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Constitutional Rules and Redistributive Policy: Evidence from Global Agricultural Protection

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Constitutional Rules and Redistributive Policy: Evidence from Global Agricultural Protection*

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Abstract

The paper studies how electoral rules and forms of government shape agricultural policy across a large sample of developing and developed countries over the 1955-2005 period. Results from difference-in-difference and cross-country regressions give support to the idea that political institutions systematically affect agricultural protection. Democratic reform induces an increase in agricultural protection that is conditional to the country choice of the form of democracy. Indeed, what matters are reforms into proportional democracies (*vis-à-vis* majoritarian democracies) and, to a lesser extent, reforms into presidential democracies (*vis-à-vis* parliamentary democracies). Moreover, we show that the redistributive attitude of proportional democracy is not driven by the (left) ideology orientation of the governments.

Keywords: Electoral Rules; Forms of Government; Ideology; Agricultural Protection; Political Economics.

JEL Classification: D72, F13, H23, O13, P16, Q18

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1. Introduction

Literature on the impact of political regimes on growth and public policy is an important new research area in the field of comparative politics. The initial focus was on ‘democracy’ versus ‘autocracy’ or the shift from one to the next (political reforms) (Barro, 1997; Rodrik and Wacziarg, 2005; Giavazzi and Tabellini, 2005). However, evidence that democratization systematically affects growth and public policy is quite weak (see Persson and Tabellini, 2008; Acemoglu *et al.* 2008).

As a consequence scholars have shifted their attention to the specific details of democratic constitutions, like electoral rules and forms of government. For example, Persson and Tabellini (2006) showed that while a shift from autocracy to democracy does not have any effect on either fiscal or trade policy, there is a positive and large effect when one considers transition to parliamentary (*vs.* presidential) or proportional (*vs.* majoritarian) democracies. These results are important as they complement previous cross-section evidence about the effect of constitutional rules on public policy outcomes (Persson and Tabellini, 2003).

However actual evidence linking constitutional rules to policy outcomes is problematic. First, because it is largely confined to macro-economic policies like government spending and fiscal policy. Thus, what about the link between these constitutional features and other important areas of public policy? Second, with some notable exceptions (e.g. Persson and Tabellini, 2006), a serious limitation comes from the cross-sectional nature of this literature, as it is notoriously difficult in comparative politics to extract casual inference from cross-country data (see Acemoglu, 2005). Constitutions are indeed equilibrium outcomes and their policy effects can be confounded with other (not controlled for) cultural, historical and institutional factors.

In this paper we try to address some of these problems. First, by focusing on an important structural (agricultural) policy, rarely investigated from this point of view. Second, by using an econometric approach less susceptible to the endogeneity concerns rise by Acemoglu (2005), exploiting the within- and across-country variation in constitutions and public policies. Finally, by conducting a battery of robustness checks ruling out the possibility that electoral rule effects are, instead, the consequence of the endogenous selection of constitutions and government ideology, as recently suggested by Iversen and Soskice (2006) and Ticchi and Vindigni (2010).

Agricultural policy (subsidization or taxation) is an excellent policy instrument to study the impact of political institutions across a wide variation of countries. This is because agricultural policies are an important component of a country trade policy, as well as a critical element of the world trading system. In fact, agricultural policy is actually one of the main areas of disagreement between developed and developing countries in the current round of trade liberalization.

The paper uses better policy indicators and improved methodologies to measure the impact of political institutions on government policy making. In terms of better policy indicators we make use of a new dataset covering both trade and fiscal policies, which is the dataset on agricultural policy distortions recently developed by the World Bank (see Anderson and Valenzuela, 2008). The dataset includes annual data for 74 countries and several agricultural products from 1955 to 2005. These data represent a considerable advance with respect to the standard practice of using the Sachs and Warner index of trade openness to study similar questions at the aggregated level (see Giavazzi and Tabellini, 2005; Milner and Kubota, 2005; Persson, 2005; Persson and Tabellini, 2006, among others).¹

Our empirical strategy follows the recent tendency of including democracies as well as non-democracies in the sample, to overcome the fact that established democracies do not display sufficient (time) variation in their constitutional features (Persson, 2005). This gives us the possibility of using a more robust approach exploiting the within-country and across-group variation in political institutions. Specifically, we estimate multiple treatment effects to disentangle the effect of democracy *per se*, from the effects of its constitutional details in terms of electoral rules and forms of government.

The main results show that the nature of democratic institutions matter a lot for the economic policy of agricultural policy. Specifically, we document a significant *positive* protection effect of political reforms toward democracy, showing that what matters are reforms into proportional democracies (*vis-à-vis* majoritarian democracies) and, to a lesser extent, reforms into presidential democracies (*vis-à-vis* parliamentary democracies). Moreover, our analysis suggests that the magnitude of the political reform effect on agricultural protection tends to be ‘sectors’ specific, with import-competing sectors and

¹ Whether the Sachs and Warner openness index measures exclusively trade openness remains an unanswered question precisely because trade openness is correlated with institutions (Rodriguez and Rodrik, 2001). Tavares (2007) showed that the effect of democratization on trade protection is, in fact, sensitive to how openness is measured, finding significant results especially when trade policy is proxy through the (criticized) Sachs and Warner index. On the virtues and limits of Sachs and Warner index, see also the more optimistic view of Wacziarg and Welch (2008).

staple food crops being significantly more sensitive to the institutionally induced policy changes. Finally, we show that the redistributive properties of proportional election toward agriculture is independent of the partisan nature of government: left government orientation, in fact, tends to tax agriculture, not subsidize it.

Our paper is related to growing literature studying how political institutions affect public policy. Formal models and theoretical predictions, especially, rely on the differentiated policy effect of electoral rules and forms of government.² Important contributions of Persson *et al.* (1997; 2000; 2007), Persson and Tabellini (2000), Austen-Smith (2000), Lizzeri and Persico (2001) and Milesi-Ferretti *et al.* (2002), predict that proportional electoral systems and parliamentary regimes should be associated with a greater provision of public goods and a broad form of redistribution, like welfare programs, as well as with higher levels of government spending and redistribution. Cross-country evidence substantially confirms predictions about the effect of electoral rules (see Kontopoulos and Perotti, 1999; Persson and Tabellini, 2003, 2004; Milesi-Ferretti *et al.* 2002; Persson *et al.* 2007). Differently, the results about the effect of forms of government are mixed. In fact, Persson *et al.* (2000) and Persson and Tabellini (2003) supports the notion of greater public spending and government redistribution in parliamentary systems than in presidential ones, instead Blume *et al.* (2007) showed that this evidence is weak.

Similar weak evidence came from the few studies that applied this reasoning to trade policy. For example, Roelfsema (2004) find a positive effect of majoritarian election on trade protection, by contrast Rogowski and Kayser (2002) and Hatfield and Hauk (2003) obtain exactly the opposite result, namely trade protection tends to be higher under proportional representation.

An important issue of the above predictions is that electoral rules affect public policy through their effects on the incentive structure of political parties of all kinds. More recently, the explanation for the theoretical and empirical association between proportional election and redistributive policies has been attributed to the partisanship of the government. Specifically, Iversen and Soskice (2006) stressed that since 1945 government participation by left parties has been much more common in proportional countries than in majoritarian ones in the OECD. In a similar vein, Ticchi and Vindigni

² These constitutional dimensions are not the only democratic details investigated by the literature. For example, Besley *et al.* (2008) focus their attention on the effect ‘political competition’ play on policy and growth across and within US states. Differently, the recent political science literature stress that what matters are the ‘number of access points’ provided by ‘overall’ institutions, more than the nature of electoral rule (see Ehrlich 2007).

(2010) highlighted that proportional systems should be expected to be ruled relatively more often by centre-left government coalitions, that are more willing to tax and redistribute income. Thus, this correlation, rather than the prevalence of coalition governments or the direct effect of electoral rule on political incentives, could explain why proportional systems spend and redistribute more. However, until now a rigorous test of these competitive hypotheses is lacking in the literature.

Finally, only a few studies have investigated the policy effect of democracy in agriculture. In this context, cross-country studies displayed inconclusive results (see Swinnen et al. 2000; Olper 2001). More robust and interesting evidence can be found in studies that exploit the panel dimension of the data (see Swinnen *et al.* 2001; Olper *et al.* 2009). These studies have found that democratization affects agricultural protection positively, and also that the effect is heterogeneous. For example, Olper *et al.* (2009) showed that when semi-parametric matching methods are used to measure the average effect of democratization, compared to difference-in-difference estimation, then the magnitude of the democracy effect increases substantially, suggesting that underlying heterogeneity in the democracy effect could be at work in driving the results. Our paper supports the notion that a large fraction of this heterogeneity in the democracy effect on agricultural protection is indeed attributable to electoral rules.

The remainder of the paper is organised as follows. Section (2) summarizes the intuitions of recent theory and derive some testable hypotheses. Section (3) introduces the econometric strategy and discusses the identification issues. Section (4) presents how democratic reforms are measured and classified, the dependent variable and other controls, giving also some preliminary evidence on how agricultural protection maps across different political institutions. In Section (5) the results are presented and discussed. Finally, Section (6) concludes.

2. Theory and hypotheses

How do political institutions affect public policy outcomes? A small but growing political economics literature has recently started studying these important policy issues by especially focusing on two key dimensions of any constitution: electoral rules and forms of government. In what follows, a synthetic overview of the main theoretical arguments and propositions will be given. Then we draw implications for agricultural policies.

