Agricultural Price Distortion and Stabilization: Stylized Facts and Hypothesis Tests

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Abstract

This paper describes agricultural policy choices and tests some predictions of political economy theories. It begins with three broad stylized facts: governments tend to tax agriculture in poorer countries, and subsidize it in richer ones, tax both imports and exports more than nontradables, and tax more and subsidize less where there is more land per capita. We test a variety of political-economy explanations, finding results consistent with hypothesized effects of rural and urban constituents' rational ignorance about small per-person effects, governance institutions' control of rent-seeking by political leaders, governments' revenue motive for taxation, and the role of time consistency in policy-making. We also find that larger groups obtain more favorable policies, suggesting that positive group size effects outweigh any negative influence from more free-ridership, and that demographically driven entry of new farmers is associated with less favorable farm policies, suggesting the arrival of new farmers erodes policy rents and discourages political activity by incumbents. Another new result is that governments achieve very little price stabilization relative to our benchmark estimates of undistorted prices, and governments in the poorest countries actually destabilize domestic prices.

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William A. Masters and Andres F. Garcia

This chapter describes agricultural policy choices and tests some predictions of major political economy theories, exploiting the new Anderson and Valenzuela (2008) dataset. We start by establishing three broad stylized facts: the development paradox (governments tend to tax agriculture in poorer countries, and subsidize it in richer ones), the prevalence of anti-trade bias (governments tend to tax both imports and exports more than nontradables), and the importance of resource abundance (governments tax more and subsidize less where there is more land per capita). We then test a variety of political-economy explanations, finding results consistent with hypothesized effects of rural and urban constituents' rational ignorance about small per-person effects, governance institutions' control of rent-seeking by political leaders, governments' revenue motive for taxation, and the role of time consistency in policy-making.

We find that larger groups obtain more favorable policies, suggesting that positive group size effects outweigh any negative influence from more free-ridership. Some of these results add to the explanatory power of our stylized facts, but others help explain them. A novel result is that demographically driven entry of new farmers is associated with less favorable farm policies, which is consistent with a model in which the arrival of new farmers erodes policy rents and discourages political activity by incumbents. Another new result is that governments achieve very little price stabilization relative to our benchmark estimates of undistorted prices, and governments in the poorest countries have actually destabilized domestic prices over the full span of our data. Price stability is often a stated goal of policy, and would be predicted by status-quo bias or loss aversion, but the stockholding or fiscal policies used to limit price changes are often unsustainable and prices tend jump when the intervention ends.

The chapter begins with an outline of the methodology adopted for the study. It then presents evidence for the three stylized facts mentioned above. The third section seeks to explain agricultural policy choices empirically, drawing on six political economy theories. It also tests a new explanation, based on demographic influences on political pressures. The final section of the chapter offers some conclusions.

Methodology

Following Anderson et al. (2008), our principal measure of agricultural trade policy is a tariff-equivalent “Nominal Rate of Assistance” (NRA), defined as:

$$NRA \equiv \frac{P_d - P_f}{P_f} \quad (1)$$

where P_d is the observed domestic price in local currency for a given product, country and year, and P_f is the estimated domestic price that would hold in the absence of commodity-market or exchange-rate intervention. By definition, such an NRA would be zero in a competitive free-trade regime, and positive where producers are subsidized by taxpayers or consumers. The NRA is negative where producers are taxed by trade policy, for example through export restrictions or an overvalued exchange rate. In a few cases, we use the absolute value of NRA in order to measure distortions away from competitive markets. Where national-average NRAs are used, they are value-weighted at the undistorted prices.

The NRA results we use are based on the efforts of country specialists to obtain the best possible data and apply appropriate assumptions about international opportunity costs and transaction costs in each market (see Anderson et al. 2008). There is inevitably much measurement error, but by covering a very large fraction of the world’s countries and commodities, over a very long time period, we can detect patterns and trends that might otherwise remain hidden.

The Anderson et al. project is designed mainly to measure policy effects on price levels, but it can also be used to measure policy effects on price variability from year to year, by comparing the variability of domestic prices with the variability of estimated free-trade prices, both expressed in natural logs. Ratio-detrending is used here to remove the time trend on prices, by regressing observed prices ($\ln(P_i)_O$) on time (t) as in equation (2) below, and using the resulting predicted values ($\ln(P_i)_{\Pr}$) defined in (3) to generate detrended prices ($\hat{P}_i$) in equation (4) as the ratio of observed over predicted prices:

$$\ln(P_i)_O = \alpha + \beta_i \cdot t + \varepsilon \quad (2)$$

$$\ln(P_i)_{\Pr} \equiv \alpha + \beta_i \cdot t \tag{3}$$

$$\hat{P}_i = \frac{\ln(P_i)_O}{\ln(P_i)_{\Pr}} \tag{4}$$

To compare the relative variation of domestic and free-trade prices, we use the standard deviation (*sd*) of each price, in a ratio that we call the Stabilization Index (SI):

$$SI \equiv \frac{sd(\hat{P}_f) - sd(\hat{P}_d)}{sd(\hat{P}_f)} \cdot 100 \tag{5}$$

A policy that does not influence proportional price stability at all, such as a strictly ad valorem tax or subsidy, would generate an SI of zero. Policies that stabilize domestic prices, such as a variable tariff that is negatively correlated with the world price, would generate a positive SI. And policies that de-stabilize domestic prices, such as import quotas that leave domestic prices vulnerable to large local supply or demand shocks, would generate a negative SI. Note that the SI for a particular product in a particular country is calculated over the 1960-2004 period for which our data are most complete, and refers to the ensemble of all policies over that time period. In this way, we capture not only the impact of a given policy on price stability while that policy is in place, but also the impact on stability of introducing or removing policies. Doing so is very important because many policies achieve short-term stability in unsustainable ways, causing prices to jump when the policy itself is changed.

