a very similar sample and estimation strategy, but restricted to the period 1995–2007, Gu et al. (2019) reach the same conclusion.

[2] See Bazillier and Hericourt (2017) and van Treeck (2014) for detailed surveys on that issue.

To estimate proper causal relationships, we need to identify exogenous sources of variations in income inequality. To this end, we propose two sets of country-year, original instruments: the number of ratifications of International Labor Organization (ILO) conventions and factor endowments. Since the second half of the 1970s, the ILO has been autonomously implementing various strategies to promote common labor standards and decent work, characterized by an increasingly dynamic process of (country-level) ratifications over time, which is mostly orthogonal to country-specific developments and other international economic policies. The exclusion restrictions are strengthened by the inclusion of several variables (GDP per capita, housing investment) controlling for standards of living, and therefore the ability to borrow, which may be affected by higher wages, and consequently by ILO conventions. For the variables reflecting factor endowments, we rely on land and capital endowments, as well as proxies for skill intensity. An extensive literature has shown these variables are strongly correlated with inequality (see Bourguignon and Morrisson, 1990; Spilimbergo et al., 1999; Gourdon et al., 2008). The general idea of both sets of instruments is to offer predictors of reforms and structural changes affecting labor market policies and trade openness with significant impacts on income inequality. At the same time, these instrumental variables (IVs) should be orthogonal to any other coincident factors, especially liberalization policy packages that are likely to be correlated with household debt. To the best of our knowledge, our paper is the first in the literature to use such instruments to identify the causal impact of inequality on credit expansion. This is a methodological contribution compared to the existing literature, which tends to directly use country-level trade openness and labor market regulation indicators as instruments. However, these country-level trade openness and labor market regulation indicators are likely to be correlated with credit market dynamics through common deregulation packages.

We find that an exogenous increase in inequality triggers an expansion of household credit. More precisely, a one-standard-deviation increase in the Gini index is associated with a significant 5- to 8- percentage-point (pp) increase in the household-credit-to-GDP ratio. When inequality is measured through the top income share, a one-standard-deviation increase lifts the credit-to-GDP ratio by 5–10 pp. All in all, these values represent between 9% and 18.5% of the average value of the credit-to-GDP ratio in our sample (55.7%), and between 18.7% and 38.6% of its average increase (+26.7 pp). These effects are sizeable. We then show the magnitude of the causal impact of inequality on credit varies substantially with the structure of income inequality. We find this effect is significantly higher when middle incomes are involved: a one-standard-deviation increase in the ratio of top-income share to the middle-income share (implying relative impoverishment of the middle class relative to the top 10%) brings an increase in household credit to GDP equivalent to 1.5–1.8 times the one stemming from a one-standard-deviation increase in the ratio of the top 10% to the bottom incomes.

A substantial part of the paper is devoted to exploring the sensitivity of our results to alternative empirical strategies. Our results hold with various combinations of instruments as well as different databases, definitions of income groups, and control variables. We also check throughout the paper that our results are mostly unaltered by the dynamics arising from the financial crisis and the Great Recession of 2007–2008. Additionally, we replicate our estimates on a sample based on 19 developing/emerging countries, as a falsification test. Indeed, we do not expect our two key results to hold here, because financial market imperfections and subsequent binding credit constraints in developing countries prevent low- and middle-income households from borrowing in response to an income loss (Kumhof et al., 2017). As a consequence, we find that in almost all specifications, inequality indicators display an insignificant impact on household leverage. Note also that in additional results (Tables E.3 to E.6 in the OA), we find that emerging countries displaying a sufficient level of openness to international capital flows do exhibit a positive impact of inequality on household credit.[3] This finding supports the assertion that incoming financial flows, by relaxing credit constraints, allow wider categories of the population to access credit, and consequently to react to variations in inequality.

Our work has important implications regarding financial-crises prevention. Indeed, our results point out that income-inequality dynamics play a significant role in the development of household leverage bubbles. More specifically, the impoverishment of the middle class relative to top-income- households drives a significant part of destabilizing credit booms, as well as the business cycle following the “credit-driven household demand channel” described by Mian and Sufi (2018). Policies designed to reduce income inequality, especially those hitting the middle class, could therefore help alleviate the risk of financial crises.

The next section presents some stylized facts and theoretical underpinnings. Section 3 details the data. Section 4 details our empirical methodology and our identification strategy. Section 5 reports our baseline results and a number of robustness checks. The last section concludes.

2. Stylized facts and theoretical background

This section documents two key stylized facts characterizing income inequality and household leverage for our sample. Each of these two stylized facts is then rationalized through various strands of the literature, all delivering the same theoretical conclusions.

[3] This finding is also consistent with the idea that inequality has an impact on the current account as shown by Behringer and van Treeck (2018).

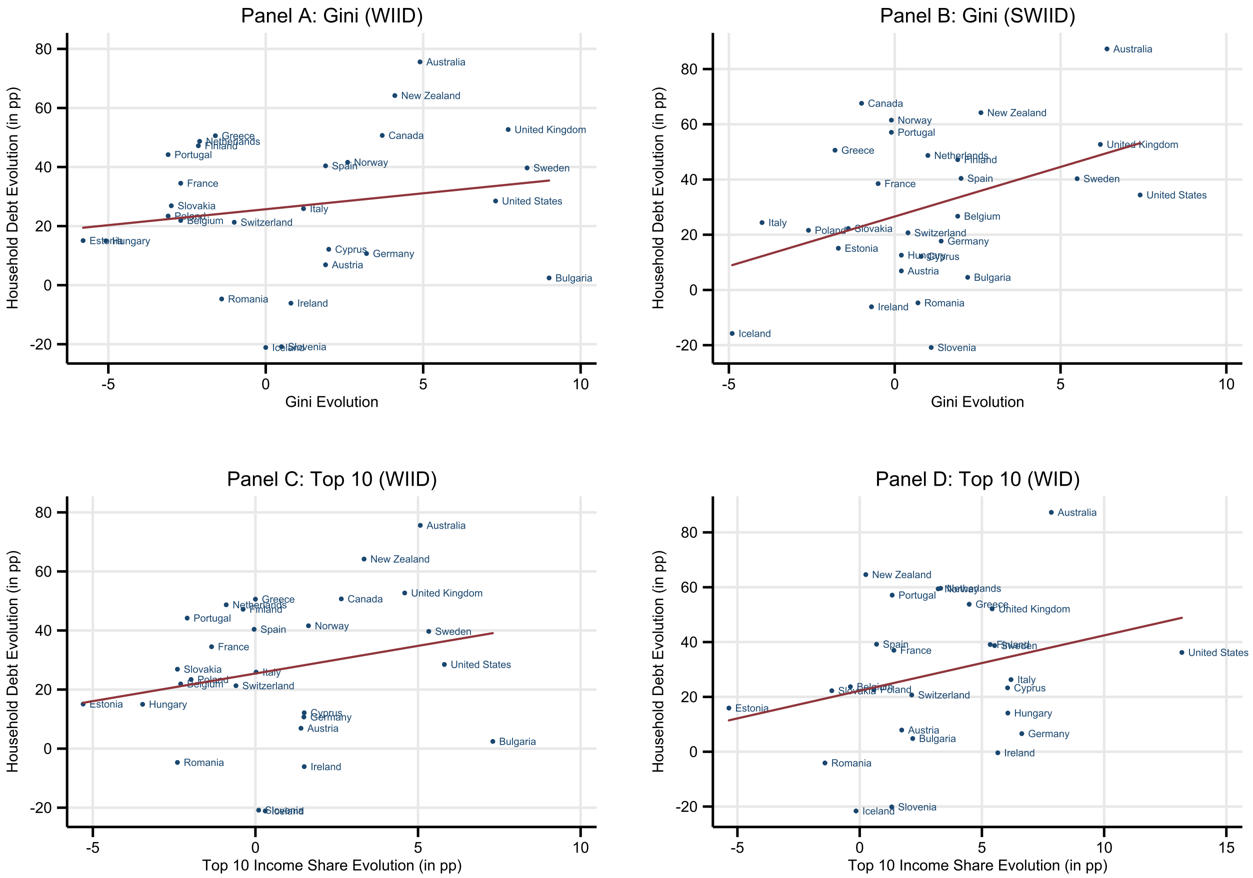

Fig. 1. Evolution of Inequalities and Household Debt Note: Household credit to GDP comes from the Bank of International Settlements, Gini comes from WIID (Panel A) and SWIID (Panel B) databases, and the top 10% income share comes from WIID (Panel C) and WID (Panel D) databases. Each graph plots the variations between the last and first observations available for each country of our sample. See section 3 for a presentation of data and Table A.2 for the time coverage for each country.

2.1. Income inequality and aggregate household debt

Fig. 1 reports plots of variations in income inequality (measured by the Gini coefficient and the top 10% income share) and household debt in each country of our sample of 30 developed economies,[4] between the first and last observations available (see Table A.2 in Appendix A for the time coverage specific to each country). We provide plots based on the main database used for our estimations, namely, WIID, as well as two other datasets used for several robustness checks, SWIID and WID (see section 3 for an extended presentation and discussion of differences between the different databases). Overall, we find a positive correlation between the evolutions of inequality and household debt. Countries that have experienced the largest increase in inequality have also observed the largest increase in their household indebtedness, especially in Anglo-Saxon countries, such as the US or the UK. For these countries, when using inequality data from WIID, the Gini index increased, respectively, by 7.3 pp (between 1979 and 2016) and by 7.7 pp (between 1970 and 2017), while household leverage rose by 29.9 and 52.7 pp, respectively. Interestingly, similar dynamics can be observed in countries with more redistributive welfare systems. In Sweden, for example, the Gini index rose by 8.3 pp between 1981 and 2017, and household debt increased by almost 40 pp. We observe similar patterns when using SWIID data, even though quantitative differences are perceptible.[5] We observe this positive correlation also when looking at the top 10% income share (either from the WIID or the WID database).[6]

[4] As stated in the Introduction, the relationship between inequality and household credit is likely to differ in developing countries for various reasons. We discuss extensively the specific case of developing countries in the falsification test reported in section 5.3.

[5] These differences between SWIID and WIID can be explained by different time coverage and methodologies between the two databases. For example, SWIID data start in 1970 for the US versus 1979 in the WIID database.

[6] The WID database reports a higher increase in the top-income share in average than the WIID. This database measures pre-tax income, which explains most of these differences. See Section 3 for a discussion. Note also that the time coverage differs for some countries (see Table A.2).

How can we make some causal sense of these positive correlations? A burgeoning academic literature has recently found that the increase in inequality observed in the last several decades results from a shift of permanent income between social groups (Piketty and Saez, 2013), consistent with evidence from various countries.[7] In case of permanent shocks, permanent-income theory would predict a proportional adjustment of consumption, without permanent alteration of savings/indebtness.[8] Therefore, departing from the latter framework to explain why households may decide to increase their borrowing in response to permanently stagnating incomes is necessary.

In this regard, Kumhof et al. (2015) provide a formal approach within a DSGE model relying on income inequality between two household groups, top and bottom earners. Top earners display preferences for wealth. The latter can represent different saving motives, such as, following, for example, Carroll (2000), agents deriving direct utility from the social status and power conferred by wealth.[9] Formally, preference for wealth enters top earners' utility function directly, which implies a positive marginal propensity to save out of permanent-income shocks. Put differently, top earners will use most of their additional income to increase their financial wealth through loans to bottom earners, whose marginal propensity to save following a permanent income shock is assumed to be zero. Consequently, the share of top earners in aggregate income has increased, together with higher leverage of bottom-income households allowing the latter to support their consumption level. Calibrated on US data, the framework replicates fairly well the profiles of the income distribution and the debt-to-income ratio for the three decades preceding the Great Recession.

