The Distributional Effects of Government Spending Shocks in Developing Economies

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Davide Furceri, Jun Ge, Prakash Loungani and Giovanni Melina[1]

International Monetary Fund

Abstract

We construct unanticipated government spending shocks for 103 developing countries from 1990 to 2016 and study their effects on income distribution. We find that unanticipated fiscal consolidations lead to a long-lasting increase in income inequality, while fiscal expansions lower inequality. The results are robust to several measures of income distribution and size of the fiscal shocks, across expansions and recessions and across country groups (low-income countries versus emerging markets). The effect is larger for government investment than government consumption, likely due to the stronger medium-run effect of the former on output and employment.

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[1] Davide Furceri: dfurceri@imf.org; Jun Ge: jge@imf.org; Prakash Loungani: ploungani@imf.org; Giovanni Melina: gmelina@imf.org. International Monetary Fund, 700 19th Street, N.W., Washington DC 20431, United States. We are grateful to … and seminar participants at IMF and the G24 meeting on macroeconomic policy and income inequality for comments and suggestions. This working paper is part of a research project on macroeconomic policy in low-income countries supported by U.K.'s Department for International Development. The views expressed in this Working Paper are those of the authors and do not necessarily represent those of the IMF, IMF policy, or of DFID.

I. INTRODUCTION

Over the last two decades, inequality has risen in many emerging and developing economies (EMDEs) (Figure 1), including populous countries such as China and India, and remains stubbornly high in others. Although in Latin America and Sub-Saharan Africa the Gini coefficient slightly decreased on average, its level in these regions remains among the highest in the world (see e.g. IMF, 2014). At the same time, public debt-to-GDP ratios have recently increased in many EMDEs because of weaker economic activity and subdued commodity prices. Hence, many EMDEs are facing the challenge of addressing high and/or rising inequality while maintaining/regaining fiscal sustainability.

In this context, a very relevant empirical question is: What is the effect of government spending shocks on income distribution? Answering this question is important because a worsening in income inequality could reduce the political support for the government to implement consolidation measures, but also because high levels of inequality could harm long-run growth (Berg and Ostry, 2017).

While there is a growing body of literature on the distributional effects of fiscal policy for advanced economies, empirical evidence for EMDEs is scant and fragmented. A key reason is a methodological challenge, that is, the difficulty of identifying changes in fiscal variables that are uncorrelated with contemporaneous macroeconomic shocks, and hence that can be considered as exogenous.

The literature has so far proposed four main approaches to overcome this issue. The first is the "natural experiment" approach proposed by Barro (1981) and further developed by Ramey in subsequent papers (Ramey and Shapiro, 1998; Ramey, 2011a; Ramey, 2011b; Owyang et al., 2013). This approach uses fluctuations in military spending to identify government spending shocks. The second is the structural vector autoregressions (SVARs) approach developed by Blanchard and Perotti (2002), and applied to some emerging economies by Ilzetzki et al. (2013), where exogenous fiscal shocks are identified by assuming that government spending is unlikely to respond to unexpected macroeconomic shocks within the same quarter. The third method is the one proposed by Kraay (2012) and Kraay (2014) for developing countries, which uses loans from official creditors as exogenous sources of fluctuations in government spending. Last, the fourth identification scheme, which we also adopt in our paper, is the one proposed by Auerbach and Gorodnichenko (2013a, 2013b)—AG henceforth—which identifies government spending shocks as the forecast errors in government spending.[2]

In this paper, we adopt the AG approach for two reasons. First, data limitations (including lack of government spending data at quarterly frequency) preclude the natural experiment and the SVAR approaches for a large set of emerging and developing economies.[3] The second reason is that this approach overcomes the problem of "fiscal foresight" (see Forni and Gambetti, 2010; Leeper et al., 2012; Leeper et al., 2013; and Ben Zeev and Pappa, 2015). Agents receiving news about changes in government spending in advance may alter their consumption and investment decisions well before the changes occur. An econometrician who uses the information contained in the change in actual spending would be relying on a different information set than that used by economic agents, which may lead to biased estimates. By

[2] See Abiad et al. (2015) and Furceri and Li (2017) for an application of this approach to the macroeconomic effects of public investment shocks in advanced and developing economies, respectively.

[3] Ilzetki et al. (2013) assembled quarterly data on government spending for 24 emerging market economies. In contrast, the methodology chosen in the paper allows us to cover an unbalanced sample of 103 EMDEs (see Table A1 in the Appendix for the list of countries and dates in which observations are available).

using forecast errors, the econometrician's information is aligned to that of economic agents. In addition, as stressed by Ramey (2011b), it is likely that private agents respond to unanticipated changes of government spending, rather than to actual changes. Therefore, considering exogenous actual changes in government spending may underestimate its impact.

After identifying government spending shocks, we use the local projections approach of Jordà (1995) to trace out the short- and medium-run responses of a set of inequality measures for a panel of 103 EMDEs over the period 1990-2016.

The paper's main results can be summarized as follows. An unanticipated fiscal contraction leads to a long-lasting decrease in net income inequality. Translating this effect in terms of changes in the spending-to-GDP ratio suggests that a reduction in government spending of 2 percent of GDP increases the Gini index by 2.74 percent. This effect is economically significant, given that the Gini coefficient is quite stable over time, and it corresponds to about 1 standard deviation of the average change in the Gini coefficient in our sample.

The results are robust to several measures of income distribution, sign and size of the fiscal shocks, across expansions and recessions, and across country groups (low-income countries versus emerging markets). The effect is larger for government investment than government consumption. One possible reason for this result is the stronger stimulus investment gives to the economy. In fact, our estimates suggest not only that public investment has a larger output multiplier than government consumption—confirming results by Furceri and Li (2017) for a similar set of countries—but also a larger effect on employment, a key driver of inclusion.

The paper contributes to the literature on the effects of fiscal policy shocks on income distribution and inequality. One obvious feature that differentiates our study from the bulk of contributions on the topic is that we focus on EMDEs while almost all other papers look at advanced economies. Ball et al. (2013) use episodes of fiscal consolidation for a sample of 17 OECD countries over a period approximately coinciding with the Great Moderation and find that fiscal consolidation has typically had significant distributional effects by raising inequality, decreasing the labor share of income and increasing long-term unemployment. Also drawing on evidence for OECD countries, Woo et al. (2017) find that spending-based adjustments tend to worsen inequality more significantly—relative to tax-based adjustments—and that progressive taxation and targeted social benefits can help offset some of the adverse distributional impacts. These results echo those of Agnello and Sousa (2014), who find that, in industrialized economies, income inequality significantly rises during periods of expenditure-driven fiscal consolidations and that tax hikes have an equalizing effect. The closest contribution to ours is that of Furceri and Li (2017), which looks at the macroeconomic effects of public investment in developing economies using a similar methodology, and provides a fist attempt in examining the effect of public investment shocks on inequality. While their focus is on the determinants of the public investment multiplier, we take a broader approach and compare the effects of various types of government spending shocks on several measures of inequality. Relative to the studies on advanced economies, we do not investigate the effects of tax shocks. The reason is that our approach is not suitable for this purpose: since revenues automatically responds to changes in economic activity (Blanchard and Perotti, 2002), it is hard to identify exogenous changes in revenues from unexpected changes in economic activity.

The rest of the paper is organized as follows. Section II presents the empirical analysis used to assess the distributional effects of government spending and describes the data. Section III presents the main findings and reports several robustness checks. Finally, Section IV concludes.

II. Data and Empirical Methodology

This section discusses the data and explains the empirical strategy employed in the analysis.