The NRA and SI estimates allow us to describe key stylized facts about policy choices, and then test the degree to which the relationships implied by political economy models actually fit the data. Our tests are all variations on equation (6):

$$Y = \alpha + \beta \cdot X + \gamma \cdot Z + \varepsilon \tag{6}$$

Where Y represents the policy measures of interest (variously NRA at the country level, NRA at the product level, the absolute value of NRA, or SI), X is a set of regressors that describe stylized facts which could be explained by many different policymaking mechanisms (income, direction of trade, resource abundance, continent dummies), and Z represents regressors that are associated with a specific mechanism hypothesized to cause the policies we observe. Our empirical analysis aims to test the significance of introducing each variable in Z when controlling for X, and to ask whether introducing Z explains the stylized facts (that is, reduces the estimated value of β) or adds to them (that is, raises the equation's estimated R-squared without changing the estimated value of β), or perhaps adds no additional significance at all. Regressors for X and Z are drawn from public data disseminated by the World Bank, FAO, the Penn World Table or others, as detailed in the Annex.

The stylized facts of agricultural policy

Our dataset covers an extraordinary diversity of commodities and countries, with huge variation in agricultural policies. In this section we explore a few key stylized facts, to establish the background variation for which we will want to control when testing the predictions of specific theories. A given theory could help explain these patterns, or could fit the residual variation they leave unexplained. In either case, controlling for key characteristics of commodities and countries allows us to test each theory's explanatory power in a simple, consistent framework.

The stylized facts we consider include the oldest and most general observations about agricultural policy, linking policy choices to a commodity's direction of trade, a country's real income per capita, and its endowment of farmland per capita. The direction of trade might matter to the extent that agricultural policy is simply trade policy, and so could be linked to a government's more general anti-trade bias. A country's real income might matter to the extent that the role of agriculture changes with economic growth, so that it is subject to the development paradox. Finally, land abundance might matter because agriculture is a natural-resource intensive sector, and could be subject to a natural resource effect. We address each of these in turn below.

The anti-trade bias of governments is a key concern of economists, dating back to Adam Smith and David Ricardo who first described how restrictions on imports and exports affect incentives for specialization. In this chapter we capture anti-trade bias of domestic instruments as well as trade restrictions, by linking measured NRAs to whether a commodity is importable or exportable in a given country and year.

A second stylized fact is the development paradox, in which the governments of poorer countries are typically observed to impose taxes on farm production, while governments in richer countries typically subsidize it. The modern literature documenting this tendency begins with Bale and Lutz (1981), and includes notable contributions from Anderson, Hayami and Others (1986), Lindert (1991), Krueger, Schiff and Valdes (1991) among others. This pattern is paradoxical insofar as farmers are the majority and are poorer than non-farmers in low-income countries, whereas in high-income countries farmers are a relatively wealthy minority.

A third kind of pattern involves natural resource effects, whereby countries with a greater resource rent available for extraction from a sector may be tempted to impose a heavier tax burden on it. The political economy of resource taxation is often discussed regarding oil and other mineral resources, as in Auty (2001), while applications to agriculture include McMillan and Masters (2003) and Isham et al. (2005). For our purposes, the resource rent which may be available in agriculture is measured crudely here by arable land area per capita, allowing us to ask whether more land-abundant countries tend to tax the agricultural sector more (or subsidize it less), when controlling for both anti-trade bias and the development paradox.

Note that anti-trade bias could help account for the development paradox, to the extent that low-income countries tend to be net exporters of farm products while richer countries tend to be net importers of them. And both could be driven by changes in the relative administrative cost of taxation, insofar as a country's income growth and capital accumulation allows government to shift taxation from exports and imports (at the expense of farms and farmers) to other things (at the expense of firms and their employees). Thus we need to control for income when testing for anti-trade bias, and control for anti-trade bias when testing for the development paradox, while controlling for both of these when looking at resource effects.

To test the magnitude and significance of these patterns in the NRA data, we use data on the direction of trade from our own database, and data on a country's average income per capita data from the Penn World Table (2007). Income is defined here as real gross domestic product in PPP prices, chain indexed over time in international dollars at year-2000 prices. Finally, data on the agricultural sector's land abundance comes from FAOSTAT (2007), as the per-capita availability of arable land, defined as the area under temporary crops, temporary meadows for mowing or pasture, land under market and kitchen gardens, and land temporarily fallow.

A graphical view

Our analysis of stylized facts begins with a graphical view of the data, focusing on the development paradox and anti-trade bias across countries and regions. One way to test for significant differences in NRAs across the income spectrum is to draw a smoothed nonparametric regression line through the data, which then allows us to compare these relationships across trade sectors. The general tendency of governments in poorer countries to tax their farmers while governments in richer countries tend to subsidize them is illustrated with smoothed lines in Figure 1, showing countries' aggregate NRAs relative to their level of real per-capita income in that year. These are weighted averages of the NRAs for all covered products, summing across commodities by their value at undistorted prices, so as to represent the total burden of taxes or subsidies on farm production.

The relationship between taxation/protection and average per-capita income is strong but non-linear in the log of income, and is different for exportables and importables. Governments in the poorest countries have imposed heavy taxes on all kinds of farmers. Tax rates move rapidly towards zero as incomes rise, then at income levels of about one to eight thousand dollars per year they stabilize with slight protection of importables and strong taxation of exportables, and as incomes rise above that all products become heavily protected.

Before we turn to detailed hypothesis tests, we must ask whether the stylized facts in the historical data still apply today. Have liberalizations and other reforms eliminated these relationships? Each country case study provides an analytical history of policymaking by successive governments,[1] and it is clear from those studies that national trade policies are not determined in isolation: there are waves of policy change that occur more or less simultaneously across countries, driven by economic conditions and the spread of ideas. These policy trends are often geographically concentrated, perhaps due to common economic circumstances or intellectual conditions.