Various types of preferences for bottom earners delivering similar outcomes are also possible to imagine, as long as they depart from the permanent-income hypothesis. For instance, the relative-income hypothesis can also deliver a causal, positive impact of inequality on household debt following a permanent-income shock. Going back to Duesenberry (1949), this approach suggests household consumption is a function of the household's own past consumption levels ("habit-formation") as well as its position in the income distribution: consumption standards are framed according to those of a reference group just above in the income scale. Based on the same idea, Frank et al. (2014) propose a theory of "expenditure cascade" where any change at the top of the income distribution affects the consumption of the group just below, and then leads to an expenditure cascade, also affecting the consumption of low- and middle-income households. Based on the US Consumer Expenditure Survey, Bertrand and Morse (2016) provide a strong case for this kind of "trickle-down consumption" from top to bottom incomes. A similar argument is made by approaches presuming the overall level of satisfaction derived from a given level of consumption depends not only on the actual current consumption level but also on how it compares with some benchmark levels: the individual's own past consumption levels ("habit-formation") or the past consumption of some outside reference group ("keeping up with the Joneses," see e.g., Christen and Morgan, 2005). We now characterize the common testable prediction of these various setups regarding the link between aggregate household credit and income inequality.

Testable Relationship 1: An increase in inequality leads to an expansion in household credit at the aggregate level.

2.2. The key role of the middle class

Based on indebtedness data by deciles of incomes, Panel A of Fig. 2 shows the shares of credit (restricted to mortgages and car loans, due to data availability) originated by the bottom (first three deciles), middle (from the third though eighth decile), and top (ninth decile, or top 10%) incomes for the US between 1999 and 2017. The share of middle incomes in household credit is higher than 60%, consistent with their income share.[10] Fig. A.1 in the online appendix (OA hereafter) reports a similar pattern for the UK, but restricted to mortgage debt due to limitations in the data. Overall, middle incomes account for the bulk of household leverage, although this share has decreased slightly in the aftermath of the 2007–2008 crisis in the UK.

Table 1 reports similar information for Canada and New Zealand, but based on quintiles of incomes and for a few years, again due to data availability. Bottom incomes now represent the first 40% of the income distribution, middle incomes represent the following 40%, and the last 20% are the top incomes. Table 1 delivers a message very similar to the one stemming from Fig. 2: middle incomes represent between 60% and 70% of total household debt.

[7] For the US case, Kopczuk et al. (2010) show that income mobility decreased slightly since the 1950s. Decreasing social mobility is inconsistent with inequalities explained by transitory income shocks. Moffitt and Gottschalk (2002, 2011) also find the variance in transitory income declined or remained constant after 1980, unlike the variance in permanent income. Cappellari and Jenkins (2014) and Jenkins (2015a) report very similar evidence (lack of changes in social mobility over time, decrease in observed income volatility) for the UK. From a cross-country perspective, Andrews and Leigh (2009) confirm this negative link between income inequality and social mobility over a sample of 16 countries. Similar evidence of an increase in between-group inequality, reflecting permanent-income shocks, has also been found in emerging countries (see Ferreira and Litchfield, 2008, on Brazil; Kanbur and Zhuang, 2014, on some Asian countries including China, and India).

[8] The permanent-income theory does allow for an increase in leverage following a decrease in the transitory component of income (Krueger and Perri, 2006, Krueger and Perri, 2011 or Iacoviello, 2008). But this increase in indebtedness can be only transitory, and will be compensated by deleveraging in response to positive shocks bringing income back at least to its long-run level. Therefore, these theories cannot explain the persistent increase in indebtedness following the increase in income inequality over the last several decades.

[9] Kumhof et al. (2015) provide an extensive survey of the literature on preferences for wealth. The latter have been suggested as a way to address the difficulties that models with standard preferences have accounting for the saving behavior of the richest households. For example, Carroll (2000) shows the permanent-income-hypothesis model can match the aggregate saving behavior only by overpredicting the saving behavior of median households and by underpredicting the saving behavior of the richest households.

[10] Bartscher et al. (2020) report similar results for the US using the Survey of Consumer Finances. Defining middle incomes as households between the 50th and 90th percentiles of the income distribution, they find that "middle-class households have always accounted for the largest share of total debt, on

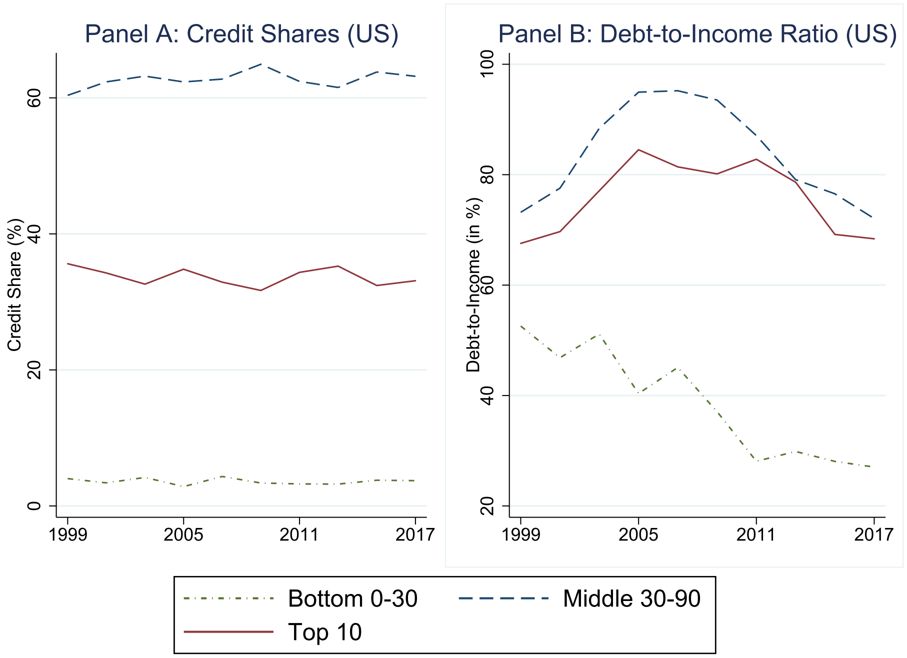

Fig. 2. Credit Shares (in %) and Debt-to-Income Ratios by Income Group, US Note: Authors' calculations (based on sample weights). Credit data includes mortgage and car loans. Data source is the Panel Study of Income Dynamics.

Table 1
Credit Shares (in %) per Income Group, Canada and New Zealand.

Country	Year	Bottom 40%	Middle 40–80%	Top 20%
Canada	1999	12.3	62.0	25.7
Canada	2005	12.6	66.2	21.2
Canada	2012	13.4	60.0	26.6
Canada	2016	10.8	58.7	30.5
New Zealand	2015	15.0	60.0	25.0
New Zealand	2018	18.0	73.0	9.0

Note: Authors' calculations. Data source is Statistics New Zealand and Survey of Financial Security for Canada. Median values for debt.

Panel B of Fig. 2 and Fig. 2 provide a complementary perspective by showing the debt-to-income ratios along the same division between the bottom, middle, and top incomes. Panel B of Fig. 2 provides some evidence for the US between 1999 and 2017. The debt-to-income ratio appears to be 1.5 to more than 2 times higher for middle incomes than for bottom incomes. Interestingly, the debt-to-income ratio increased much more for the middle incomes than for the two other groups before the 2008 crisis. If debt-to-income ratios have decreased both for bottom and middle incomes in the aftermath of the 2008 crisis, the increase in indebtedness for the latter until the 2007–2008 crisis is striking.[11] In Fig. A.2 in the OA, we also show the debt service (mortgage principal and interest payments, as shares of income) to income ratio appears to be 2–3 times higher for middle incomes than for low incomes in the US, and also for the UK.[12] In both countries, debt service increased mainly for middle incomes before the 2007–2008 crisis.

Fig. 2 also provides debt-to-income ratios for 14 EU countries belonging to our sample, for the last available wave of the Household Finance and Consumption Survey (2017). For comparison purposes, we also report the debt-to-income ratios

average about 50% to 60% of total outstanding debt" (Bartscher et al., 2020, p.12). Interestingly, they also note that middle incomes accounted for 55% of the total debt *increase* from 1950 to 2007.

[11] For the US, see Bartscher et al. (2020, Table 7) for an historical representation of the debt-to-income ratios from 1950 to 2017 using the Survey of Consumer Finance. Their findings confirm our observation: debt-to-income ratio appears systematically higher for the middle than for the bottom incomes (and top incomes) over the whole period. Using more exhaustive data on credit, they also report higher debt-to-income ratios. They find the debt-to-income ratios of the middle incomes (50% to 90%) reached almost 140% by 2007. This ratio has increased by around 100 pp since 1950. Contrary to our findings, the auhtors report higher debt-to income ratios for the bottom incomes than for the top incomes (Bartscher et al., 2020, Table 8). This difference can be explained by the inclusion of all types of loans (available in SCF and not in PSID), in addition to the mortgages and car loans we show in Fig. 2.

[12] For the UK, we cannot report the evolution of (total) debt-to-income ratios, due to various methodological issues and overabundant missing observations on total debt.

Table 2
Debt-to-Income Ratio (in %) per Income Group, 2017.

Country	Bottom	Middle	Top
	Bottom 30%	*30–90%*	*Top 10%*
US	27.1	72.1	68.4
	Bottom 40%	*40–90%*	*Top 10%*
European Union	58.2	72.3	78.7
Austria	13.4	43.9	49.6
Belgium	34.0	124.9	71.6
Estonia	23.9	21.4	52.2
Finland	50.5	83.7	95.9
France	32.4	74.6	96.2
Germany	26.7	46.7	66.5
Greece	281.2	51.6	68.9
Hungary	21.8	20.1	17.9
Ireland	22.4	78.7	79.5
Italy	56.6	35.9	64.3
Netherlands	454.1	253.8	152.4
Poland	12.7	18.9	31.4
Portugal	147.6	146.1	96.1
Spain	116.7	123.4	86.6

Source: Panel Study of Income Dynamics (US), Household Finance and Consumption Survey (European Union), authors' calculations. Note: the average debt-to-income ratio in the US, median debt to income ratio in the European Union. The debt-to-income ratio is the ratio of debt to gross household income. European Union data include total debts: mortgages and non-mortgage loans - consumer credit loans, private loans – credit lines/bank overdrafts debt and credit card debt. For the US, it includes mortgages and car loans.

for the US. In a majority of cases, the debt-to-income ratio is 1.5–4 times higher for middle than for bottom incomes. They are equivalent for Estonia, Hungary, and Portugal. In only three cases (Greece, Italy and Netherlands) is the debt-to-income ratio lower for middle than for bottom incomes.[13] On average, the debt-to-income ratio is 72.3% for the middle incomes versus 58.2% for the bottom incomes. They are also higher than debt-to-income ratios for top incomes in Belgium, Hungary, Netherlands, Portugal, and Spain, although the top incomes show, on average, higher level of indebtedness than middle incomes (78.7% versus 72.28%).

All in all, Fig. 2 as well as Tables 1 and 2 support the view that, across several recent decades and various countries, the middle class generates the bulk of aggregate household leverage and consistently tends to have higher debts relative to their income.

Relying on the same strands of the literature as previously, we can highlight several mechanisms pointing to the middle class as the major source of aggregate household leverage. In Kumhof et al. (2015), main results would be strengthened if the bottom earners' marginal propensity to save was not assumed to be zero, because a nonzero marginal propensity to save creates a desire to dissave (i.e., to borrow) in response to a negative income shock. In addition, Kumhof et al. (2015) consider two kinds of agents, top and bottom earners, corresponding roughly to the top 5% and bottom 95% in the US case. Therefore, bottom earners involve *de facto* low- and medium-income households. An explicit distinction between the former and the latter seems relevant, by assuming the middle class has a higher marginal propensity to save, and will consequently accumulate proportionally more debt than low incomes following an inequality shock permanently transferring income to top incomes.

The middle class's higher marginal propensity to save appears to be consistent both with the "fundamental psychological law" of consumption put forward by Keynes (1936, p. 96)[14] and by empirical evidence. Based on US micro data, Dynan et al. (2004) and Kumhof et al. (2015) find the savings rate steeply increases with income: slightly above 2% for the first quintile, it varies between 10.5% and 16.5% for the third and fourth quartiles (likely to embody the middle class), and between 30% and 40% for the top 5% income share. Based on those estimates, Dynan et al. (2004) compute the marginal propensity to save for the same income categories, with 8.9% for the first quintile, between 7.5% and 22.7% for the middle class, and 50.5% for the top 5% income share.[15] All this evidence supports the idea of a higher marginal propensity to save for the middle class, compared with low incomes.

