Distortions to Agriculture and Economic Growth in Sub-Saharan Africa

Kym Anderson

University of Adelaide, Australian National University, and CEPR

kym.anderson@adelaide.edu.au

and

Markus Brückner

National University of Singapore

ecsbm@nus.edu.sg

June 2012

Distortions to Agriculture and Economic Growth in Sub-Saharan Africa

by

Kym Anderson and Markus Brückner*

Abstract: To what extent has Sub-Saharan Africa's slow economic growth over the past five decades been due to price and trade policies that discouraged production of agricultural relative to non-agricultural tradables? This paper uses a new set of estimates of policy induced distortions to relative agricultural prices to address this question econometrically. We first test if these policy distortions respond to economic growth, using rainfall and international commodity price shocks as instrumental variables. We find that on impact there is no significant response of relative agricultural price distortions to changes in real GDP per capita growth. We then test the reverse proposition and find a statistically significant and sizable negative effect of relative agricultural price distortions on the growth rate of Sub-Saharan African countries. Our fixed effects estimates yield that, during the 1960-2005 period, a ten percentage points increase in distortions to relative agricultural prices decreased the region's real GDP per capita growth rate by about half a percentage point per annum.

Key words: Economic growth, Trade restrictions, Agricultural incentives

JEL codes: F1, F4, O1, O2, O4

---

1. Introduction

Economic growth in Sub-Saharan Africa has been slow for decades (Easterly and Levine, 1997; Ndulu and O'Connell, 2007). According to data from the Penn World Table (Heston et al., 2009), Sub-Saharan African real income per capita grew at less than one percent per year over the past half

century. In this paper we examine whether and to what extent policy induced distortions to agricultural and non-agricultural production are responsible for Sub-Saharan Africa's dismal growth performance. The average share of GDP from agriculture in Sub-Saharan African countries during the past half century has been more than one-third (WDI, 2011). Even in recent years agricultural production in Sub-Saharan Africa has constituted about a quarter of total GDP and more than half of total employment (Sandri, Valenzuela and Anderson, 2007). Given these large shares, the question of how much policy induced distortions to relative agricultural prices have slowed the region's economic growth is economically relevant -- both for the academic debate on the determinants of Africa's growth tragedy as well as for the longstanding debate in the development economics literature on whether the net positive growth externalities from industrial activity exceed those from agriculture.

Our estimation strategy to identify the causal effects that policy distortions to relative agricultural prices have on economic growth in Sub-Saharan African countries is based on a two-step estimation approach. In the first step, we estimate the response of these policy distortions to economic growth, using plausibly exogenous variations in rainfall and international commodity price shocks as instrumental variables.[1] The instrumental variables approach enables us to examine how distortions to relative agricultural prices respond to exogenous changes in GDP per capita growth. Importantly, beyond informing the political economy debate on the determinants of policy distortions, the results from this first step provide useful information on the extent to which these policy distortions are endogenous to changes in Sub-Saharan African countries' GDP per capita growth. In the second step, we use this information to estimate the effects that policy induced distortions to relative agricultural prices have on economic growth.

Our first main finding is that there is no systematic response of relative agricultural price

---

1 We thus build on the prior literature that has shown that rainfall and international commodity price shocks have a significant effect on real GDP per capita growth of Sub-Saharan African countries. See for example Miguel et al. (2004), Barrios et al. (2010), or Brückner and Ciccone (2010, 2011).

distortions to economic growth. Our instrumental variables regressions that control for country and year fixed effects yield a statistically insignificant effect of economic growth on relative agricultural price distortions. The estimated effects are also quantitatively small. They imply that a one percent higher GDP per capita growth decreased distortions to relative agricultural prices by at most 0.003 standard deviations. We document that the effects of economic growth on agricultural policy distortions are insignificant and quantitatively small regardless of whether we use rainfall as an excluded instrument or international commodity price shocks. Furthermore, we show that the effects continue to be quantitatively small and statistically insignificant when we use a distributed lag model, exclude outliers, restrict the sample to the post-1985 period, or include additional within-country controls that capture changes in the size of government, political institutions, and the incidence of civil war. Our first main finding therefore indicates that growth in average incomes does not trigger significant changes in distortions to the price of agricultural relative to non-agricultural tradables.

In the second part of the paper we examine the effects that these policy induced price distortions have on economic growth. Our main finding there is that increases in distortions to relative agricultural prices have a statistically significant and quantitatively sizable negative effect on the rate of economic growth. Our panel fixed effects estimates yield that a ten percentage points increase in distortions to relative agricultural prices over the 1960-2005 period reduced real GDP per capita growth by about half a percentage point per annum on average. We document that this result is robust to allowing for country-specific growth effects; using a distributed lag model to distinguish short-run from longer-run growth effects; and using a 5-year non-overlapping panel data set to eliminate short-run business-cycle fluctuations. We also document that there continues to be a significant negative and quantitatively sizable effect of relative agricultural price distortions on economic growth when we control for dynamics in GDP per capita growth.

There are a number of telling country episodes that fit the pattern documented by our panel

fixed effects analysis. Tanzania, for example, halved its negative distortion to the relative price of farm products over the 1985-2005 period, during which time income per capita increased by over 30 percent. By contrast, over the same period Zimbabwe worsened its distortions to relative agricultural prices by over 50 percent and experienced a drop in income per capita of more than 25 percent. Other less-extreme examples during the 1960-1980 period include Madagascar, which increased its relative agricultural price distortions by more than 50 percent leading to a drop in real income per capita of more than 10 percent, and Uganda, which increased its relative agricultural price distortions four-fold leading to a drop in real income per capita of more than 25 percent.

Over 85 percent of the partial equilibrium welfare cost of policy distortions to agricultural prices in Sub-Saharan African countries has been attributed to restrictions on exports and imports (Croser and Anderson, 2011). Our paper is thus closely related to the empirical literature on the growth effects of policy distortions to international trade.[2] More generally, our paper is related to the large literature that has examined the link between trade openness and economic growth.[3] It also relates to literature that focuses more specifically on the relative contributions of agricultural and industrial activities to growth. McMillan and Rodrik (2011), for example, raise again the question of whether the net positive growth externalities from industrial activity exceed those from agriculture. One recent view put forth by Aghion et al. (2012) is that subsidizing one sector at the expense of the other is only likely to have positive productivity effects if these subsidies are allocated to the competitive sector or increase competition in that sector.

Our paper contributes to the above literature in several ways. First, we use a new measure of trade distortions – relative price distortions to agriculture – that is of particular relevance in the

---

2 See for example Edwards (1992) or Wacziarg and Welch (2008) for empirical evidence that policy distortions to international trade have significant negative growth effects. For historical evidence that trade policy distortions had a positive or insignificant effect on economic growth see, for example, O'Rourke (2000) and Clemens and Williamson (2004). A new study of pre-World War I industrialization by Schularick and Solomou (2011) calls into question earlier findings that suggested tariffs boosted growth in that first globalization wave.

3 See for example Sachs and Warner (1995), Frankel and Romer (1999), Alcala and Ciccone (2004), or Wacziarg and Welch (2008) for evidence of a positive effect of trade openness on economic growth. For a critique, see Rodriguez and Rodrik (2001). For empirical evidence that suggests that the positive effect of trade openness on economic growth is a more recent phenomenon of the later 20th century, see Vamvakidis (2002).

context of estimating the growth effects in largely agrarian Sub-Saharan African countries. Second, we control in the panel regressions for country and year fixed effects, identifying the effects that distortions to trade have on economic growth from the within-country variation of the data. Third, we provide an instrumental variables estimate of the response of policy induced price distortions to economic growth, thereby informing the political economy debate on the extent to which trade and price policies are endogenous to economic growth.

The remainder of our paper is organized as follows. Section 2 describes the data. Section 3 discusses the estimation strategy. Sections 4 and 5 present the empirical results. Section 6 concludes.

2. Data

Relative Agricultural Price Distortions. The World Bank recently completed a major global empirical study that estimated annual policy induced price distortions to agricultural incentives since 1955.[4] The study estimates nominal rates of assistance (NRAs) to agricultural industries as well as nominal rates of assistance to producers of nonagricultural tradables. The NRA is defined as the percentage by which government policies directly raise the gross return to producers of a product above what it would be without the government's intervention (or lowered it, if NRA<0). For agriculture this was carefully estimated by comparing domestic and border prices of like products (at similar points in the value chain) for each of the covered farm industries, drawing on national statistical sources supplemented where necessary by producer prices and unit values of exports and imports from FAO (2011). To obtain the weighted average NRA for the agricultural sector as a whole, the World Bank study's contributors calculated the weighted average of product NRAs for enough farm products (an average of 8 per country) to cover at least 70 percent of farm

---

4 The study is summarized in four regional volumes, including one on Africa that also describes the myriad policies adopted and their drivers (Anderson and Masters, 2009), and a global overview volume (Anderson 2009). The panel dataset of estimates of price distortions has been made freely available (Anderson and Valenzuela, 2008), and the methodology is documented in Anderson et al. (2008).

production valued at undistorted prices, and used the latter shares as weights.[5] The non-agricultural

tradable sectors' NRA was obtained by generating a weighted average of trade taxes from national

sources for such sectors as forestry, manufacturing and mining, using those sectors' shares of GDP

as weights.[6]

Our main variable that captures policy induced distortions to *relative* agricultural prices is

the relative rate of assistance (RRA). This variable is defined as:

$$RRA = [(100+NRAag^t)/(100+NRAnonag^t)] - 1$$

where $NRAag^t$ and $NRAnonag^t$ are the percentage NRAs for the tradables parts of the agricultural

and non-agricultural sectors, respectively.[7] Since the NRA cannot be less than -100 percent if

producers are to earn anything, neither can the RRA (since the weighted average $NRAnonag^t$ is

non-negative in all the country case studies). And if both of those sectors are equally assisted, the

RRA is zero.

Thus, economic policy reform that reduces sectoral bias is characterized by movements of

the RRA towards zero from below (or from above, if a pro-agricultural policy bias had been in

place). Note that the RRA takes into account that it is distortions to *relative* prices that affect

aggregate outcomes: farmers are affected not only by prices of their own products but also by prices

faced by nonagricultural producers bidding from the same national pool of inter-sectorally mobile

---

5 It should be noted that these NRA estimates of agricultural price distortions are far superior to the normal indicators used by trade economists to measure trade distortions; these indicators are typically based on trade-weighted (rather than production weighted) averages of import tariffs and export taxes. The only other study of the kind similar to the NRA estimates is by Krueger, Schiff and Valdés (1988). That study covered three farm products for each of just three Sub-Saharan African countries over an average of 21 years: Cote d'Ivoire (1960-82), Ghana (1955-77) and Zambia (1966-84). A crude set of estimates of pre-1980 export tax equivalents for an average of two products in seven African countries is reported in Bates (1981, Appendix B). All other estimates known to the authors have smaller time series and are mostly single-country or single-commodity studies.