Figure 2 decomposes and summarizes the country NRAs into each region's average for all exportables, importables, and total tax/subsidy burden for all farm production. In each panel of Figure 2, the gap between the top and bottom lines measures the region's average degree of anti-trade bias: the top line is average NRA on importables, the bottom line is average NRA on exportables, and the gap between them is the degree to which production incentives are distorted towards serving the home market as opposed to international trade. The central line measures the region's average degree of anti-farm bias, which includes any policy intervention on nontradable products.

The Africa data in Figure 2 reveal a decade-long trend from the early 1960s to the early 1970s towards greater anti-farm bias, due to less protection on importables and more taxation of exportables. After 1980 this was followed by twenty years of slow reduction in

[1] The detailed country case studies are reported in four regional volumes covering Africa (Anderson and Masters 2009), Asia (Anderson and Martin 2009), Latin American (Anderson and Valdés 2008) and Europe's transition economies (Anderson and Swinnen 2008).

the taxation of exportables, and a rise then fall in protection on importables, so that anti-trade bias actually expanded in the early 1980s and was then reduced substantially after 1990.

The data for other regions in Figure 2 show a range of experiences, but all except ECA (Eastern Europe and Central Asia) show a trend towards reduced anti-trade bias in the 1990s. In Asia there were increasingly heavy taxes on farm exports through the 1970s, but reform came earlier and faster than in Africa so that export taxes were largely eliminated by the 1990s. Latin America during the 1970s shares some of Africa and Asia's growing anti-farm bias, and has had an even greater degree of reform towards freer trade (NRAs of zero) in the 1990s. The ECA region, on the other hand, experienced a rapid rise in its NRA levels towards the norms seen in high-income countries, whose NRA levels fluctuate but show little trend from the 1960s to today.

The stylized facts: antitrade bias, the development paradox and resource abundance

Table 1 describes the three kinds of stylized facts simultaneously, using a series of OLS regressions to show the correlations between NRAs and each kind of determinant. In each column we control for the link to income in logarithm form, with log income as the only regressor in columns 1 and 4. The additional regressors in other columns are often significant, but they raise the regression's R^2 relatively little. Income alone explains most of the variance that is explained in any of the regressions shown here, including the variance within countries presented in column 4. Columns 1-4 use over 2,000 observations of national average total NRA for all covered products as the dependent variable, while column 5 uses the much larger number of individual commodity-level NRAs.

One of our stylized facts is that governments across the income spectrum tend to tax all kinds of trade, thus introducing an anti-trade bias in favor of the home market. From column 5, controlling for income the average NRA on an importable product is 16.5 percent higher and on an exportable it is 27.6 percent lower than it otherwise might be. Latin America has NRAs that are a further 16 percent lower (column 3) than those of other regions. Relative to Africa, Latin America and the omitted region (Eastern Europe), Asia and the high-income countries have unusually high NRAs when controlling for their income level.

Trade policy and price stabilization

Trade policy often aims to stabilize domestic prices as well as change their level. As detailed by Timmer (1989) and Dawe (2001) among others, stabilization of agricultural product prices may be especially important for low-income countries, where food prices have a large impact on consumer expenditure and farmgate prices have a large impact on real incomes in rural areas. In practice, however, while stockholding or variable-rate subsidies and taxes can achieve stabilization in the short run, such effects may be offset by the jumps in prices that occur when these policies are changed. Empirically the link between a country's income and the degree to which its trade policies actually stabilize prices is shown in Table 2. As it happens, the estimated coefficient on income is positive and the constant is negative: lower-income countries provide less stability. Less stabilization also occurs in land-abundant countries, and for importables and exportables relative to nontradables. Using column (4) as our preferred model, the estimated coefficients imply that the crossover level of per-capita income below which governments have tended to destabilize prices is $1,600 for importables and $2,400 for exportables. On average in those countries, over the full period of our data, actual domestic prices have been less stable than undistorted prices would have been.

Testing political economy theories of agricultural policy

The policy choices presented above could be driven by many different influences. What kinds of political economy models can best explain the patterns we see? In these models, observed policies are an equilibrium outcome to be explained, like a market price. If policymaking were to operate with full competitive efficiency, a political Coase theorem would apply: individuals would "buy" and "sell" their policy interests and thereby acquire a Pareto-optimal set of policies. But the policies we observe appear to impose costs on some people that exceed their gains to others, so our explanations all involve one or another mechanism that might prevent the competitive market sketched in Coase (1960) from applying. Each model posits a specific mechanism which prevents losers from buying out the gainers and thereby obtaining Pareto-improving reforms, and suggests certain variables that might therefore be correlated with the particular policies we observe. Identifying which kinds of political market failures have been most important could help policymakers circumvent these constraints, through rules and other interventions that help shift the political-economy equilibrium towards Pareto-improving policy outcomes.

The following sections describe various possible mechanisms, drawing on the last half-century of political economy modeling. The theories are well known so we describe them only briefly, and focus on the empirical correlations between variables. Our results are organized into two sets: regressions using aggregate national-average data are in Table 3, and those using product-level data are in Table 4. Note that none of our tests make any attempt to control for endogeneity. These are all exploratory regressions aimed at establishing correlations, comparing a large number of competing hypotheses in a common framework. Future work to test particular mechanisms would call for more specialized models and datasets.

Explaining the data: six major political economy theories

The simplest kind of explanation for observed policies is rational ignorance, by which individuals will not invest in learning or taking action about a policy if the policy's cost (or benefit) to them exceeds their cost of political organization. This mechanism could help to explain why observed policies tend to generate highly concentrated gains that provide substantial benefits to a few people, thereby motivating them to act politically and obtain that policy. In many cases the gains come at the expense of others who, if the cost per person is small, can be expected to remain on the sidelines. Such a focus on per-capita incidence is associated with Downs (1957), and could be the most powerful explanation for the patterns we observe. Influential applications to agriculture include Anderson (1995), who demonstrates how the concentration of gains and losses shifts during economic development.