A. Data

In order to estimate the causal effect of government expenditures on inequality, we first need to construct unanticipated government spending shocks. We follow the approach proposed by AG and identify unanticipated changes in government expenditures using forecast errors. In particular, we use the forecast errors from various vintages of the IMF World Economic Outlook (WEO) publications, which have a good time series and cross-sectional coverage of government expenditure forecasts for EMDEs. In this framework, government spending shocks (*FE*) are computed as the difference between the growth rate of actual government spending ($\Delta \ln G$) and the growth rate forecasted by IMF analysts as of October of the same year ($\Delta \ln G^E$):

$$FE_{i,t} = \Delta lnG_{i,t} - \Delta lnG_{i,t}^{E} \qquad (1)$$

where *i* and *t* denote the cross-sectional and time dimension, respectively.

As discussed in Section I, this methodology solves by construction the problem of "fiscal foresight" (see Forni and Gambetti, 2010; Leeper et al., 2012; Leeper et al., 2013; and

Ben Zeev and Pappa, 2015). In the real world, agents receive news about changes in fiscal spending in advance and they may alter their consumption and investment behavior well before the changes occur. An econometrician who uses the information contained in the change of actual government spending would be relying on a different information set than that used by economic agents, which could lead to inconsistent estimates. By using forecast errors, the AG methodology brings the two information sets into closer alignment.

We use the forecasts of government expenditures made in October of the same year to minimize the likelihood that unanticipated changes in government spending do not arise due to the potentially endogenous response of fiscal policy to the state of the economy. In fact, even if shocks are unanticipated, they may still occur in response to business cycle conditions: for example, the government may be forced to cut spending because growth turns out to be unexpectedly weak. However, for this to materially affect our estimates, such adjustments need to happen within the same quarter when news about the state of the economy is received (i.e. between October and December).[4] This is highly unlikely, as amendments to spending typically needs to be passed by parliaments and the legislative process usually takes a few months to be completed (see also Blanchard and Perotti, 2002). In any case, we later show that our findings are robust when controlling for unexpected changes in economic activity.

Figures A1-A3 in the Appendix report the distributions of government spending shocks for EMDEs. The bulk of shocks (between the 1st and the 99th percentile) lie between -41 and

[4] All fiscal and junctural information up to October of a given year is incorporated in the forecasts made in October.

45 percent; between -42 and 49 percent; and between -101 and 95 percent, for total government spending, consumption, and investment, respectively.

Using these measures of unanticipated fiscal shocks, we estimate the average impact of government spending shocks on several measures of income inequality: (i) market Gini; (ii) net Gini, i.e. net of transfers and taxes; (iii) the ratios between incomes of the top 10 percent (and 20 percent) earners and the bottom 10 percent (and 20 percent) earners.[5]

We use the local projection method (Jordà, 2005) to estimate impulse-response functions. This approach has been advocated by Stock and Watson (2007) and AG, among others, as a flexible alternative that does not impose the dynamic restrictions embedded in vector autoregressions (or autoregressive-distributed lag) specifications and it is particularly suited to estimating nonlinearities in the dynamic responses. The baseline regression is specified as follows:

$$y_{i,t+k} - y_{i,t-1} = \alpha_i^k + \vartheta_t^k + \beta^k FE_{i,t} + \theta^k X_{i,t} + \varepsilon_{i,t}^k, \qquad (2)$$

where y is the log of the Gini coefficient; α_i are country fixed effects, included to control for all time-invariant differences across countries (such as countries' trend growth rates); ϑ_t are time fixed effects, included to control for global shocks such as shifts in oil prices or the global business cycle; $FE_{i,t}$ is the government spending shock discussed above; X_{it} is a set a of control variables including two lags of the shocks, as well as two lags of the growth rate of the Gini coefficient; θ^k is a vector of coefficients; and $\varepsilon_{i,t}^k$ is the error term.

[5] Market and net Gini are taken from the Standardized World Income Inequality Database (Solt, 2016). Top and bottom income shares, are from the World Development Indicators (WDI) database of the World Bank.

Equation (2) is estimated for each k = 0,1,…6, where k = 0 is the year when the shock takes place. Impulse-response functions are computed using the estimated coefficients β^k, while the associated confidence bands are obtained using the estimated standard errors of the coefficients β^k, based on clustered robust standard errors at the country level.

III. Results

A. Baseline

Figure 2 presents the results obtained by estimating equation (2) using Gini coefficients for gross and disposable income (market and net Gini, respectively) as measures of inequality. The figure shows the estimated effect of unanticipated government expenditure shocks and the associated 90 percent confidence bands (dashed lines). The results show that an unanticipated fiscal expansion leads to a long-lasting decrease in income inequality, with stronger effects for net inequality. An unanticipated increase in government expenditures of 10 percent curbs market (net) Gini by more than ½ percent (almost 1 percent) after 4 years. At a 6-year horizon the fall in gross (net) inequality amounts to 1 percent (almost 1.5 percent).

These effects are not only statistically significant but also economically important given the high persistence and the low variability of the Gini indices in our sample—the standard deviation of the average change in the market (net) Gini is 3.5 percent (2.5 percent). To put things in perspective a 1-standard-deviation increase in government spending (16 percent in real terms), on average, leads to a fall in the net Gini index of 2.4 percent points in our sample, amounting to almost one standard deviation of the change in the Gini coefficient.

To translate this effect in terms of changes in the spending-to-GDP ratio, we apply the two-step procedure proposed by Ramey and Zubairy (forthcoming), in which changes in the ratio of government spending to GDP are instrumented with the spending shocks identified in the analysis. The results of this exercise suggest that an increase (reduction) of government spending by 2 percent of GDP reduces (increases) the Gini index by 2.7 percent.

These effects are of the same order of magnitude found by Ball et al. (2013) and Agnello and Sousa (2014) for advanced economies.

B. Robustness checks

To check the robustness of the results, several alternative estimations are carried out.[6] First, we check whether the sign of government spending shocks matter—that is, whether the response of inequality varies between fiscal expansions and consolidations. This question is important because, at the current juncture, many EMDEs are carrying out fiscal consolidations. To answer this question, we modify equation (2) as follows:

$$y_{i,t+k} - y_{i,t-1} = \alpha_i^k + \vartheta_t^k + \beta_1^k D_{it} FE_{i,t} + \beta_2^k (1 - D_{it}) FE_{i,t} + \theta^k X_{i,t} + \varepsilon_{i,t}^k, \qquad (3)$$

where *D* is a dummy variable that takes one for positive shocks and zero otherwise. In Figure 3 we report the responses of net inequality to positive and negative shocks, along the baseline average responses. The results suggest that the response of inequality to fiscal consolidation is not statistically different from the response to fiscal expansions. This means that the conclusions we draw from the average results can be applied to both types of fiscal actions.

Second, we check whether the size of the fiscal shock matters—that is, whether the effect of a one-percent exogenous change in government spending on inequality depends on the size of the shock. For this purpose, we estimate two versions of equation (3). In the first, *D*

[6] In the remainder of the paper, we report the response of net inequality to fiscal shocks. Similar results, available upon request, are obtained for gross inequality.

is a dummy variable that takes value one for large positive government spending shocks—greater than the 75th percentile (5%)—and zero otherwise; in the second instance, *D* is a dummy variable that takes value one for large negative government spending shocks—smaller than the 25th percentile (-6%)—and zero otherwise. In Figure 4 we report the responses that large positive shocks and large negative shocks have on net inequality, along the baseline average responses.[7] These results suggest that neither the size nor the sign of the shocks significantly affect the response of inequality to government spending shocks.

Next, we check whether the responses of inequality to fiscal shocks vary across states of the business cycle—that is, depending on whether the economy is in recessions or expansions This question is important given that several contributions (see, e.g., Batini et al., 2012; Auerbach and Gorodnichenko 2013a, 2013b; Blanchard and Leigh 2013; Abiad et al. 2015; Furceri and Li, 2017) find a different output response to fiscal shocks across states of the business cycle. It is also policy relevant at the current juncture, given that several EMDEs are adopting fiscal consolidation measures in a period of weak economic activity. To answer this question, we use a smooth-transition-function approach as in Auerbach and Gorodnichenko (2013b). We modify equation (2) as follows:

$$y_{i,t+k} - y_{i,t-1} = \alpha_i^k + \vartheta_t^k + \beta_1^k G(z_{it}) FE_{i,t} + \beta_2^k \big(1 - G(z_{it})\big) FE_{i,t} + \theta^k X_{i,t} + \varepsilon_{i,t,}^k \qquad (4)$$

with $G(z_{it}) = \frac{\exp(-\gamma z_{it})}{1+\exp(-\gamma z_{it})}, \quad \gamma > 0,$ in which z is an indicator of the business cycle, normalized to have zero mean and unit variance and $G(z_{it})$ is the corresponding smooth transition function.