2.1 *The economic policy of constitutional rules*

Electoral systems. The literature contrasts elections in multiple versus single electoral districts. The former traditionally refers to majoritarian election with plurality rule, the latter with proportional representation.³

A few papers have started to formalize how these features of electoral rules influence the *level* and *composition* of government spending (Austen-Smith, 2000; Lizzeri and Persico, 2001; Persson and Tabellini, 1999, 2000; Milesi-Ferretti *et al.*, 2002; among others). A first prediction is that proportional elections tend to address government spending towards large programs, benefiting large groups in the population like welfare programs, while majoritarian elections give politicians a greater incentive to target transfers to geographically smaller constituency groups.

In proportional elections the legislators are elected from large districts, giving politicians a strong incentive to get support from large coalitions in the population. Instead, in majoritarian elections the districts are small, creating a strong incentive for politicians to target policies towards key district constituencies. Furthermore, the electoral formula has a reinforcing effect. Indeed, in proportional election the voters choose a list of candidates, while in majoritarian elections a single candidate is chosen. Thus, in the former case the implemented policy is likely to reflect what is optimal for the party, often reflecting the national perspective and favouring broad forms of redistribution. The opposite applies in majoritarian systems where the individual legislator tends to ‘look after’ the interests of the represented district, thus favouring a more narrow and targeted distribution.

Almost all models predict that electoral rules also affect the *level* of government spending, with proportional elections normally associated with larger overall spending. Indeed, both Kontopoulos and Perotti (1999) and Milesi-Ferretti *et al.* (2002), claim greater spending in proportional systems.⁴ More recently, Persson *et al.* (2007) highlighted an indirect mechanism through which electoral rules can affect public policies. They stressed that proportional elections induce a greater incidence of coalition

³ As shown by Persson and Tabellini (2003), this clear cut distinction finds empirical support, as in reality there exists a strong correlation between district magnitude and the so called ‘electoral formula’.

⁴ Persson and Tabellini (1999) found greater overall government spending for majoritarian elections. However, their empirical work does not support this prediction.

governments than do majoritarian elections, giving rise to larger budget spending as minority interests are more represented in the legislature.⁵

Finally, Grossman and Helpman (2005) applied a model based on ‘party discipline’⁶ to trade policy predicting a protectionism bias in majoritarian democracies. The underline mechanism lie on the idea that if ‘party discipline’ is less than perfect, then protectionism arises whenever national parties cannot precommit to a policy and when the majority delegation does not fully incorporate the preferences of the minority.

Forms of government. While there exists a large political science literature contrasting different types of democratic regimes (see, e.g., Cox, 1997), only few formal models assess the effect of different forms of government on public policy. Here, the classical distinction is between presidential and parliamentary forms of government. In the former, the appointment is direct, through citizen election, in the latter it is indirect, through a vote of confidence from an elected parliament.

Persson *et al.* (1997, 2000) compare these two regimes, focusing on different features like the separation of power over legislation (agenda setting) and the degree of legislative cohesion. In parliamentary regimes the government has stronger powers to initiate legislation than in a presidential regime, facilitating collusion among politicians at the voters’ expense, resulting in higher taxes and spending. Moreover, in parliamentary systems the vote of confidence induces more discipline within the government coalition. Thus a stable majority tends to satisfy the broad interests of its constituents.

These models give predictions about the *level* and *composition* of government spending that tend to mimic and reinforce the previous discussion on electoral systems. Specifically, in presidential regimes the prediction is for *lower* overall spending and taxation than in parliamentary regimes. Moreover, presidential regimes should also be associated with target programs like local public goods, whereas parliamentary systems with broader spending programs, like national public goods.

⁵ The last paper formalized the so-called common pool problem: if different groups have partial control over some component of government, then none of them fully internalizes the fiscal costs. This problem is clearly exacerbated under proportional elections, because, as suggested by political science literature (see, e.g. Lijphart, 1990), proportional election makes coalition governments more likely.

⁶ By ‘party discipline’ the authors mean the commitment problems that parties face due to their *ex ante* incentives to promise trade policies to win elections, which can diverge from more partisan concerns that elected legislators will confront *ex-post*.

2.2 Implications for agricultural policy

What are the implications of these general predictions for the comparative politics of agricultural policy? The propositions summarized above suggest two quite clear predictions about the *level* and the *composition* of government spending. The prediction about the *level* of spending translates directly to agricultural policy, suggesting higher protection and support under parliamentary and proportional democracies than under presidential and majoritarian systems, *ceteris paribus*.

However, the results of government spending *composition* (target vs. broad) is more complex, and can take different directions depending on the nature of agricultural policy transfers (Rausser and Roland, 2009). If agricultural policy takes mainly the form of local public goods or specific forms of redistribution, as is often the case in developed countries, then we should observe relatively more distortions in presidential and majoritarian systems than in parliamentary and proportional systems. Differently, if the agricultural policy takes mainly the form of national public goods or a broad form of redistribution, as could be the case in developing countries where farmers often exceed 50% of the population, then the opposite prediction should hold, and we should observe relatively more protection in parliamentary and proportional systems than in presidential and majoritarian regimes.

As emphasised by Rausser and Roland (2009), an additional consideration linked to the probability of finding agricultural voters as pivotal voters should be added. In a developed country this probability is higher under majoritarian systems than under proportional as there is less likelihood of finding a farmer whose income is median in the country. Indeed, it is much more plausible that a farmer may be median in a rural district if that district is pivotal for elections. Clearly, this reasoning goes exactly in the opposite direction when a large fraction of the population is involved in farm activities.

Summarizing, in developed countries the farmers' group is small, representing a classic special interest group, whereas in developing countries, where the rural population share is often above 50%, the farmers' group represents the broad interests of the population. Thus, the effect of regime types and electoral rules on agricultural protection should be *conditional* to the level of development. Because the key aim of this paper is to test predictions concerning the potential effect of *regime changes* to different electoral rules and forms of government, a transition that largely happens in developing countries (see next), the above considerations suggest that in our specific context agricultural

protection should be considered a *broad* form of redistribution. Thus, keeping this qualification in mind, we expect to find that a reform to parliamentary (presidential) democracy and/or to a proportional (majoritarian) democracy, from an autocratic status, will, on average, result in a greater (lower) increase in agricultural protection and support, *ceteris paribus*.

3. Econometric method

Following Giavazzi and Tabellini (2005) we define *political reforms* in specific constitutional status as a ‘treatment’ experienced by some countries but not others. Then we estimate the average treatment effect through a difference-in-difference regression, exploiting the fact that any democratic transition (*political reform*) can be characterized on the basis of its specific constitutional feature in terms of electoral rules and forms of government.

Following this strategy, we exploit both the time series and the cross-sectional variation in the data, as well as the (potential) heterogeneous effect of democratization across different constitutional rules and agricultural sectors (see below). Indeed, in our sample we include countries that experience changes in their constitution in the observed period, called the *treated*, as well as countries that do not experience any constitutional transition, called the *controls*.

More formally, this approach could be represented as follows. Denote by $S = \{0, 1\}$ the treatment indicator, equal to 1 for treated countries, i.e. those that experience a constitutional reform, and equal to 0 for control countries, i.e. those that do not change the constitution throughout the observed period for which data on agricultural protection are available. Let $Y_{i,t}^S$ be the outcome of interest, namely the level of agricultural protection in country i in time t and constitutional state S . Let $t = 0$ be the period *before* the change in constitution, and $t = 1$ the period *after* this transition. The individual treatment effect of this political reform in country i and period t is then $Y_{i,t}^1 - Y_{i,t}^0$, namely the change in protection in period 1 when this country switched its constitutional status.

Denoting by X_i a set of observable characteristics, our objective is to estimate the *average treatment effect on the treated (ATT)* that can be expressed as follows ⁷

⁷ For an in depth discussion on different methods to estimate the average treatment effect of political institutions on policy outcomes, see Persson and Tabellini (2003, 2008).

$$E(ATT) = E(Y_{i,t}^1 - Y_{i,t}^0 | S_{i,t} = 1, X_i). \quad (1)$$

The *ATT* measures the effect of constitution on agricultural protection in countries that make a constitutional reform, relative to the (unobserved) counterfactual, which is the outcome variable that would be observed in the treated country if it were off-treatment. Thus the relevant counterfactual for measuring the reform effect is what would have happened in the country protection without any constitutional reform. Clearly, the key problem in estimating this average effect using relation (1) is that we do not observe the counterfactual, $E(Y_{i,t}^0 | S_{i,t} = 1, X_i)$.

A potential solution to this problem is to construct a counterfactual using propensity score matching methods, as in Persson and Tabellini (2008) and Olper *et al.* (2009). However, the few specific constitutional transitions in our sample preclude the use of this approach. An alternative strategy, followed in this paper, is to use difference-in-difference regression. Indeed, under the assumptions that, conditional on X , the treatment S and outcomes $Y_{i,t}^S$ are independent, a conventional linear parametric method to estimate the average treatment effect is the difference-in-difference estimator

$$E(Y_{i,t}^1 - Y_{i,t}^0 | S_{i,t} = 1, X_i) - E(Y_{i,t}^1 - Y_{i,t}^0 | S_{i,t} = 0, X_i). \quad (2)$$

Relation (2) gives the difference-in-difference estimate of the average reform effect by comparing the average protection after the constitutional transition, minus protection before the transition in the treated countries, with the change in protection in control countries during the same period.