Rational ignorance effects are tested in column 2 of Table 3, where the dependent variable is the value-weighted average of all commodity NRAs for the country as a whole, and the independent variable used to test for rational ignorance is its total cost (benefit) per capita in that sector. This test is applicable only to observations with positive total NRAs, so that a larger NRA imposes a greater cost (benefit) per urban (rural) person. Results show a large and significant pattern: when costs (benefits) per capita are larger, the percentage NRA levels are correspondingly smaller (higher). Furthermore, the effect is larger for people living in urban areas, perhaps because city-dwellers are more easily mobilized than their rural counterparts, when controlling for other factors.

Column 3 of Table 3 tests a related but different explanation: the absolute size of each group. This may influence outcomes through free-ridership, if individuals in larger groups have more incentive to shirk as in Olson (1965). An opposite group-size effect could arise if larger groups are more influential, perhaps because they can mobilize more votes, political contributions, or other political forces. As it happens, column 3 of Table 3 shows that larger groups do obtain more favorable policies, perhaps because all of these groups are very large and have similar levels of free-ridership. Again the magnitude is larger for urban people than for rural people, suggesting that on average an additional urbanite has more political influence than an additional rural person.

Relative to the unconditional regression in column 1, the estimated coefficient on national income is markedly lower when controlling for rational ignorance in (2), and somewhat greater when controlling for group size in (3). In that sense, rational ignorance helps to account for the development paradox, while group size is an additional influence. These regressions are not necessarily comparable, however, because of differences in the sample size.

A third kind of explanation is tested in column 4 of table 3, concerning the rent-seeking behavior of political leaders themselves. This terminology is associated with Krueger (1974), and suggests that Pareto-inefficient policy choices will persist as long as government officials can avoid accountability. By focusing on policymakers' behavior, the rent-seeking approach explains the observed pattern of policy intervention in terms of the checks and balances that constrain policymakers differently across countries and across sectors. The clear prediction is that governments facing more checks and balances will choose policies that are closer to Pareto-optimality. In column 4 of Table 3, we test this view using the absolute value of NRA as our dependent variable, and a variable for "checks and balances" from the World Bank's Database of Political Institutions (Beck et al. 2001, 2008) as our measure of politicians' power. Results are significant, suggesting that after controlling for income, governments that impose more checks and balances on their officials do have less distortionary policies.

Columns 5 and 6 of Table 3 tests a fourth type of model, in which observed policies may be by-product distortions caused by measures chosen for other reasons, such as a tax revenue motive. Governments with a small nonfarm tax base may have a stronger motive to tax agricultural imports and exports, or conversely governments with a larger tax base may be less constrained by fiscal concerns and hence freer to pursue other political goals. Here the variable we use to capture the extent of taxable activity is the country's monetary depth, as measured by the ratio of M2 to GDP. Since greater taxation of trade is associated with negative NRAs for exportables but positive NRAs for importables, this test is divided into two subsamples. What we find is that governments in more monetized economies have lower levels of NRA in both samples: they tax exportables more, and tax importables less. On average in our sample, import taxes are associated with revenue motives (so they are smaller when other revenues are available), but export taxes are not.

The four major theories described above are tested in Table 3 using data at the national level, using value-weighted averages over all products; in the table below, we test two additional kinds of theories that apply at the product level, with a much larger number of observations. This is done for the fifth and sixth kinds of theory, namely time consistency and status-quo bias.

The fifth type of explanation tested at the product level involves time consistency and commitment mechanisms. Such theories are associated with Kydland and Prescott (1977), who show that current policy choices depend in part on how easily future governments can change those policies. Without an institution for credible commitment, introducing and sustaining a desirable policy may be impossible – particularly for products that are more dependent on irreversible private investments. Differences across products in the importance of irreversible investment thus allow us to test how much time consistency matters: if products with irreversible investments attract high taxation, then commitment devices that help governments maintain low taxes might be helpful. This idea is applied to help explain agricultural policy in Africa by McMillan and Masters (2003), who show that tree crops and other irreversible investments are more vulnerable to high taxation and simultaneously attract less public services. The same effect holds in these data: the results in columns 2 and 3 of Table 4 are consistent with such a time-consistency effect, as perennials are taxed more than annuals. Other differences across crops are also important. Column 4 of Table 4 shows that sugar and dairy are taxed more than other commodities at low incomes, and then as income grows, policies switch towards subsidization of these previously taxed commodities.

A sixth kind of political-economy mechanism is pure status-quo bias, in which political leaders resist change as such, even if the change would be desirable in retrospect. Status quo bias could lead policymakers to resist both random fluctuations and persistent trends, even when accepting these changes would raise economic welfare. Several different mechanisms have been proposed to explain why change would be resisted *ex ante*, despite the desirability of reform *ex post*. An informal version of this idea that is specific to policy-makers is described by Corden (1974) as a “conservative welfare function.” A micro-foundation for this idea could be individual-level “loss aversion”, as formalized by Kahneman and Tversky (1979): people systematically place greater value on losing what they have than on gaining something else. Status quo bias can also arise for other reasons too.

Fernandez and Rodrik (1991) show how Pareto-improving reforms may lack political support if those who will lose know who they are, whereas those who could gain do not yet know if they will actually benefit. If status-quo bias leads policymakers to resist change in world prices, observed NRAs would be higher after world prices have fallen. NRAs could also try to resist changes in crop profitability more generally, and therefore be higher after acreage planted in that crop has fallen. We test for both kinds of status quo bias in columns 5 and 6 of Table 4. With our usual controls, we find support for status-quos bias in prices, as there is a negative correlation between policies and lagged changes in world prices. However, there is no remaining correlation between policies and lagged changes in crop area.