[7] For comparability purposes, we report the response of inequality to negative fiscal shocks with the opposite sign.

We use real GDP growth as a measure of the business cycle.[8] As discussed in AG, the local projection approach to estimating non-linear effects is equivalent to the smooth transition autoregressive (STAR) model developed by Granger and Teravistra (1993). The main advantage of this approach relative to estimating STVARs for each regime is that it uses a larger number of observations to compute the impulse response functions of only the dependent variables of interest, improving the stability and precision of the estimates. This estimation strategy can also more easily handle the potential correlation of the standard errors within countries, by clustering at the country level.

Figure 5 reports the responses of net inequality to a government spending increase across recessions and expansions, along with the average response. Results point to the evidence that controlling for the state of the business cycle does not produce significantly different results, leading to the conclusion that the *average* results can be applied to both states of the business cycle.

As a fourth robustness check, we look at subsamples along the time dimension. Given that our sample spans between 1990 and 2016, we divide it into two equal parts—taking 2003 as the cut-off year—to verify whether the responses of inequality to government spending shocks vary across the first and the second portion of the sample. The cut-off year is an appropriate one also because many LICs experienced a structural change approximately in the mid-2000s with significantly higher average real GDP growth rates and lower real output and inflation volatilities (see, e.g., Melina and Portillo, 2017). We use a variant of equation (3)

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[8] As in Abiad et al (2015) we set $\gamma = 1.5$. The results do not qualitatively change if we use alternative values of γ.

where *D* is a dummy variable that takes value one for pre-2003 observations and zero for post-2003. The results reported in Figure 6 suggest, except for the response at the 6-year horizon for the post-2003 subsample, that the responses of net inequality to government spending shocks is rather stable across time, with subsample estimates not differing significantly from those obtained for the full sample.

The countries in our sample vary significantly by their level of per-capita income. To check whether the results are different across different income groups, we distinguish between Emerging Markets (EMs) and Low Income Countries (LICs) by using a variant of equation (3), where *D* is now a dummy variable that takes value one for EMs and zero for LICs. Figure 7 shows that the responses of inequality to government spending shocks in LICs and EMEs are not statistically significantly different from each other, as well as from the results for the entire sample.

A possible concern with the identification of government spending shocks is that they may be endogenous to output growth or revenue surprises. While the use of forecasts made in October of the same year mitigates this concern, we check first the robustness of our results to adding current and lagged output growth innovations—defined as the difference between actual GDP growth and the rate forecasted by analysts in October of the same year—as controls.[9] Second, we perform a similar exercise using revenue surprise. Results (Figure 8) are

[9] In practice, we adopt a two-stage approach. First, we regress government spending forecast errors on GDP growth forecast errors and then use the residuals from this regression as our measure of government spending shocks.

very close to those obtained in the baseline when we add output growth surprises, and become even stronger if we add revenue surprises.

Although the use of the forecasts made in October of each year mitigates endogeneity issues, a concern with our identification is that it focuses only on unanticipated changes in government spending occurring in the last quarter of each year and these may be less informative than changes in spending conducted over the entire year. To check whether this is the case, we repeat the analysis using the forecasts made in October of the previous year. The results—also reported in Figure 8—show that the response of inequality is not statistically significantly different from that reported in the baseline.

Finally, we check whether the effects of government spending shocks on inequality are robust to the inclusion of other drivers of income inequality. In particular, similarly to Dabla-Norris et al. (2015), we consider as controls changes in: (i) trade openness (measured as the sum of exports and imports in GDP); (ii) financial openness (defined as the share of total assets and liabilities to GDP); (iii) financial depth (proxied by the private credit-to-GDP ratio); and (iv) financial crises.[10] The results also in this case are very similar to those obtained in the baseline (Figure 9), further validating that the fiscal policy shocks identified in the analysis can be deemed as exogenous.

[10] The source of the data for these variables are respectively: IMF WEO; Lane and Milesi Ferretti (2007); the World Bank Development Indicator; Leaven and Valencia (2012).

C. Components of government expenditures

This section looks at the composition of government expenditures, by disentangling the effects that government consumption and investment have on inequality.[11] To this end, we construct unexpected government consumption and investment shocks—FE^C and FE^I respectively—in an analogous way as we do for total government expenditures (as explained in Section II), and replace *FE* in equation (2) with the newly constructed fiscal shocks, one at a time.

The resulting response functions are reported in Figure 10. An increase in both types of government expenditure, on average, have a persistent and negative effect on inequality, which is statistically significant from a 2-year and 3-year horizon onwards for consumption and investment, respectively. An unexpected increase in government consumption (investment) by 10 percent, in the medium term, reduces net Gini by about 0.5 (0.3) percent, with the effect of investment being similar to that computed by Furceri and Li (2017). Given that public investment is on average only about 30 percent of government consumption, the results also suggest that public investment shocks, on average, have larger medium-term effect on income distribution than government consumption shocks.

Translating this effect in terms of percent points of GDP, using the abovementioned approach, we find that an increase in government consumption (investment) of two percent of GDP reduces the Gini index by 1.04 percent (1.52 percent). The effects of government consumption and investment on inequality are lower than that of total government expenditures

[11] Limited data availability for government transfers forecasts preclude examining the effect of government transfers shocks on inequality.

likely because the latter include transfers, which have a direct redistribution role by directly affecting net inequality.

D. Other distributional measures

The Gini index is the most commonly used measured of income inequality, mostly owing to its wider availability compared to other measures. However, some of the observations in the SWIID database are obtained by model-based imputations and therefore subject to measurement errors (Solt, 2016).[12] To check the validity of our results, we re-estimate equation (2) using the ratios between incomes of the top 10 percent (and 20 percent) earners and the bottom 10 percent (and 20 percent) earners. Figure 11 reports the response functions of the log of these ratios to unexpected shocks to government consumption and investment. Both ratios decline in response to an unanticipated change in public spending. Given the smaller share of public investment on GDP relative to government consumption, the results also suggest that public investment shocks, on average, have larger medium-term effect on the income ratios than government consumption shocks. In addition, while the responses to government consumption become statistically significant only after 6 years, the responses to government investment are significant from a 2-year horizon onwards.

E. Effects on output and employment

One of the reason why public investment has larger medium-term distributional effects than public consumption could be due to larger fiscal multiplier. Another possibility is that

[12] The index is also criticized for being more sensitive to the income of the middle class and not capturing the exact distribution of income as identified by the Lorenz curve.

public investment affects more employment than public consumption. In sum, public investment may boost economic activity to a larger extent and, through wages, this may curb income inequality. This section empirically tests these conjectures.

To check the first of our conjectures, we replace the dependent variable in equation 2 with real output growth and use FE^C and FE^I one at a time as fiscal shocks. Results are reported in Figure 12. Both an increase in government consumption and investment have a positive and statistically significant effect on real GDP, with the effect of consumption being more immediate and that of investment building over time and being stronger in the medium term. This result is consistent with the fact that while government consumption mainly affects aggregate demand, government investment affects both aggregate demand and supply via its gradual effect on public capital and on total factor productivity. The results also suggest that the public investment multiplier (computed as the ratio of the estimated coefficients to the public investment-output share) is stronger than that of consumption (computed in an analogous way). In particular, at a five-year horizon the fiscal multiplier is 0.40 for investment and 0.28 for consumption.