[13] Tables A.1 and A.2 in the OA provide figures for the two other waves (2010–2011 and 2014–2015), with very similar patterns, even though some countries occasionally show some interesting differences; for example, Portugal in 2014–2015 displays a higher debt-to-income ratio for middle incomes.

[14] Put simply, the marginal propensity to consume decreases with income, and symmetrically, the marginal propensity to save increases with income.

[15] Kumhof et al. (2015) find a marginal propensity to save of 40% for the top 5%, by excluding capital gains.

As a result, the possibility that the middle class suffering the same income loss as the bottom earners would contribute more to the dissaving at the aggregate level seems natural, because it has a higher marginal propensity to save. In addition, middle-class households are, by definition, higher in the income distribution, so that they have higher past levels of income and consumption, and their reference group is closer to top incomes. In other words, relative income and relative consumption approaches presented above would predict that the middle class has a higher level of consumption to support, requiring higher borrowing than bottom incomes. Therefore, we bring to the data the following relationship.

Testable Relationship 2: When the share of top incomes increases relative to bottom incomes, the bulk of the positive impact of this rising inequality on household credit is driven by the middle class rather than by lower incomes.

3. Data

Our empirical analysis relies primarily on a country-level yearly dataset for 30 developed countries over the period 1970–2017[16] based on two building blocks: income inequality and credit.

3.1. Inequality

The use of inequality data in cross-country studies raises several challenges. More specifically, the choice for a specific database is a crucial issue. Jenkins (2015b), among others, shows how it can have major implications on empirical results. We rely primarily on the WIID, which offers the best compromise in terms of coverage and variety of income-inequality indicators used. Jenkins (2015b) recommends the use of the WIID. The latter includes new estimates from National Survey statistics, TransMonEE (2011), the Commitment to Equity Project (CEQ), the Socio-Economic Database for Latin America and the Caribbean (SEDLAC, 2016), the Luxembourg Income Study, OECD, and EUROSTAT. It covers all countries over the world between 1867 and 2018.

However, we report at each key step robustness checks of our results based on the SWIID and the WID. The SWIID (Solt, 2009) has more systematic coverage than the WIID in recent decades, with a lower number of missing observations. But beyond the fact that the imputation procedure that is used to fill missing data raises potential issues,[17] this database does not provide information on deciles of income distribution. The WID database, however, contains information on income deciles based on administrative tax data, together with survey data. Bordo and Meissner (2012) and Perugini et al. (2016), among others, use top-income shares from the WID database. This database, built by Alvaredo et al. (2017), is available for 116 countries with high time coverage for some countries. However, their key income-inequality indicators are based on pre-tax and not disposable income; that is, they do not take into account the effect of fiscal redistribution on disposable income.

One of our aims in this paper is to focus on the potential heterogeneous role of different shocks along the income distribution on the inequality-credit relationship. Indeed, as stated by Atkinson and Morelli (2010, p. 66) in the context of banking crises, “different parts of the income distribution react differently, and the conclusions drawn regarding the origins and the impact of the crisis may depend on which part of the parade we are watching.” Top income share indexes will not capture any distributional change *within* the bottom 90%. Consequently, we do not focus exclusively on the Gini coefficient and top-income shares usually studied in the literature, but we also investigate ratios between income-share deciles.

The use of the Gini index remains useful because it takes into account the entire distribution of income and not only tails dynamics. Afterwards, we go one step further by investigating different income-shares categories. We start with an indicator commonly used in the literature, the top incomes, defined as the share of income of the top 10% (corresponding to incomes after the ninth decile). We complement this top-income share by using ratios of the latter to other income categories, in order to assess the impact of relative variations, that is, the gain or impoverishment of one category versus another one. More precisely, we study the impact of the ratio of top incomes to middle-class incomes: Top 10/middle 30–90, where “middle 30–90” corresponds to incomes after the third and through the eight decile. Finally, we focus on the ratio of top incomes to bottom incomes, top 10/bottom 30, where “bottom 30” is defined as the share of income owned by the bottom 30% (corresponding to incomes up to the third decile).[18] These indicators will allow us to disentangle the specific effect of income shocks for the poorest and for the middle class, and consequently to assess the empirical validity of our two testable relationships.[19]

Going into the details of data sources for each indicator, we use the Gini coefficient predominantly from the WIID,[20] but also report estimates based on the Gini index from the SWIID. Furthermore, the ratios between income shares come

[16] We extend the analysis to a sample of 19 developing and emerging countries in Section 5.3.

[17] This debate falls within the trade-off between the geographical coverage and the reliability of the data. See Jenkins (2015b) and Solt (2015).

[18] Note these ratios are intuitively closed to the Palma (Palma, 2011) index that combines the top 10% income share with the bottom 40% income share.

[19] Tables D.7, D.8, and D.9 in the OA report results based on alternative definitions of top and middle incomes. Tables D.7 and D.8 define top incomes as the top 30% (corresponding to incomes after the seventh decile), and middle incomes as incomes after the third and up to the seventh decile. Table D.9 define top incomes as the Top 20% (corresponding to incomes after the eighth decile), middle incomes as incomes after the fourth and up to the eight decile, and bottom incomes as those below the fourth decile. None estimates display any major qualitative change compared with our benchmark results.

[20] We provide a transparent process for using WIID rigorously. Our rules of selection ensure high-quality data within and between countries. We keep only observations with specific characteristics: they are coded as high (or medium) quality, and they concern post-tax income. Our selection promotes the use of one unique dataset per country. Table F.3 in the OA shows the primary source of inequality measures chosen after processing WIID.

both from the WIID and the WID. The former provides the complete distribution of incomes per deciles, whereas the latter distinguishes the top 10%, the middle 50%-90%, and the bottom 50%. Tables A.1 and A.2 in Appendix A report the primary sources used for each country.

3.2. Credit

To contrast to the few existing works based on cross-country samples, we focus on household credit.[21] The various frameworks surveyed in Section 2 converge to predict an impact of income inequality on household credit, but not necessarily on the other sources of private credit, such as business credit. Although any supply-driven phenomenon should theoretically increase all kinds of credit through a decrease in interest rates in Kumhof et al. (2015), the demand-driven conclusions of the various relative income and consumption approaches clearly point to an impact limited to household credit. In this regard, Table E.10 in the OA shows the impact of inequality on firm credit and bank credit is unclear and mostly insignificant. In addition, Buyukkarabacak and Valev (2010) find business credit is a much weaker predictor of financial crises.

We rely on the ratio of household credit to GDP, because recent literature (e.g., Atkinson and Morelli, 2015) emphasizes that the excessive level of credit relative to output may lead to financial instability. Increasing levels of credit do not imply instability if productive investment is funded, triggering an increase in the long-run output. However, we also check through additional estimates how our results behave when we use the log of household credit.[22]

Our main datasource for household credit is the Bank for International Settlements (BIS): over 76% (23 countries) of household credit comes directly from BIS. The remainder of household credit data comes from Central Banks, and has been carefully checked and harmonized (see Data Appendix A).

3.3. Other variables

The classical determinants of credit that the literature identifies are financial liberalization, monetary dynamics, and the level of economic development. Regarding financial liberalization, we use indexes of credit market deregulation provided by the Fraser Institute, concerning private ownership of banks, the existence of interest rate controls and negative interest rates, and the extent to which government borrowing crowds out private borrowing. We also include Chinn and Ito's (2006) now well-known index measuring a country's degree of capital-account openness.

Monetary dynamics are a key determinant of credit in various theoretical contexts. We proxy the monetary environment by the broad money supply, that is, the M2/GDP ratio from the World Bank, following the previous literature, notably Elekdag and Wu (2011) and Perugini et al. (2016). The level of economic development also affects the depth of the domestic financial system on the one hand, and the level of the financial exclusion frontier similar to French et al. (2013) on the other hand. We use the standard proxy, GDP per capita, provided once again by the World Bank.

We also add two variables controlling for the dynamics of the real estate market: real house prices, provided by the BIS and Cesa-bianchi et al. (2015), and the ratio of housing gross fixed capital formation to GDP (coming from the OECD), representing households' investment in real estate. These two latter variables control for the dynamics of indebtedness specifically driven by households' housing investment, which proved to be increasingly important over a significant part of the studied period. As we note in Section 4.2, these variables, together with GDP per capita, also play an important part in supporting the validity of our IV strategy.

Finally, some estimates also include four variables representing common trends and shocks: world GDP (from the World Bank) and oil prices (from FRED Saint-Louis) control for common business-cycle and inflation conditions, whereas the Fed Funds rate and the VIX index (both coming from FRED Saint Louis) represent the world financial cycle (see Miranda-Agrippino and Rey, 2015, and Rey, 2015).

4. Empirical methodology

4.1. Baseline specification

Our main objective is to identify how inequality and its structure affect household credit at the country level. Fig. 1 in Section 2 shows a positive correlation between the evolution of income inequality and household leverage within countries (taking the first and last observations available for each country). This correlation is also found in Fig. 3, which shows the partial correlations between inequality (measured alternatively by the Gini and top 10) and household debt from various specifications, starting with basic cross-country regressions (Panels A and B). The correlation is stronger within countries when country fixed effects are included (Panels C and D). Nevertheless, this correlation tends to become insignificant once we also control for year fixed effects (Panels E and F), although the coefficient remains positive.[23]

[21] Bordo and Meissner (2012) use the log of bank credit to the private sector, and Perugini et al. (2016) the ratio of total private credit to GDP. Gu et al. (2019) alternate between both indicators.

[22] Reported in Table E.11 in the OA, results of these estimates are qualitatively and quantitatively very similar to our benchmark estimates, though with a generally lower significance.

[23] This last result shows common shocks affecting countries are likely to explain a large part of the concomitant increase in inequality and household debt observed in various countries. Our empirical strategy takes this issue into account to identify the causal effect of income inequality on leverage.