A new explanation: demographic influences on political pressures

The six political economy models tested above could all potentially explain the results we observe, and are often mentioned in the political economy literature. A seventh kind of explanation is more novel: it is based on exogenous but predictable changes in employment that affect whether other people are likely to enter the sector in the future. This could drive the level of political support in a dynamic political economy model, where individuals' incentives to invest in politics depend crucially on the probability of others' future entry to their sector and the resulting level of expected future rent dissipation.

A forward-looking model of lobbying effort driven by the entry of new agents has been suggested by Hillman (1982) and also Baldwin and Nicoud (2007), who used it to help explain why governments protect declining industries. In their models, declining industries invest more to seek policy-induced rents because their secular decline creates a barrier to entry in the future. Agriculture experiences this kind of secular decline in its labor force only after the "structural transformation turning point", when total population growth is slow enough and nonfarm employment is large enough for the absolute number of farmers to decline (Tomich, Kilby and Johnston 1995). Before then, the number of farmers is rising, whereas after that point the number of farmers falls or remains constant.

The secular rise and then fall in the number of farmers could help explain NRA levels, to the extent that the entry of new farmers erodes policy rents obtained from lobbying. This would discourage farmers from organizing politically as long as new farmers are entering the sector, and facilitate organization once the entry of new farmers stops. Focusing on this dynamic of entry, as opposed to the absolute size of the group, could help explain the timing of transition from taxation to protection and also help explain the persistence of protection even where agriculture is not a declining industry. In many industrialized countries, for example, agricultural output grows but a fixed land area imposes a strong barrier to the entry of new farmers, helping incumbent producers capture any policy rents they may obtain through lobbying.

To test for an entry-of-new-farmers effect, we return to country-level data in Table 3, where the last column tests for the correlation with NRA of an indicator variable set to one if there is demographic entry of new farmers, defined as a year-to-year increase in the "economically active population in agriculture" reported by the FAO. The variable is set to zero when the number of farmers remains unchanged or declines. In column 7 of Table 3, with our usual controls, observed policies remain less favorable to farmers as long as the farm population is rising. This result is quite different from the predictions of other models, and offers a potentially powerful explanation for the timing of policy change and the difficulty of reform.

This section has tested seven hypothesized mechanisms, using our generic stylized facts as control variables. One important question is whether these mechanisms are explaining the stylized facts, or adding to them. As it happens, the specific mechanism mainly add to the explanatory power of our regressions: introducing them raises the equations' R-squared but does not reduce the magnitude or significance of the stylized factors with respect to national income, land abundance, or the direction of trade. There are, however, three important exceptions which account for some of the observed correlation with income: the effect of peoples' rational ignorance from having larger transfers per person, the effect of a government's revenue motive from having greater monetary depth, and the effect on rent seeking behavior of having more checks and balances in government. Variables specific to these effects capture a share of the variance in NRAs that would otherwise be associated with per-capita income, suggesting that they are among the mechanisms that might cause the development paradox, while other results are additional influences on governments' policy choices.

Conclusions

This chapter tests standard political-economy theories of why governments intervene to influence agricultural prices. Our key data source (Anderson and Valenzuela 2008) provides estimates for the tariff-equivalent effect on agricultural prices of all types of trade-related policies across around 70 countries from 1955 through 2007. Policy impacts are measured for 72 products, chosen to account for over 70 percent of agricultural value added in each country, resulting in a total of over 25,000 distinct estimates from particular products, countries and years.

Our analysis begins by confirming three previously observed stylized facts: a consistent anti-trade bias in all countries, the development paradox of anti-farm bias in poorer countries and pro-farm bias at higher incomes, and the resource abundance effect towards higher taxation (or less subsidization) of agriculture in more land-abundant countries. We find strong support for a number of mechanisms that could help explain government policy choices. Results support rational ignorance effects as smaller per-capita costs (benefits) are associated with higher (lower) proportional NRAs, particularly in urban areas. Results also support rent-seeking motives for trade policy, as countries with fewer checks and balances on the exercise of political power have smaller distortions, and we find support for time-consistency effects, as perennials attract greater taxation than annuals. We find partial support also for status-quo bias, as observed NRAs are higher after world prices have fallen, but there is no correlation between policies and lagged changes in crop area.

Three of our results run counter to much conventional wisdom. First, we find support for a revenue motive function of taxation only on importables, and the opposite effect on exportables. Second, we find no support for the idea that larger groups of people will have more free-ridership and hence less political success. Our results are consistent with the alternative hypothesis of a group-size effect in which larger groups tend to be given more favorable levels of NRA. Third, we find that governments in lower-income countries actually destabilize domestic prices, relative to what those prices would be with freer trade, over the full time period of our data. A given policy may achieve short-term stability, but on average these policies are not (or perhaps cannot be) sustained, leading to large price jumps when policies change.

An important novelty in our results is the finding that demographically-driven entry of new farmers is associated with less favorable policies. This result is consistent with models in which new entrants erode policy rents, making political organization depend on barriers to entry that allow incumbents to capture the benefits of policy change.

We find robust support for some theories and not others, but none of our regressions account for more than half of the variance across countries and over time. To explain the remainder would require deeper analyses of policies' institutional context in particular countries and commodities, and further econometric tests. Such research will also point the way towards improvements in data quality to reduce measurement error. The World Bank's project methodology aimed for much more consistency in data sources, definitions and assumptions than is usually possible to achieve over such a large sample, but the data are inevitably noisy with random and also systematic variance in the NRA estimates. Future work could produce even more useful datasets, as well as further analysis of the hypotheses tested here.