6 The NRA for non-agricultural tradable sectors is underestimated in so far as the inclusion of any omitted non-tariff import restrictions would have raised the manufacturing NRA more than the inclusion of any omitted export restrictions would have lowered the NRA for non-farm primary products. Both of those sets of instruments would have been more important in the past than since the early 1990s. Also important prior to the 1990s were overvalued exchange rates. Where data permitted, the effect of exchange rate distortions are included as implicit trade taxes in the estimation of the NRA for each industry producing exportable or import-competing products, following Dervis, de Melo, and Robinson (1981).

7 The $NRAnonag^t$ is a weighted average of the trade taxes in the manufacturing and in the non-farm primary sectors, using sectoral shares of non-agricultural GDP as weights. See Anderson et al. (2008) and the Appendices of Anderson and Masters (2009) for further details.

resources. More than seventy years ago Lerner (1936) provided his Symmetry Theorem to prove that, in a two-sector economy, an import tax has the same effects on production, consumption, trade and national economic welfare as an export tax. This carries over to a model that has many sectors, and is unaffected if there is imperfect competition domestically or internationally or if some of those sectors produce only nontradables (Vousden, 1990, pp. 46-47).

Figure 1 plots the time-series evolution of the RRA for each of the 14 large Sub-Saharan African countries in our sample (which accounts for more than three-quarters of the population and even more of the GDP of Sub-Saharan Africa excluding South Africa).[8] It shows that, for all of those Sub-Saharan African countries except Nigeria, the RRA is negative almost all of the time. Hence on average there has been a strong policy bias against agriculture over the past half-century. However, there is also substantial RRA variation across time and countries. For example, in Ethiopia, Mozambique, and Tanzania there was a continuous reduction in policy biases against agriculture, while in countries such as Zambia and Zimbabwe the strong bias against agriculture was firmly maintained.

We note here that the RRA measure we use has several important advantages over other existing measures of policy distortions available for Sub-Saharan African countries. First, the estimated nominal rates of assistance to agriculture and non-agriculture reported in Anderson and Valenzuela (2008) provide by far the longest and most consistent annual time-series data on policy distortions in Sub-Saharan African countries. Second, measures of just trade policy distortions typically use trade weights to obtain sectoral averages, whereas the new World Bank study uses more-appropriate weights based on production valued at undistorted prices. Third, most other estimates of agricultural trade policy distortions focus on just import tariffs (see, e.g., WTO 2010), thereby missing export distortions (as well as occasional food import subsidies) which turn out to have been far more important in Africa over the past half-century (see Croser and Anderson 2011).

---

8   This is the largest possible sample and time-span given the availability of data on the relative rate of assistance estimates for the Sub-Saharan African region.

Commodity export price index. We construct a country-specific international commodity export price index for agricultural and natural resource commodities as:

$$ComPrice_{i,t} = \sum_{c} \theta_{i,c} ComPrice_{c,t}$$

where $ComPrice_{c,t}$ is the international price of commodity c in year t, and $\theta_{i,c}$ is the average (time-invariant) value of exports of commodity c in the GDP of country i.[9] We obtain data on annual international commodity prices from UNCTAD Commodity Statistics and our data on the value of commodity exports are from the NBER-United Nations Trade Database. The commodities included in the agricultural commodity export price index are beef, coffee, cocoa, cotton, maize, rice, rubber, sugar, tea, tobacco, wheat, and wood. The commodities included in the natural resource export price index are aluminum, copper, gold, iron, and oil. In case there were multiple prices listed for the same commodity a simple average of all the relevant prices is used.

Rainfall. The annual rainfall data are from Matsuura and Willmott (2007). The rainfall data come at a high resolution (0.5°x0.5° latitude-longitude grid) and each rainfall observation in a given grid is constructed by interpolation of rainfall observed by all stations operating in that grid. We aggregate rainfall data to the country level by assigning grids to the geographic areas of countries.

GDP per capita and other data. Data on real per capita GDP, trade openness (exports plus imports over PPP GDP), private consumption per capita, investment, the government expenditure share, the exchange rate, and the price level are from the Penn World Table, version 6.3 (Heston et al., 2009). Data on political institutions are from the Polity IV database (Marshall and Jaggers, 2009). The data on civil war incidence are from the PRIO/UPPSALA database on armed conflicts (CSCW, 2010). Data on GDP of agricultural value added, manufacturing value added, and the GDP share of exports

---

9   This functional form of the commodity export price index follows common practice in the literature. See for example, Collier and Goderis (2007) and the references cited therein.

are from WDI (2011). Data on the agricultural labor force, the share of the agricultural labor force in the total labor force, and an index of agricultural production are from FAOSTAT (2011). For some summary statistics see Table 1.

3. Baseline Estimation Strategy

To examine the effects that within-country changes in distortions to relative agricultural prices ($\Delta \text{abs}(rra_{i,t})$) have on real GDP per capita growth ($\Delta \ln(GDP_{i,t})$), we estimate the following econometric model:

$$(1) \qquad \Delta \ln(GDP_{i,t}) = \alpha_i + \beta_t + \eta \Delta \text{abs}(rra_{i,t}) + \Gamma \Delta X_{i,t} + u_{i,t}$$

where α_i are country fixed effects that account for cross-country differences in geography, history, ethnicity and other time-invariant determinants of economic growth such as initial income per capita levels. The year fixed effects, β_t, capture common year shocks that affect both GDP per capita growth in Africa and changes in agricultural product price distortions (for example, common shocks to economic growth that are due to changes in the world business cycle or political events such as the end of the Cold War). The vector $X_{i,t}$ includes additional within-country controls such as variations in a country-specific international export price index, rainfall, political institutions, the government expenditure share, civil war incidence, the exchange rate, inflation, and trade openness.

We note that our main measure of within-country changes in relative price distortions is the change in the *absolute* rate of assistance. The motivation for using the change in the absolute RRA value is that – as argued in Section 2 – economic policy reform which reduces inter-sectoral bias is characterized by movements of the RRA towards zero: either from below if an anti-agricultural policy bias had been in place, or from above if a pro-agricultural policy bias had been in place. By using the change in the absolute value of the RRA, we impose the restriction that the effects of changes in the RRA are symmetric for an anti-agricultural (negative RRA) and a pro-agricultural bias (positive RRA). Imposing this restriction is a more efficient way of estimating the effects that

relative price distortions have on economic growth, under the assumption that the restriction holds. It is an empirical question as to whether the restriction holds, and we come back to testing this restriction in Section 5.1.

We use the change in the log of GDP per capita because the Maddala and Wu (1999) panel unit root test does not reject the null hypothesis of the level of the log of GDP per capita containing a unit root (see Appendix Table 1). Yet this test comfortably rejects the null hypothesis of the change in the log of GDP per capita containing a unit root at the 1 percent significance level. The panel unit root test also rejects the null hypothesis of unit root in the relative rate of assistance. Thus, we relate the change in GDP per capita to the change in the relative rate of assistance, noting that equation (1) has a level form representation

(2) $$\ln(GDP_{i,t}) = a_i + b_t + c_i\, t + \eta \mathrm{abs}(rra_{i,t}) + BX_{i,t} + e_{i,t}$$

Unless there exists a cointegration relationship, the error term e will be non-stationary. However, a cointegration relationship between GDP and the RRA is not possible: the RRA is a stationary variable, as indicated by the unit root test results in Table 2. Hence, our main estimating equation relates the change in the log of GDP per capita to the change in the RRA.

As a baseline, we estimate the average marginal effect η that within-country changes in relative agricultural price distortions have on economic growth. We then examine lagged effects of these price distortions on economic growth by means of a distributed lag model where we include further lags of the relative rate of assistance on the right-hand side of the estimating equation. By doing so, we can examine both short-run and medium/long-run growth effects. For example, the short-run growth effects could differ from the longer-run growth effects if there are adjustment costs to capital so that it takes time for the capital stock in the sectors to fully adjust to the relative agricultural price distortions.

It is possible that the growth effects of relative agricultural price distortions may be country-specific. Country-specific growth effects could arise, for example, due to cross-country differences

in sectoral compositions or due to cross-country differences in political economy factors that drive the relative agricultural price distortions. An important econometric issue is, therefore, whether the restricted form of equation (1) provides a consistent estimate of the average marginal effect of agricultural price distortions on economic growth in Africa. To check this, we use the mean-group estimator developed by Pesaran and Smith (1995). This estimator computes country-specific slope estimates and allows us to check whether the mean of these country-specific slope estimates is close to the estimate of the average marginal effect obtained in equation (1).

A further necessary condition for consistent estimation of the growth effects of agricultural price distortions is that our distortions variable is exogenous to within-country changes in economic growth. To examine whether this is the case, we use a two-stage least squares estimation approach that regresses the within-country change in the absolute RRA on real GDP per capita growth which we instrument by within-country variations in rainfall and an international commodity export price index. We therefore make use of prior research by Miguel et al. (2004) and Brückner and Ciccone (2010, 2011) that used these instruments for economic growth in African countries to examine how growth shocks affect civil war risk and within-country variations in political institutions. The exclusion restriction in this two-stage least squares estimation is that, conditional on economic growth, year-to-year variations in rainfall and international commodity prices only affect relative agricultural price distortions through their income per capita effects. We examine this exclusion restriction in detail in the section that follows.

Exogeneity of within-country variations in the RRA to economic growth is a necessary but not a sufficient condition for consistent estimation of within-country variation in the RRA on economic growth. For example, reform to RRA distortions might be accompanied by other policies that are growth promoting. In that case, even if the within-country variations in the RRA are not driven by economic growth, the least squares estimate would represent an upper bound of the true causal effect that RRA variations have on economic growth. In order to address this concern, we

will present as a robustness check estimates that control for within-country variations in political institutions and civil war incidence as well as variables that capture macroeconomic policies, such as the government expenditure share, inflation, the exchange rate, and trade openness.