In our specific context we are interested in the (possible) heterogeneous effects induced by reform in *different* constitutional features, so the implementation of this difference-in-difference estimator requires additional qualification as we need a multiple treatments specification (Persson, 2005). Thus, our regression model can be expressed as follows

$$Y_{i,t} = \sum_{f=1}^F \beta^f S_{i,t}^f + \rho X_{i,t} + \alpha_i + \theta_t + \varepsilon_{i,t} \quad (3)$$

where $Y_{i,t}$ denotes our measure of interest, namely agricultural protection, α_i and θ_t are respectively the country and year fixed effects, $X_{i,t}$ is a set of control variables, and $S_{i,t}^f$ is a binary variable for a sub-set of different *forms* of democracy $f = 1, \dots, F$, namely majoritarian vs. proportional democracy or parliamentary vs. presidential democracy. Our

parameters of interest, β^f , are the difference-in-difference estimate of the reform effects, and are obtained by comparing average protection after constitutional reform, minus protection before transition in treated countries, to the change in protection in the control countries over the same period. Here the control countries are those that do not experience any reform episodes, thus those that have either $S_{i,t}^f = 1$ or $S_{i,t}^f = 0$ over the entire sample period.

3.1 Identification

The difference-in-difference estimator (3) addresses many limitations of standard cross-section regressions. Indeed, it accounts for time-invariant country characteristics such as geography, resource endowments and historical events, which may influence both economic and policy choices. At the same time it accounts for many ‘structural’ determinants of protection, such as comparative advantage and lobby structure, which typically move slowly over time and thus do not particularly affect the estimate on the reform variable. Moreover, the model also controls for common global shocks, removing the effect on protection of cyclical fluctuations in world prices.

However, the identification of the causal effect of political reforms obtained from equation (3) is based on two key restrictive assumptions (see Abadie, 2005; Persson and Tabellini, 2008). First of all, it is assumed that, in the absence of any political reform, the average growth rate in protection in treated countries should be the same as in control countries. This assumption can be quite restrictive in our specific context as the protection dynamics, for example between developing and developed countries, have been quite different, at least this was the case since the mid-eighties.⁸ We tackled this potential source of bias by adding several covariates to the vector X , with the aim of increasing the ‘similarity’ between treated and control countries. Moreover, as the identifying assumption may be violated if political reforms are not random, we follow the common practice of including, in the vector X of additional controls, the interaction between year fixed effects and time invariant continental dummies for Africa, Asia and Latin America countries. Note that these continent specific trends will also control for the (potential) differences in the protection dynamics across reforming and control countries.

⁸ Indeed, developing countries progressively dismantle their import-substitution policies, while developed countries start to reform their agricultural policies (see Anderson and Valenzuela, 2008).

The second main restriction is that equation (3) does not take into account the (potential) heterogeneity of constitutional effects on agricultural protection. In this case the unexplained component of protection, $\varepsilon_{i,t}$, also includes the term $(\beta_{i,t} - \beta)S_{i,t}^f$, where $\beta_{i,t}$ is the country-specific effect of constitution in country i and year t .⁹ However, because the main objective of this paper is to understand the potential heterogeneous effect of different constitutional features, as well as of different agricultural sectors, our approach should be immune to this potential source of bias.

4. Data

The sample includes 74 countries, comprising yearly data from 1955 to 2005 and covering many agricultural products (see Table A1 for country and year coverage). Overall we worked with an unbalanced panel with more than 25,000 observations. The reform effects on protection are estimated using both the full data set, and sub-samples. Specifically, we contrast import-competing sectors vis-à-vis exportable sectors, as well as four different product groups.¹⁰

4.1 Political reforms

In classifying democratic reforms, previous literature is followed (see Persson and Tabellini, 2003; Giavazzi and Tabellini, 2005; Persson, 2005). Thus, the interested reader should refer to these papers for a deeper description and justification of this not marginal problem. Here we give only a summary of the key criteria and data sources.

First, we classify countries into democracy or autocracy using the Polity2 index of the Polity IV data set. The Polity2 index assigns a value ranging from -10 to +10 to each country and year, with higher values associated with better democracies. Following Giavazzi and Tabellini (2005), we code a country as democratic in each year that the Polity2 index is strictly positive, setting a binary indicator called *democracy* = 1 (0 otherwise). A reform into (or out of) democracy occurs in a country-year when the democracy indicator switches from 0 to 1 (and *vice versa*). In order to render the before-after analysis plausible it is also necessary that the outcome of interest, agricultural

⁹ See, for example, Ashenfelter and Card (1985) for a general discussion on this.

¹⁰ The four groups and their composition are as follows: *Grains and Tubers*: rice, wheat, maize, cassava, barley, sorghum, millet, oat; *Oilseeds*: soybean, groundnut, palm oil, rapeseed, sunflower, sesame; *Livestock products*: pigment, milk, beef, poultry, egg, sheep meat, wool; *Tropical crops*: sugar, cotton, coconut, coffee, rubber, tea, cocoa.

protection, be observed for at least two years before and after each reform episode. Applying these criteria to the dataset, we reach 66 transitions into or out of democracy, of which 41 are transition into democracy and 25 are into autocracy.

Second, following Persson (2005), we construct four binary indicators to classify each country with respect to the specific nature of its constitution. Among democracies, countries are coded as *presidential* ($PRES = 1$, and $PARL = 0$) when the chief executive is not accountable to the legislature through a vote of confidence. In all other situations we have a *parliamentary* system ($PARL = 1$, and $PRES = 0$).¹¹ Note that, following this logic, we have countries with a directly elected president, such as Portugal and France, classified as parliamentary, and countries without a popularly elected president, such as Switzerland, coded as presidential.¹² Moreover, countries are classified as *majoritarian* if their election to the lower house relies strictly on plurality rule ($MAJ = 1$, and $PROP = 0$). Differently, all the other electoral systems are classified as *proportional* ($PROP = 1$, and $MAJ = 0$).

The primary source for mapping the sample into this classification is the database of Persson and Tabellini (2003), supplemented by the *Database on Political Institutions* (DPI) of the World Bank (Beck *et al.* 2001), and the *Comparative Data Set on Political Institutions* (Lundell and Karvonen, 2003).

All the reform episodes discussed above are listed with their specific classifications in Table A.2, panel (a). Differently, panel (b) reports the few (eleven) constitutional reforms that happen in permanent democracies. As clearly shown in the figures, a few countries have reversal episodes during political reforms: countries that start as autocracy and then, after a democratization episode, return to dictatorships. As some of these democratization episodes are very brief, following Giavazzi and Tabellini (2005) and Papaioannou and Siourounis (2008), we define treatment considering not only *all* reform episodes but also *permanent reforms*, namely those reforms that are not reversed in the sample within the period considered.

4.2 *Dependent variables and other covariates*

We test our hypotheses using two ‘different’ dependent variables: the aggregate agricultural nominal rate of assistance (*NRA*) and the nominal rate of assistance at the

¹¹ As discussed in Persson and Tabellini (2003), this represents a quite crude classification, especially because the conceptual model also relies on separation of powers in the legislative process. However, using also this dimension to classify countries as presidential or parliamentary systems introduces difficulties that are beyond the scope of this study.

¹² Note that this apparent contradiction in the classification, does not affect our results to any degree.

product level (*nra*). Both variables are from the World Bank *Agdistortions Database* (see Anderson and Valenzuela, 2008 for calculation details).

The *NRA* is measured as the weighted average of the *nra* at the product level, using as a weight the industry' value share of each product. At the product level the *nra* can be defined as $nra \equiv (P - P^*)/P^*$, where P is the actual domestic price in local currency and P^* is the estimated domestic price that would hold in the absence of any commodity-market or exchange-rate intervention. Thus the *nra* (*NRA*) is like an equivalent tariff measuring total transfer to agriculture products (sector) as a percentage of the undistorted unit values, taking into account both border and domestic protection, as well as protection/taxation due to input subsidies and exchange rate distortions. The *nra* (*NRA*) is positive when the product is subsidized, negative when it is taxed, and 0 when net transfers are zero. Working at both aggregate (agricultural) and commodities level offers two main advantages. First, as the weight used for aggregation, the undistorted values of production, could be measured with errors, it is important to test if our results are robust to potential aggregation bias. Second, working at product level gives us the possibility of investigating whether the constitutional policy effects are heterogeneous across groups of commodities, such as import-competing *vs.* export sectors.

In the empirical specifications we include additional structural controls that are likely to affect the level of agricultural protection, as suggested by previous studies (Anderson, 1995; Beghin, and Kherallah, 2004; Swinnen *et al.* 2000; Olper, 2001). In particular, our basic specification always includes the following covariates: the level of development, measured by the log of real per capita GDP; the share of agricultural employment on total employment; the log of agricultural land per capita; the log of total population; and, in regressions run at the product level, we always include each sector value share over total agricultural production value. All these variables are computed starting from FAO, World Bank (WDI), and *Agdistortions database* sources, or from national statistics.

Moreover, to take into account that change in political regime could be related to the occurrence of conflict (both domestic and international) we also include a variable of conflict years (and its lagged value) equal to 1 (0 otherwise) if in that year(s) there has been conflict in the considered country. These data come from the UCD/PRIO Armed Conflict Dataset Version 4-2008 (see Gleditsch *et al.* 2002). Finally, we also collect data on land Gini inequality and government ideology orientation, to check the robustness of our findings and the 'channels' through which electoral rules affect public policy. Land

inequality is based on FAO data, and comes from Daininger and Olinto (2000), differently, ideology orientation is based on the World Bank DPI dataset, following Dutt and Mitra (2005) and Olper (2007). Specifically, starting from the DPI ideology classification we build an (average) ideology index by attributing a score of 1, 2 and 3 to right, centre and left-wing government orientation, respectively. Then the index is averaged over the time span covered by each country, which normally refers to the 1975-2005 period.

4.3 Constitutional rules and agricultural protection: A first look

Figures 1, 2 and 3 display a non-parametric test for unconditional differences in average nominal rate of assistance (*nra*) across different constitutional features and trade status of agricultural products. Three clear patterns emerge from the figures.