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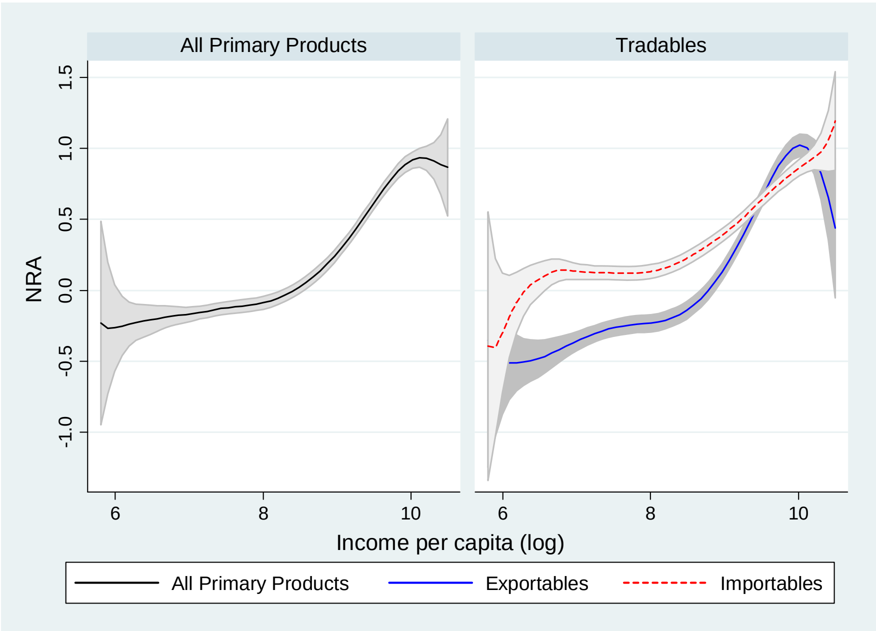
Annex: List of variables

Variable name	**Definition**	**Source**
Border prices	Price at which a commodity could be imported (cif) or exported (fob), as applicable, in each country and year	Anderson and Valenzuela (2008)
Crop area	The area from which a crop is gathered. Area harvested, therefore, excludes the area from which, although sown or planted, there was no harvest due to damage, failure, etc.	FAOSTAT (2007)
Checks and balances	Measures the effectiveness of electoral checks on government decision makers or according to electoral rules that influence party control over members	Beck, Keefer and Clarke (2008)
Entry of new farmers	Dummy variable which takes the value of one if the year change in the economically active population in agriculture is positive.	FAOSTAT (2007)
Exchange rate variation	Calculated as the standard deviation of the de-trended ratio of the exchange rate between 1960 and 2004.	Penn World Table 6.2
Importable (Exportable)	Indicator variable for commodity-level NRAs, equal to 1 if the NRA is observed in a year when the commodity was imported (exported) and 0 otherwise.	Anderson and Valenzuela (2008)
Income	Real gross domestic product per capita, at PPP prices, chain indexed. Expressed in international dollars of 2000.	Penn World Table 6.2
Income growth variation	Calculated as the coefficient of variation of the growth rate of real GDP per capita between 1960 and 2004.	Penn World Table 6.2
Land per capita	Area of arable land as defined by the FAO, divided by the total population.	FAOSTAT (2007)
Monetary depth (M2/ GDP)	Money and quasi money comprise the sum of currency outside banks, demand deposits other than those of the central government, and the time, savings, and foreign currency deposits of resident sectors other than the central government	World Bank (2007)
Policy transfer cost per rural (urban) person	The sum of each commodity NRA times the value of production at border prices, divided by populations as defined above. Results are shown as costs of policy, so NRAs per rural person are multiplied by -1.	Anderson and Valenzuela (2008)
Rural (Urban) population	Rural population estimates are based on UN Population Projection estimates of total population, minus urban population using varying national definitions of urban areas	FAOSTAT (2007)

Figure 1: National average NRAs and real income per capita[a]

(percent/100)