4. Main Results

4.1 The response of relative agricultural price distortions to economic growth

We begin our empirical analysis by estimating the response of changes in agricultural price distortions to within-country changes in real GDP per capita. This first-step exercise serves the purpose of clarifying whether indeed our policy distortions variable – the change in the absolute value of the relative rate of assistance – is exogenous to economic growth. Our first-step exercise also sheds light on the question of how and to what extent plausibly exogenous growth shocks affect the political process of setting relative price distortions in the economy.

Table 2 presents our instrumental variables estimates of the effect that economic growth has on the change in the absolute RRA. Column (1) reports country and year fixed-effects estimates where we instrument the within-country change in real GDP per capita with the within-country change in rainfall and the within-country change in the international commodity export price index. The main result in Panel A is that the estimated coefficient on real GDP per capita growth is statistically insignificant and quantitatively small. The estimated coefficient implies that at most a change in real GDP per capita of one percentage points leads to a 0.003 standard deviation change in the absolute value of the RRA.

To ensure that the insignificant coefficient on GDP per capita growth is not driven by outliers, we report in column (2) instrumental variable estimates that exclude the top/bottom 1 percentile of GDP per capita growth. In this case the obtained estimates are also quantitatively small and statistically insignificant. In column (3) we show that similar results are obtained if we restrict the sample to the post-1985 period (thus excluding events such as the oil price shock of the 1970s

and the upward trend in the relative rate of assistance that began to occur during the pre-1985 period); and in column (4) we show that there is also no significant effect of economic growth on the RRA when including on the right-hand side of the estimating equation additional within-country control variables such as the polity2 score, an indicator variable for the incidence of civil war, the government expenditures share, inflation, the exchange rate, and trade openness.[10] Hence, the main result of these instrumental variables estimates is that on impact there is no systematic response of changes in the absolute RRA to economic growth.

For comparison purposes with the instrumental variables estimates, we report in Panel B of Table 2 the corresponding least-squares estimates. The least squares estimates are negative in sign but statistically insignificant. We note that if policy induced distortions to relative agricultural prices have a negative effect on economic growth, reverse causality bias implies that the least-squares estimates are biased downward. This downward bias can explain why the least-squares estimate on the impact response of the absolute RRA to economic growth is negative. The negative reverse causality bias can also explain why in absolute size, the least squares coefficient is larger than the coefficient that is obtained from the instrumental variables regression.

We note that the quality of our instrumental variables is reasonable. The first-stage estimates reported below the second stage in Panel A of Table 2 are all individually highly significant. Moreover, the joint first-stage F-statistic is well above 10. Given this first-stage F-statistic we can reject at the 5 percent significance level, based on the tabulations reported in Stock and Yogo (2005), that the maximal IV relative bias is larger than 5 percent.[11] Bias due to weak instruments is therefore unlikely to be an issue in our instrumental variables regressions. Moreover, the validity of our instruments in terms of being uncorrelated with the second-stage error term cannot be rejected.

---

10 Research by Miguel et al. (2004) and Brückner and Ciccone (2010, 2011) has shown that rainfall and international commodity price shocks have a significant effect on civil war and political institutions. Reporting results that control for within-country changes in the incidence of civil war and political institutions is therefore an important robustness check.

11 Unfortunately, the critical values in Stock and Yogo (2005) are based on homoscedastic errors. No critical values have been established yet when errors are heteroskedastic. Nevertheless, the critical values tabulated in Stock and Yogo (2005) are often referred to in the applied instrumental variables literature, even in a panel data context.

The Hansen J test produces an insignificant p-value on the joint hypothesis that our instruments are uncorrelated with the second-stage error term. This is a first reassuring indication that our instruments do not systematically violate the exclusion restriction.

In Table 3 we show that there are no significant reduced form effects. Changes in neither the agricultural commodity price index nor the natural resource commodity price index are significantly correlated with changes in the absolute value of the RRA. We also do not find a significant reduced form effect of year-to-year variations in rainfall. This is true regardless of whether we consider the largest possible Sub-Saharan African sample (column (1)); exclude observations in the top/bottom 1 percentile of GDP per capita growth (column (2)); exclude the pre-1985 period (column (3)); or include additional within-country control variables on the right-hand side of the regression (column (4)). Hence, despite Table 2 showing that international commodity price shocks and rainfall shocks have a highly significant effect on GDP per capita growth of Sub-Saharan African countries, Table 3 shows that there are no significant reduced-form effects. The reduced-form estimates in Table 3 therefore echo the insignificant instrumental variables estimates, reported in Panel A of Table 2, which show that there is no significant impact response of changes in the absolute RRA to plausibly exogenous variations in real GDP per capita growth.

A more intuitive way to demonstrate that rainfall and international commodity price shocks are valid instruments is to report the effects that rainfall and international commodity price shocks have on the absolute RRA conditional on GDP per capita growth. In Panel A of Table 4 we report estimates for instrumenting GDP per capita growth with the change in the international commodity export price index and including rainfall on the right-hand side of the second-stage equation. In Panel B of Table 4 we report estimates for instrumenting GDP per capita growth with the change in rainfall and including the international commodity export price index on the right-hand side of the second-stage equation. Both panels show that, conditional on GDP per capita growth, rainfall and international commodity price shocks do not have significant effects on the change in the absolute

RRA. Hence, when conditioning on GDP per capita we find that there are no significant effects of rainfall and international commodity price shocks on the absolute relative rate of assistance. This more intuitive examination of the exclusion restriction therefore reconfirms the results of the Hansen J test that showed that there is no systematic evidence of the instruments being correlated with the second-stage error term.

A further issue in the estimation of the effects that economic growth has on policy induced distortions to the relative price between agricultural and non-agricultural products is whether there are significant lagged effects. Recall that Table 2 reports the contemporaneous response of the change in the absolute RRA to economic growth. The sample autocorrelation of economic growth is fairly low (0.1) and hence examining the contemporaneous effect that economic growth has on the absolute RRA without controlling for additional lags of economic growth is unlikely to lead to inconsistent estimates of the impact effect. To show that this is indeed the case, Table 5 reports estimates from a distributed lag model that include up to two additional lags of GDP per capita growth on the right-hand side of the estimating equation. The main result is that in these augmented regressions the contemporaneous effect of economic growth on the absolute RRA continues to be quantitatively small and statistically insignificant. Note also that the lagged effects of economic growth are insignificant for most of the specifications.[12]

To summarize, the main message of our instrumental variables regressions is that the impact and lagged effects of the change in the absolute value of the RRA are exogenous to within-country variations in GDP per capita growth. This is an important result because it implies that the necessary condition of exogeneity of the absolute RRA to economic growth is satisfied in the following part of our empirical analysis where we examine the effects that changes in the absolute RRA have on economic growth.

---

12 We have also explored the effects of further lags of GDP growth at t-3 and t-4. The estimates on these lags turned out to be insignificant and quantitatively small.

4.2 The effects of relative agricultural price distortions on economic growth

Table 6 reports the least-squares estimates of the impact effect that changes in the absolute value of the relative rate of assistance have on economic growth. The results for the largest possible sample are in column (1). The estimated coefficient on the RRA is -0.04 and this estimate is significant at the 10 percent level. Quantitatively, the estimate implies that on average a one standard deviation change in the absolute RRA (0.15) is associated with a lower GDP per capita growth rate of about 0.6 percentage points. In column (2) we show that the precision of our estimates improves somewhat when we exclude potential outliers. Excluding observations that fall in the top/bottom 1 percentile of the GDP per capita growth distribution yields an estimated coefficient of -0.03. This coefficient is statistically significant at the 10 percent level. When we exclude the pre-1985 period the effect of relative agricultural price distortions on economic growth becomes quantitatively larger (column (3)). However, the smaller sample size also leads to a substantial increase in the standard error so that we cannot reject that the estimated effect in column (3) is significantly different from the estimated effect in column (1). In column (4) we show that results are quantitatively similar to our baseline estimates if we include additional within-country control variables such as the polity2 score, an indicator variable for the incidence of civil war, the government expenditures share, inflation, the exchange rate, and trade openness.

An important econometric issue is whether our results are robust to allowing for country-specific growth effects of policy distortions to relative agricultural prices. It is well known from the panel data literature that if the country-specific slope parameters are correlated with the right-hand side regressors this produces inconsistent estimates in the restricted panel data model of the average marginal effect. To check whether cross-country parameter heterogeneity leads to inconsistent estimates of the average marginal effect in our sample, we use the mean-group estimator developed by Pesaran and Smith (1995) that allows for country-specific coefficients. We report the results of this regression graphically in Figure 2, where we provide a kernel density plot of the distribution of

the country-specific slope estimates. The mean (median) of the kernel density plot is -0.04 (-0.03). Therefore, cross-country parameter heterogeneity does not lead to a significant bias of the average marginal effect in our sample.

An interesting question is whether beyond the significant negative contemporaneous effect of price distortions on economic growth there is also a significant negative lagged effect. A lagged effect could arise if, for example, there are significant adjustment costs to capital that differ across sectors in the economy. Our panel data set has a fairly large T dimension (the average T is about 35) and, therefore, is well suited to explore lagged effects of within-country changes in policy distortions to relative agricultural prices on economic growth. In fact, it is worth to restate here that a key advantage of using annual panel data is that this allows us to examine not only short-run growth effects, which are of substantial interest in and of themselves, but also medium/long-run growth effects by means of a distributed lag model.

In Table 7 we report dynamic panel data estimates as a first approximation to characterize the medium/long-run effect of relative agricultural price distortions on economic growth. It is well known that the presence of country fixed effects leads to inconsistent least-squares estimates in the dynamic panel data specification. Yet in our regressions this fixed effects bias should be relatively small since the average T is fairly large. To check whether this fixed effects bias is indeed small in our regressions, we report, in addition to least squares estimates, system-GMM estimates (Blundell and Bond, 1998). For these system-GMM estimates, we use the first lag as an instrument to ensure that not too many moment conditions are used.

Panel A of Table 7 shows that the dynamic panel data regression produces a coefficient on lagged GDP per capita growth of about 0.05 to 0.1. A test for second-order serial correlation produces insignificant results in all cases. Thus, specification tests indicate that the model is well specified. We compute the long-run growth response from the dynamic panel data model by inverting the characteristic polynomial. This yields a cumulative (long-run) growth effect of a

permanent increase in relative agricultural price distortions of about -0.05. This estimate is very similar to the static panel data model where we concentrate on the impact growth effect of relative agricultural price distortions. Panel B of Table 7 shows that very similar results are obtained if we use the fixed effects least squares estimator instead of the system-GMM estimator.