First, on average, democracies have consistently higher protection than autocracies, which are even taxed, especially when exportable sectors are considered (see Figure 1). This is consistent with econometric evidence reported in Olper *et al.* (2009). Second, the differences across forms of democracies are impressive, and this is especially true when we contrast electoral systems (Figure 2). Proportional democracies, both on average and in import-competing sectors, have protection levels from 2 to 4 times higher than majoritarian democracies. Interestingly, these differences are increasing over time. Third, a similar pattern emerges across different forms of government (Figure 3). Parliamentary democracies consistently protect agriculture more than presidential democracies, although the magnitude of these differences decreases over time. Finally, across commodity groups, the constitutional-protection differences are particularly relevant, especially for import-competing sectors.

Summing up, different constitutional features display stark differences in agricultural protection patterns. Obviously one cannot draw conclusions about the effect of forms of government on protection from these simple correlations. Constitutional rules and protection are correlated with several other factors, like the level of development, which need to be controlled for. Moreover, constitutional rules are not random, but are correlated with other (often unobservable) characteristics such as history and culture that introduce not trivial identification problems. The next section will present econometric results that tackle all these things properly.

5. Regression results

Table 1 reports regression results based on the specification (3). In these regressions the dependent variable is the average nominal rate of assistance (*NRA*) in agriculture. All the covariates discussed in Section (3) are included in the vector of controls X_{it} . We also include the continent years interaction effect to control for differences in regional protection dynamics and the non-stationary nature of the democracy dummy.¹³ Finally, the reported standard errors are clustered at country level, allowing arbitrary country-specific serial correlation (see Bertrand *et al.* 2004).

Columns 1 and 2 replicate earlier evidence reported in Olper *et al.* (2009), who studied the political reforms effect of transitions from autocracy to democracy on agricultural protection. These regressions represent our benchmark to evaluate the effect of different constitutional rules on agricultural protection. Regression 1 yields an estimate of the democracy coefficient that is positive and significant (p -value < 0.05). The magnitude of the democracy variable suggests that a transition from autocracy to democracy increases agricultural protection, on average, by 7.8 percent points, a relevant effect from an economic point of view.¹⁴ Because some of the democratization episodes are very brief, in column 2 we also run a regression considering only *permanent reforms*, namely those reforms that are not reversed in the sample within our sample period. Not surprisingly, considering only permanent reforms, the estimated democratization effect increases to 11.7 percent points, suggesting that democracy needs time to display its redistributive effect on agricultural policy. The above results highlight the redistributive nature of democratic institutions toward agriculture, giving broad confirmation to the findings of Olper *et al.* (2009).

To disentangle the effect of constitutional rules, in the subsequent regressions of Table 1 the relevant variables are obtained by interacting the democracy dummy with the respective dummies for government systems, *PARL* and *PRES* (columns 3-4) and electoral rules, *PROP* and *MAJ* (columns 5-6). Thus, the estimated coefficients on these dummies capture the average constitutional effect, namely the percentage points increase in

¹³ Indeed, as emphasized by Papaioannou and Siourounis, (2008), the *democracy* indicator tends to exhibit a trending behaviour, as countries experiencing (successful) transition to democracy hardly ever switch back to autocracy.

¹⁴ The democracy effects reported in Table 1 are slightly lower in magnitude than the findings reported in Olper *et al.* (2009), for two main reasons. First, use was made of criteria considering a political reform episode only when the outcome of interest, protection, was observed at least four years before and after the reform. Thus they are closer to the permanent reform effect reported in column 7. Second, here we have used the last version of the *Agdistortions* database (relies 06-09) that presents some minor changes.

agricultural protection when a country experiences a shift from autocracy to democracy with a particular constitutional status.

Considering first the forms of government, regression results suggest that a reform from autocracy to a presidential democracy induces an increase in agricultural protection of about 8.8 percent points, a magnitude that goes up to 14% points if only permanent reforms are considered (column 4). These effects are statistically significant at the 5% level. Differently, a political reform from autocracy to a parliamentary democracy induces an increase in protection, still positive (4% or 5%, depending on the treatment) but never significant. Thus, it appears that presidential democracies tend to support agriculture more than parliamentary democracies. However, performing an *F*-test to check the statistical difference between parliamentary and presidential coefficients, we cannot reject the equality of the two coefficients at the conventional statistical level (see the bottom of table 1). Thus, the magnitude of the effect of transitions from autocracy to presidential democracy, although positive and significant, is not statistically different from transition to parliamentary democracies, namely having a presidential or a parliamentary system does not matter for agricultural policy.

Regressions 5 and 6 display the results when the democratization effect is split with respect to electoral rules, thus considering political reforms from autocracy to a proportional or a majoritarian democracy. As is clear from the magnitude of the estimated coefficients on *PROP* and *MAJ*, differences in electoral rules matter for agricultural protection. A country that experiences a reform to proportional democracy, from an autocratic status or from a majoritarian democracy, increases agricultural protection by about 15-17% points, and the effect is precisely estimated (*p-value* < 0.01). Differently, reform to a majoritarian democracy has an effect of about 2.5-5%, never statistically significant. Not surprising, an *F*-test for the equality of the coefficients on proportional and majoritarian indicators is rejected at the 5% statistical level.

Summarizing, the two sets of results suggest interesting constitutional effects on agricultural protection. Specifically, a political reform from autocracy to a proportional democracy increase agricultural support or, differently, reduce the level of agricultural taxation,¹⁵ of about 17% points, whereas transition to majoritarian democracy does not. For transition to presidential democracies (*vis-à-vis* parliamentary) the evidence points in the same direction, although the estimated effect is lower in magnitude, and less robust.

¹⁵ Note that in the dataset, several countries experiencing democratic reform have a negative agricultural protection, namely they tax agriculture (see table A.1).

The regressions above are based on average *NRA* obtained by aggregating the level of protection across several agricultural products. This obviously can introduce potential aggregation bias. For that reason, it could be useful to evaluate whether our results are the same when we work at the commodities level. This strategy allows us to investigate the existence of possible heterogeneity in the constitutional policy effects across different commodity groups. Indeed, as show by Anderson and Valenzuela (2008), there exists a huge variation in sectoral protection level.

We start by considering import-competing (Table 2) and export sectors (Table 3), the former traditionally more protected than the latter and with a quite different political economy logic (see Krueger, 1990; Baldwin and Robert-Nicoud, 2007).¹⁶ In these regressions the dependent variable is the *nra* at the product, not the agricultural, level, resulting in a significant increase of the usable observations. Thus, the specification includes country and sector fixed effects, as well as the sector value shares to control for heterogeneity at the sector level.¹⁷

Regressions at the sectors level broadly confirm the previous evidence, giving also new insight. First, a political reform toward democracy always increases agricultural protection, although the magnitude of the estimated effect is sector specific: it reaches about 17.4% points for import-competing sectors but is only 6.8% points for export sectors. Across forms of government, what matters is again transition to presidential (*vs.* parliamentary) democracy. Differently across electoral rules only transition to proportional (*vs.* majoritarian) democracy significantly increases agricultural protection. Once again the estimated coefficients of different electoral rules are always statistically different from each other, while across government systems they are not. Thus, what is interesting from these additional results is the huge difference in the estimated effect between import-competing and export sectors. In the former, transition to presidential and proportional democracy, increases agricultural protection by about 22-23% points, whereas in the latter the same numbers are never higher than 10%.

¹⁶ For example, Richard Baldwin and Frédéric Robert-Nicoud (2007) explain the difference in protection between (declining) import-competing sectors with respect to (expanding) export sectors, suggesting that in the latter a new entrant tends to erode the policy rents, while, in the former, sunk costs rule out entry as long as the rents are not too high. This asymmetric appropriability of rents means losers lobby harder.

¹⁷ The sign and significance of other covariates in this specification are as follows. First, and not surprisingly, agricultural protection is strongly positively associated with the level of development, but negatively with the share of each commodity production value over total production value. Moreover, protection is often positively related to the log of population, and negatively to both the land per capita and the employment share of agriculture. The last variables in these 'sectoral' *nra* specification are often estimated with more precision, with respect to what we found in the 'aggregate' *NRA* regressions of Tables 1 and 2. Finally, the conflict and war dummy (as its lagged value) is always positive but never significant.

Table 4 repeats a similar exercise considering four different product groups. Overall, the results from these additional regressions confirm previous findings, however, with some notable exceptions. Once again democratization exerts a positive effect on protection irrespective of the sector considered, and, in addition, transition to presidential and, especially, to proportional democracy (from an autocratic status) always dominates the reform effect of transition to parliamentary and majoritarian democracy. However, all these effects are precisely estimated only, or especially, in the grains-tubers sector that represent typical staple food crops. What appears puzzling at the product group level is the result of livestock products. Contrary to the results above, in this important sector reform to parliamentary democracy leads to a significant protection growth effect of about 25%, which is also statistically different from the reform effect of presidential democracy. Finally, a democratic transition, although positive, does not exert any significant effect on tropical crops protection, no matter what kind of form of democracy is considered.

5.1 Robustness checks and extensions

To what degree are the results above robust to alternative specifications and explanations? This section will deserve attention to these issues, also with the objective of better understanding the channel through which political institutions may affect agricultural policy.

5.1.1 Dynamic panel model

The first issue is to analyze how critical is the *ceteris paribus* condition or, put differently, how well have all the other variables explaining agricultural protection been controlled for (de Gorter 2010). At a general level, we should emphasize that our specifications are less parsimonious than previous similar exercises conducted on agricultural protection. Indeed, other than the key determinates of agricultural protection found relevant in other studies, we also control for time invariant country heterogeneity, unobserved common shocks, as well as differences in continental trends in protection.