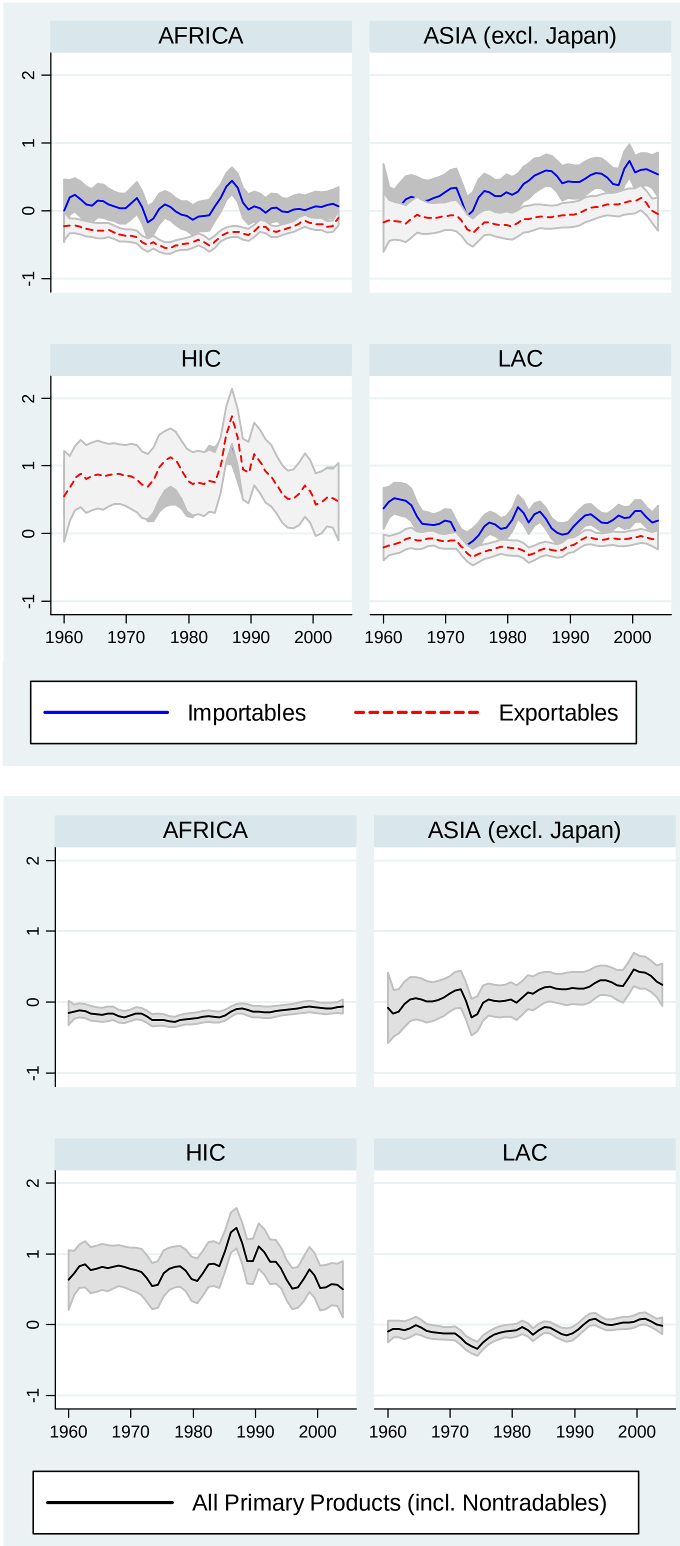


[a] Smoothed line and 95% confidence interval computed with Stata's *lpolyci* using bandwidth 1 and degree 4. Income per capita is expressed in I$ (2000 constant prices).

Source: Authors' derivation based on estimates in Anderson and Valenzuela (2008)

Figure 2: National average NRA over time, by trade status and region[a]

(percent/100)



[a] LAC – Latin America, HIC – High income countries. Smoothed line and 95% confidence interval computed with Stata's *lpolyci* using bandwidth 1 and degree 2.
*Source: Authors' derivation based on estimates in Anderson and Valenzuela (2008)*Table 1: Stylized facts of the covered total NRA

Explanatory variables	*Model* (1)	(2)	(3)	(4)	(5)
Income (log)	0.3420***	0.3750***	0.2643***	0.2614***	0.2739***
	(0.0121)	(0.0130)	(0.0230)	(0.0226)	(0.0579)
Land per capita		-0.4144***	-0.4362***		
		(0.0264)	(0.0256)		
Africa			0.0651		
			(0.0404)		
Asia			0.1404***		
			(0.0418)		
Latin America			-0.1635***		
			(0.0176)		
High income countries			0.4311***		
			(0.0340)		
Importable					0.1650*
					(0.0829)
Exportable					-0.2756***
					(0.0849)
Constant	-2.6759***	-2.8159***	-2.0352***	-1.9874***	-2.0042***
	(0.0941)	(0.0965)	(0.2024)	(0.1920)	(0.4174)
R^2	0.28	0.363	0.418	0.827	0.152
No. of obs.	2520	2269	2269	2520	28118

[a] Covered total NRA is the dependent variable for models 1-4, and NRA by commodity for model 5. Model 4 uses country fixed effects. Results are OLS estimates, with robust standard errors (models 1-4), country clustered standard errors (model 5) and significance levels shown at the 99% (***), 95% (**), and 90% (*) levels. The Europe and Central Asia region is the omitted continent variable.

Source: Authors' calculations

Table 2: Stylized facts of the stabilization index[a]

	Model					
Explanatory variables	***(1)***	***(2)***	***(3)***	***(4)***	***(5)***	***(6)***
Income (log)	5.6507***		7.0059***	7.4730***	9.4113***	8.8422*
	(1.0515)		(2.1454)	(2.5982)	(3.1381)	(4.7925)
Importable		6.5568*	-7.1127	-9.4289*		-10.3265*
		(3.4489)	(4.3119)	(4.8711)		(5.8565)
Exportable		1.5545	-8.4469**	-9.5703**		-11.6999**
		(3.4652)	(3.8169)	(4.1644)		(5.5625)
Land per capita			-9.8402**	-9.4037**		-9.6186**
			(4.1771)	(4.0466)		(4.2018)
Income growth				-444.8959		-547.3185
Variation				(481.5131)		(656.6352)
Exchange rate				2.0297***		1.0391
Variation				(0.6763)		(0.9372)
Africa					8.2332	1.1559
					(7.3334)	(7.5259)
Asia					15.2604**	6.2383
					(7.0633)	(8.3245)
Latin America					-4.4882	-10.931
					(6.3745)	(8.0996)
High income					-3.0503	-1.5757
Countries					(8.5204)	(9.3760)
Constant	-37.7412***	4.6606**	-40.9054**	-44.9126**	-75.4189***	-53.9286
	(8.8035)	(2.1175)	(15.7140)	(20.7327)	(27.7500)	(41.7300)
R^2	0.029	0.005	0.035	0.047	0.032	0.055
No. of obs.	757	766	722	722	771	724
Dropped obs.	20	11	6	6	6	4

[a] Dependent variable for all regressions is the Stabilization Index by country and product. Influential outliers were dropped from the sample based on the Cook's distance criteria [(*K*-1)/*N]*. Results are OLS estimates, with clustered standard errors and significance levels shown at the 99% (***), 95% (**), and 90% (*) levels.