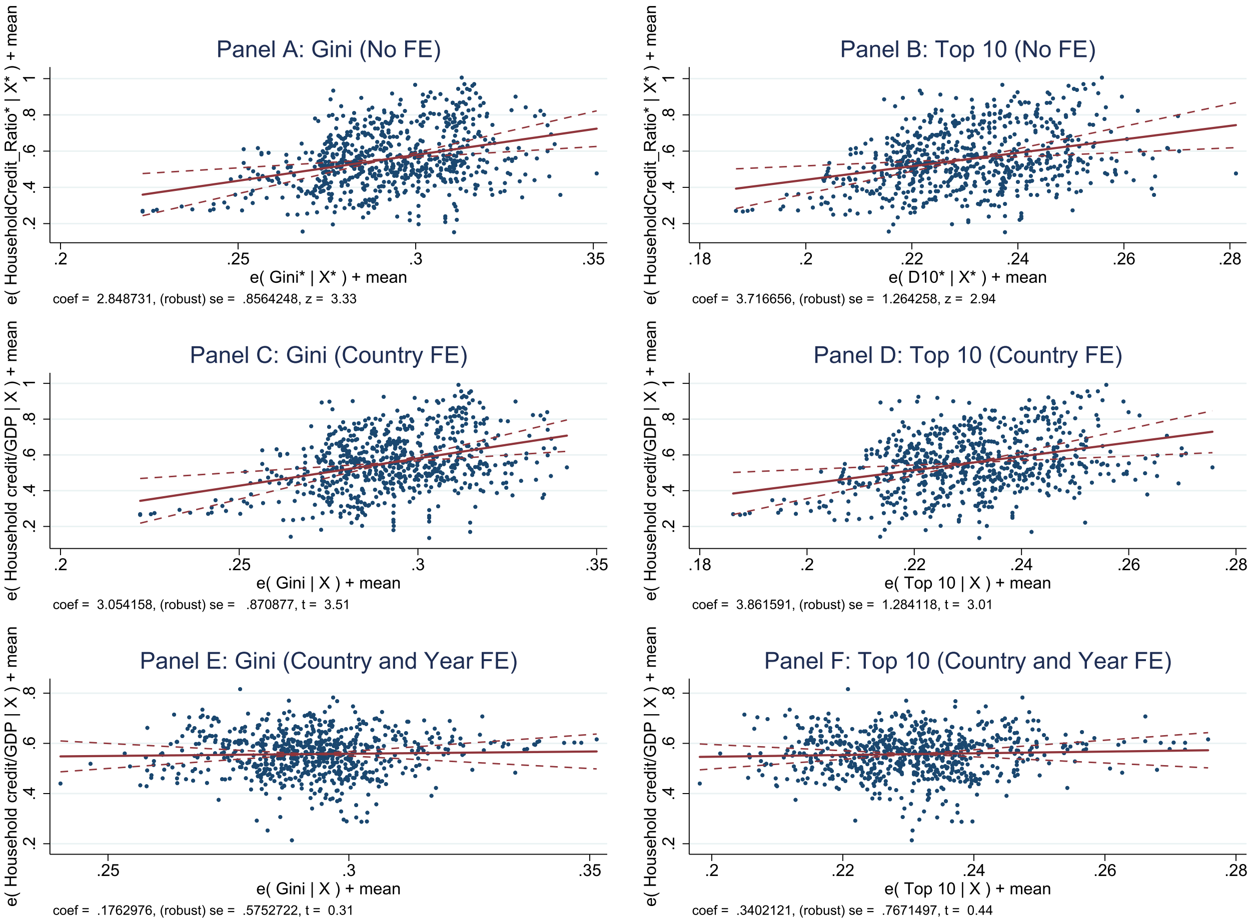

Fig. 3. Partial Correlations between Inequality and Household Leverage Note: These partial-regression plots show the partial correlation between inequality and household leverage from panel regressions: (1) $Credit_{i,t} = \beta Ineq_{i,t} + \epsilon_{i,t}$, (2) $Credit_{i,t} = \beta Ineq_{i,t} + \mu_i + \epsilon_{i,t}$, and (3) $Credit_{i,t} = \beta Ineq_{i,t} + \mu_i + \lambda_t + \epsilon_{i,t}$ with μ_i and λ_t country and year fixed effects, respectively. The fitted line is the least-square fit between the residuals and has the same slope as the estimated coefficient in the regression. Scatter-plot values are rescaled to be centered on the mean value of both variables.

Our goal is to build on these correlations and estimate a specification of the following form:

$$Credit_{i,t} = \beta Ineq_{i,t} + \Gamma X_{i,t} + \lambda Y_t + \mu_i + \epsilon_{i,t} \tag{1}$$

where $Credit_{i,t}$ and $Ineq_{i,t}$ are, respectively, the household credit to GDP and inequality in country i during year t. We assess the impact of inequality through various measures (Gini index, share of top incomes, ratios of deciles of income) in order to clarify the role of the structure of income distribution. $X_{i,t}$ is a vector of controls including M2 over GDP, the log of GDP per capita, and the index of financial deregulation, as well as the real house prices and the ratio of housing gross fixed capital formation to GDP. λY_t is a vector of variables representing common trends and shocks, consisting alternatively of year dummies, or four variables including: world GDP, oil prices, the Fed Funds rate, and the VIX index. Finally, μ_i denotes country fixed effects, capturing all time-invariant country characteristics.

We are specifically interested in changes in credit driven by exogenous variations in inequality. Our coefficient of interest is β: the various strands of the literature surveyed in Section 2 predict $\beta > 0$ when inequality rises, that is, when the Gini index, the share of top incomes (top 10%) in the total income, or the ratio of top incomes to low or middle incomes increase.

When Eq. (1) is estimated by OLS (see Table B.1, in the OA), the correlation between inequality and credit is massively insignificant in all specifications, not unlike what is observed in Fig. 3 when country and year fixed effects are included.[24] This echoes the findings of Bordo and Meissner (2012), who find insignificant correlations when using a similar specification - but with log of credit as a dependent variable.

[24] We also note this insignificant correlation in extensive additional OLS estimates reported in Tables B.3 and B.4 in the OA.

Table 3
Descriptive Statistics: Credit and Inequality.

	Mean	Min	1st quartile	Median	3rd quartile	Max	S.D.*within* All period	S.D. *within* Bef. 2008
Levels								
Gini	0.292	0.197	0.257	0.29	0.329	0.402	0.019	0.019
Top 10	0.231	0.171	0.212	0.232	0.25	0.312	0.014	0.014
Top 10/Middle 30–90	0.37	0.265	0.336	0.369	0.4	0.534	0.028	0.029
Top 10/Bottom 0–30	1.62	0.926	1.296	1.556	1.891	2.97	0.176	0.179
Top 10/Middle 50–90	0.493	0.366	0.451	0.49	0.53	0.69	0.035	0.036
Top 10/Bottom 0–50	0.776	0.471	0.65	0.762	0.89	1.32	0.076	0.077
Household credit/GDP	0.557	0.039	0.342	0.521	0.723	1.394	0.161	0.130
log(real household credit)	5.94	1.38	4.89	6.23	7.09	9.69	0.533	0.480
Nbr. of ratified ILO Conv.	74.3	7	57	75	96	133	6.83	6.77

S.D.: standard deviation.

However, for a number of reasons, these OLS estimates may be heavily biased. First, credit and inequality are likely to be simultaneously determined by shocks, such as the deregulation waves in the 1980s and the 1990s,[25] which simultaneously increased the two variables; in that case, β is positively biased. We reduce the bias by controlling for financial liberalization and capital-account openness, but other dimensions and shocks might still be at play.

Another obvious issue relates to reverse causality: credit is very likely to have an impact on inequality. Since Banerjee and Newman (1993) and Galor and Zeira (1993), the literature has long asserted the latter should be negative: because financial market imperfections are mainly binding on the poor, better access to credit markets, allowing more poor people to become entrepreneurs or to invest in human capital, will help reduce inequalities. This long-standing conventional wisdom about financial development and inequality is summarized in Levine (2005, p. 920): “The results indicate that finance exerts a disproportionately large, positive impact on the poor and hence reduces income inequality.” In addition, Beck et al. (2007) find the reduction in inequality allowed by increased access to finance is sizeable. Consequently, this negative bias is likely to offset the previously mentioned positive bias, leading to the OLS estimates from Eq. (1) that are noisy, non-statistically different from zero.

Finally, Table 3 reports some basic descriptive statistics for our key variables. Within-country standard deviations, in particular, are useful in computing meaningful and comparable quantifications from our estimations (see Section 5 below).

4.2. Identification strategy

To identify how variations in inequality driven by exogenous shocks affect household credit to GDP, we need an instrument that affects inequality without influencing directly credit (exclusion restriction). This instrument has to be orthogonal to any country-specific characteristic which may drive simultaneously both variables (inequality and credit). Therefore, we cannot use indicators of labor market institutions and trade openness, such as the ones used in Perugini et al. (2016) and Gu et al. (2019). Indeed, labor market, trade, and financial liberalization often belong to the same policy packages, with two consequences: an increase in the demand for credit due to the decrease in workers’ bargaining power (see Tridico, 2012), and a joint increase in credit supply and trade openness explained by simultaneous financial and trade liberalization. In other words, credit, labor, and product market regulation levels and trade openness are likely to be driven by deeply related dynamics, casting strong doubts on the validity of exclusion restrictions in such a context.

Therefore, we propose two strategies of identification, relying on two sets of country-year, original instruments: the number of ratifications of ILO conventions and factor endowments (land and capital endowments, and skill intensity/education level). The general idea of both sets of instruments is to offer predictors of reforms and structural changes affecting labor market policies and trade openness with significant impacts on income inequality, but simultaneously orthogonal to any other coincident factors, especially liberalization policy packages that are likely to be correlated with household debt. To the best of our knowledge, our paper is the first in the literature to use such instruments to identify the causal impact of inequality on credit expansion.

[25] Because the deregulation wave occurs simultaneously in most developed countries, part of this effect is captured through the time dummies. However, differences in the timing of financial deregulation may still bias our OLS estimates.

Instrumental Variable (1): The number of ratified ILO conventions

The International Labor Organisation (ILO) is a UN agency with 187 member states setting international labor standards by adopting conventions and recommendations.[26] Today, 189 conventions cover all fields related to labor relations (e.g., collective bargaining, forced labor, child labor, equality of opportunity and treatment, labor administration and inspection, employment policy, vocational guidance and training, job security, wages, working time, occupational safety and health, social security, maternity protections, etc). Areas covered by these conventions are therefore much broader than labor market institutions.

Highlighting the uniqueness of the tripartite structure of the ILO (each state is represented by its government, by workers' representatives, and by employers' representatives) and its consequences in terms of policy agenda is important. Governance differs drastically from other international organizations such as the IMF or the World Bank, where voting power is determined by national quotas and each country is represented by a governor nominated by the government only. Workers' representatives have much more power in the ILO than in any other international organizations. Beyond such institutional difference, the fundamental goals of the ILO also explained why its policy agenda is so specific. This organization focuses specifically on social justice, individual well-being, economic security and equal opportunity.[27]

Such differences may explain why the ILO policy agenda is largely autonomous from other international economic policies. For instance, the increasing influence of free-market economics in international economic policies had little effect on the ILO agenda, even during the 1980s when the ILO model of tripartite dialogue was contested. The dynamic in the number of ILO conventions' ratifications is mainly explained by the evolution of the ILO strategy over time (see Rodgers et al., 2009, for a global overview of ILO history).[28]

One particular threat to identification is that ILO conventions might be correlated with other country-level variables that would affect household credit. For example, if governments aimed at strengthening labor regulations are also ratifying ILO conventions, our instrument would be correlated with broader, country-level labor market regulations. A possible correlation between labor market deregulation and financial deregulation may be a concern, because the latter is likely to have a direct effect on our dependent variable, household credit. For this reason, we do not use indexes of country-level, labor-market regulations as instruments. Indeed, we find a significant correlation between credit and labor deregulation when controlling for country and year fixed effects. By contrast, we do not find any significant correlation either between credit deregulation and ILO conventions or between labor deregulation and ILO conventions.[29] We therefore argue that dynamics of ratification depend mainly on the international policies and strategies of the ILO, and are largely orthogonal to other international (deregulation) policy packages and national circumstances.

Based on the history of ILO policies, we also identify two waves of ratifications that are driven by specific ILO internal changes. The first one is the period 1973–1977, with the new leadership of ILO Director-General Francis Blanchard and the start of the International Program for the Improvement of Working Conditions and Environment. The second one is the period 1995–2008, starting with the Social Summit of Copenhagen and the Declaration on Fundamental Principles and Rights at work, which boosted a new dynamic of ratifications, under the leadership of ILO Director-General Juan Somavia. We suggest that the argument supporting the orthogonality condition is even stronger during these periods because additional ratifications are strongly explained by new dynamics specific to the ILO.

Another identification concern would come from a direct effect of ILO conventions on household debt, which would not go through a change in income distribution. The potential positive effect of ILO conventions on *total* wages and income might also increase the ability to refund loans, and consequently, to access credit. Our main specification includes country-year variables controlling for this channel through which ILO conventions may affect directly household leverage. GDP per capita controls for the *average* level of wages. In addition, we also control for the ratio of housing gross fixed capital formation to GDP representing households' investment in real estate, jointly with real house prices. Indeed, a very significant part of the increase in household leverage over the past decades has been devoted to housing investment; therefore, the latter should be able to capture the additional indebtedness capacity allowed by better wages/labor incomes, based on the idea that a better income improves the ability to refund bigger loans, which are typical of housing investments. We are therefore confident that the effect we capture through our identification strategy is the effect of ILO conventions on income *distribution* rather than a direct effect on household leverage.[30]

[26] The ratification of conventions is voluntary. Once a country has ratified a convention, it becomes binding. Ratifying countries commit themselves to applying the convention in national law and practice, and to reporting on its implementation at regular intervals.

[27] The 1919 preamble of the ILO Constitution states that "universal and lasting peace can be established only if it is based upon social justice." The 1944 constitutive Declaration of Philadelphia emphasizes as a central aim that "all human beings, irrespective of race, creed or sex, have the right to pursue both their material well-being and their spiritual development in conditions of freedom and dignity, of economic security, and equal opportunity" and states that "all national and international policies and measures, in particular those of an economic and financial character, should be judged in this light and accepted only in so far as they may be held to promote and not to hinder the achievement of this fundamental objective," source : http://www.ilo.org/global/about-the-ilo/history/lang--en/index.htm.