To check the second conjecture, we repeat the analysis looking at the effect of government consumption and investment shocks on employment. The results reported in the top panels of Figure 13 suggest that employment significantly increases in the same year and the year after a positive public investment shock, while the effect of a public consumption shock on overall employment is not statistically significant.

Finally, to examine the distributional effect of fiscal shock on employment, we repeat the analysis for youth employment. The results of this empirical exercise, reported in the bottom panels of Figure 13, suggest that both public consumption and investment shocks have statistically significant short-term effects on youth employment (about 4 percent). Again, given that public investment is only about 30 percent of government consumption, the results also suggest a larger multiplier effect for public investment shocks.

IV. CONCLUSIONS

The empirical fiscal literature is increasingly devoting closer attention on the distributional effects of government spending shocks. So far, however, the focus has been on advanced economies and some frontier emerging markets. Using the various vintages of the IMF WEO publications, we construct unanticipated government spending shocks for 103 developing countries from 1990 to 2016 and find that, in these countries, an unanticipated fiscal expansion (contraction) leads to a delayed but long-lasting decrease (increase) in net income inequality. This effect is both statistically and economically significant. In particular, an increase (reduction) in government spending of 2 percent of GDP reduces (increases) the Gini index by 2.74 percent, which corresponds to about 1 standard deviation of the average change in the Gini coefficient in our sample.

The results survive a wide battery of robustness checks and do not significantly differ across fiscal stimuli and consolidations, across booms and busts, or across LICs and EMs. Total government expenditures are found to have a stronger distributional impact than government consumption or government investment alone, likely because total expenditures include government transfers, an important redistributive tool.

The effect is larger for government investment than government consumption, likely due to the stronger medium-run effect of the former on economic activity. In fact, our estimates suggest a bigger output and employment multiplier effect for government investment relative to consumption. This results has important policy implications. In particular, it suggests that shifting the composition of government expenditure away from consumption and toward public investment could be not only output-enhancing but could also help reducing inequality in the medium-term.

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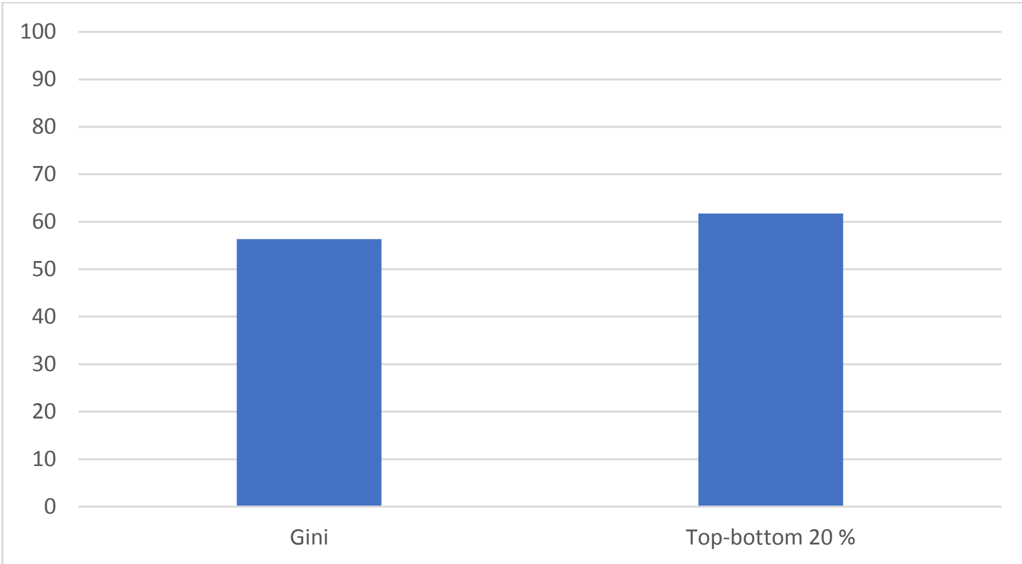
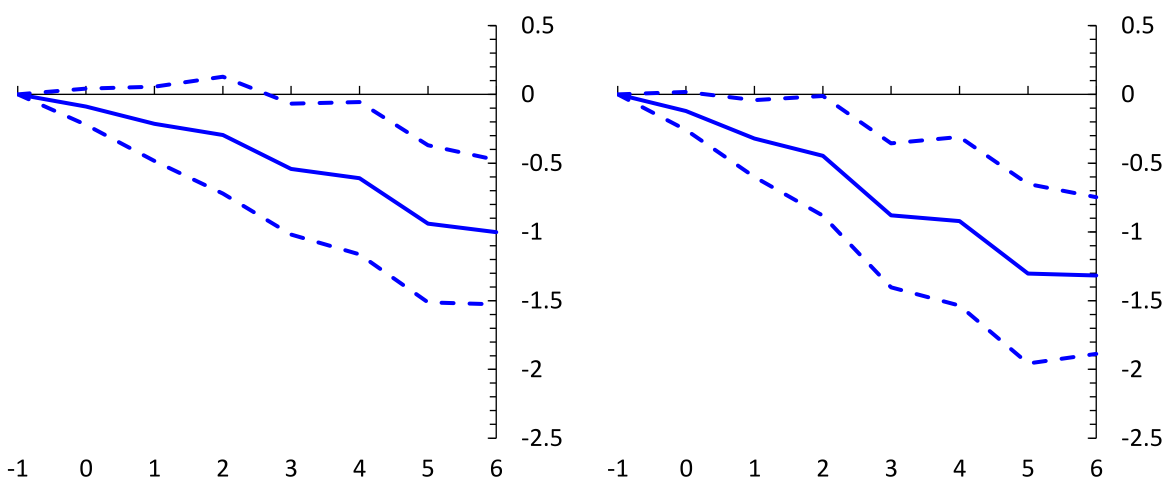
Figure 1. Share of EMDEs with rising inequality since the 1990



Source: Standardized World Income Inequality Database (Solt, 2016), WDI.

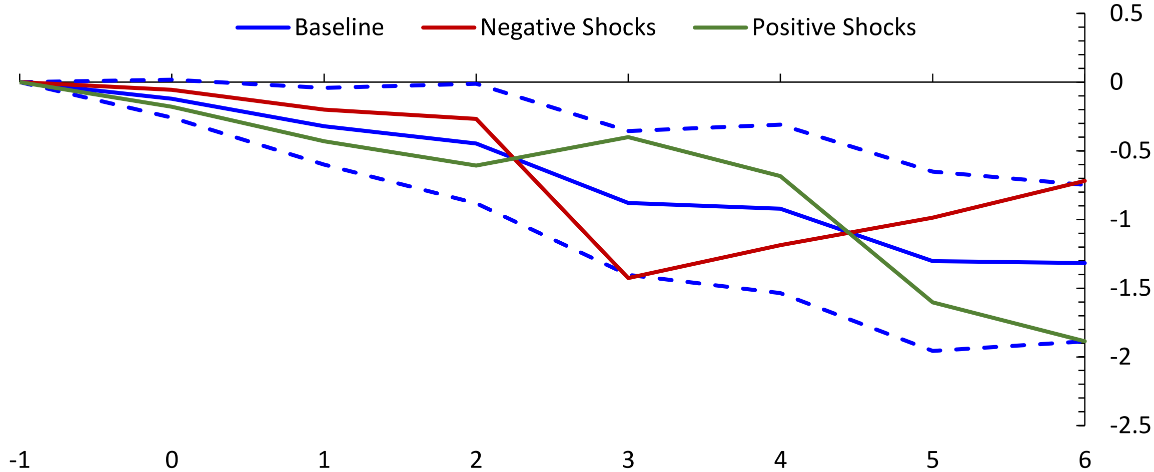
Figure 2. Effect of Total Government Expenditure Shocks on Income Inequality (Gini Coefficients)