Source: Authors' calculations

Table 3: Testing political economy hypotheses at the country level[a]

Dependent variable Total NRA for:	***(1)*** All Prods.	***(2)*** All Prods.	***(3)*** All Prods.	***(4)*** |All Prods.|	***(5)*** Exportables	***(6)*** Importables	***(7)*** All Prods.
Explanatory variables							
Income (log)	0.2643***	0.1234***	0.3175***	0.1913***	0.2216***	0.1142***	0.2461***
	(0.0230)	(0.0440)	(0.0242)	(0.0291)	(0.0184)	(0.0299)	(0.0248)
Land per capita	-0.4362***	-0.2850***	-0.4366***	-0.4263***	-0.7148***	-0.6360***	-0.4291***
	(0.0256)	(0.0467)	(0.0245)	(0.0277)	(0.0818)	(0.0338)	(0.0266)
Africa	0.0651	0.1544***	0.0964**	0.2612***	-0.1071***	-0.0628	0.0844**
	(0.0404)	(0.0489)	(0.0419)	(0.0522)	(0.0363)	(0.0575)	(0.0423)
Asia	0.1404***	0.2087***	0.1355***	0.1007**	-0.1791***	0.0217	0.1684***
	(0.0418)	(0.0515)	(0.0457)	(0.0504)	(0.0361)	(0.0564)	(0.0472)
LAC	-0.1635***	-0.0277	-0.1189***	-0.0947***	-0.2309***	-0.1780***	-0.1460***
	(0.0176)	(0.0242)	(0.0203)	(0.0189)	(0.0245)	(0.0311)	(0.0212)
HIC	0.4311***	0.2789***	0.4203***	0.3761***	1.0694***	0.8807***	0.4346***
	(0.0340)	(0.0456)	(0.0343)	(0.0390)	(0.1332)	(0.0604)	(0.0338)
Policy transfer cost per rural person		-0.0773*					
		(0.0422)					
Policy transfer cost per urban person		-1.2328***					
		(0.2830)					
Rural population			1.4668***				
			(0.1528)				
Urban population			-3.8016***				
			(0.3717)				
Checks and balances				-0.0173***			
				(0.0063)			
Monetary depth (M2/GDP)					-0.0310***	-0.0401***	
					(0.0041)	(0.0073)	
Entry of new farmers							-0.0737*
							(0.0407)
Constant	-2.0352***	-0.9046**	-2.4506***	-1.2465***	-1.5957***	-0.4652*	-1.8575***
	(0.2024)	(0.3576)	(0.2102)	(0.2568)	(0.1629)	(0.2696)	(0.2210)
R^2	0.4180	0.45	0.437	0.294	0.373	0.397	0.419
No. of obs.	2269	1326	2269	1631	1629	1644	2269

[a] Dependent variables are the total NRA for all covered products in columns 1, 2, 3 and 7; the absolute value of that NRA in column 4, and the total NRA for exportables and importables in columns 5 and 6, respectively. For column 2, the sample is restricted to countries and years with a positive total NRA. Monetary depth is expressed in ten-thousandths of one percent. Results are OLS estimates, with robust standard errors and significance levels shown at the 99% (***), 95% (**), and 90% (*) levels.

Source: Authors' calculations

Table 4: Testing political economy hypotheses at the product level[a]

Explanatory variables	*Model* (1)	(2)	(3)	(4)	(5)	(6)
Income (log)	0.2605**	0.2989***	0.2363**	0.2159**	0.3160**	0.2804**
	(0.1089)	(0.0576)	(0.1039)	(0.0965)	(0.1230)	(0.1295)
Importable	0.0549	0.0048	-0.0061	0.1039	0.1106	0.0331
	(0.0753)	(0.0937)	(0.0901)	(0.0972)	(0.0882)	(0.1018)
Exportable	-0.2921***	-0.3028***	-0.2918***	-0.2868***	-0.3614***	-0.3414***
	(0.0697)	(0.0868)	(0.0749)	(0.0805)	(0.0728)	(0.0756)
Land per capita	-0.3066***	-0.3352***	-0.3478***	-0.3140***	-0.4738***	-0.1746**
	(0.0884)	(0.1080)	(0.1035)	(0.0950)	(0.1532)	(0.0760)
Africa	0.0553		0.1171	0.0901	0.0554	0.1236
	(0.1898)		(0.1956)	(0.1874)	(0.2207)	(0.2127)
Asia	0.2828		0.2998	0.2903	0.1833	0.2311
	(0.2250)		(0.2110)	(0.2140)	(0.2311)	(0.2355)
LAC	-0.0652		-0.0309	-0.0515	-0.1426	-0.0863
	(0.0880)		(0.0998)	(0.1053)	(0.1066)	(0.1151)
HIC	0.2605*		0.3388**	0.3136**	0.4837*	-0.0298
	(0.1395)		(0.1430)	(0.1393)	(0.2770)	(0.1762)
Perennials		-0.1315**	-0.1492***			
		(0.0540)	(0.0549)			
Animal Products		0.2589***	0.2580***			
		(0.0889)	(0.0892)			
Others		-0.1764**	-0.1956**			
		(0.0820)	(0.0795)			
Sugar				-1.0903**		
				(0.5398)		
Rice				-1.1926		
				(1.2711)		
Milk				-4.1447***		
				(1.0724)		
Wheat				-0.6149		
				(0.4403)		
Other Cereals				0.6198		
				(0.4822)		
Sugar*Income				0.1790***		
				(0.0620)		
Rice*Income				0.1502		
				(0.1663)		
Milk*Income				0.5476***		
				(0.1214)		
Wheat*Income				0.068		
				(0.0471)		
Other*Income				-0.0678		
				(0.0526)		
Lagged Change in Border Prices					-0.0025***	
					(0.0006)	
Lagged Change in Crop Area						0.0083
						(0.0358)
Constant	-1.8516*	-2.0109***	-1.6685*	-1.5914*	-2.1625**	-2.0549*
	(0.9409)	(0.3957)	(0.8978)	(0.8445)	(1.0507)	(1.1023)
R^2	0.1950	0.2100	0.2240	0.2800	0.3020	0.1940
No. of obs.	25599	20063	20063	20063	15982	9932

[a] The dependent variable is the commodity level NRA. Observations with a lagged change in border prices lower than -1000% were dropped from the sample. Results are OLS estimates, with clustered standard errors and significance levels shown at the 99% (***), 95% (**), and 90% (*) levels.

Source: Authors' calculations