[28] Fig. A.3. in OA shows the evolution of ILO's ratification over time. We observe an increasing trend in ILO's ratification over time, largely independent from other international economic policies.

[29] See Fig. A.5 in the OA for partial-correlation plots.

[30] In Table B.2 in the OA, we include ILO ratification variables in an OLS regression similar to Table B.1. The estimated coefficient is always negative, which goes against the argument that ILO conventions might have a direct and *positive* effect on household leverage, through easier access to credit. Furthermore,

On the other side, the ratification of ILO conventions is likely to have an effect on inequality, ensuring the strength of our instrument. ILO conventions cover a wide range of topics related to wages, working conditions, and labor relations, with an explicit goal of improving workers' well-being (as described in the 1919 ILO Constitution and the 1944 Declaration of Philadelphia). They are legally binding once ratified by a country.[31] Beyond the diversity of such conventions and their potential heterogeneous effects on different labor market outcomes, one common characteristic of these conventions is to contribute to increase workers' bargaining power by providing them a more protective regulatory framework. Such an increase in workers' bargaining power is associated with an increase in wage compression and therefore a decrease in inequality. Various empirical studies have confirmed the overall distributional effect of labor market institutions (Betcherman, 2012; Calderón and Chong, 2009; Checchi and García-Peñalosa, 2008; Koeniger et al., 2007).[32]

Instrumental Variables (2): Factor endowments.

Although we are confident that the number of ratified ILO conventions is an adequate IV for our purpose, we want to assess how our results behave for alternative types of exogenous inequality shocks. The idea is to show the precise type of exogenous inequality shocks considered does not matter for the results. In this respect, trade openness is also a potential source of inequality, though obviously it cannot be used directly as an instrument for the aforementioned reasons: trade openness is likely to be jointly determined with credit dynamics, for example, because trade and financial liberalization have gone hand in hand in most countries. That said, the *determinants* of trade openness, or factor endowments, are much more exogenous to these joint dynamics, whereas an extensive literature has shown they are strongly correlated with inequality (see, e.g., Spilimbergo et al., 1999; Bourguignon and Morrisson, 1990; Gourdon et al., 2008). These determinants are usually the land and capital endowments, and skill intensity/education level. They will therefore correspond to three instruments, whose definitions and sources are provided in full details in Table A.1 in Appendix A: the agricultural land share as a percentage of total territory, the ratio of net capital stock to total hours worked, and the average number of years of total schooling.[33] Using alternative instruments also allows us to perform Hansen's J-test of overidentifying restrictions. As we show later, almost all test statistics are insignificant, indicating the orthogonality of the overidentifying instruments and the error term cannot be rejected; thus, our choice of instruments is appropriate on that ground.

Therefore, we use as IVs first the number of ratified ILO conventions, then our various proxies for factor endowments, and finally, combinations between the former and the latter. Our main econometric strategy estimates the effect of exogenous changes in inequality (predicted by our various sets of instruments) on the ratio of household credit to GDP:

$$Ineq_{i,t} = \alpha IV_{i,t} + \delta X_{i,t} + \Phi Y_t + \mu_i + \nu_{i,t}, \tag{2}$$

where $IV_{i,t}$ is the number of ratified ILO conventions, or the land/capital endowments and skill intensity/education level, or various combinations implying those variables. The second stage is:

$$Credit_{i,t} = \beta \widehat{Ineq}_{i,t} + \Gamma X_{i,t} + \Psi Y_t + \mu_i + \epsilon_{i,t}, \tag{3}$$

where $\widehat{Ineq}_{i,t}$ is the predicted value of the inequality index from Eq. (2).

We perform the Durbin-Wu-Hausman test for exogeneity of regressors. Unsurprisingly, the null hypothesis of exogeneity is rejected in almost all cases, which confirms the need to use IVs.[34] In all estimations, we also report the F-stat form of the Kleibergen-Paap statistic ("KFP" at the bottom of each table), the heteroskedastic robust version of the Cragg-Donald statistic suggested by Stock and Yogo (2005) as a test for weak instruments. In most cases, statistics are comfortably above the critical values, confirming that our instruments are strong predictors of inequality.

As a robustness check, we finally propose a modified specification of Eq. (2) to measure the specific effects of ILO ratifications during the two waves 1973–1977 and 1995–2008 (Eq. (4)).[35] The goal is to rely on a "quasi-natural experiment" environment provided by the strategy of the ILO. During these two periods, the increase in ratifications has been largely explained by new ILO policies pursued by their Directors-General at that time. Then, Eq. (2) becomes:

$$Ineq_{i,t} = \alpha_1 ILO_{i,t} + \alpha_2 ILO_{i,t} \times Year_{73-77} + \alpha_3 ILO_{i,t} \times Year_{95-08} + \delta X_{i,t} + \Phi Y_t + \mu_i + \nu_{i,t}, \tag{4}$$

where $ILO_{i,t}$ is the number of ratified ILO conventions.

estimated coefficients are not significant or are very weakly significant, which supports the exclusion restriction. Significance is even lower for ratifications during the two waves.

[31] These conventions appear to be binding in advanced economies as well. International treaties and conventions have a higher jurisdictional power than national laws. As a result, any court can base on a ratified ILO convention any decision opposing a governmental regulation or legislation. An illustrative example is the *Contrat Nouvelle Embauche* adopted in France in 2005. Several court decisions contested this new labor contract, with the argument that the national law contradicted the ILO convention 158. The French Court of Cassation confirmed this analysis and rejected the claims of the government, which had no other choice but to repeal the law and forgive this new labor contract in 2008.

[32] Note Calderón and Chong (2009) use specifically the number of ILO conventions to assess the distributional effects of labor regulations on inequalities.

[33] Unfortunately, we could not retrieve sufficiently numerous and comparable measures of skilled labor intensity for our sample.

[34] All statistics and corresponding *p*-values are available upon request.

[35] For this specification, Y_t includes 73–77 and 95–08 period dummies (in addition to the vector of variables representing common trends and shocks) when year dummies are not included.

Table 4
TR 1: First Stage of Table 5.

Dep. Var.	(1) Gini	(2)	(3)	(4)	(5) Top 10	(6)	(7)	(8)
Sample	All	All	Bef. 2008	Bef. 2008	All	All	Bef. 2008	Bef. 2008
ILO conv.	−0.000797***	−0.000927***	−0.00123***	−0.00134***	−0.000576***	−0.000661***	−0.000792***	−0.000826***
	(0.000215)	(0.000216)	(0.000281)	(0.000294)	(0.000158)	(0.000155)	(0.000201)	(0.000215)
GDP per capita	0.0420***	0.0356**	0.0500**	0.0677***	0.0432***	0.0322***	0.0438**	0.0594***
	(0.0134)	(0.0139)	(0.0250)	(0.0242)	(0.0105)	(0.0104)	(0.0172)	(0.0170)
Broad Money Ratio	0.0180***	0.0154**	0.0447***	0.0387***	0.00786	0.00730	0.0224***	0.0185***
	(0.00668)	(0.00605)	(0.00964)	(0.00948)	(0.00482)	(0.00451)	(0.00692)	(0.00714)
Financial Openness	−0.0203***	−0.0317***	−0.0133*	−0.0258***	−0.0177***	−0.0260***	−0.0126**	−0.0198***
	(0.00656)	(0.00618)	(0.00752)	(0.00753)	(0.00458)	(0.00444)	(0.00523)	(0.00557)
Credit Deregulation	0.00462***	0.00434***	0.00893***	0.00835***	0.00248***	0.00194**	0.00585***	0.00475***
	(0.00119)	(0.00125)	(0.00207)	(0.00202)	(0.000880)	(0.000903)	(0.00141)	(0.00155)
Real House Prices	0.00125	0.00360	−0.0113**	−0.00625	−0.00219	−0.00237	−0.0156***	−0.0137***
	(0.00354)	(0.00362)	(0.00566)	(0.00586)	(0.00275)	(0.00269)	(0.00432)	(0.00412)
Housing GFCF Ratio	−0.0378***	−0.100***	−0.0310**	−0.108***	−0.0287***	−0.0792***	−0.0166*	−0.0851***
	(0.0128)	(0.0194)	(0.0127)	(0.0204)	(0.00855)	(0.0142)	(0.00862)	(0.0165)
World GDP	0.0518**		0.0727*		0.0366**		0.0734**	
	(0.0233)		(0.0417)		(0.0175)		(0.0295)	
Oil Price	−0.00722***		−0.0105***		−0.00519***		−0.00662***	
	(0.00165)		(0.00268)		(0.00123)		(0.00189)	
VIX	−0.000282		−0.00375*		0.000460		−0.00242*	
	(0.00141)		(0.00201)		(0.00110)		(0.00142)	
FED Rate	−0.0333		0.0122		0.000319		0.0291	
	(0.0367)		(0.0452)		(0.0276)		(0.0337)	
Year Dummies	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	726	726	467	467	726	726	467	467
Countries	30	30	27	27	30	30	27	27

Robust standard errors are in parentheses. All specifications include country fixed effects. *, ** and *** denote, respectively, significance at the 10%, 5%, and 1% levels.

5. Results

5.1. Impact of income inequality on household leverage

In this section, we focus on the empirical assessment of our first testable relationship, namely, the positive impact of an exogenous variation in income inequality on the ratio of household credit to GDP.

Baseline estimates (Instrumental Variable (1): ILO conventions). Table 4 shows the first-stage results associated with the estimates reported in Table 5. The latter displays our baseline results for Eq. (3), focusing on two indicators of income inequality widely used in the literature: the Gini index (which gives an idea of the "average" inequality of the income distribution, columns (1) to (4)) and the share of income going to the top 10% ((columns (5) to (8)). Both are instrumented by the number of ILO conventions ratified at the country level. In this regard, the first-stage estimates reported in Table 4 confirm the theoretical intuitions presented in Section 4.2 regarding the (very) significant negative association between the number of ratified ILO conventions and inequality, due to the higher protection and bargaining power they grant to workers. Put differently, a higher number of ratified ILO conventions decreases the Gini index (columns (1) to (4) in Table 4) and the share of top incomes (columns (5) to (8) in Table 4).

To make meaningful comparisons regarding the second-stage estimates shown in Table 5, we report in the "Quantification" row the product between the estimated parameter for each inequality indicator and its within-country standard deviation. As previously mentioned in Section 4.1, we check how our results behave when common time dynamics are included through a set of control variables (columns (1), (3), (5), and (7)) or, alternatively, year dummies (columns (2), (4), (6), and (8)). In this regard, we devote specific attention to the 2007-2008 financial crisis, which may have affected the relationship we are interested in: for this reason, we report in columns (3), (4), (7), and (8) estimates over a period restricted to years before 2008.[36]

The first testable relationship (TR 1) is validated: positive changes in inequality, as predicted by changes in the number of ratified ILO conventions, are positively related to the ratio of household credit to GDP. This result holds regardless of which specification is estimated, though significance appears slightly weaker in columns (1) and (5). As expected, this is likely to be a consequence of the post-financial-crisis years: significance of the Gini index and top 10% income is very strong in all other columns, where post-financial-crisis years are either controlled for in a more exhaustive way through

[36] Three countries are excluded from these pre-2008 estimates due to data availability: Romania, Switzerland, and Slovenia – see Table A.2 in the Data Appendix. Table B.3 in the OA presents the OLS results with the same specifications.