Another way to examine short-run and longer-run growth effects is by means of a distributed lag model. In Table 8 we show the results for the case of including, in addition to the year t effect, the year t-1 and t-2 effects of within-country changes in the absolute RRA. The least-squares estimates on the lagged effects are negative in sign, but statistically insignificant and quantitatively much smaller in absolute size than the estimated impact effect for most of the specifications. Moreover, these regressions show that including additional lags of the RRA on the right-hand side of the estimating equation changes little the coefficient on the contemporaneous effect of policy distortions on economic growth. The main conclusion from our panel fixed-effects estimates is thus that increases in distortions to relative agricultural prices have a significant negative effect on average income in Sub-Saharan African countries.

For welfare purposes, it is also of interest to examine whether these distortions had a significant negative effect on private consumption. In Table 9 we therefore report panel fixed effects estimates of the effect that changes in the absolute relative rate of assistance had on real consumption per capita growth. In similar spirit to Table 8, we report estimates of the cumulative effect of changes in the absolute RRA in the years t to t-2, which allows us to focus on the longer-run effects. Our main finding is a significant negative effect of changes in the absolute RRA on consumption growth. This is true regardless of whether we consider the largest possible Sub-Saharan African sample (column (1)); exclude outliers (column (2)); exclude the pre-1985 period (column (3)); or include additional within-country control variables on the right-hand side of the regression (column (4)). Hence, Table 9 reinforces the earlier finding that decreases in the absolute RRA led to a significant increase in economic well-being in Sub-Saharan Africa.

5. Extensions

5.1 Anti- versus Pro-Agricultural Bias

Our regressions, so far, used the absolute relative rate of assistance as a measure for relative price distortions. Thus, those estimates imposed the restriction that the effects of changes in the RRA are symmetric for an anti-agricultural bias (negative RRA) and a pro-agricultural bias (positive RRA). As discussed in Section 2, the further the RRA is away from zero the larger the price distortions. Hence, our prior results focused on the question of whether reducing distortions towards the free market setting is growth promoting.

However, there are two slightly different, but related, questions that our prior results are unable to speak to. One is: do movements of the relative rate of assistance towards zero have similar growth effects when reducing a pro-agricultural bias as when reducing an anti-agricultural bias? The other question is: could it be that increases in the relative rate of assistance have in general a negative effect on economic growth (i.e., regardless of whether a pro- or anti-agricultural bias is in place), for example due to more positive growth externalities in the non-agricultural sector?

To explore these two questions we now use the change in the relative rate of assistance and, allow the coefficient on the change in the relative rate of assistance to differ depending on whether there is a pro-agricultural bias (positive RRA) or an anti-agricultural bias (negative RRA). Hence, we estimate an unrestricted version of equation (1) that can be written as:

(3) $$\Delta\ln(GDP_{i,t}) = \omega_i + \varphi_t + \eta^1\Delta(rra_{i,t}{}^{positive}) + \eta^2\Delta(rra_{i,t}{}^{negative}) + \varepsilon_{i,t}$$

Clearly, if in the above equation (3) $-\eta^1 = \eta^2$, then we can estimate equation (3) more efficiently by having just one parameter η for the change in the *absolute* value of the relative rate of assistance (as we did in Section 4). Economically, the implication of $-\eta^1 = \eta^2$ is that the effects on economic growth of reducing distortions from an anti-agricultural bias (negative RRA) towards the free market allocation are symmetric to the effects of reducing distortions from a pro-agricultural bias (positive RRA). Indeed, this is what one might expect if there is no difference in the extent of growth

externalities between the agricultural and non-agricultural sectors.

We also note that equation (3) allows us to examine the question of whether, in general, distorting agriculture in favor of non-agricultural production has an effect on economic growth. This can be done by testing the null hypothesis that $\eta^1=\eta^2$, and then testing the joint null hypothesis that $\eta^1=\eta^2=0$. If indeed we cannot reject that $\eta^1=\eta^2$, then a more efficient way to estimate equation (3) would be to use just one parameter η for the change in the relative rate of assistance, $\Delta(rra_{i,t})$. On the other hand, if we reject the restriction $\eta^1=\eta^2$, then this indicates that, in general, distorting agriculture in favor of non-agricultural production does not have a significant growth effect of the sort discussed in McMillan and Rodrik (2011). Using in that case just one parameter η for $\Delta(rra_{i,t})$ would obscure differences in effects that changes in the RRA have on economic growth for a pro-agricultural bias (when the RRA is above zero) and for an anti-agricultural bias (when the RRA is below zero).

Panel A of Table 10 presents our estimates of equation (3).[13] In column (1) we report estimates where the dependent variable is the growth rate of real GDP per capita; in column (2) we report estimates where the dependent variable is the growth rate of real consumption per capita. In order to focus on the longer-run effects as in Tables 9 and 10, we report estimates of the cumulative effect of changes in the RRA in the years t to t-2. The main findings are as follows:

1. Increases in the relative rate of assistance in the presence of a pro-agricultural bias (i.e. when the RRA is above zero) have a significant negative effect on GDP per capita growth and consumption growth. This can be seen from the negative coefficient on $\Delta(\text{RRA}^{\text{Positive}})$. Quantitatively, the estimate implies that a one percentage point increase in the relative rate of assistance above zero reduced per capita growth by about 0.11 percentage points for GDP, and by about 0.17 percentage points for consumption.

---

13 In Appendix Table 2 we document, using instrumental variables estimation (the instruments are the same as in Section 4.1), that there continues to be a statistically insignificant effect of economic growth on changes in the relative rate of assistance, when distinguishing between positive and negative RRA values.

2. Increases in the relative rate of assistance in the presence of an anti-agricultural bias (i.e. when the RRA is below zero) have a significant positive effect on growth in GDP and consumption per capita. This can be seen from the positive coefficient on $\Delta(\text{RRA}^{\text{Negative}})$. Quantitatively, the estimate implies that a one percentage point increase in the relative rate of assistance from below zero increased per capita growth by about 0.09 percentage points for GDP, and by about 0.11 percentage points for consumption. These estimates thus imply that deviations in the RRA away from zero – the free market equilibrium – have a significant negative effect on consumption and GDP per capita growth.

3. The coefficients on the positive and negative RRA variable are quantitatively – in absolute value – of almost similar size. Therefore, we cannot reject the null hypothesis that $-\eta^1 = \eta^2$. The p-value of this test is 0.68 for GDP per capita growth and 0.30 for consumption growth. Hence, there is no evidence that the restriction we imposed for our baseline analysis in Section 4 is violated.

4. We can reject the null hypothesis, at the 1 percent significance level, that $\eta^1 = \eta^2$: the p-value of this test is 0.004 for GDP per capita growth and 0.005 for consumption growth. This indicates that, in general, discouraging agricultural production to favor of non-agricultural sectors does not have a significant growth effect. To convey this result in a more intuitive and direct way, we report in Panel B of Table 10 our estimates from a regression of GDP per capita and consumption growth on $\Delta(rra_{i,t})$. The main finding is that the average effect of changes in the relative rate of assistance on GDP per capita and consumption growth is insignificant. Econometrically, this result is not surprising since Panel A shows that the coefficient on the negative RRA variable is close to the negative of the coefficient on the positive RRA variable. Hence, using just one RRA variable that does not distinguish between whether there is a pro- or anti-agricultural bias leads to an insignificant average effect.

In sum, result (3) confirms our finding in Section 4.2 that movements in the RRA away from zero have a significant negative effect on GDP per capita and consumption growth. Result (3) also shows that reductions in RRA distortions in an anti-agricultural regime have similar growth and consumption effects as reductions in RRA distortions in a pro-agricultural regime. Hence, that result does not lend support to the view that there is an asymmetry in the effects of RRA distortions on economic growth and consumption. Furthermore, in conjunction with result (3), result (4) rejects the hypothesis that, in general, decreasing the assistance to agricultural relative to non-agricultural production has a significant positive growth effect. Instead Table 10 shows that this effect depends on whether a pro-agricultural bias or an anti-agricultural bias is in place; and hence whether distortions are reduced towards the free market allocation.

5.2 Level vs. Growth Effects

As we pointed out in Section 3, our estimating equation that relates the change in the log of GDP to the change in the absolute RRA is consistent with a permanent increase in the absolute RRA having a permanent effect on the level of GDP per capita. We can also examine whether there is evidence that a permanent increase in the level of the RRA has a permanent effect on the GDP per capita growth rate. If there is only a level effect, then we should not expect that the sum of the coefficients on the current and lagged level of the RRA has a significant effect on GDP per capita growth. To see this in the simplest possible way, note that equation (1) can also be written as

(1') $\quad\quad\quad\quad \Delta\ln(GDP_{i,t}) = \alpha_i + \beta_t + \eta\,\text{abs}(rra_{i,t}) - \eta\,\text{abs}(rra_{i,t-1}) + u_{i,t}$

We can then test whether there is also a growth effect of a permanent change in the level of the RRA by estimating the following model:

(4) $\quad\quad\quad\quad \Delta\ln(GDP_{i,t}) = \kappa_i + \lambda_t + \eta^3\,\text{abs}(rra_{i,t}) + \eta^4\,\text{abs}(rra_{i,t-1}) + \sigma_{i,t}$

As can be clearly seen from (1'), if there is an effect of a change in the level of the RRA on only the level of GDP per capita, it must be that $\eta^3 + \eta^4 = 0$. On the other hand, if $\eta^3 + \eta^4 \neq 0$, then there is also an

effect on the GDP per capita growth rate. To see this more directly, note that if $\eta^3+\eta^4 \neq 0$, then we can rewrite equation (4) as

(4')
$$\Delta\ln(GDP_{i,t}) = \kappa_i + \lambda_t + \eta^3\Delta\text{abs}(rra_{i,t}) + (\eta^3+\eta^4)\text{abs}(rra_{i,t-1}) + \sigma_{i,t}$$

Hence, $\eta^3+\eta^4$ captures the effect that a permanent change in the level of the RRA has on the GDP per capita growth rate.[14]

In Table 11 we report the relevant estimates for testing whether there is a significant growth effect associated with changes in the level of the absolute RRA. As discussed, we can conclude there is a significant growth effect if the sum of the coefficients on the contemporaneous and lagged RRA is significantly different from zero. In column (1) we report estimates of the sum for the t and t-1 effect, i.e. $\eta^3+\eta^4$ as suggested by the simplest possible model in equation (4). To ensure that we are really picking up the effect of permanent changes in the level of the absolute RRA, we report results from regressions that add additional lags of the level of the absolute RRA on the right-hand side of the estimating equation. Hence, in column (2) we report the results for the sum of the t to t-2 effect, column (3) the sum of the t to t-3 effect, and column (4) the sum of the t to t-4 effect. In columns (5) to (8) we repeat the exercise using the growth rate of consumption per capita instead of the growth rate of GDP per capita as the dependent variable. As can be seen, the main result is that the sum of coefficients on the current and lagged level of the RRA is negative and significant at the conventional confidence levels. Quantitatively, the estimates in columns (4) and (9) suggest that a one standard deviation increase in the absolute relative rate of assistance (0.23) reduced GDP per capita and consumption growth by over one and half percentage points per annum. Hence, our analysis suggests that distortions to the relative price between agricultural and non-agricultural products had a significant negative effect on Sub-Saharan African countries' GDP and consumption per capita growth rates.