However, a potential critical element of the results is that they are based on a static model. This forces all the dynamics to be captured either by the included controls or by the time dummies. Since several controls included in the vector X exhibit a limited time variation, we attribute a large fraction of the dynamics in protection to unobserved common events. However, agricultural protection is highly persistent over time due to inertia and *status quo* bias. Thus, it would be a mistake to attribute all unexplained

variation in protection in a particular year to unobserved common events in that same year, and this is particularly true for regressions based on sectoral *nra*.

To allow for such persistency, table 5 presents a dynamic version of equation (3), estimating autoregressive specifications that control for persistence in agricultural protection. Note that although the joint presence of fixed effects and the lagged dependent variable could yield inconsistent estimates, our long time period (35 years for the average countries) strongly reduces this potential source of bias. As expected, agricultural protection is highly persistent over time, implying that actual protection is an important predictor of future protection. Not surprisingly, this version of the model shows a smaller magnitude of the political institution effects, as now part of the dynamics in protection is captured by the lagged dependent variable. However, all the relevant institutional coefficients display signs and significance levels close to the static versions.

In fact we find a significant positive effect on protection of a democratization episode of about 3.8% points. This effect changes only slightly for transitions to presidential democracy (4% points), but increase significantly to 6.2% points on passing to proportional democratic transitions.¹⁸ At the same time, presidential and parliamentary coefficients are never statistically different from each other, while these differences persist when considering electoral rules. Finally, once again the import-competing sectors display higher sensitivity to institutionally induced policy changes, and the same pattern emerges considering the four sector groups (results not shown). Thus, on the basis of this additional evidence, we conclude that our main findings and conclusions are very robust to problems of omitting variable bias.

5.1.2 Endogenous constitutions

A more subtle critical element of our findings is related to the consideration that constitutions are endogenous. As stressed by Ticchi and Vindigni (2010, p. 1) ‘if different constitutional provisions lead to different fiscal policies and, therefore, generate different benefits for the various groups in the society, we should expect individuals to have different preferences over constitutions and take this into account at the time of the

¹⁸ Note that these numbers have to be considered short-run effects. If we retrieve the long-run political reform effects – namely the short-run coefficient divided by one minus the lagged dependent variable coefficient – then their magnitude is closed to the estimated effects of static models. For example, the (short-run) democratization-protection effect of 3.8% points reported in column 1 of Table 4, correspond to a long-run effect of 12.2% points, thus very close to the 11.7% points effect of the static model reported in column 1 of Table 1.

constitutional choice'. In our specific context, this reasoning raises at least two main problems.

First, countries with a better assets distribution — for example a more equal distribution of land — may be more likely to make transitions to proportional democracy. If this is the case, then the higher redistributive nature of proportional democracy could simply be the result of a more effective agricultural coalition, such as an effect of the lower *heterogeneity* in the agricultural group (see Olper 2007). There is no obvious support for this proposition in the data as assets inequality moves very slowly over time, thus it is largely captured by country fixed effects in our difference-in-difference specification. However this possibility cannot be ruled out without some additional tests. Indeed, as is evident from table 6, columns 1 to 3, inequality in land distribution (land Gini) reduces (increases) the probability of having an electoral rule based on a proportional (majoritarian) system. Specifically, running a Probit regression, where the dependent variable is equal to 1 for majoritarian countries (0 otherwise), on land Gini and the level of development, we find that inequality increases the probability of having a majoritarian electoral system in OECDs. Hence, to check the robustness of our findings one needs to control also for the (possible) protection effect of inequality in land distribution.

A second concern relates to evidence that left-wing governments are more likely in a proportional democracy (see Iversen and Soskice, 2006; Ticchi and Vindigni, 2010). In such a situation, the risk is that we are confounding the redistributive effect of proportional democracy with those of a left government. Table 6, columns 4-6, indicates the relevance of this reasoning in our dataset. Specifically, we regress an indicator of left-wing orientation on our majoritarian dummy, controlling for the level of development. In line with the prediction of Iversen and Soskice (2006), we find that countries with proportional electoral rules have a higher frequency of left-wing government, although this effect is never statistically significant.

The consideration above calls for a deeper investigation into the effect of electoral rules on agricultural protection, taking into consideration the potential effect of both inequality and government ideology. To deal with this, we use a cross-section of countries, averaging the level of protection across fifteen years, from 1990 to 2004. We are forced to work with a cross-section as neither land Gini nor government ideology

display sufficient time variation to pick up relevant effects in a difference-in-difference specification.¹⁹

Table 7, columns 1 and 2, shows the benchmark cross-country results across two different samples: a *broad* sample, which considers all the countries with data on our constitutional indicators, thus also including countries with doubtful democratic institutions; a *narrow* sample, which includes countries with a Polity2 index of democracy higher than 5, thus only well established democracies. The specifications include all the controls reported in the regressions of table 1.

The estimated coefficients on electoral rule, *MAJ*, and government system, *PRES*, strongly confirm the results presented in section 5.2. Indeed, a country with a majoritarian electoral rule has, on average, a level of agricultural protection from 17% to 28% lower than countries with a proportional system, a result significantly at the 5% level. On the contrary, different forms of government do not affect the level of protection to any degree. What is remarkable about these results is the marked coincidence of the electoral rule effects between cross-country and difference-in-difference regressions.

In columns 3 and 4 we add land Gini to the specification. Its estimated coefficient is negative and significant, but the electoral rule effect is totally unaffected. A similar story is reached when we add our left government orientation measures to the specification. Indeed, in the *broad* sample, its coefficient is negative and significant at the 10% level showing that, on average, left-wing governments tend to protect agriculture less than right-wing governments, although this effect is weak when only well established democracies are considered. Finally, the magnitude of the electoral rule effect, if any, increases when land Gini and left-orientation are included together (see columns 9 and 10).

In columns 11 and 12 we test a specification that includes the interaction of land Gini and government ideology, to assess the possibility that the response of redistributive trade policy to land inequality and ideology is conditional to the interaction between these two variables. For example, as stressed by Olper (2007), one can suppose that if politicians are partisan, then in an unequal society a left-wing government could have a strong rationale for redistribution, potentially affecting the ideology-protection relationship. The results

¹⁹ Moreover, land Gini data with (some) time variation exist only for a few countries of our data set, and are based on a *decennial* period from the FAO Agricultural Census. Similarly, data on left government orientation, based on the World Bank DPI dataset, start only in 1975 and are lacking for several countries experiencing a democratic transition. Thus, for practical reasons, it is impossible to run meaningful fixed effects regressions.

are interesting, and strongly support the above hypothesis. Indeed, the interaction term is positive and strongly significant. At the same time the (absolute) linear coefficients of both land Gini and the ideology variables increase in magnitude, and are now significant at the 5% level. However, most relevant for our purpose, is the fact that the estimated effect of electoral rule is only slightly affected by this modification, and retains its significant level.

Summing up, these additional regressions show that the idea that electoral rules affect public policies through their effect on the partisan composition of governments, as suggested by Iversen and Soskice (2006) and Ticchi and Vindigni (2010), is not supported by the data in the context of agricultural protection.

5.2 Discussion and interpretation

Are these results consistent with actual political economy predictions? First, existing theories (and evidence) suggest that proportional democracies (vis-à-vis majoritarian ones) redistribute more, have larger government spending, larger budget deficits and larger transfers to broad population groups (see Section 2). According to our estimate, reform to proportional democracy increases protection, on average, by about 17% points, an effect consistent with the idea that proportional systems redistribute more. At the same time, the result appears consistent also in terms of the composition of government spending (broad vs. narrow), as in our specific context redistribution towards agriculture has to be considered a *broad* form of redistribution.²⁰ However, our results on electoral rules are contrary to the Grossman and Helpman (2005) model who predict a protectionism bias in majoritarian democracies: for the agricultural sector we find an opposite result.

Second, the findings about the effect of forms of government are less clear. Indeed, theory predicts that presidential countries should have lower government spending, less budget deficit and smaller transfers to broad population groups. Econometric evidence shows that agricultural protection tends to increase more after transitions to presidential democracies, although this effect is sometimes weak, and even reversed in some

²⁰ Indeed, in countries undergoing democratic transition, the average and the median share of agricultural population is higher than 50% (see Olper *et al.* 2009).

circumstances. From this point of view, given the structure of our reforming countries, our econometric evidence tends to be contrary to theory.²¹

Third, our analysis suggests also that the magnitude of the constitutional effects on agricultural protection tends to be ‘sector’ specific, with import-competing sectors and staple food crops being significantly more sensitive to institutionally induced policy changes. We do not have theoretical priors to interpret this evidence. However, if farmers producing for import-competing sectors and/or staple food crops are more numerous, and have incomes close to the median or, differently, are more able to form effective coalitions because losers tend to lobby harder, then it is not surprising to find that, after a democratization process, they are more able to influence government behavior.

A final outcome of our results is related to the channel through which electoral rules exert its effect on redistributive policy. From this point of view, we show that in our country sample neither inequality, nor government ideology, are at the root of the agricultural protectionism bias of proportional democracies. Thus, direct political incentive effects summarized in Section 2 appear the most plausible interpretation of our findings, *ceteris paribus*. Because the last result is also obtained working on a cross-country sample of only democracies, it raises an interesting question on how agricultural sector, and the related policy, should be considered in modern democracies. Indeed, one can argue that the popular view that sees agriculture as a typical ‘narrow’ special interest group in rich countries, could be substituted by a view that sees the sector as a ‘broad’ interest of the population.²² Future investigations of this idea appear important to better understand the extent to which actual political economy theory is able to explain actual patterns of agricultural protection.

6. Conclusion

Motivated by recent developments in political economy theory about the effect of rule-based political institutions on public policy outcomes, we investigated how democratic

²¹ Unconditional evidence supports the notion that presidential democracy has lower agricultural protection than parliamentary democracy, in line with theoretical predictions (see fig. 1). However, it is difficult to find this constitutional effect in data after controlling for other determinants of policy, suggesting that the lower protection in presidential democracies can be attributed to other country features.