Table 5
Testable Relationship (TR) 1: Baseline Estimates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	Household Credit/GDP							
Sample	All	All	Bef. 2008	Bef. 2008	All	All	Bef. 2008	Bef. 2008
Gini	2.861*	3.686***	3.869***	4.143***				
	(1.541)	(1.350)	(1.339)	(1.246)				
Top 10					3.962*	5.172***	6.016***	6.739***
					(2.061)	(1.855)	(2.186)	(2.227)
GDP per capita	−0.394***	−0.455***	−0.0984	−0.233	−0.445***	−0.490***	−0.168	−0.353*
	(0.0936)	(0.0998)	(0.151)	(0.157)	(0.108)	(0.105)	(0.164)	(0.185)
Broad Money Ratio	0.0958**	0.0734*	−0.0385	−0.0469	0.116***	0.0924**	−0.000252	−0.0116
	(0.0443)	(0.0412)	(0.0841)	(0.0780)	(0.0361)	(0.0362)	(0.0767)	(0.0766)
Financial Openness	0.00832	0.0599	−0.0895*	−0.0313	0.0202	0.0773	−0.0653	−0.00457
	(0.0509)	(0.0634)	(0.0466)	(0.0556)	(0.0533)	(0.0656)	(0.0523)	(0.0647)
Credit Deregulation	−0.0168*	−0.0191**	−0.0367**	−0.0340**	−0.0134*	−0.0131*	−0.0373**	−0.0314**
	(0.00937)	(0.00898)	(0.0152)	(0.0143)	(0.00789)	(0.00759)	(0.0160)	(0.0154)
Real House Prices	0.183***	0.154***	0.192***	0.178***	0.196***	0.179***	0.242***	0.245***
	(0.0253)	(0.0290)	(0.0389)	(0.0423)	(0.0249)	(0.0281)	(0.0499)	(0.0515)
Housing GFCF Ratio	0.116	0.432**	0.167**	0.712***	0.122	0.471**	0.147**	0.840***
	(0.0851)	(0.181)	(0.0767)	(0.176)	(0.0809)	(0.188)	(0.0739)	(0.230)
World GDP	0.569***		0.495**		0.572***		0.335	
	(0.132)		(0.241)		(0.131)		(0.262)	
Oil Price	0.0918***		0.0909***		0.0918***		0.0903***	
	(0.0147)		(0.0204)		(0.0145)		(0.0212)	
VIX	0.0105		0.00358		0.00787		0.00361	
	(0.00886)		(0.0129)		(0.00905)		(0.0132)	
FED Rate	−0.833***		−0.448*		−0.929***		−0.576**	
	(0.198)		(0.250)		(0.190)		(0.274)	
Quantification								
$\beta_{Ineq} * SD_{within}$	0.053	0.068	0.075	0.08	0.055	0.072	0.086	0.097
$KPF - stat$	13.753	18.359	19.237	20.899	13.305	18.112	15.46	14.803
Year Dummies	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	726	726	467	467	726	726	467	467
Countries	30	30	27	27	30	30	27	27

Robust standard errors are in parentheses. All specifications include country fixed effects. The critical value for the weak-instruments test is based on a 10% (resp. 15%) 2SLS size at the 5% significance level, which is 16.4 (8.96) in all estimations. *, **, and *** denote, respectively, significance at the 10%, 5%, and 1% levels.

year dummies (columns (2) and (6)), or removed from estimations (columns (3)/(4) and (7)/(8)). In all cases, the strength of our instruments is confirmed: the Kleibergen-Paap statistic is above, or at least very close to, the threshold value pointed by Stock and Yogo (2005), and in any case, above the value of 10 prescribed by Staiger and Stock (1997). Given the first-stage coefficients (Table 4, column (1)), the ratification of seven additional ILO conventions (i.e., hardly more than one additional within-country standard deviation, see Table 3) is found to generate a decrease in the Gini (on a [0-1] scale) ranging from -0.0056 to -0.0091 (until the 2008 financial cris), which in turn implies a 1.6–3.8 (until the 2008 financial crisis) pp decrease in credit to GDP.

Regarding control variables, GDP per capita and broad money ratio are significant only over the whole period. The broad money ratio is positively associated with household leverage, consistent with the well-known idea that a higher money supply brings additional credit. GDP per capita displays a negative impact;[37] as for the two proxies for financial liberalization, whereas financial openness is largely insignificant, financial deregulation exhibits a negative impact on household credit to GDP regardless of the considered period.

Interestingly, Table E.11 in the OA shows that when the dependent variable is simply the log of household credit, GDP per capita and the financial-openness indicator display the expected positive sign, whereas financial deregulation turns simply insignificant. Therefore, regarding credit deregulation, a positive deviation from the mean across countries does not bring additional household credit, decreasing its share to GDP. Additionally, Tables E.1 and E.2 in the OA show that without country fixed effects, estimated parameters on GDP per capita and financial openness are always positive and highly significant, as is financial deregulation. This finding suggests that higher levels of development and financial openness do bring an increase in household credit, but the latter does not move faster than GDP. Though delivering some definite explanations for these estimates is beyond the scope of this paper, a common rationale may be underlying them. Beyond a certain (average over countries) level of development (GDP per capita) and financial liberalization, the latter variables keep supporting the growth of household credit, but at a slower pace than GDP. In this regard, note that in Table E.10 from the OA, financial

[37] Interestingly, the sign on GDP per capita is reverted when we run our estimation on a sample of emerging and developing countries; see Table 10 in section 5.3.

Table 6
TR 1: Results with ILO's Waves of Ratifications.

Model	First stage				Second stage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var	Gini		Top 10		Household Credit/GDP			
Sample	All	Bef. 2008	All	Bef. 2008	All	Bef. 2008	All	Bef. 2008
Gini					1.684***	2.524***		
					(0.637)	(0.614)		
Top 10							3.356***	3.277***
							(1.033)	(0.732)
ILO conv.	−0.001**	−0.001*	−0.0004***	−0.0002				
	(0.000)	(0.000)	(0.000)	(0.000)				
ILO conv. *7377	−0.003***	−0.003***	−0.002***	−0.002***				
	(0.001)	(0.001)	(0.000)	(0.001)				
ILO conv. *9508	−0.0001**	−0.0002***	−0.0001**	−0.0002***				
	(0.000)	(0.000)	(0.000)	(0.000)				
GDP per capita	0.040***	0.069***	0.035***	0.054***	−0.402***	−0.175	−0.444***	−0.215*
	(0.014)	(0.025)	(0.011)	(0.018)	(0.081)	(0.118)	(0.086)	(0.113)
Broad Money Ratio	0.012**	0.039***	0.005	0.021***	0.116***	0.036	0.113***	0.079**
	(0.006)	(0.010)	(0.005)	(0.007)	(0.033)	(0.046)	(0.031)	(0.037)
Financial Openness	−0.033***	−0.018**	−0.026***	−0.007	−0.006	−0.072*	−0.028	−0.072*
	(0.006)	(0.009)	(0.005)	(0.006)	(0.038)	(0.040)	(0.044)	(0.037)
Credit Deregulation	0.004***	0.007***	0.002*	0.004***	−0.011	−0.021**	−0.010	−0.015
	(0.001)	(0.002)	(0.001)	(0.002)	(0.007)	(0.010)	(0.006)	(0.010)
Real House Prices	0.002	−0.007	−0.003	−0.014***	0.164***	0.175***	0.177***	0.207***
	(0.004)	(0.006)	(0.003)	(0.004)	(0.025)	(0.035)	(0.025)	(0.034)
Housing GFCF Ratio	−0.110***	−0.120***	−0.086***	−0.093***	0.225*	0.552***	0.324**	0.565***
	(0.020)	(0.020)	(0.014)	(0.015)	(0.120)	(0.107)	(0.130)	(0.098)
Quantification								
$\beta_{Ineq} * SD_{within}$					0.031	0.049	0.047	0.047
$KPF - stat$					25.992	25.527	17.772	26.358
Obs.	726	467	726	467	726	467	726	467
Countries	30	27	30	27	30	27	30	27

Robust standard errors are in parentheses. All specifications include country fixed effects and year dummies. The critical value for the weak-instruments test is based on a 10% 2SLS size at the 5% significance level, which is 22.3 in all estimations. For the 5% 2SLS bias at the 5% significance level, it is 13.9. *, **, and *** denote, respectively, significance at the 10%, 5%, and 1% levels.

openness has a positive impact on the ratio of private credit to GDP (at least, until the 2007-2008 financial crisis), and that credit deregulation also has a positive impact on bank credit to GDP for the whole period. These two credit aggregates also encompass corporate credit, suggesting that beyond a certain level of development and/or financial liberalization, the expansion of credit as a share of GDP benefits mainly firms, rather than households.

In addition, Tables E.3 and E.4 (columns (1) to (4)) in the OA report estimates for a specification in which the Gini index and the top 10% are interacted alternatively with financial deregulation and financial openness. If the interactions with the former bring mostly insignificant results, those with the latter tend to show a positive impact. This result is consistent with a supply-side effect: if additional inequality triggers more demand for credit through the various channels surveyed in Section 2, financial openness magnifies this effect by bringing capital in the country, allowing an increase in credit supply.

As expected, real estate controls (house prices and housing GFCF) are both positively associated with household leverage. Finally, the latter moves in the same direction as the world business cycle (world GDP and oil prices) and financial cycle (a decrease in the Fed rate brings additional country-year household credit, the VIX index appearing insignificant in all estimates).[38]

Regarding the size of the effects, a one-standard-deviation increase in the Gini index is associated with a 5.3–6.8 (all period) to a 7.5–8 (pre-financial crisis) percentage point (pp) increase in the household-credit-to-GDP ratio. When inequality is measured through the top-income share, we find that a one-standard-deviation increase lifts the credit-to-GDP ratio by almost identical figures over the whole period (+5.3 to +7.2 pp), but slightly higher amounts for the pre-2008 financial-crisis period (+8.6 to +9.7 pp).

In addition, Table 6 reports estimates where inequality is instrumented not only with the number of ratified ILO conventions, but also with interactions between the latter and time dummies for specific time periods (1973–1977, 1995–2008) corresponding to particular waves of ratifications that are specifically explained by ILO's internal dynamics (see Section 4.2). The first-stage results (columns (1) to (4)) confirm the latter bring their own, specific reduction in inequality, especially

[38] Because the specification with year dummies is more saturated, it clearly dominates the one that controls for common trends using observable variables. Therefore, for the sake of space, following tables reporting second-stage results are exclusively based on the specification including year dummies. Results are mostly unchanged for estimates with observable time controls and are available in the OA or upon request.

Table 7
TR 1: Alternative Instrument Sets.

Dep. Var.	(1) Household Credit/GDP	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gini	9.561**	3.249***	3.555***	3.728***				
	(3.821)	(0.949)	(0.768)	(0.788)				
Top 10					17.691**	3.391***	3.919***	3.765***
					(8.295)	(0.923)	(0.857)	(0.846)
GDP per capita	−0.935**	−0.415***	−0.440***	−0.455***	−1.347**	−0.377***	−0.413***	−0.403***
	(0.400)	(0.149)	(0.147)	(0.151)	(0.611)	(0.126)	(0.128)	(0.126)
Broad Money Ratio	−0.252	0.020	0.007	−0.001	−0.253	0.080*	0.068	0.072*
	(0.176)	(0.057)	(0.051)	(0.052)	(0.205)	(0.045)	(0.044)	(0.044)
Financial Openness	0.162	−0.020	−0.011	−0.006	0.288	−0.036	−0.024	−0.028
	(0.131)	(0.049)	(0.049)	(0.050)	(0.208)	(0.042)	(0.043)	(0.042)
Credit Deregulation	−0.118***	−0.057***	−0.060***	−0.061***	−0.121***	−0.044***	−0.046***	−0.046***
	(0.039)	(0.014)	(0.013)	(0.013)	(0.047)	(0.011)	(0.010)	(0.010)
Real House Prices	0.302***	0.236***	0.239***	0.241***	0.483***	0.256***	0.264***	0.261***
	(0.072)	(0.040)	(0.040)	(0.041)	(0.148)	(0.037)	(0.037)	(0.037)
Housing GFCF Ratio	1.164***	0.502***	0.534***	0.553***	1.718**	0.460***	0.506***	0.493***
	(0.451)	(0.138)	(0.131)	(0.134)	(0.791)	(0.113)	(0.115)	(0.114)
Quantification								
$\beta_{Ineq} * SD_{within}$	0.189	0.064	0.070	0.074	0.260	0.050	0.058	0.055
Instruments	Agri	$\frac{K}{L}$+School	$\frac{K}{L}$+School + ILO	Agri+$\frac{K}{L}$ + School+ILO	Agri	$\frac{K}{L}$+School	$\frac{K}{L}$+School + ILO	Agri+$\frac{K}{L}$ + School+ILO
$KPF-stat$	9.562	27.068	27.639	20.662	5.374	49.355	40.534	32.714
$KPF\ size\ -crit.value$	16.38	19.93	22.3	24.58	16.38	19.93	22.3	24.58
$KPF\ bias\ -crit.value$			13.91	16.85			13.91	16.85
$Hansen-stat$		0.050	0.422	5.264		0.171	3.686	11.081
$Hansen-p-value$		0.823	0.810	0.153		0.679	0.158	0.011
Obs.	483	483	483	483	483	483	483	483
Countries	20	20	20	20	20	20	20	20

Robust standard are errors in parentheses. All estimations include country fixed effects and year dummies. The critical values for the weak-instruments test are based on a 10% 2SLS size and a 5% IV bias at the 5% significance level. See Table B.3 in Appendix B for first stage results. *, **, and *** denote, respectively, significance at the 10%, 5%, and 1% levels.

the 1973–1977 wave, with the 1995–2008 displaying a much more modest impact. In any case, this specification including explicitly the impact of ratifications waves does not bring any significant alterations to our second-stage results (columns (5) and (6) report those for the Gini; those for the top 10% are reported in columns (7) and (8)).