---

14 See, for example, Bond et al. (2010) for an application of this strategy to testing whether the positive effect of an increase in the level of investment is limited to the level of GDP per capita, or whether it also has an effect on the GDP per capita growth rate.

To strengthen the above result of a significant long-run growth effect of distortions to the relative price between agricultural and non-agricultural products, we report in Table 12 estimates that are based on 5-year, non-overlapping panel data. Before discussing those results, we would like to re-state here that using 5-year, non-overlapping panel data has the disadvantage of reducing the number of observations, and thus statistical power, substantially. In addition, the short-run relationship between the RRA and GDP growth is of substantial interest, too. By construction, 5-year panel data eliminates much of this short-run relationship. Nevertheless, we realize that it has been common practice in the growth literature to report estimates that are based on 5-year, non-overlapping panels. These estimates should be viewed as complimentary to the distributed lag estimates that we reported in Table 11.

Thus, with the above in mind, it is gratifying that the estimates in Table 12 produce very similar results to the distributed lag estimates that are based on annual panel data. Controlling for country fixed effects as well as time (i.e. 5-year) fixed effects, Table 12 shows that increases in the absolute relative rate of assistance had a significant negative effect on GDP and consumption growth. Quantitatively, the estimates in columns (1) and (2) imply that, on average, a ten percentage point increase in the absolute relative rate of assistance reduced GDP growth over a 5-year period by around one percentage point; and consumption growth by around half a percentage point. Columns (3) and (4) document that this significant negative effect of increases in the absolute RRA on GDP and consumption growth continues to hold when excluding outliers. These fixed effects estimates, that are based on 5-year non-overlapping panel data, therefore reconfirm the main finding of our baseline analysis: distortions to the relative price between agricultural and non-agricultural products have had a significant negative GDP and consumption growth effect in Sub-Saharan African countries.

5.3 Effects on Sector Size, Agricultural Production, and Investment

We now examine the effects that changes in the relative rate of assistance to agriculture had on the size of the agricultural sector, real agricultural output growth, and aggregate investment. To start off the discussion on these channels through which variations in the RRA have real effects on the macroeconomy, we document in column (1) of Table 13 that increases in the relative rate of assistance to agriculture significantly increased agriculture's share of GDP. Controlling for country and year fixed effects, the estimates in column (1) imply that, on average, a ten percentage points increase in the RRA increased agriculture's share of GDP by nearly 0.3 percentage points. Column (2) of Table 13 shows that increases in the RRA led to significant decreases of the manufacturing sector's share of GDP. Quantitatively, the estimates in column (2) imply that a ten percentage points increase in the RRA decreased the share of manufacturing in GDP by around 0.15 percentage points. The estimated effects of increases in the RRA on the relative size of the manufacturing sector are negative and, in absolute terms, they are around half the size of the estimated effects that changes in the RRA had on the relative size of the agricultural sector. This difference is not surprising given that, during the sample period, the average share of Sub-Saharan Africa's GDP from agriculture was nearly three times that of manufacturing. In statistical terms, the estimated effects in columns (1) and (2) are significant at the 10 and 5 percent level, respectively. The results in columns (1) and (2) are therefore a first indication that variations in the RRA have real effects on the economy because these variations significantly affect the relative size of the agricultural sector.

We provide further evidence that variations in the RRA had real effects on the economy by documenting that changes in the RRA were positively associated with changes in FAOSTAT's agricultural production index. The FAOSTAT agricultural production index is an index of real agricultural production. This index is based on the Laspeyre formula and reflects the relative level of the aggregate volume of agricultural production for each year in comparison with the base period 2004-2006 (FAOSTAT, 2011). All intermediate primary inputs of agricultural origin are deducted. Thus, the FAOSTAT agricultural production index is an index of disposable production. Column

(3) of Table 13 shows that increases in the RRA induced significant increases in the agricultural production index: a ten percentage points increase in the RRA increased the agricultural production index by around 0.3 percent. This estimated effect is significantly different from zero at the 5 percent significance level. Hence, column (3) provides supportive evidence for the hypothesis that reductions in disincentives to agricultural production yielded real increases in agricultural output.

FAOSTAT also provides data on the economically active population in agriculture; i.e. the labor force that is active in the agricultural sector. Unfortunately, no long time-series data exist for actual employment (and unemployment) in agriculture, but data are available from FAOSTAT (2011) on the allocation of the labor force between the agricultural and non-agricultural sectors. Variations in the labor force share in agriculture provide another useful indication of the real effects that variations in the RRA had on the macroeconomy. Column (4) shows that increases in the RRA induced significant decreases in the share of the labor force that is in the agricultural sector. The estimates in column (4) imply that, on average, a ten percentage points increase in the RRA decreased the share of the labor force in the agricultural sector by around 0.01 percentage points. Given that the sample standard deviation of the change in the share of the labor force in agriculture is less than 0.003, this is not a small effect.[15] And, statistically, the estimated effect is significant at the 5 percent level.

In particular, column (4) tells us that reductions in disincentives to agriculture were associated with a significant release of labor from agriculture to the non-agricultural sector. One possible part-explanation for this is that, if work in agriculture has the objective of providing a minimum level of income for the family -- which, in the context of extremely poor Sub-Saharan African countries, appears plausible -- then labor is released from agriculture to allow some members of now-higher-income farm families to search for better prospects in other sectors of the

---

15 Another way to see this is by interpreting the estimates in column (4) in terms of standard deviations. Recalling that the sample standard deviation for the change in the relative rate of assistance is 0.167, the estimates in column (4) imply that a one standard deviation change in the relative rate of assistance induced a change in the share of the labor force in agriculture of 0.06 standard deviations.

economy.[16] Another possible part-explanation is that a rise in the RRA induces innovation in agriculture, and if that involves adopting new farm technologies from higher-wage countries then it is likely to be labor-saving and thus reduce the need for as many laborers on farms (Hayami and Ruttan, 1985).

To complete the picture we would, ideally, like to examine the effect of variations in the RRA on the capital stock in the agricultural sector. Unfortunately, no time-series data on investment or the capital stock in the agricultural sector are available for the set of African countries in our sample.[17] In order to examine at least the effects that reductions in disincentives to agriculture had on aggregate investment, we report in column (5) of Table 13 estimates for the overall investment to GDP ratio. Controlling for country and year fixed effects, our estimates yield a significant positive within-country effect of variations in the RRA on the investment to GDP ratio. Quantitatively, the estimates imply that a ten percentage points increase in the RRA increased the investment to GDP ratio by around 0.1 percentage points. The estimated effect is significant at the 10 percent significance level. Hence, column (5) suggests that reductions in disincentives to agricultural production were associated with systematic increases in real aggregate investment.

5.4 Effects on International Trade

In this section we explore whether and to what extent distortions to relative agricultural prices affected Sub-Saharan African countries' exports and imports. Trade openness is one of the most robust determinants of economic growth in growth regressions (e.g. Ciccone and Jarocinski, 2010). It appears natural, therefore, to also explore here the effects of within-country RRA variations on

---

16 We note, without further evidence on the causal relationship between the labor force in the non-agricultural sector and GDP growth, that it is unclear whether the re-allocation to the non-agricultural sector had direct positive GDP growth effects. Brückner (2012) shows that in the context of the group of Sub-Saharan African countries (of which many are characterized by unusually large primal cities and strong ethnic divisions) increases in the urbanization rate had a significant negative GDP per capita growth effect.

17 The panel data set of 62 industrial and developing countries that is provided by Larson et al. (2000) only covers one of the countries in our sample (Tanzania). That sample was expanded by Butzer, Mundlak and Larson (2010) and even more by Daidone and Anríquez (2011), but even the latter set includes only 4 of our 16 African countries and for a shorter time period.

Sub-Saharan African countries' international trade.

Table 14 shows that distortions to relative agricultural prices were associated with significant reductions in exports and imports. The estimated coefficient on the absolute relative rate of assistance to agriculture is negative and significant at the 5 percent level. This is true when the standard measure of trade openness is used (exports plus imports over PPP GDP), see column (1); and it is also true when focusing more narrowly on exports, see column (4). Quantitatively, the estimated coefficients imply that, on average, a one percentage point increase in the absolute relative rate of assistance reduced the GDP share of exports plus imports by 0.18 percentage points; and the GDP share of exports by 0.05 percentage points. Hence, distortions to relative agricultural prices had negative effects on both exports and imports. This evidence again suggests that consumers and producers in Sub-Saharan African countries were hurt by distortions to relative agricultural prices, from the export side for producers and from the import side for consumers.

There is also no evidence that, in general, subsidizing other sectors at the expense of agriculture is beneficial for a country's international trade. Columns (2) and (5) of Table 14 show that the average effects of variations in the relative rate of assistance on exports and imports are insignificant. To understand that result, note from columns (3) and (6) that the effects of changes in the relative rate of assistance on international trade depend on whether a pro- or anti-agricultural bias had been in place. There is a significant negative effect on international trade from increasing the relative rate of assistance to agriculture when a pro-agricultural bias is in place. However, when an anti-agricultural bias is in place the opposite holds, in which case increasing the relative rate of assistance to agriculture has a positive effect on international trade. Evidently the average effect of changes in the relative rate of assistance on international trade is insignificant because these two effects offset each other. These results make it clear that distortions to relative agricultural prices from their free market levels affect the ability of Sub-Saharan African countries to compete in the international market.