²² Note that this view fits with the idea that agricultural protection persists also because the population at large (wrongly or rightly) tends to give high national priority to food security related issues. On this point see the recent evidence of taxpayer beliefs about US farm income and policy, reported in Ellison, Lusk and Briggeman (2010).

transitions into different constitutional systems affect agricultural protection. The results strongly support the notion that democratic institutions tend to be friendly to agriculture. Specifically, a shift from autocracy or a majoritarian democracy to a proportional democracy induces a strong increase in agricultural protection. A similar but weaker effect was detected for transition to a presidential system, at least when we exploit the within country variation in institutions. Interestingly, the magnitude of these effects tends to be ‘sector’ specific, with import-competing sectors and staple food crops being significantly more sensitive to the institutionally induced policy changes. Finally, we show that our main finding—namely, that proportional democracies redistribute more towards agriculture—is robust to the (potential) *selection bias* of constitutions and, more importantly, it is not driven by the (left) ideology orientation of the government. Indeed, in our dataset, left-wing governments tend, on average, to tax agriculture and not to support it.

These findings reinforce the idea that institutions matter to redistributive policy outcomes in a direction at least partially consistent with theory. Obviously, one has to be careful to attribute causality in the appropriate direction when interpreting the observed association between democracy, electoral rules and agricultural policy. However, this paper provides evidence that suggests that the effect of political regime is causal, as the results come from panel data analyses using difference-in-difference regressions focusing on changes in agricultural protection following specific political regime transitions.

Several further improvements need to be made to better understand the interaction between institutions and agricultural policy. For example, this paper assumes that electoral rules affect political incentives directly. However, there is evidence that electoral rules shape public policy only indirectly, through their effect on party and government structure (see Persson *et al.* 2007). Extension in this and other directions could significantly improve our understanding of the interlink between political institutions and agricultural policy.

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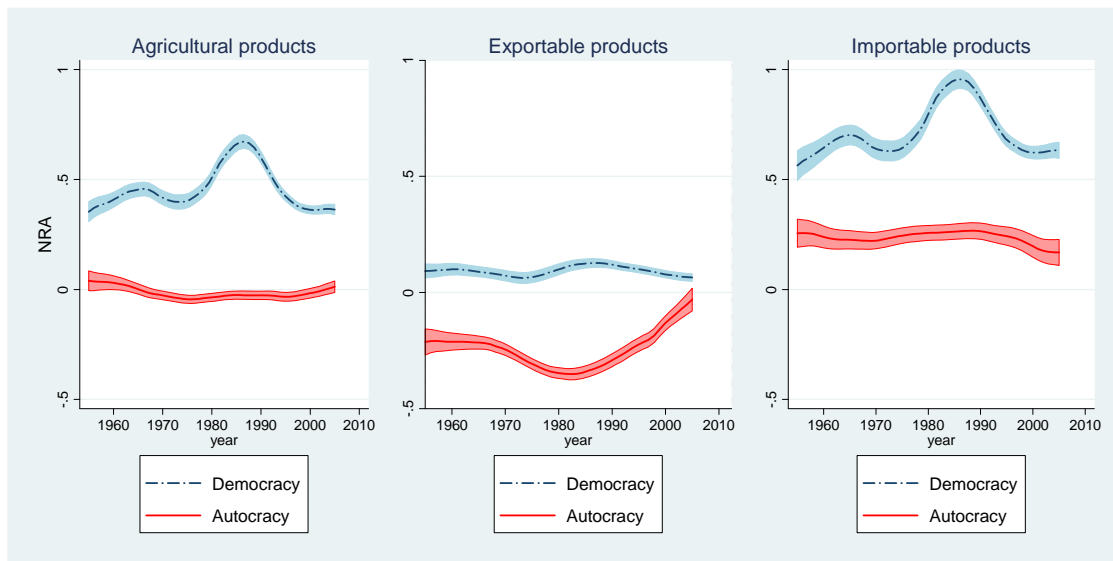


Figure 1. Average nominal rate of assistance to agriculture across autocracies and democracies, 1955 to 2005

Notes: The figures show the evolution of the (smoothed) average *nra*, and their 95 percent confidence interval (computed using Stata's *lpolyci*) calculated across different political regimes for overall agriculture products and for exportable and importable products.(see text).

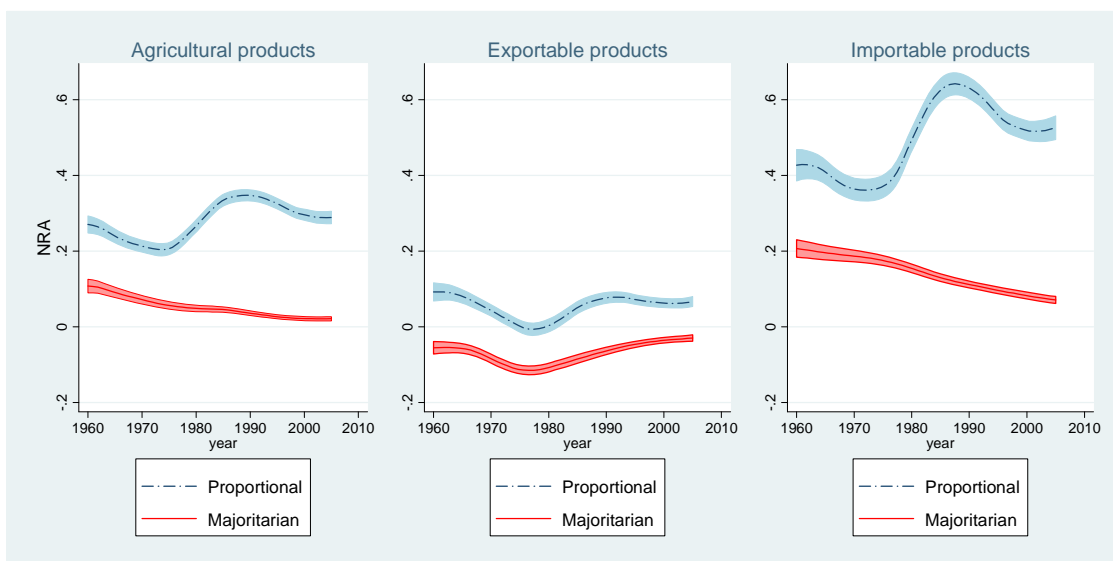


Figure 2. Average nominal rate of assistance to agriculture over electoral systems, 1960 to 2005

Notes: The figures show the evolution of the (smoothed) average *nra*, and their 95 percent confidence interval (computed using Stata's *lpolyci*), calculated across electoral systems for overall agriculture products and for exportable and importable products.(see text).

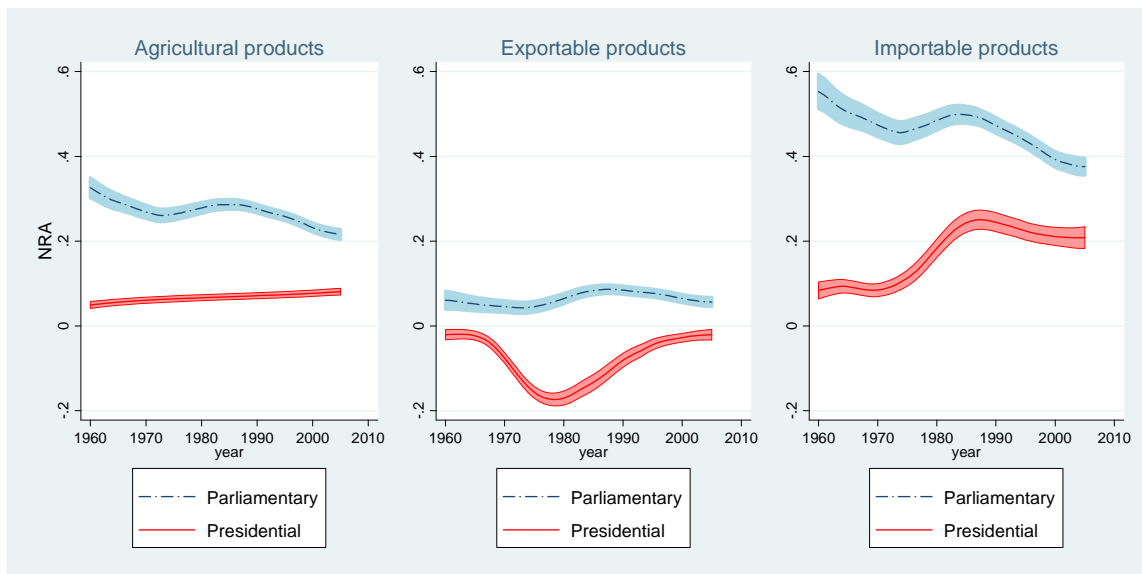


Figure 3. Average nominal rate of assistance to agriculture over government types, 1960 to 2005

Notes: The figures show the evolution of the (smoothed) average *nra*, and their 95 percent confidence interval (computed using Stata's *lpolyci*), calculated across government types for overall agriculture products and for exportable and importable products.(see text).