Finally, we check how estimates behave with alternative inequality datasets: the SWIID (for which we can retrieve Gini indexes but no income deciles) and the WID, which contains information on income deciles based on administrative tax data (rather than the survey data used in WIID). The WID is especially useful for investigating our second testable relationship that the relative impoverishment of the middle class compared with top-income households matters quantitatively more for the evolution of the household leverage than the impoverishment of low-income households relatively to top-income households (see Section 5.2 below). Results are reported in Tables C.2 (first stage), C.3 (second stage), C.4 (second stage restricted to the pre-2008 period), and C.5 (second stage including Japan, for which data were not available in WIID, in the sample) in the OA. Qualitatively and quantitatively, our results are basically unchanged compared with those stemming from our baseline estimates.

Instrumental variables (2): Factor endowments. In this section, we check the consistency of our results with alternative instruments that are also very unlikely to be correlated with globalization trends or country-level policy packages. As detailed in Section 4.2 above, we use three variables proxying for factor endowments: the agricultural land share, the ratio of net capital stock to total hours worked, and the average number or years of total schooling; see Table A.1 in the Data Appendix A for more details, and Table F.2 in the OA for the time coverage by country. Table B.3 in Appendix B reports first-stage coefficients. The latter emphasize that alternative instruments have mostly the expected effects on inequality indicators.

Table 7 reports estimates of Eq. (3), where the Gini (columns (1) to (4)) and the top 10 (columns (5) to (8)) are instrumented with these alternative instruments, as well as combinations of the latter with the number of ratified ILO conventions. We start with the agricultural land share as a single instrument (columns (1) and (5)). In columns (2) and (6), inequality measures are instrumented using a combination of the ratio of capital to total hours worked and the average number of years of total schooling. Columns (3) and (7) include the number of ratified ILO conventions together with the latter (capital/hours worked and years of schooling) in the set of instruments, and finally columns (4) and (8) use all four instruments by adding the agricultural land share.

In all estimations, the coefficients on inequality measures are positive and significant, and, in specifications relying on more than one instrument, the Hansen test cannot reject our overidentifying restrictions (except in column (8), but the

Table 8
Testable Relationship (TR) 2: Baseline Estimates.

	(1)	(2)	(3)	(4)
Dep. Var.	Household Credit/GDP			
Ineq. measure	$\frac{Top\ 10}{Mid.\ 30-90}$	$\frac{Top\ 10}{Bot.\ 0-30}$	$\frac{Top\ 10}{Mid.\ 50-90}$	$\frac{Top\ 10}{Bot.\ 0-50}$
Ineq. measure	2.039***	0.222***	1.874**	0.526***
	(0.782)	(0.077)	(0.753)	(0.185)
GDP per capita	−0.466***	−0.414***	−0.492***	−0.419***
	(0.095)	(0.083)	(0.104)	(0.084)
Broad Money Ratio	0.119***	0.111***	0.114***	0.118***
	(0.033)	(0.034)	(0.034)	(0.032)
Financial Openness	0.064	0.030	0.080	0.031
	(0.058)	(0.046)	(0.065)	(0.046)
Credit Deregulation	−0.010	−0.011*	−0.010	−0.010
	(0.007)	(0.007)	(0.007)	(0.006)
Real House Prices	0.174***	0.151***	0.181***	0.156***
	(0.027)	(0.025)	(0.028)	(0.025)
Housing GFCF Ratio	0.376**	0.180	0.442**	0.216*
	(0.162)	(0.116)	(0.190)	(0.119)
Quantification				
$\beta_{Ineq} * SD_{within}$	0.057	0.039	0.066	0.04
Quantif. *Middle/Bottom*	1.462		1.650	
KPF − stat	20.275	44.453	15.328	41.658
Obs.	698	698	698	698
Countries	29	29	29	29

Robust standard errors are in parentheses. All specifications include country fixed effects and year dummies. The critical value for the weak-instruments test is based on a 10% 2SLS size at the 5% significance level, which is 16.4 in all estimations. See Table D.1 in the OA for first-stage results, and Table D.2 in the OA for estimates restricted to the period before 2008. *, **, and *** denote, respectively, significance at the 10%, 5%, and 1% levels.

latter reports estimates that are almost identical to those in columns (6) and (7)). This observation indicates that in almost all cases, the orthogonality of the overidentifying instruments and the error term cannot be rejected; thus, these various sets of instruments are appropriate on that ground. These results suggest that regardless of the (exogenous) shock causing them, variations in inequality are positively related to the variation of household leverage. The coefficients and resulting quantifications are quantitatively larger in the estimations using the agricultural land share as a single instrument (columns (1) and (5)), but our estimates are also noisier, with higher point estimates together with higher standard deviations - this instrument is arguably more exogenous, but also weaker because time variations are limited. In all other estimations (columns (2) to (4), and (6) to (8)), estimated parameters and quantifications are similar to those found in Table 5.

Lastly, we propose two additional robustness checks in this area. First, we add real house prices and housing GFCF ratio to the set of instrumented variables, together with the inequality indicators, without any perceptible effect on our results (see Table C.6 in the OA).[39] Second, we propose alternative control variables: other proxies for financial openness such as *de facto* financial openness, that is, the ratio (External Assets + External Liabilities)/GDP, or gross portfolio investments to GDP; the short-term real interest rate as an additional control, together with money supply, for monetary-policy stance; and the long-term real interest rate to control for term premium (see Table E.7 in OA for data sources). Again, our results remain unchanged (see Table E.8 in OA).

5.2. The key role of middle incomes

An important insight from the various strands of the literature surveyed in Section 2 is that for a given inequality shock increasing the share of top incomes relatively to incomes below, middle incomes should contribute more to the variation of household leverage than low incomes. We test this intuition in this section.[40]

Baseline estimates. Table 8 shows our main results, with column (1) relying on the ratio of top 10% incomes to middle ones (share of the third to the ninth decile) as the inequality indicator, whereas column (2) is dedicated to the ratio of top 10% incomes to bottom ones (share of incomes up to the third).[41] In columns (3) and (4), we replicate the exercise by

[39] We choose ILO convention, the capital/labor intensity, and the number of years of schooling as instruments for our three instrumented variables. We do not use arable land, which appears to be a very weak predictor of real estate variables.

[40] Note the sample under study is identical to the one used in Section 5.1 but for New Zealand, which had to be excluded for lack of required data on income deciles. We checked that this slight change in the sample does not affect results from Table 5. Table C.1 in OA shows the results using this sample without New Zealand. Results are unchanged.

[41] First stage is shown in Table D.1. in the OA.

Table 9
TR 2: Alternative Instrument Sets.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	Household Credit/GDP							
$\frac{Top\ 10}{Mid.\ 30-90}$	6.325**	4.592**	2.751***	3.228***				
	(2.757)	(2.013)	(0.769)	(0.869)				
$\frac{Top\ 10}{Bot.\ 0-30}$					0.857**	0.537**	0.315***	0.357***
					(0.433)	(0.259)	(0.076)	(0.083)
GDP per capita	−1.014**	−0.771**	−0.513***	−0.580***	−0.711**	−0.493**	−0.341***	−0.370***
	(0.417)	(0.312)	(0.164)	(0.179)	(0.347)	(0.217)	(0.121)	(0.127)
Broad Money Ratio	−0.085	−0.015	0.060	0.040	−0.152	−0.031	0.052	0.037
	(0.128)	(0.098)	(0.050)	(0.055)	(0.180)	(0.114)	(0.049)	(0.052)
Financial Openness	0.183	0.106	0.025	0.046	0.196	0.087	0.012	0.026
	(0.142)	(0.107)	(0.057)	(0.062)	(0.159)	(0.104)	(0.052)	(0.055)
Credit Deregulation	−0.081***	−0.065***	−0.049***	−0.053***	−0.094**	−0.068***	−0.049***	−0.053***
	(0.028)	(0.021)	(0.011)	(0.012)	(0.039)	(0.025)	(0.012)	(0.012)
Real House Prices	0.404***	0.343***	0.278***	0.295***	0.245***	0.221***	0.205***	0.208***
	(0.111)	(0.084)	(0.045)	(0.048)	(0.057)	(0.043)	(0.036)	(0.037)
Housing GFCF Ratio	1.270**	0.965**	0.641***	0.725***	0.678**	0.484**	0.349***	0.374***
	(0.534)	(0.401)	(0.182)	(0.201)	(0.305)	(0.203)	(0.116)	(0.120)
Quantification								
$\beta_{Ineq} * SD_{within}$	0.185	0.134	0.080	0.094	0.157	0.099	0.058	0.066
Quantif. *Middle/Bottom*	1.174	1.359	1.389	1.438				
Instruments	Agri	$\frac{K}{L}$+ Agri	$\frac{K}{L}$+ ILO	Agri+$\frac{K}{L}$ +ILO	Agri	$\frac{K}{L}$+ Agri	$\frac{K}{L}$+ ILO	Agri+$\frac{K}{L}$ +ILO
$KPF-stat$	7.048	4.201	12.912	8.925	6.296	4.634	27.482	18.641
$KPF\ size\ -crit.value$	16.38	19.93	19.93	22.3	16.38	19.93	19.93	22.3
$KPF\ bias\ -crit.value$				13.91				13.91
$Hansen-stat$		2.525	1.285	3.996		3.203	1.583	4.324
$Hansen-p-value$		0.112	0.257	0.136		0.074	0.208	0.115
Obs.	462	462	462	462	462	462	462	462
Countries	19	19	19	19	19	19	19	19

Robust standard errors are in parentheses. All estimations include country fixed effects and year dummies. The critical values for the weak-instruments test are based on a 10% 2SLS size and a 5% IV bias at the 5% significance level. See Table D.6 of the OA for first-stage results. *, **, and *** denote, respectively, significance at the 10%, 5%, and 1% levels.