6. Conclusion

In the 1960s and 1970s, farm output in Sub-Saharan African countries was discouraged by heavy export taxation, along with overvalued exchange rates and import protection for manufacturers. Since the 1980s these direct and indirect disincentives to farm have been reduced, albeit much less rapidly than in other developing countries (Anderson, 2009). The dismal growth performance of Sub-Saharan Africa's agrarian economies over the past half century provides an important case study for exploring whether (and by how much) distortions to agricultural incentives have slowed economic growth.

We have addressed this issue empirically using rigorous panel fixed effects estimation techniques. Our fixed effects analysis finds that, during the period 1960 to 2005, a one standard deviation increase in distortions to relative agricultural prices decreased real GDP per capita growth by about one and a half percentage points per annum on average. These results thus suggest that the anti-agricultural policy bias contributed significantly to Sub-Saharan Africa's disappointing growth performance.

Our findings are important for several reasons. First, they imply that reducing distortions to incentives faced by even the world's poorest farmers can be growth-enhancing. Our findings thus do not support the view that there are significant growth benefits associated with policies supporting other tradable sectors such as manufacturing at the expense of agriculture. This is fortuitous for at least two reasons. One is that there is new evidence that lowering disincentives to agriculture also reduces inequality and poverty globally (de Janvry and Sadoulet, 2010; Anderson, Cockburn and Martin, 2011). The other is that the latest surge of globalization is being spurred by off-shoring an ever-rising proportion of industrial production processes, which provides even-higher rewards than in the past to countries that reform their price-distorting policies (Baldwin, 2011).

Second, our empirical analysis shows that there is a significant within-country effect of

policy distortions on economic growth. This also is an important result: it implies that the relationship between price distortions and economic growth is unlikely to be a consequence of, for example, the strong ethnic divisions that characterize many Sub-Saharan African countries. The reason is that ethnic divisions, as measured by countries' ethnic fractionalization or polarization, are mostly time-invariant variables. Hence, these variables cannot be a cause of within-country variations in price distortions during the sample period.

Third, our findings suggest that the returns from investments in agricultural development could be greater in countries with less distorted relative prices. Funding for agricultural development in Sub-Saharan Africa is expanding rapidly at present, particularly via development assistance programs. See, for example, the wide range of major donor partners that have joined with the Alliance for a Green Revolution for Africa; the Partnership to Cut Hunger and Poverty in Africa; or the contribution from the Bill and Merlinda Gates Foundation. Our findings provide additional empirical support to those arguing that aid flows would be more effective if those numerous African countries that still have an anti-agricultural policy bias (see Figure 1) were to reduce it.

References

Alcala, F. and A. Ciccone (2004), 'Trade and Productivity', *Quarterly Journal of Economics* 119(2): 613-46, May.

Aghion, P., M. Dewatripont, L. Du, A. Harrison, and P. Legros (2012). "Industrial Policy and Competition." Unpublished Manuscript Harvard University.

Anderson, K. (ed.) (2009), *Distortions to Agricultural Incentives: A Global Perspective, 1955– 2007,* London: Palgrave Macmillan and Washington DC: World Bank.

Anderson, K., J. Cockburn and W. Martin (2011), 'Would Freeing Up World Trade Reduce Poverty and Inequality? The Vexed Role of Agricultural Distortions', *The World Economy* 34(4): 487- 515, April.

Anderson, K., M. Kurzweil, W. Martin, D. Sandri and E. Valenzuela (2008), 'Measuring Distortions to Agricultural Incentives, Revisited', *World Trade Review* 7(4):1-30.

Anderson, K. and W. Masters (eds.) (2009), *Distortions to Agricultural Incentives in Africa*, Washington DC: World Bank.

Anderson, K. and E. Valenzuela (2008), *Global Estimates of Distortions to Agricultural Incentives, 1955 to 2007*, data spreadsheets available at www.worldbank.org/agdistortions.

Baldwin, R. (2011), 'Trade and Industrialization After Globalization's 2nd Unbundling: How Building and Joining a Supply Chain are Different and Why it Matters', NBER Working Paper 17716, Cambridge MA.

Barrios, S., L. Bertinelli and E. Strobl (2010), 'Trends in Rainfall and Economic Growth in Africa: A Neglected Cause of the African Growth Tragedy', *Review of Economics and Statistics* 92(2): 350-66, May.

Bates, R.H. (1981), *Markets and States in Tropical Africa,* Berkeley and Los Angeles: University of California Press.

Blundell, R. and S. Bond (1998), 'Initial Conditions and Moment Restrictions in Dynamic Panel

Data Models', *Journal of Econometrics* 87(1): 115-43.

Bond, S., A. Leblebicioglu and F. Schiantarelli (2010), 'Capital Accumulation and Growth: A New Look at the Empirical Evidence', *Journal of Applied Econometrics* 25: 1073-99.

Brückner, M. (2012), 'Economic growth, size of the agricultural sector, and urbanization in Africa', *Journal of Urban Economics* 71: 26-36.

Brückner, M. and A. Ciccone (2011), 'Rainfall and the Democratic Window of Opportunity', *Econometrica* 79(3): 923-47, May.

Brückner, M. and A. Ciccone (2010), 'International Commodity Price Shocks, Growth, and the Outbreak of Civil War in Africa', *Economic Journal* 120(544): 519-34, May.

Butzer, R.,Y. Mundlak and D. Larson (2010), 'Measures of Fixed Capital in Agriculture', World Bank Policy Research Working Paper No. 5472, Washington DC, November.

Ciccone, A. and M. Jarocinski (2010), "Determinants of Economic Growth: Will Data Tell?", *American Economic Journal: Macroeconomics* 2: 223-247.

Clemens, M. and J. Williamson (2004), 'Why Did the Tariff-Growth Correlation Reverse After 1950?', *Journal of Economic Growth* 9(1): 5–46, March.

Collier, P. and B. Goderis (2007), 'Commodity Prices, Growth, and the Natural Resource Curse: Reconciling a Conundrum', CSAE Working Paper 276, University of Oxford (revised December 2009).

Croser, J.L. and K. Anderson (2011), 'Agricultural Distortions in Africa: Trade and Welfare Indicators, 1961 to 2004', *World Bank Economic Review* 25(2): 250-77.

CSCW (2010), 'Armed Conflict Dataset', Online Database. www.prio.no/CSCW/Datasets/Armed-Conflict

Daidone, S. and G. Anríquez (2011), 'An Extended Cross-country Database for Agricultural investment and Capital', ESA Working Paper 11-16, FAO, Rome, June.

de Janvry, A. and E. Sadoulet (2010), 'Agricultural Growth and Poverty Reduction: Additional Evidence', *World Bank Research Observer* 25(1): 1-20, February.

Dervis, K., J. de Melo, and S. Robinson (1981), 'A General Equilibrium Analysis of Foreign Exchange Shortages in a Developing Country', Economic Journal 91: 891–906.

Edwards, S. (1992), 'Trade Orientation, Distortions and Growth in Developing Countries', *Journal of Development Economics* 39(1): 31-57, July.

Easterly, W. and R. Levine (1997), 'Africa's Growth Tragedy: Policies and Ethnic Divisions,' *Quarterly Journal of Economics* 112(4): 1203-50, November.

FAOSTAT (2011), *FAOSTAT Database on Agriculture,* Online Database.

Frankel, J.A. and D. Romer (1999), 'Does Trade Cause Growth?' *American Economic Review* 89(3): 379-99, June.

Hayami, Y. and V. Ruttan (1985), *Agricultural Development: An International Perspective* (revised and expanded edition), Baltimore MD: Johns Hopkins University Press.

Heston, A., R. Summers and B. Aten (2009), *Penn World Table Version 6.3,* Center for International Comparisons of Production, Income and Prices, University of Pennsylvania.

Krueger, A.O., M. Schiff and A. Valdés (1988), 'Agricultural Incentives in Developing Countries: Measuring the Effect of Sectoral and Economy-wide Policies', *World Bank Economic Review* 2(3): 255-72, September.

Larson, D., R. Butzer, Y. Mundlak, and A. Crego (2000), 'A Cross-Country Database for Sector Investment and Capital', *World Bank Economic Review* 14: 371-391.

Lerner, A. (1936), 'The Symmetry Between Import and Export Taxes', *Economica* 3(11): 306-13, August.

Maddala, G. and S. Wu (1999), 'A Comparative Study of Unit Root Tests With Panel Data and A New Simple Test', *Oxford Bulletin of Economics and Statistics* 61, 631-652.

Marshall, M. and K. Jaggers (2009), 'Polity IV Project: Political Regime Characteristics and

Transitions: 1800-2000', Online Database.

Matsuura, K. and C, Willmott (2007), 'Terrestrial Air Temperature and Precipitation: 1900-2006, Gridded Monthly Time Series, Version 1.01', University of Delaware, http://climate.geog.udel.edu/~climate/.

McMillan, M.S. and D. Rodrik (2011), 'Globalization, Structural Change and Productivity Growth', NBER Working Paper 17143, Cambridge MA, June.

Miguel, E., S. Satyanath and E. Sergenti (2004), 'Economic Shocks and Civil Conflict: An Instrumental Variables Approach', *Journal of Political Economy* 112(4): 725-53, August.

Ndulu, B.J. and S.A. O'Connell (2007), 'Policy Plus: African Growth Performance 1960-2000', Ch. 1 (pp. 3-75) in B.J. Ndulu, P. Collier, R.H. Bates and S. O'Connell (eds.), *The Political Economy of Economic Growth in Africa, 1960-2000*, Cambridge: Cambridge University Press.

O'Rourke, K. (2000), 'Tariffs and Growth in the late 19th Century', *Economic Journal* 110(463): 456–83, April.

Pesaran, M. and R. Smith (1995), 'Estimating Long-Run Relationships from Dynamic Heterogeneous Panels', *Journal of Econometrics* 68(1): 79-113.

Rodriguez, F. and D. Rodrik (2001), 'Trade Policy and Economic Growth: A Skeptic's Guide to Cross-National Evidence', in B.S. Bernanke and K.S. Rogoff (eds.), *NBER Macroeconomics Annual 2000*, Cambridge MA: MIT Press.

Sachs, J.D. and A. Warner (1995), 'Economic Reform and the Process of Global Integration', *Brookings Papers on Economic Activity* 1: 1-95.

Sandri, D., E. Valenzuela and K. Anderson (2007), 'Economic and Trade Indicators, 1960 to 2004', Agricultural Distortions Working Paper 02, World Bank, Washington DC.

Schularick, M. and S. Solomou (2011), 'Tariffs and Economic Growth in the First Era of Globalization', *Journal of Economic Growth* 16(1): 33-70, March.