Table 1. Constitutional rules and agricultural protectionDependent variable: country average nominal rate of assistance (*NRA*)

Estimation	Difference-in-difference estimates					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Democracy</i>	0.078 <i>(0.030)</i>	0.117 <i>(0.023)</i>				
<i>PARL</i>			0.043 <i>(0.346)</i>	0.051 <i>(0.366)</i>		
<i>PRES</i>			0.088 <i>(0.037)</i>	0.142 <i>(0.032)</i>		
<i>PROP</i>					0.155 <i>(0.008)</i>	0.176 <i>(0.007)</i>
<i>MAJ</i>					0.025 <i>(0.490)</i>	0.053 <i>(0.263)</i>
Wald test:						
<i>F-statistic</i>			0.69	1.36	4.85	4.70
<i>p-value</i>			<i>(0.409)</i>	<i>(0.247)</i>	<i>(0.031)</i>	<i>(0.033)</i>
Treatment	All	Permanent	All	Permanent	All	Permanent
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Continental trends	Yes	Yes	Yes	Yes	Yes	Yes
R squared (within)	0.369	0.374	0.372	0.378	0.379	0.382
Countries	74	74	74	74	74	74
Observations	2574	2574	2511	2511	2511	2511

Notes: P-value based on robust standard errors clustered by country in parentheses. All regressions also include the log of population, agricultural employment share, land per capita, and conflict year dummies. Interaction between years and continent dummies (Africa, Asia, and Latin America) included as indicated (see text). Figures in **bold** (*italics*) when the significant level is higher than 95% (90%).

Table 2. Constitutional rules and agricultural protection of import-competitive sectors

Dependent variable: country-sectors nominal rate of assistance (<i>nra</i>)						
Estimation	Difference-in-difference estimates					
Sector	Import-competitive sectors					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Democracy</i>	0.138 <i>(0.003)</i>	0.174 <i>(0.007)</i>				
<i>PARL</i>			0.108 <i>(0.301)</i>	0.048 <i>(0.682)</i>		
<i>PRES</i>			0.148 <i>(0.002)</i>	0.231 <i>(0.002)</i>		
<i>PROP</i>					0.225 <i>(0.001)</i>	0.223 <i>(0.003)</i>
<i>MAJ</i>					0.054 <i>(0.259)</i>	0.084 <i>(0.210)</i>
Wald test						
<i>F-statistic</i>			0.13	1.89	6.60	3.97
<i>p-value</i>			0.724	0.169	0.011	0.047
Treatment	All	Permanent	All	Permanent	All	Permanent
Continental trends	Yes	Yes	Yes	Yes	Yes	Yes
R squared (within)	0.239	0.239	0.238	0.239	0.240	0.239
Countries-sectors	519	519	519	519	519	519
Observations	13278	13278	13206	13206	13206	13206

Notes: P-value based on robust standard errors clustered by country-sector in parentheses. All regressions include: the log of per-capita GDP, the log of population, agricultural employment share, land per capita, the product value shares, conflict year dummies, and interaction between years and continent dummies (Africa, Asia, and Latin America). Figures in **bold** (*italics*) when the significant level is higher than 95% (90%).

Table 3. Constitutional rules and agricultural protection of export sectors

Dependent variable: country-sectors nominal rate of assistance (*nra*)

Estimation Sectors	Difference-in-difference estimates					
			Export sectors			
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Democracy</i>	0.046 <i>(0.043)</i>	0.068 <i>(0.026)</i>				
<i>PARL</i>			0.003 <i>(0.933)</i>	0.025 <i>(0.608)</i>		
<i>PRES</i>			0.062 <i>(0.009)</i>	0.091 <i>(0.003)</i>		
<i>PROP</i>					0.092 <i>(0.013)</i>	0.109 <i>(0.009)</i>
<i>MAJ</i>					0.014 <i>(0.477)</i>	0.027 <i>(0.309)</i>
Wald test						
<i>F-statistic</i>			2.84	2.06	5.62	6.21
<i>p-value</i>			0.093	0.152	0.018	0.013
Treatment	All	Permanent	All	Permanent	All	Permanent
Continental trends	Yes	Yes	Yes	Yes	Yes	Yes
R squared (within)	0.146	0.147	0.145	0.146	0.147	0.148
Countries-sectors	440	440	440	440	440	440
Observations	9558	9558	9558	9558	9558	9558

Notes: P-value based on robust standard errors clustered by country-sector in parentheses. All regressions include: the log of per-capita GDP, the log of population, agricultural employment share, land per capita, the product value shares, conflict year dummies, and interaction between years and continent dummies (Africa, Asia, and Latin America). Figures in **bold** (*italics*) when the significant level is higher than 95% (90%).

Table 4. Constitutional rules and protection for specific commodities

Dependent variable: country-sectors nominal rate of assistance (*nra*)

	Difference-in-difference regressions											
	Grains and tubers			Livestock products			Oilseeds			Tropical Crops		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Democracy</i>	0.126 <i>(0.000)</i>			0.087 <i>(0.322)</i>			0.150 <i>(0.106)</i>			0.059 <i>(0.284)</i>		
<i>PARL</i>		0.041 <i>(0.611)</i>			0.256 <i>(0.036)</i>			0.047 <i>(0.628)</i>			0.039 <i>(0.759)</i>	
<i>PRES</i>		0.150 <i>(0.000)</i>			0.004 <i>(0.969)</i>			0.186 <i>(0.138)</i>			0.064 <i>(0.179)</i>	
<i>PROP</i>			0.201 <i>(0.001)</i>			0.106 <i>(0.311)</i>			0.359 <i>(0.091)</i>			0.146 <i>(0.159)</i>
<i>MAJ</i>			0.064 <i>(0.047)</i>			0.041 <i>(0.655)</i>			0.030 <i>(0.648)</i>			0.004 <i>(0.924)</i>
Wald test												
<i>F-statistic</i>		1.27	3.93		3.76	0.46		0.66	2.55		0.04	2.34
<i>p-value</i>		0.261	0.049		0.054	0.496		0.417	0.114		0.834	0.129
Treatment	All	All	All	All	All	All	All	All	All	All	All	All
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Continental trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R squared (within)	0.227	0.228	0.229	0.279	0.278	0.277	0.378	0.381	0.386	0.424	0.424	0.426
Countries-sectors	269	269	269	238	238	238	80	80	80	112	112	112
Observations	8932	8764	8764	6920	6890	6890	2510	2508	2508	3869	3778	3778

Notes: P-value based on robust standard errors clustered by country-sector in parentheses. All regressions include: the log of per-capita GDP, the log of population, agricultural employment share, land per capita, the product value shares, conflict year dummies, and interaction between years and continent dummies (Africa, Asia, and Latin America). Figures in **bold** (*italics*) when the significant level is higher than 95% (90%).

Table 5. Robustness Checks: Dynamic panel modelDependent variable: country-sectors nominal rate of assistance (*nra*)

Regression	<i>All sectors</i>			<i>Import-competing</i>			<i>Exportables</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Democracy</i>	0.038 <i>(0.000)</i>			0.061 <i>(0.001)</i>			0.023 <i>(0.048)</i>		
<i>PARL</i>		0.027 <i>(0.113)</i>			0.052 <i>(0.206)</i>			0.003 <i>(0.860)</i>	
<i>PRES</i>		0.040 <i>(0.000)</i>			0.062 <i>(0.002)</i>			0.031 <i>(0.013)</i>	
<i>PROP</i>			0.062 <i>(0.000)</i>			0.092 <i>(0.000)</i>			0.038 <i>(0.035)</i>
<i>MAJ</i>			0.018 <i>(0.046)</i>			0.028 <i>(0.205)</i>			0.014 <i>(0.241)</i>
<i>Lagged NRA</i>	0.693 <i>(0.000)</i>	0.692 <i>(0.000)</i>	0.692 <i>(0.000)</i>	0.669 <i>(0.000)</i>	0.669 <i>(0.000)</i>	0.668 <i>(0.000)</i>	0.580 <i>(0.000)</i>	0.579 <i>(0.000)</i>	0.579 <i>(0.000)</i>
Wald test									
<i>F-statistic</i>		0.45	7.50		0.06	4.54		2.08	2.05
<i>p-value</i>		<i>0.504</i>	<i>0.006</i>		<i>0.808</i>	<i>0.034</i>		<i>0.150</i>	<i>0.153</i>
Treatment	All	All	All	All	All	All	All	All	All
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Continental trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R ² (within)	0.576	0.575	0.575	0.610	0.609	0.610	0.444	0.443	0.443
Country-sectors	801	801	801	517	517	517	440	440	440
Observations	25301	24976	24976	13045	12978	12978	9355	9229	9229

Notes: P-value based on robust standard errors clustered by country-sector in parentheses. All regressions include: the log of per-capita GDP, the log of population, agricultural employment share, land per capita, the product value shares, conflict year dummies, and interaction between years and continent dummies (Africa, Asia, and Latin America). Figures in **bold** (*italics*) when the significant level is higher than 95% (90%).

Table 6. Robustness checks: Relationship between electoral rules, inequality and ideology

Estimation	<i>Probit</i>			<i>OLS</i>		
	MAJ (1)	MAJ (2)	MAJ (3)	LEFT (4)	LEFT (5)	LEFT (6)
<i>Land Gini</i>	-0.060 (0.956)	0.593 (0.442)	5.306 (0.035)			
<i>MAJ</i>				-0.147 (0.408)	-0.288 (0.116)	-0.034 (0.826)
<i>Log GDP per-capita</i>	-0.255 (0.014)	-0.006 (0.973)	1.437 (0.021)	-0.112 (0.018)	-0.014 (0.808)	-0.201 (0.198)
Sample	Broad	Narrow	OECD	Broad	Narrow	OECD
R ²	0.085	0.004	0.220	0.084	0.053	0.123
Obs.	55	39	24	59	47	26

Notes: P-value based on robust standard errors in parentheses. Columns 1-3 report Probit regressions; Columns 4-6 report OLS regressions. *MAJ* is a dummy variable equal to 1 for majoritarian electoral systems, and 0 otherwise; land Gini values refer to circa 1980; Left is a variable increasing in left government orientation equal to 1, 2 and 3 for countries with right, center and left-wing government orientation, respectively, averaged across the period 1975-2005. Figures in **bold** (*italics*) when the significant level is higher than 95% (90%).