Panel A: Market Gini *Panel B: Net Gini*



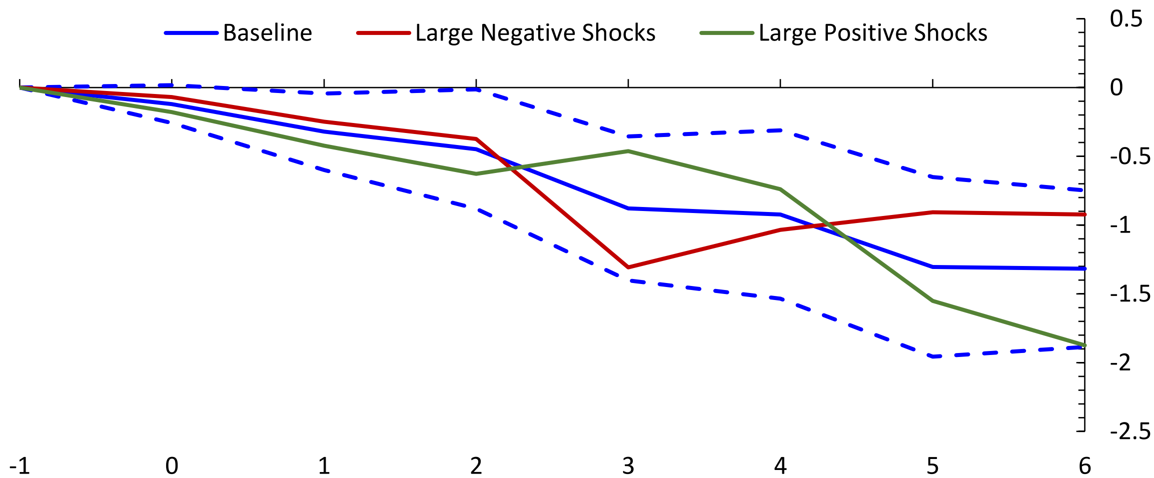
Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure; dashed lines denote 90 percent confidence bands. Estimates based on equation (2).

Figure 3. Effect of Total Government Expenditure Shocks on Net Income Inequality (Net Gini Coefficient)—Positive versus Negative Shocks



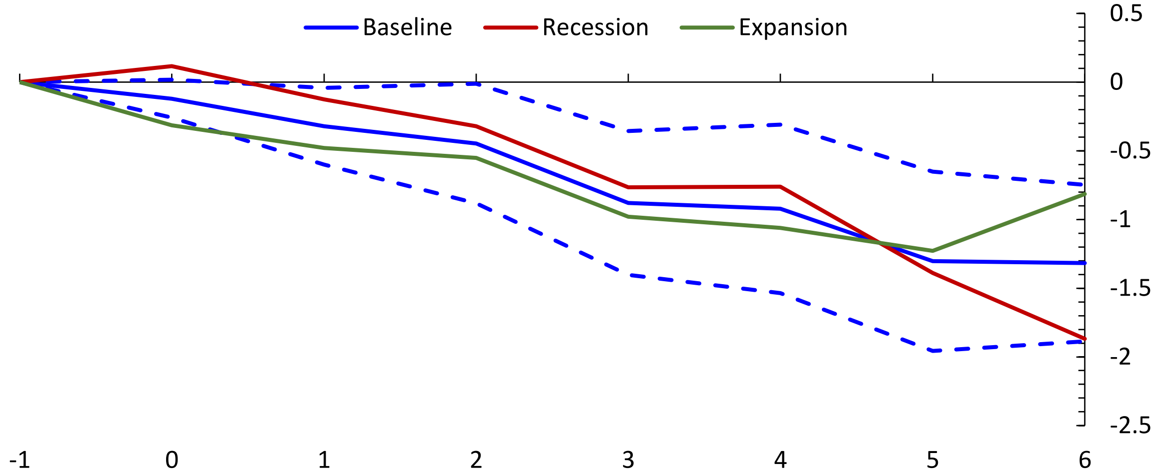
Note: x-axes denots years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure in the baseline model; dashed lines denote 90 percent confidence bands in the baseline model; solid red and green lines denote alternative models. Estimates based on equation (3).

Figure 4. Effect of Total Government Expenditure Shocks on Net Income Inequality (Net Gini)—Large Positive versus Large Negative Shocks



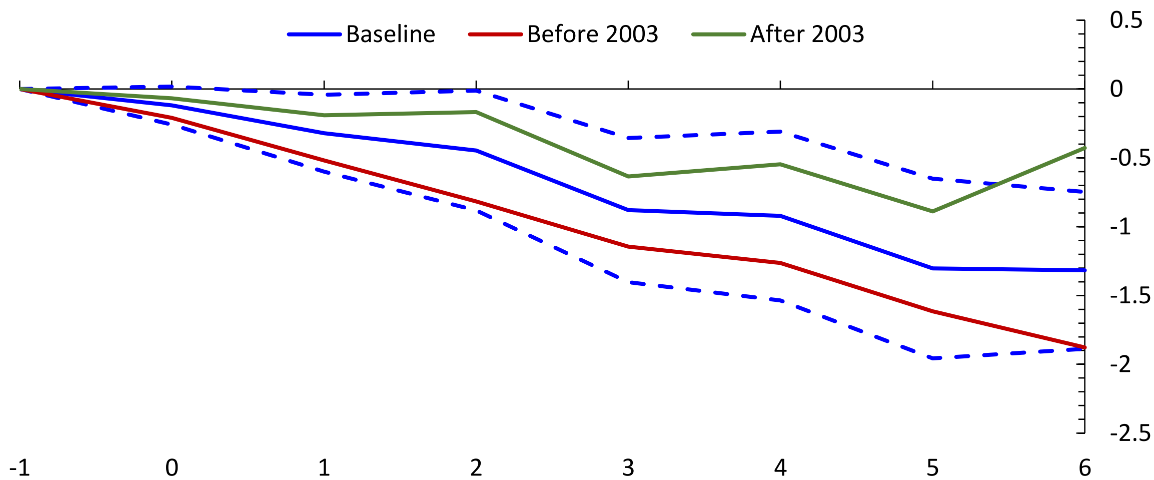
Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses (minus percent responses) to an unticipated 10 percent increase (decrease) in government expenditure in the baseline model; dashed lines denote 90 percent confidence bands in the baseline model; solid red and green lines denote alternative models. Estimates based on equation (3).

Figure 5. Effect of Total Government Expenditure Shocks on Net Income Inequality (Net Gini Coefficient)—The role the Business Cycle



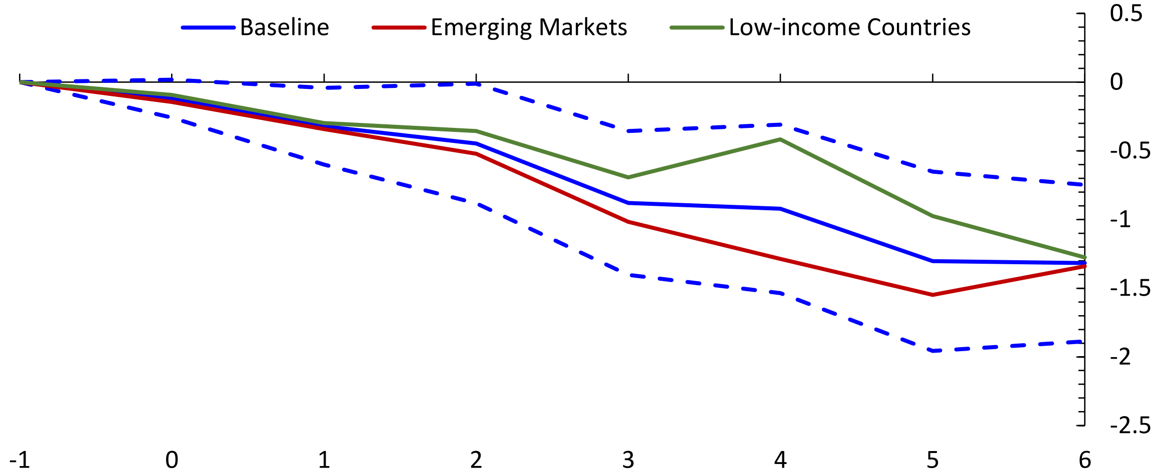
Note: x-axes denots years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure in the baseline model; dashed lines denote 90 percent confidence bands in the baseline model; solid red and green lines denote alternative models. Estimates based on equation (4).