Stock, J. and M. Yogo (2005), 'Testing for Weak Instruments in Linear IV Regression', pp. 80-108

in *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg*, ed. D. Andrews and J. Stock, Cambridge: Cambridge University Press.

Vamvakidis, A. (2002), 'How Robust is the Growth-Openness Connection? Historical Evidence', *Journal of Economic Growth* 7(1): 57–80, March.

Vousden, N. (1990), *The Economics of Trade Protection,* Cambridge and New York: Cambridge University Press.

Wacziarg, R. and K.H. Welch (2008), 'Trade Liberalization and Growth: New Evidence', *World Bank Economic Review* 15(3): 393-429, October.

WDI (2011), *World Development Indicators,* Online Database.

WTO (2010), *Tariff Profiles 2010,* Geneva: World Trade Organization.

Table 1. Descriptive Statistics

	Mean	Stdv.	Observations
Real GDP Per Capita	1637.1	713.0	532
Real GDP Per Capita Growth	0.004	0.078	532
Abs. Relative Rate of Assistance	0.372	0.225	532
Change Abs. Relative Rate of Assistance	-0.004	0.148	532
Relative Rate of Assistance	-0.323	0.292	532
Change Relative Rate of Assistance	0.002	0.167	532
Manufacturing GDP Share	0.123	0.057	408
Change in Manufacturing GDP Share	0.001	0.016	408
Agricultural GDP Share	0.334	0.138	408
Change in Agricultural GDP Share	-0.003	0.032	408
Agricultural Labor Force Share	0.732	0.119	518
Change in Agricultural Labor Force Share	-0.005	0.003	518
Exports plus Imports GDP Share	0.473	0.198	459
Change in Exports plus Imports GDP Share	0.005	0.101	459
Exports GDP Share	0.219	0.111	459
Change in Exports GDP Share	0.002	0.042	459
Growth in Agricultural Production	0.026	0.099	518
Investment GDP Share	0.087	0.062	518
Change in Investment GDP Share	0.001	0.003	518
Polity2 Score	0.685	7.498	394
Civil War Incidence	0.058	0.233	532

Table 2. The Effects of Economic Growth on the Relative Rate of Assistance
(2SLS and LS Estimates)

	Δabs(RRA)			
	Panel A: 2SLS			
	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within- Country Controls
Δln(GDP)	-0.04	0.35	-0.09	-0.10
	(0.10)	(0.27)	(0.27)	(0.12)
Hansen J, p-value	0.15	0.60	0.19	0.49
	First Stage for Δln(GDP)			
Δln(ComPIAgri)	2.03**	1.74*	5.34***	1.82***
	(0.80)	(0.96)	(0.38)	(0.65)
Δln(ComPINatres)	1.80***	1.62***	3.11**	2.93**
	(0.60)	(0.54)	(1.25)	(1.25)
Δln(Rainfall)	0.05**	0.05***	0.06***	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)
First-Stage, F-Statistic	18.45	14.55	149.46	10.52
	Panel B: LS			
Δln(GDP)	-0.15	-0.16	-0.25	-0.29*
	(0.13)	(0.15)	(0.25)	(0.15)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	532	522	280	390
Countries	14	14	14	12

Note: The dependent variable is the change in the absolute relative rate of assistance. The method of estimation in Panel A is two-stage least squares; Panel B least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variables the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 3. The Reduced-Form Effects of International Commodity Price Shocks and Rainfall Shocks
on the Relative Rate of Assistance
(Reduced Form Estimates)

	Δabs(RRA)			
	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within-Country Controls
Δln(ComPIAgri)	1.81 (1.17)	1.92 (1.16)	-0.37 (2.57)	2.25 (2.39)
Δln(ComPINatres)	-0.46 (0.32)	0.10 (1.17)	-1.00 (1.36)	-0.40 (0.31)
Δln(Rainfall)	0.02 (0.03)	0.02 (0.03)	0.05 (0.04)	0.00 (0.04)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	532	522	280	390
Countries	14	14	14	12

Note: The dependent variable is the change in the absolute relative rate of assistance. The method of estimation is least-squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variables the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 4. The Effects of Economic Growth on the Relative Rate of Assistance
(2SLS Estimates; Additional Tests of Exclusion Restriction)

Δabs(RRA)

Panel A: Excluded Instrument is Commodity Price Index

	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within-Country Controls
	2SLS	2SLS	2SLS	2SLS
Δln(GDP)	-0.09 (0.10)	0.38 (0.46)	-0.18 (0.26)	-0.11 (0.15)
Δln(Rainfall)	0.02 (0.03)	0.00 (0.05)	0.06 (0.04)	0.00 (0.04)
First-Stage, F-Statistic	11.41	7.97	164.13	9.42
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes

Panel B: Excluded Instrument is Rainfall

	(1)	(2)	(3)	(4)
Δln(GDP)	0.32 (0.57)	0.32 (0.66)	0.82 (0.76)	0.14 (1.19)
Δln(ComPIAgri)	1.25 (1.88)	1.38 (1.77)	-2.93 (3.83)	1.83 (4.70)
Δln(ComPINatres)	-1.10 (-0.64)	-0.45 (-0.22)	-5.41 (3.95)	-0.66 (2.56)
First-Stage, F-Statistic	8.14	8.87	12.26	2.82
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	532	522	280	390
Countries	14	14	14	12

Note: The dependent variable is the change in the absolute relative rate of assistance. The method of estimation is two-stage least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variables the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 5. The Effects of Economic Growth on the Relative Rate of Assistance
(2SLS Estimates; Distributed Lag Estimates)

		Δabs(RRA)		
	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within- Country Controls
Δln(GDP)	-0.07 (0.21)	0.54 (0.43)	-0.03 (0.22)	-0.20 (0.32)
Δln(GDP), t-1	0.37 (0.28)	0.45 (0.36)	-0.33 (0.24)	0.43 (0.27)
Δln(GDP), t-2	0.75 (0.46)	0.86 (0.56)	0.49*** (0.18)	0.58 (0.42)
First-Stage F-statistic	9.69	5.44	137.75	18.31
Hansen J, p-value	0.24	0.35	0.28	0.46
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	502	522	280	390
Countries	14	14	14	12

Note: The dependent variable is the change in the absolute relative rate of assistance. The method of estimation is two-stage least squares. The instrumental variables are rainfall and the international commodity export price indices. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variables the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 6. The Effects of Changes in the Relative Rate of Assistance on Economic Growth

	Δln(GDP)			
	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within-Country Controls
	LS	LS	LS	LS
Δabs(RRA)	-0.04*	-0.03*	-0.07	-0.06***
	(0.02)	(0.02)	(0.05)	(0.02)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	532	522	280	390
Countries	14	14	14	12

Note: The dependent variable is real GDP per capita growth. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variables the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 7. The Effects of Changes in the Relative Rate of Assistance on Economic Growth
(Controlling for Growth Dynamics)

Δln(GDP)

Panel A: SYS-GMM

	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within-Country Controls
	SYS-GMM	SYS-GMM	SYS-GMM	SYS-GMM
Δabs(RRA)	-0.04*	-0.04**	-0.05	-0.06***
	(0.02)	(0.02)	(0.03)	(0.02)
Δln(GDP), t-1	0.05	0.02	-0.01	0.11
	(0.07)	(0.08)	(0.09)	(0.08)
AR (1) Test, p-value	0.00	0.00	0.00	0.00
AR (2) Test, p-value	0.20	0.44	0.38	0.14
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	531	521	280	389
Countries	14	14	14	12

Panel B: LS

	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within-Country Controls
Δabs(RRA)	-0.04*	-0.03*	-0.07	-0.06***
	(0.02)	(0.02)	(0.05)	(0.02)
Δln(GDP), t-1	0.07	0.06	0.01	0.12
	(0.06)	(0.06)	(0.10)	(0.08)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	531	521	280	389
Countries	14	14	14	12

Note: The dependent variable is real GDP per capita growth. The method of estimation in Panel A is system-GMM. In Panel B the method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variables the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 8. The Effects of Changes in the Relative Rate of Assistance on Economic Growth
(Distributed Lag Estimates)

	Δln(GDP)			
	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within-Country Controls
	LS	LS	LS	LS
Δabs(RRA), t	-0.05**	-0.05***	-0.09	-0.07***
	(0.02)	(0.02)	(0.05)	(0.02)
Δabs(RRA), t-1	-0.03	0.00	-0.07*	-0.04
	(0.02)	(0.02)	(0.04)	(0.03)
Δabs(RRA), t-2	-0.01	0.00	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Sum of coefficients t to t-2	-0.09***	-0.05	-0.16**	-0.12**
	(0.03)	(0.04)	(0.07)	(0.06)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	517	502	280	382
Countries	14	14	14	12

Note: The dependent variable is the change in the log of real GDP per capita. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variables the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 9. The Effects of Changes in the Relative Rate of Assistance on Consumption

	Δln(Consumption)			
	(1)	(2) Excluding Top/Bottom 1 Percentile	(3) Excluding the Pre-1985 Period	(4) Additional Within-Country Controls
	LS	LS	LS	LS
Δabs(RRA) [Sum of Coefficients t to t-2]	-0.13** (0.05)	-0.11* (0.06)	-0.21 (0.13)	-0.16** (0.06)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	517	502	280	382
Countries	14	14	14	12

Note: The dependent variable is the change in the log of real consumption per capita. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Column (2) excludes observations of the top and bottom 1 percentile of the real GDP per capita growth distribution. Column (3) excludes observations for the pre-1985 period. Column (4) adds as control variable the within-country changes of the government expenditures share, the log of the price level, the exchange rate, the share of exports plus imports over PPP GDP, the polity2 score, and an indicator variable for the incidence of civil war. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 10. Effects of Changes in the Relative Rate of Assistance: Anti- vs Pro-Agricultural Bias

	Δln(GDP)	Δln(Consumption)
	(1)	(2)
	LS	LS
Panel A: Estimates Using Δ(RRA^{Positive}) and Δ(RRA^{Negative})		
Δ(RRA^{Positive})	-0.11**	-0.17**
[Sum of Coefficients t to t-2]	(0.03)	(0.04)
Δ(RRA^{Negative})	0.09*	0.11*
[Sum of Coefficients t to t-2]	(0.05)	(0.06)
P-value: Test that Δ(RRA^{Positive}) = -Δ(RRA^{Negative})	0.68	0.30
P-value: Test that Δ(RRA^{Positive}) = Δ(RRA^{Negative})	0.00	0.00
P-value: Test that Δ(RRA^{Positive}) = Δ(RRA^{Negative})= 0	0.00	0.00
Panel B: Estimates Using Δ(RRA)		
Δ(RRA)	0.01	0.01
[Sum of Coefficients t to t-2]	(0.06)	(0.07)
Country Fe	Yes	Yes
Year Fe	Yes	Yes
Observations	517	517
Countries	14	14