Table 6. Robustness Checks: Cross-section regressions on 1990-2004 average values

Dependent variable: country average nominal rate of assistance (*NRA*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>MAJ</i>	-0.169 (0.031)	-0.280 (0.022)	-0.189 (0.018)	-0.266 (0.023)	-0.182 (0.041)	-0.304 (0.024)	-0.221 (0.009)	-0.308 (0.014)	-0.241 (0.008)	-0.335 (0.008)	-0.193 (0.024)	-0.255 (0.045)
<i>PRES</i>	-0.014 (0.906)	0.061 (0.694)	0.016 (0.903)	0.142 (0.448)	-0.021 (0.870)	0.050 (0.749)	-0.012 (0.930)	0.092 (0.610)	-0.053 (0.747)	0.029 (0.884)	-0.080 (0.629)	-0.017 (0.933)
<i>Land Gini</i>			-0.862 (0.043)	<i>-1.044</i> (0.087)			-0.923 (0.035)	<i>-1.028</i> (0.084)	-0.648 (0.115)	-0.570 (0.352)	-3.561 (0.006)	-4.468 (0.010)
<i>Left-orientation</i>					<i>-0.124</i> (0.088)	<i>-0.074</i> (0.405)	-0.155 (0.045)	<i>-0.128</i> (0.236)	<i>-0.104</i> (0.170)	<i>-0.048</i> (0.661)	-0.845 (0.010)	-1.174 (0.022)
<i>Left * Land Gini</i>											0.012 (0.016)	0.018 (0.023)
Sample	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Continental dummies	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Adj R ²	0.523	0.545	0.569	0.512	0.518	0.470	0.581	0.511	0.588	0.505	0.614	0.563
Observations	65	48	54	39	59	47	50	39	50	39	50	39

Notes: P-values based on robust standard errors in parentheses. *MAJ* is a dummy variable equal to 1 for majoritarian electoral systems, and 0 otherwise; land Gini values refer to circa 1980; Left measures government orientation, and is equal to 1, 2 and 3 for countries with right, center and left-wing government ideology, respectively, averaged across the 1975-2004 period. Each regression include the following set of controls: the log of per-capita GDP and population, agricultural employment share, land per capita, and regional fixed effects as indicated.. Figures in **bold** (*italics*) when the significant level is higher than 95% (90%).

Table A1. Country sample and years coverage

#	Country	<i>Years coverage</i>		#	Country	<i>Years coverage</i>	
		Start	End			Start	End
1	Argentina	1960	2005	38	Malaysia	1960	2005
2	Australia	1955	2005	39	Mali	1970	2005
3	Austria	1956	2005	40	Mexico	1979	2005
4	Bangladesh	1974	2004	41	Morocco	1961	2004
5	Benin	1970	2005	42	Mozambique	1975	2005
6	Brazil	1966	2005	43	Netherlands	1956	2005
7	Bulgaria	1992	2005	44	New Zealand	1955	2005
8	Burkina Faso	1970	2005	45	Nicaragua	1991	2004
9	Cameroon	1961	2005	46	Nigeria	1961	2004
10	Canada	1961	2005	47	Norway	1956	2005
11	Chad	1970	2005	48	Pakistan	1962	2005
12	Chile	1960	2005	49	Philippines	1962	2005
13	China	1981	2005	50	Poland	1992	2005
14	Colombia	1960	2005	51	Portugal	1956	2005
15	Cote d'Ivoire	1961	2005	52	Romania	1992	2005
16	Czech Republic	1992	2005	53	Rep. of South Africa	1961	2005
17	Denmark	1956	2005	54	Russia	1992	2005
18	Dominican Republic	1955	2005	55	Senegal	1961	2005
19	Ecuador	1970	2003	56	Slovakia	1992	2005
20	Egypt	1955	2005	57	Slovenia	1992	2005
21	Estonia	1992	2005	58	Spain	1955	2005
22	Ethiopia	1981	2005	59	Srilanka	1955	2004
23	Finland	1956	2005	60	Sudan	1958	2004
24	France	1956	2005	61	Sweden	1956	2005
25	Germany	1955	2005	62	Switzerland	1956	2005
26	Ghana	1960	2004	63	Taiwan	1955	2002
27	Hungary	1992	2005	64	Tanzania	1976	2004
28	India	1960	2005	65	Thailand	1978	2004
29	Indonesia	1970	2005	66	Togo	1970	2005
30	Ireland	1956	2005	67	Turkey	1961	2005
31	Italy	1956	2005	68	Uganda	1961	2004
32	Japan	1955	2005	69	UK	1956	2005
33	Kenya	1966	2001	70	Ukraine	1992	2005
34	Korea South	1955	2005	71	USA	1955	2005
35	Latvia	1992	2005	72	Vietnam	1986	2005
36	Lithuania	1992	2005	73	Zambia	1964	2005
37	Madagascar	1960	2005	74	Zimbabwe	1970	2005

Table A2. Reform episodes (1955-2005)

(a) Exits and entries in different forms of democracy

<i>Country</i>	<i>Year</i>	<i>Into or Out of Democracy</i>	<i>Form of government</i>	<i>Electoral rule</i>
Argentina	1973	Into	Presidential	Proportional
Argentina	1976	Out	Presidential	Proportional
Argentina	1983	Into	Presidential	Proportional
Benin	1991	Into	Presidential	Proportional
Burkinafaso	1977	Into	Presidential	Proportional
Burkinafaso	1980	Out	Presidential	Proportional
Bangladesh	1991	Into	Parlamentary	Majoritarian
Brazil	1985	Into	Presidential	Proportional
Chile	1973	Out	Presidential	Majoritarian
Chile	1989	Into	Presidential	Majoritarian
Cote d'Ivoire	2000	Into	Presidential	Majoritarian
Cote d'Ivoire	2002	Out	Presidential	Majoritarian
Dominican Republic	1978	Into	Presidential	Proportional
Ecuador	1968	Into	Presidential	Proportional
Ecuador	1970	Out	Presidential	Proportional
Ecuador	1979	Into	Presidential	Proportional
Spain	1976	Into	Parlamentary	Proportional
Ethiopia	1994	Into	Parlamentary	Majoritarian
Ghana	1970	Into	Parlamentary	Majoritarian
Ghana	1972	Out	Parlamentary	Majoritarian
Ghana	1979	Into	Presidential	Majoritarian
Ghana	1981	Out	Presidential	Majoritarian
Ghana	1996	Into	Presidential	Majoritarian
Indonesia	1999	Into	Presidential	Proportional
Kenya	1966	Out	Parlamentary	Majoritarian
Kenya	2002	Into	Presidential	Majoritarian
Korea	1963	Into	Presidential	Majoritarian
Korea	1972	Out	Presidential	Majoritarian
Korea	1987	Into	Presidential	Proportional
Madagascar	1991	Into	Presidential	Proportional
Mexico	1994	Into	Presidential	Proportional
Mali	1992	Into	Presidential	Majoritarian
Mozambique	1994	Into	Presidential	Proportional
Nigeria	1966	Out	Presidential	Majoritarian
Nigeria	1979	Into	Presidential	Majoritarian
Nigeria	1984	Out	Presidential	Majoritarian
Nigeria	1999	Into	Presidential	Majoritarian
Pakistan	1970	Out	Presidential	Majoritarian
Pakistan	1972	Into	Presidential	Majoritarian
Pakistan	1977	Out	Presidential	Majoritarian
Pakistan	1988	Into	Presidential	Majoritarian
Pakistan	1999	Out	Presidential	Majoritarian
Philippines	1972	Out	Presidential	Majoritarian
Philippines	1986	Into	Presidential	Majoritarian
Portugal	1975	Into	Parlamentary	Proportional

Sudan	1958	Out		
Sudan	1965	Into	Parlamentary	Majoritarian
Sudan	1970	Out	Parlamentary	Majoritarian
Sudan	1986	Into	Presidential	Majoritarian
Sudan	1989	Out	Presidential	Majoritarian
Senegal	2000	Into	Presidential	Proportional
Thailand	1974	Into	Parlamentary	Majoritarian
Thailand	1976	Out	Parlamentary	Majoritarian
Thailand	1978	Into	Parlamentary	Majoritarian
Turkey	1971	Out	Parlamentary	Proportional
Turkey	1973	Into	Parlamentary	Proportional
Turkey	1980	Out	Parlamentary	Proportional
Turkey	1983	Into	Parlamentary	Proportional
Taiwan	1992	Into	Parlamentary	Proportional
Tanzania	2000	Into	Presidential	Majoritarian
Uganda	1966	Out	Parlamentary	Majoritarian
Uganda	1980	Into	Presidential	Majoritarian
Uganda	1985	Out	Presidential	Majoritarian
Zambia	1968	Out	Presidential	Majoritarian
Zambia	1991	Into	Presidential	Majoritarian
Zimbabwe	1987	Out	Presidential	Majoritarian

(b) Reforms in existing democracies

<i>Country</i>	<i>Reform</i>	<i>Type of reform</i>
Bangladesh	1991	Government: presidential to parliamentary
France	1986	Election: majoritarian to proportional
France	1988	Election: proportional to majoritarian
New Zealand	1996	Election: majoritarian to proportional
Philippines	1998	Election: majoritarian to proportional
Philippines	2001	Election: proportional to majoritarian
South Africa	1994	Election: majoritarian to proportional
Sri Lanka	1979	Government: parliamentary to presidential
Sri Lanka	1989	Election: majoritarian to proportional
Taiwan	1996	Government: parliamentary to presidential
Ukraine	1998	Election: majoritarian to proportional