Figure 6. Effect of Total Government Expenditure Shocks on Net Income Inequality (Net Gini Coefficient)—Time Subsamples



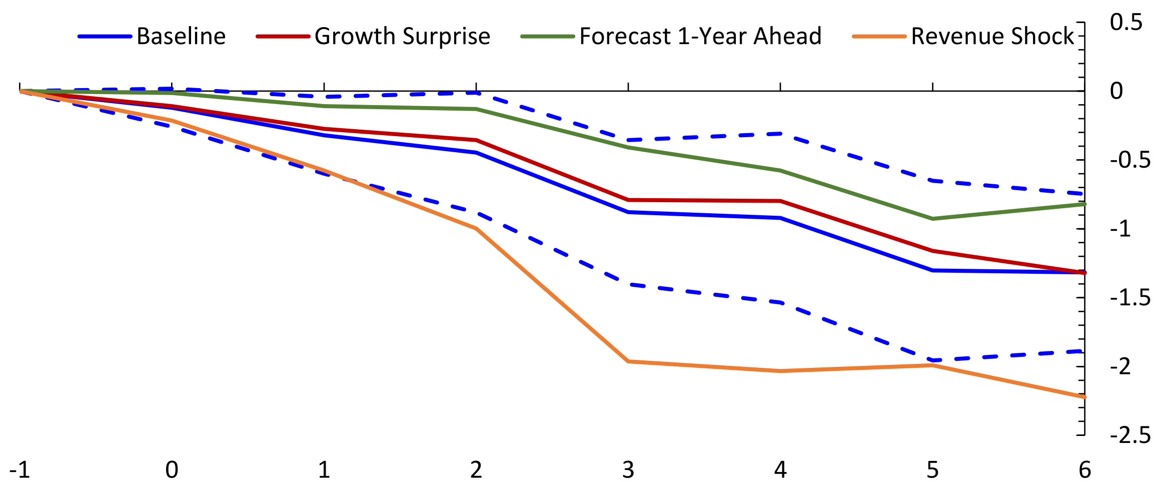
Note: x-axes denots years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure in the baseline sample; dashed lines denote 90 percent confidence bands in the baseline sample; solid red and green lines denote alternative subsamples. Estimates based on equation (3).

Figure 7. Effect of Total Government Expenditure Shocks on Net Income Inequality (Net Gini Coefficient)—Country Subsamples



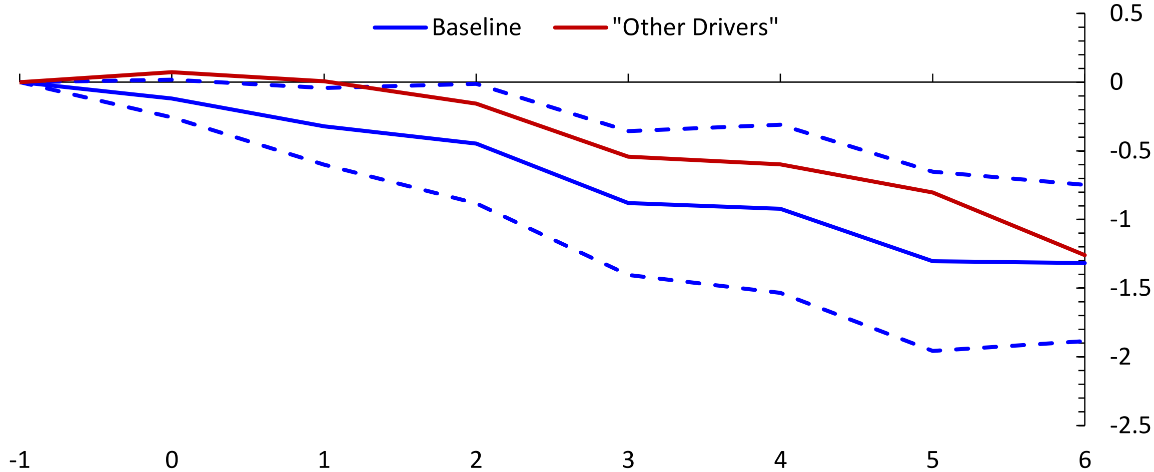
Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure in the baseline sample; dashed lines denote 90 percent confidence bands in the baseline sample; solid red and green lines denote alternative subsamples. Estimates based on equation (3).

Figure 8. Effect of Total Government Expenditure Shocks on Net Income Inequality (Net Gini)—Alternative Shocks



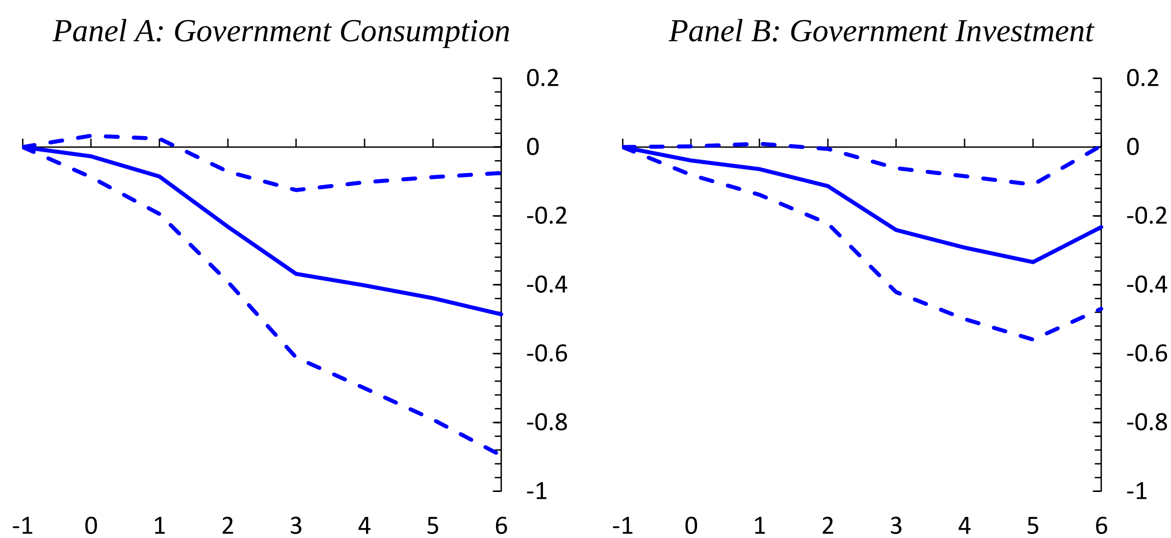
Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure in the baseline model; dashed lines denote 90 percent confidence bands in the baseline model; solid red and green lines denote alternative models. Estimates based on equation (2).