Table 10
Falsification Test: Emerging Countries.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	Household Credit/GDP							
Sample	All	Bef. 2008	All	Bef. 2008	All	Bef. 2008	All	Bef. 2008
Ineq. Measure	Gini	Gini	Top 10	Top 10	$\frac{Top\ 10}{Mid.\ 30-90}$	$\frac{Top\ 10}{Mid.\ 30-90}$	$\frac{Top\ 10}{Bot.\ 0-30}$	$\frac{Top\ 10}{Bot.\ 0-30}$
Ineq. Measure	1.849**	0.844	3.155*	1.234	1.617	0.463	0.167	0.034
	(0.893)	(0.873)	(1.751)	(1.346)	(1.020)	(0.518)	(0.123)	(0.036)
GDP per capita	0.162***	−0.019	0.185***	−0.038	0.183**	−0.059	0.175**	−0.061
	(0.052)	(0.076)	(0.067)	(0.086)	(0.074)	(0.094)	(0.079)	(0.086)
Broad Money Ratio	0.359***	−0.013	0.371***	−0.005	0.386***	−0.010	0.443**	−0.017
	(0.086)	(0.067)	(0.103)	(0.062)	(0.124)	(0.066)	(0.172)	(0.071)
Financial Openness	−0.049**	0.023	−0.066**	0.018	−0.064*	0.024	−0.025	0.047
	(0.023)	(0.015)	(0.032)	(0.016)	(0.036)	(0.015)	(0.028)	(0.031)
Credit Deregulation	−0.010	−0.012	−0.010	−0.012	−0.013	−0.012	−0.027	−0.014
	(0.008)	(0.014)	(0.010)	(0.014)	(0.013)	(0.014)	(0.025)	(0.015)
Real House Prices	0.094*	0.092	0.132*	0.105	0.166	0.097	0.216	0.089
	(0.048)	(0.066)	(0.075)	(0.081)	(0.107)	(0.076)	(0.164)	(0.065)
$KPF-stat$	8.562	3.039	4.915	1.985	3.185	1.971	2.028	2.735
Obs.	260	110	260	110	260	110	260	110
Countries	19	15	19	15	19	15	19	15

Robust standard errors are in parentheses. All specifications include country fixed effects and year dummies. The critical value for the weak-instruments test is based on a 10% (resp. 15%) 2SLS size at the 5% significance level, which is 16.4 (8.96) in all estimations. *, **, and ***, denote, respectively, significance at the 10%, 5%, and 1% levels. Inequality variables are instrumented by the number of ILO conventions.

changing the definition of middle and low incomes, using the fifth decile as the threshold between bottom- and middle-incomes. Qualitatively, we still find strong support for the positive impact of exogenous variations of inequality on household leverage.[42] Results are qualitatively similar when we use an alternative definition of middle incomes starting with the fifth decile (columns (3) and (4)).[43]

Quantifications confirm the intuitions wrapped in our second testable relationship (TR 2): following a one-standard-deviation increase in the various income-inequality measures, household credit/over GDP increases by around 4 pp when bottom incomes are hit (see columns (2), and (4)), whereas this increase stands between 5.7 and 6.6 pp when middle incomes are hit (see columns (1), and (3)). In other words, a one-standard-deviation increase in the ratio of top incomes to middle incomes (implying a relative impoverishment of the middle class compared with the top 10%) leads to an increase in household credit to GDP equivalent to 1.5-1.7 times that one stemming from an increase in the ratio of top 10% to the bottom incomes; see the row "Quantif. *Middle/Bottom*" at the bottom of the table.[44] Interestingly, this order of magnitude of 1.5–1.7 for the additional impact of an inequality shock hitting the middle class corresponds to the lower bound found for the gap regarding the debt-to-income ratios reported in Section 2.2, found to be 1.5–4 times higher for middle incomes than for low incomes in a vast majority of countries.

Finally, as we did in the previous section for our first testable relationship, we check how our results behave with alternative data. Together with the underlying first-stage estimates reported in Table D.4 in the OA, Table D.5 in the OA reports estimates based on data from the WID (the SWIID does not provide information on income deciles). Once again, results are very similar to those reported in Table 8: a one-standard-deviation increase in the ratio of top incomes to middle incomes (meaning an impoverishment of middle classes relative to the top 10%) delivers an increase in household credit to GDP ranging from 1.6 to 1.8 times the one stemming from an increase in the ratio of the top 10% to the bottom incomes. This finding clearly strengthens our point regarding the stronger impact of income-inequality shocks hitting middle classes (compared with bottom incomes).

Alternative instrument sets. We keep following the strategy implemented in Section 5.1, and assess if our results are robust to alternative types of exogenous inequality sources. We rely on two of the variables proxying for factor endowments used previously: agricultural land share and net capital/hours worked. We had to remove the average number of years of schooling in order to preserve the validity of overidentifying restrictions. More precisely, the first four columns of Table 9 are devoted to the ratio of top 10% to middle incomes, while the following four focus on the ratio of the top 10% to the bottom incomes. Columns (1) and (5) rely again on the agricultural land share as a single instrument. Columns (2) and (6) add to the previous variable the ratio of capital to total hours worked. In columns (3) and (6), we rely on a combination of the ratio of capital to total hours worked and the number of ratified ILO conventions. Finally, columns (4) and (8) add the agricultural land share to the previous set of instruments.[45]

On the whole, even though instruments are a bit weaker in a couple of specifications, estimates are completely consistent with those reported in Tables 7 and 8. The coefficients on both inequality measures are positive and significant, and once again, in specifications featuring more than one instrument, Hansen's J-test statistics of overidentifying restrictions are insignificant in all cases but in column (6), for which the null hypothesis that the over-identifying restrictions are valid cannot be rejected at the 5% threshold, though. More importantly, when the number of ratified ILO conventions is included in the set of instruments, Hansen's J-test statistics tend to be even more insignificant, supporting the orthogonality of this specific instrument with the error term.

Again, the estimated coefficients and quantifications are a bit noisier in the specifications relying only on the agricultural land share as a single instrument. This is also the case, to a lesser extent, for those relying on the combination between agricultural land share and the ratio of capital stock to hours worked. But in general, all estimates, and more importantly, resulting quantifications, are very similar to those shown in Table 8: a one-standard-deviation increase in the ratio of the top incomes to middle incomes (implying a relative impoverishment of middle classes compared to the top 10%) leads to an increase in household credit to GDP mostly around 1.4 times the one stemming from an increase in the ratio of the top 10% to the bottom incomes - see the row "Quantif. Middle/Bottom" at the bottom of the table. This order of magnitude is very close to, though slightly lower than, the one found in our benchmark estimates (1.5–1.7, see Table 8 above and related comments). This finding brings additional support to our point regarding the stronger impact of income-inequality shocks hitting the middle class (compared with a similar shock hitting the bottom incomes).[46]

[42] As we did previously, we report in Tables E.5 and E.6 (columns (1) to (4)) in the OA estimates including interactions between financial deregulation and financial openness on the one hand, and our income-inequality indicators on the other hand. Estimates are on the whole very noisy, though they remain consistent with the idea that the effect of inequality on household leverage is magnified by financial openness, that is, by the relaxation of credit constraints.

[43] Table D.2 in the OA reports estimates for the sample restricted to the pre-financial crisis period, without any significant alteration. Table D.3 in the OA reports estimates where interactions between the number of ILO conventions ratified and the two waves are included in the set of instruments, with very similar results. Table E.2 in the OA presents results without country fixed effects. Once again, our main result holds.

[44] These multipliers come from the ratio of quantifications in strictly comparable specifications; for example, for columns (1) and (3), 0.057/0.039 gives 1.462.

[45] First-stage results are presented in Table D.6 in the OA.

[46] Lastly, we also show our results are not affected (1) when instrumenting real house prices and housing GFCF ratio (Table D.10 in OA) or (2) when alternative control variables are included (Table E.9 in OA).

5.3. Falsification test: emerging and developing economies

Kumhof et al. (2017) highlight that the credit constraints are so high in the emerging world that potential borrowers have little access to (too narrow or even non-existent) domestic financial markets, and no access to international ones. In these countries, domestic top-income households cannot lend to those at the bottom, and are constrained to "deploy all their additional savings abroad," leading to current account surpluses. In our context, this less developed financial system in emerging countries imply less available credit on the supply side.

On the demand side, the various theoretical mechanisms put forward in Section 2 may also be less at play in economies where bottom incomes represent a more homogenous category, and are much too far below the top-income group for relative-consumption approaches to apply. Put differently, because the middle class in emerging and developing economies is not as developed as it is in advanced economies (see Kochhar, 2015), the quantitative importance of the ratio of top incomes to middle incomes to explain the aggregate dynamics of credit should not materialize, or at least be seriously dampened. This point is important because a key result of this paper is the part played by the impoverishment of middle incomes relative to top incomes in boosting household leverage.

Therefore, we propose, as a falsification test, checking that the positive causal link from inequality to household credit, and consequently the major part of the middle class in the latter, exists if and only if the country is sufficiently developed. We can bring this intuition to the data by estimating again our empirical model on an alternative sample focusing exclusively on emerging economies (full details about the composition of this sample are in Table F.1 in the OA). Note that due to data limitations, we cannot include the ratio of housing GFCF to GDP in the estimations. Table 10 reports the results of these exercises, using ILO conventions as an instrument.[47] Columns (1) and (2) focus on the Gini index, whereas columns (3) and (4) deals with the top 10%. Columns (5)/(6) and (7)/(8) report estimates for the ratio of the top 10% to, respectively, the middle and bottom incomes.

In all specifications, estimated parameters on the different income-inequality indicators are correctly signed (positive), but significance is much lower, with an IV appearing mostly weak. Gini and the top 10% have a positive impact on household credit at the 5% and 10% significance level, respectively (columns (1) and (3)). Coefficients are not significant when the sample is restricted before 2008 (columns (2) and (4)). Effects of the ratio of the top 10% to middle incomes and the ratio of the top 10% to the bottom incomes also appear insignificant as well (columns (5) to (8)).

Conversely, GDP per capita emerges as a positive and significant determinant in specifications estimated over the whole period. A possible interpretation suggests that, at an early stage of economic development, credit constraints are so binding that only an increase in average wealth per capita can ease access to credit; after a certain threshold of development, however, credit constraints become less binding (as suggested by the sign reversion on GDP per capita on our main sample, see, e.g., Table 5), and the inequality mechanisms driving up household credit to GDP suggested by the various theoretical frameworks surveyed in Section 2 start working.

We also investigate further the role of credit constraints in emerging economies, by examining the heterogenous response of household credit to inequality according to the degree of financial deregulation and international financial openness. Columns (5) to (8) in Tables E.3 to E.6 in the OA replicate the exercise already implemented on our main sample of developed countries. As for the latter, the results are quite noisy and insignificant regarding interactions with financial deregulation. Conversely, estimates tend to show in several specifications that emerging countries displaying a sufficient level of openness to international capital flows do exhibit a positive impact of inequality on household credit. These results again support the idea of a relaxation of credit constraints by incoming financial flows, allowing wider categories of the population to access credit, and consequently, to react to variations in inequality.

6. Conclusion

Based on a country-level yearly dataset combining household credit and detailed information on income distribution from the WIID database over the period 1970–2017, this paper shows that increases in various indicators of income inequality driven by different exogenous sources trigger expansions of household credit, and this effect is substantially higher when top incomes grow richer at the expense of the middle class, rather than at the expense of low incomes.

Our empirical strategy first identifies country-level variations in income inequality driven by exogenous changes in the number of ratified ILO conventions at the country-year level. We show such exogenous increases in income inequality deliver additional household leverage, in a setup accounting for many other relevant determinants of credit, including some controlling directly for improvement in standards of living and ability to borrow, such as housing investment and GDP per capita. We also find the impact is magnified when middle incomes, rather than low incomes, are impoverished relative to top incomes. We confirm these results using additional instruments representing country-level factor endowments (agricultural land share, capital intensity, mean years of schooling), different databases (SWIID and WID), definitions of income groups, and control variables. We also check throughout the paper that our results are not importantly altered by the period following the 2007-2008 financial crisis.

[47] Fig. A.4. in the OA reports a negative correlation between the evolution of income inequality and household debt on our sample of developing countries. This negative correlation, which can credibly be explained by the negative bias arising from reverse causality detailed in Section 4.2, illustrates the difference with developed countries, for which Fig. 1 shows a positive correlation.

Our findings are extremely robust to all these sensitivity exercises. An exogenous one-standard-deviation increase in the Gini index and top 10% income share generates, respectively, a 5–8 pp and 5–10 pp expansion in the ratio of household credit to GDP. In addition, the impact is 1.5–1.8 times stronger when top incomes increase relative to middle incomes, rather than at the expense of bottom incomes. Interestingly, when replicating our estimates on a sample exclusively based on developing/emerging countries, we find all these effects vanish, consistently with binding credit constraints preventing bottom incomes from accessing credit and insufficiently important middle-income categories.

Our work has important implications regarding financial-crises prevention. To avoid financial crises such as the one of 2007–2008, one has to therefore prevent the creation of household leverage bubbles. Our findings suggest the reduction in inequality is an important prerequisite of such a policy, especially at the middle of the income distribution.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.euroecorev.2020.103629.

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