Note: The dependent variable in column (1) is the change in the log of real GDP per capita; column (2) the change in the log of real consumption per capita. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 11. Effect in the Level of the RRA on the Growth Rate of GDP and Consumption

	Δln(GDP)				Δln(Consumption)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LS	LS	LS	LS	LS	LS	LS	LS
Sum of Coefficients	t to t-1	t to t-2	t to t-3	t to t-4	t to t-1	t to t-2	t to t-3	t to t-4
abs(RRA)	-0.07**	-0.08**	-0.08**	-0.09*	-0.06**	-0.07**	-0.05	-0.07*
	(0.04)	(0.04)	(0.04)	(0.05)	(0.03)	(0.03)	(0.04)	(0.04)
Country Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	531	531	517	503	531	531	517	503
Countries	14	14	14	14	14	14	14	14

Note: The dependent variable in columns (1)-(4) is the change in the log of real GDP per capita; columns (5)-(8) the change in the log of real consumption per capita. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 12. Effects of the RRA on GDP and Consumption Growth: 5-Year Non-Overlapping Panel

	Δln(GDP)	Δln(Consumption)	Δln(GDP)	Δln(Consumption)
			Excluding Top and Bottom 1 Percentile	
	(1)	(2)	(3)	(4)
	LS	LS	LS	LS
abs(RRA), 5 years	-0.10***	-0.06**	-0.06***	-0.05*
	(0.04)	(0.03)	(0.02)	(0.02)
Country Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	99	99	97	97
Countries	14	14	14	14

Note: The dependent variable in columns (1) and (3) is the change in the log of real GDP per capita over five, non-overlapping years; in columns (2) and (4) the dependent variable is the change in the log of real consumption per capita over five, non-overlapping years. The explanatory variable *abs(RRA), 5 years* refers to the average absolute relative rate of assistance over those five, non-overlapping years. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. Columns (3) and (4) exclude the 1st and 99th percentile of the 5-year, non-overlapping GDP and consumption growth data. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 13. Effects on Sector Shares and Real Agricultural Production

	ΔAgriGDPShare	ΔManuGDPShare	Δln(AgriProd)	ΔAgriLFShare	ΔInvGDPShare
	(1)	(2)	(3)	(4)	(5)
	LS	LS	LS	LS	LS
ΔRRA	0.027*	-0.016**	0.029**	-0.001**	0.010*
	(0.015)	(0.007)	(0.013)	(0.0003)	(0.006)
Country Fe	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes
Observations	408	408	518	518	518
Countries	14	14	14	14	14

Note: The dependent variable in column (1) is the change in the GDP share of agricultural value added; column (2) the change in the GDP share of manufacturing value added; column (3) the change in the log of the agricultural production index; column (4) the change in the share of the labor force in agriculture; and column (5) the share of investment in GDP. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Table 14. Effects on Exports and Trade Openness

	Δ(Exp+Imp/GDP)			Δ(Exp/GDP)		
	(1)	(2)	(3)	(4)	(5)	(6)
	LS	LS	LS	LS	LS	LS
Δabs(RRA)	-0.18**			-0.06**		
	(0.08)			(0.02)		
ΔRRA		0.12			0.01	
		(0.09)			(0.04)	
ΔRRA^{Pos}			-0.09**			-0.07***
			(0.05)			(0.01)
ΔRRA^{Neg}			0.21***			0.05
			(0.08)			(0.04)
P-value: Test that Δ(RRA^{Pos}) =-Δ(RRA^{Neg})			0.15			0.54
P-value: Test that Δ(RRA^{Pos}) = Δ(RRA^{Neg})			0.01			0.01
P-value: Test that Δ(RRA^{Pos}) =Δ(RRA^{Neg})=0			0.04			0.00
Country Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Observations	459	459	459	459	459	459
Countries	13	13	13	13	13	13

Note: The dependent variable in columns (1)-(3) is the change in the GDP share of exports plus imports; columns (4)-(6) the change in the GDP share of exports. The method of estimation is least squares. Huber robust standard errors (shown in parentheses) are clustered at the country level. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

Appendix Table 1. Maddala and Wu (1999) Unit Root Test

	Trend		No Trend	
	Lag 0	Lag 1	Lag 0	Lag 1
ln(GDP)	0.97	0.98	0.99	0.98
Δln(GDP)	0.00	0.00	0.00	0.00
RRA	0.00	0.00	0.00	0.00
abs(RRA)	0.00	0.00	0.00	0.00

Appendix Table 2. Response of Changes in the RRA to Economic Growth for Positive and Negative RRA Values

	Δ(RRA)	Δ(RRA^{Positive})	Δ(RRA^{Negative})
	(1)	(2)	(3)
Δln(GDP)	0.13	-0.00	0.13
	(0.10)	(0.03)	(0.10)
Hansen J, p-value	0.17	0.25	0.25
First-Stage F-statistic	18.45	18.45	18.45
Country Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Observations	532	532	532
Countries	14	14	14

Note: The dependent variable is the change in the relative rate of assistance; column (2) the change in the RRA for strictly positive RRA values (negative values are set to zero); column (3) the change in the RRA for strictly negative RRA values (positive values are set to zero). The method of estimation is two-stage least squares. The instrumental variables are the international commodity export price index and rainfall. *Significantly different from zero at the 10 percent significance level, ** 5 percent significance level, *** 1 percent significance level.

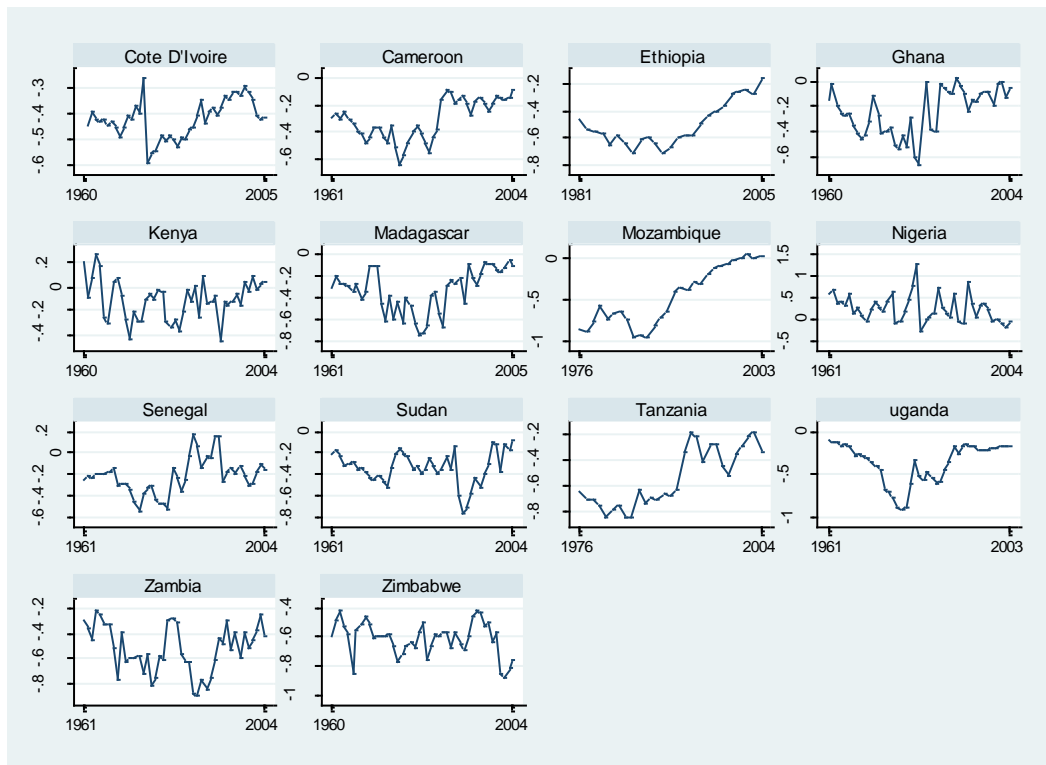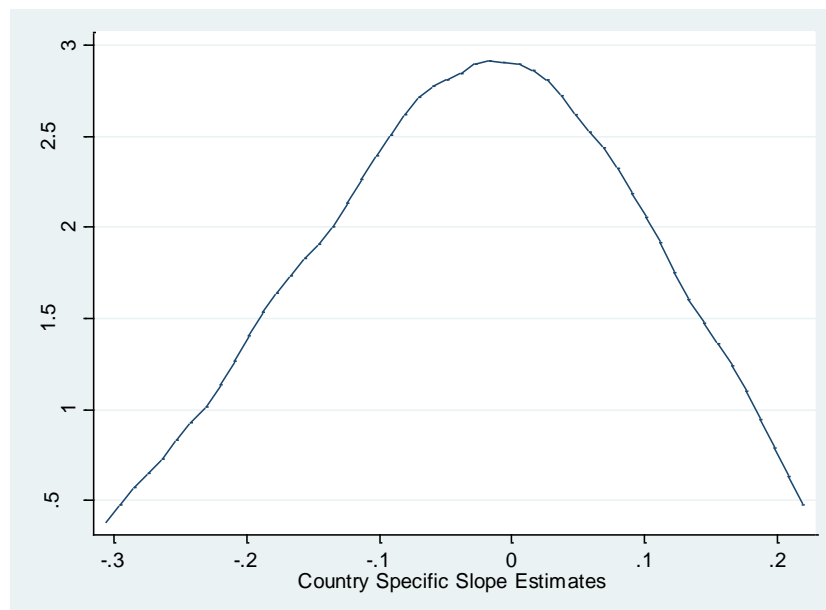Figure 1. Time-Series Plots of the Relative Rate of Assistance

Figure 2. Distribution of Country-Specific Slope Estimates

Note: The figure shows the kernel density plot of the country-specific slopes estimates that are obtained from a panel fixed effects regression where the dependent variable is the change of the log of real GDP per capita and the explanatory variable

is the change in the absolute relative rate of assistance. The kernel density plot is generated using an Epanechnikov kernel and a bandwidth of 0.08.