Figure 9. Effect of Total Government Expenditure Shocks on Net Income Inequality (Net Gini)—Other Drivers



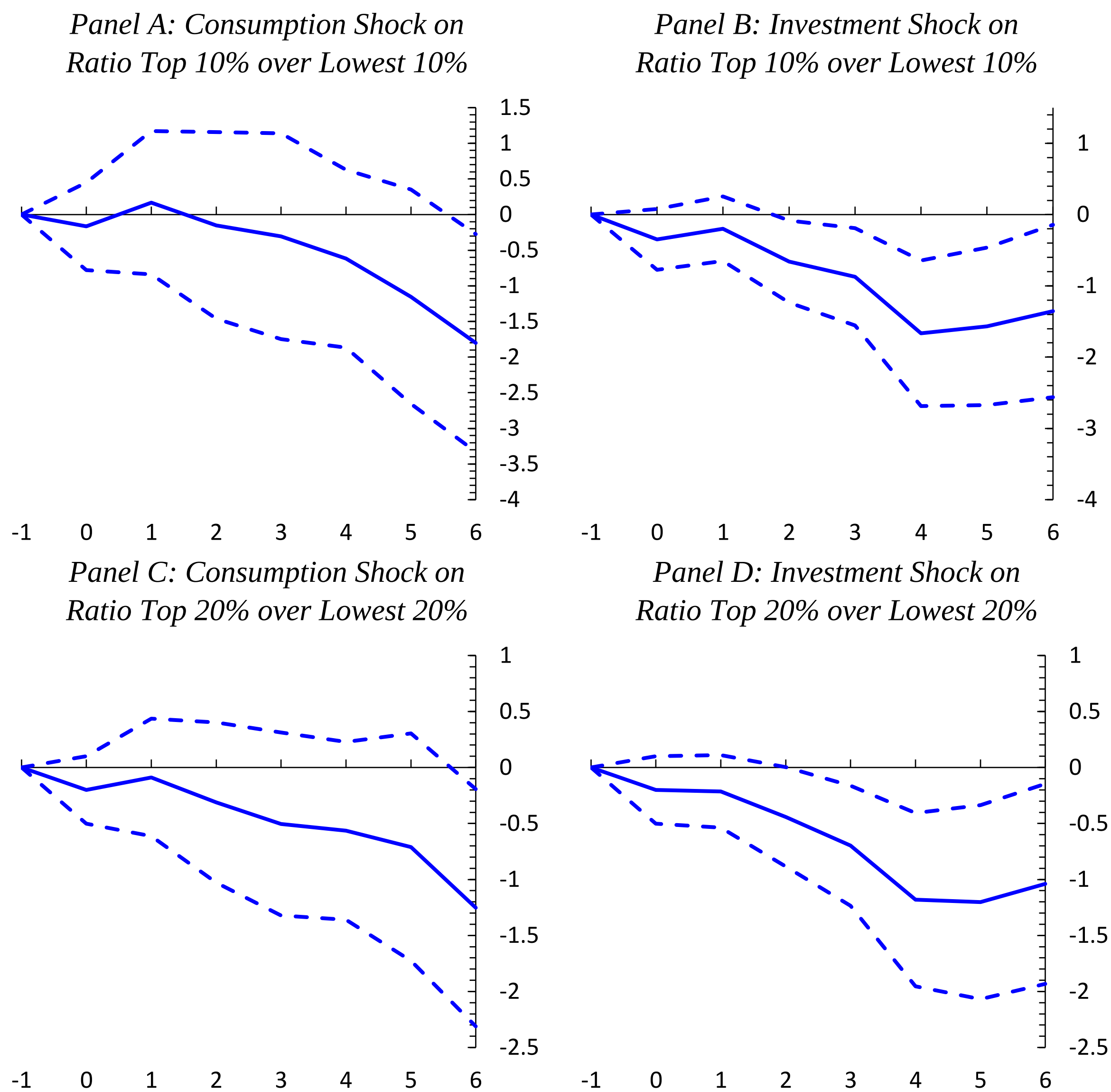
Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure in the baseline model; dashed lines denote 90 percent confidence bands in the baseline model; solid red lines denote alternative models. Estimates based on equation (2).

Figure 10. Effect of Government Consumption and Investment Shocks on Net Income Inequality (Net Gini)



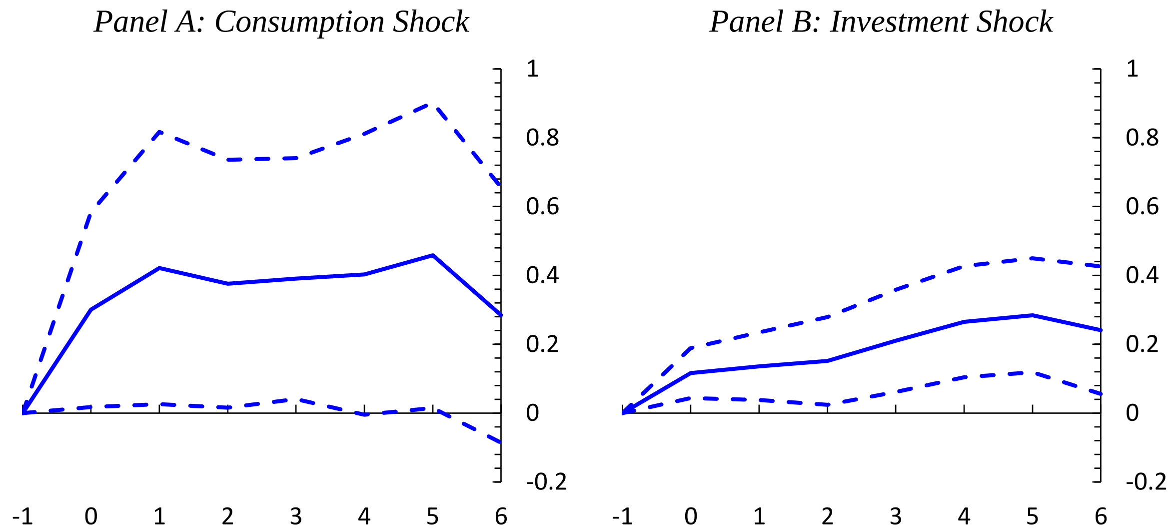
Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government conmsumption (invesmtent); dashed lines denote 90 percent confidence bands. Estimates based on equation (2).

Figure 11. Effect of Government Consumption and Investment Shocks on Income Shares



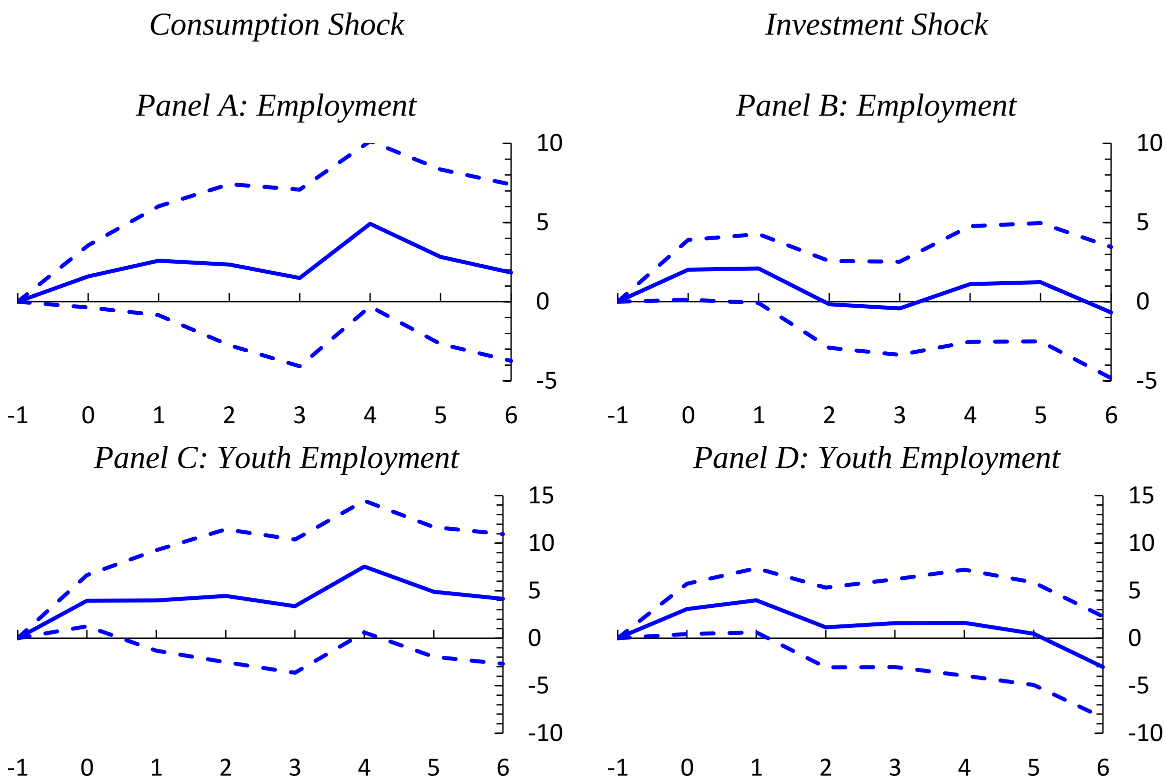
Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government consumption (investment); dashed lines denote 90 percent confidence bands. Estimates based on equation (2).

Figure 12. Effect of Government Consumption and Investment Shocks on Real GDP



Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure in the baseline model; dashed lines denote 90 percent confidence bands. Estimates based on eaution (2).

Figure 13. Effect of Government Consumption and Investment Shocks on Employment



Note: x-axes denote years; t=0 is the year of the shock; solid blue lines denote percent responses to an unticipated 10 percent increase in government expenditure; dashed lines denote 90 percent confidence bands. Estimates based on equation (2).

APPENDIX

Table A1. Country Coverage

Country	EM/LIC	Coverage	Country	EM/LIC	Coverage
Afghanistan	LIC	2005-2015	Lesotho	LIC	1991-2015
Albania	EM	1993-2015	Lithuania	EM	2000-2015
Algeria	EM	1990-2015	Macedonia, FYR	EM	1995-2015
Angola	EM	1996-2015	Madagascar	LIC	1990-2015
Argentina	EM	1990-2015	Malawi	LIC	1992-2015
Armenia	EM	1995-2015	Malaysia	EM	1991-2015
Bangladesh	LIC	1993-2015	Maldives	EM	1993-2015
Barbados	EM	1992-2015	Mali	LIC	1998-2015
Belarus	EM	2000-2015	Mauritania	LIC	1999-2015
Belize	EM	1992-2015	Mauritius	EM	1991-2015
Benin	LIC	1994-2015	Mexico	EM	1990-2015
Bolivia	LIC	1990-2015	Moldova	LIC	1995-2015
Bosnia and Herzegovina	EM	2003-2015	Mongolia	LIC	1993-2015
Botswana	EM	1996-2015	Montenegro, Rep. of	EM	2008-2015
Brazil	EM	1990-2015	Morocco	EM	1990-2015
Bulgaria	EM	1995-2015	Mozambique	LIC	1991-2015
Burkina Faso	LIC	1992-2015	Namibia	EM	1996-2015
Burundi	LIC	1996-2015	Nepal	LIC	2006-2015
Cabo Verde	EM	1996-2015	Nicaragua	LIC	1992-2015
Cambodia	LIC	1999-2015	Niger	LIC	1992-2015
Cameroon	LIC	1990-2015	Nigeria	LIC	1991-2015
Central African Republic	LIC	1995-2015	Pakistan	EM	1990-2015
Chad	LIC	1991-2015	Panama	EM	2004-2015
Chile	EM	1990-2015	Paraguay	EM	1991-2015
China	EM	2001-2015	Peru	EM	1991-2015
Colombia	EM	1990-2015	Philippines	EM	1990-2015
Comoros	LIC	1993-2015	Poland	EM	1990-2015
Costa Rica	EM	2000-2015	Romania	EM	1991-2015
Croatia	EM	1994-2015	Russia	EM	1993-2015
Côte d'Ivoire	LIC	1990-2015	Rwanda	LIC	1991-2015
Djibouti	LIC	1996-2015	Senegal	LIC	1990-2015
Dominican Republic	EM	2001-2015	Serbia	EM	2007-2015
Ecuador	EM	2002-2015	Seychelles	EM	1991-2015
Egypt	EM	1990-2015	Sierra Leone	LIC	1996-2015
El Salvador	EM	1992-2015	South Africa	EM	1990-2015
Ethiopia	LIC	1998-2015	Sri Lanka	EM	1990-2015
Gambia, The	LIC	1990-2015	Swaziland	EM	1996-2015
Georgia	EM	2010-2015	Syria	EM	1992-2010
Ghana	LIC	1990-2015	Tanzania	LIC	1998-2015
Guatemala	EM	1993-2015	Thailand	EM	1990-2015
Guinea	LIC	1992-2015	Togo	LIC	1991-2015
Guinea-Bissau	LIC	1996-2015	Trinidad and Tobago	EM	1991-2015
Haiti	LIC	1992-2015	Tunisia	EM	1991-2015
Honduras	LIC	1994-2015	Turkey	EM	1990-2015
Hungary	EM	1990-2015	Uganda	LIC	1996-2015
India	EM	1990-2015	Ukraine	EM	1998-2015
Indonesia	EM	1990-2015	Uruguay	EM	1993-2015
Iran	EM	1992-2015	Venezuela	EM	1990-2015
Kazakhstan	EM	2004-2015	Vietnam	LIC	1998-2015
Kenya	LIC	1990-2015	Yemen	LIC	2001-2015
Kyrgyz Republic	LIC	1999-2015	Zambia	LIC	1991-2015
Lebanon	EM	2001-2015			

Note: EM = emerging market; LIC = low-income country. Classification based on IMF WEO.

Figure A1. Distribution of Government Expenditure Shocks in EMDE
(Source: IMF World Economic Outlook and IMF Staff calculations)

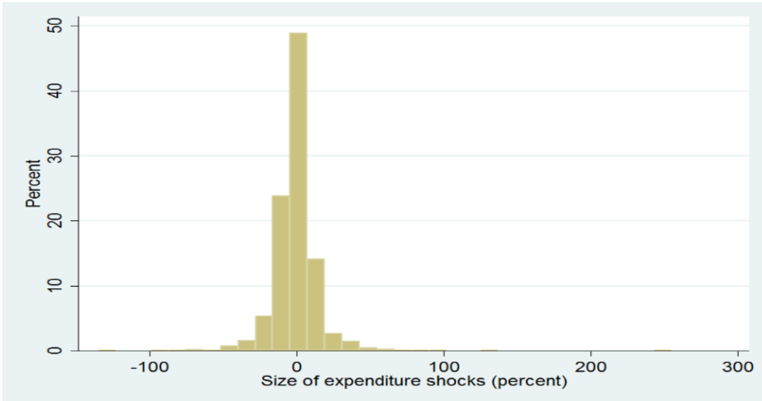


Figure A2. Distribution of Government Consumption Shocks in EMDE
(Source: IMF World Economic Outlook and IMF Staff calculations)

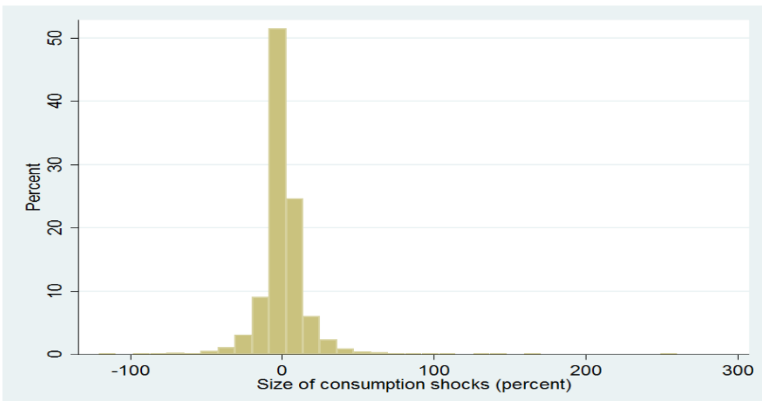


Figure A3. Distribution of Government Investment Shocks in EMDE
(Source: IMF World Economic Outlook and IMF Staff calculations)