FORMS OF DEMOCRACY AND AGRICULTURAL POLICY OUTCOMES*

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Abstract

This paper tests predictions from recent political economics literature about the effect of different forms of democracy on public policy outcomes. We identify the effect of different electoral rules and government systems on agricultural protection, by exploiting democratization episodes in a sample of 74 countries over the 1955-2005 period. Results from difference-in-difference regressions show that a democratization process induces an increase in agricultural protection that is strongly conditional to the specific nature of the political reform. Indeed, what matter 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 is 'sectors' specific, with import-competing sectors and staple food crops being significantly more sensitive to the institutionally induced policy changes.

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

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

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1. Introduction

In the last decade, the political economy of development have pose growing emphasis on the role institutions play in affecting economic growth and performance (see Hall and Jones, 1999; Acemoglu et al 2001).[1] One of the crucial questions in this debate is about the mechanism of how political institutions affect economic growth (Rodrik et al 2004). In this respect, government policies should play a key role. Political institutions affect (economic) policy making by shaping the rules of the game and determine the context in which key policy decisions are made, such as the redistribution of income, the correction of market failures and the provision of public goods (Besley, 2005).

The newly emerging field of comparative political economics places growing emphasis on the effect political institutions play in shaping public policy outcomes. The conceptualization of rule-based political institutions—like electoral rules and forms of government—into formal political economy models, has produced several testable hypotheses firmly motivated by theory (Persson and Tabellini 2000). Until now, rigorous tests of the predictions coming from this literature has been largely confined to macro-economic policies, like welfare programmes and fiscal policies. Thus, what about the link between democratic institutions and other important structural policies ?

In this paper we contribute to this literature by studying the effect of different forms of democracy on agricultural policy outcomes in both developing and developed countries. Agricultural policy (subsidization or taxation) is an excellent policy instrument to study the impact of political institutions across a wide variation of countries. Indeed, agricultural policy is an important policy for governments in both rich and poor countries.[2]

Actual literature linking political institutions to agricultural policies (see Beghin and Kherallah, 1994; Beghin et al 2006; Swinnen et al. 2001; Olper, 2001; Thiesse and

[1] See Przeworski and Limongi (2000) and Przeworski (2004), for critical reviews about the effect of institutions on economic and policy outcomes.

[2] In poor countries where agriculture is a very important share of the economy and where food is a major consumption item the importance of agricultural policy as a public policy issue is obvious. However, also in rich countries agricultural policy remains disproportionately important compared to the relatively small share of agriculture in terms of economic output. A clear symptom of this continued importance of agricultural policy for rich (and poor) countries is the stand-off in the current WTO negotiations where disagreements over agricultural policies is now threatening to undermine the entire WTO agreement.

Porsche 2007)[3] provides weak link with the 'new generation' of political economic models, lessening our understanding of the mechanism in place and, consequently, its policy implication. Some contributions have tried to go further, closing the gap between theory and evidence (Henning 2004, Olper and Raimondi 2004). However, questions still remain regarding the robustness and generalization of existing empirical findings.

This 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 many agricultural products from 1955 to 2005.

From a methodological point of view we follow 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 empirical approach exploiting the within country and across-country variation in political institutions. Specifically, we estimate a multiple treatment effects, to separate the effect of democracy *per se*, from the effects of its details in terms of electoral rules and forms of government.

We find robust evidence that the nature of democratic institutions matter for the economic policy of agricultural protection. Specifically, we document a significant positive protection effect of political reforms toward democracy. However, what matter 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 tend to be 'sectors' specific, with import-competing sectors and staple food crops being significantly more sensitive to the institutionally induced policy changes.

These findings reinforce the idea that institutions matter to redistributive outcomes in a direction that is consistent with theory. Obviously, we have to be careful to attribute causality in the appropriate direction when interpreting the observed association between

[3] Important precursors of this kind of analyses can be found in the works of Bates (1983; 1989) on agrarian development in African countries. Moreover, the relationship between democracy and agricultural protection was first highlighted by Lindert (1991), who in a cross-country analysis found a positive relationship when democracy was associated with rapid agricultural decline.

democracy, electoral rules and agricultural policy. This paper provides evidence that suggests that the effect of political regime is causal. Indeed, the evidence comes from panel regressions with country fixed effects, and specific instances of changes in agricultural protection following transitions in specific political regimes. However, at the same time, it is possible that reverse causation exists as well.

Countries with better assets distribution—reflected in a relatively more equal distribution of land, for example—may be more likely to make transitions to proportional democracy (Ticchi and Vindigni, 2009). If this is the case, then the higher redistributive nature of proportional democracy could be simple the results of a more effective agricultural coalition as an effect of the lower *heterogeneity* in the agricultural group (see Olper 2007). There is no obvious support in the data for this proposition, as assets inequality move very slowly over time, and it is thus largely captured by fixed effects in our specification. However, this possibility cannot be ruled out. Another concern could be related to the evidence that left-wing government are more likely in proportional democracy, at least in OECD countries. Then the risk is that we are confounding the redistributive effect of proportional democracy with those of center-left government. The empirical results by Olper (2007) cast some doubt on this line of though. If indeed the electoral rule influences agricultural policy through the government ideology, then we should observe a positive relationship between left government orientation and agricultural protection; however the data goes exactly in a opposite direction, at least in more unequal countries.

The remainder of the paper is organised as follow. Section (2) put our paper in the context of the related literature. Section (3) presents how democratic reforms are measured and classify, as well as the dependent variable and other structural controls. The section end with some preliminary results about how agricultural protection maps across different forms of democracy. Section (4) introduce the econometric strategy and discuss the identification issues. In Section (5) the results are presented and discussed at the light of actual predictions and evidence. Finally, Section (6) conclude.

2. Related literature

Our paper is related to a growing theoretical and empirical literature studying the effect of democratic institutions on public policy outcomes. Across this literature, we can useful distinguished two main lines of research. The first one is related to papers that try to

answer questions like: does democracies have different economic policy than autocracies? Differently, the second line of research is instead devoted to analyze, within democracies, questions like: do differences in the constitutional details of democracies affect public policy? Clearly, the two questions are strongly interrelated, but logically distinct.

The literature contrasting democratic vs. autocratic form of government is large, but little consensus has emerged on how these two different regimes affect public policy outcomes. For example, Mulligan et al. (2004) in a cross-section of countries do not find robust evidence of any difference between democracies and autocracies on several economic policies. Differently, Besley and Kudamatsu (2006), using panel data analysis show that health policy interventions are superior in democracies.

A policy that has attracted substantial interest in this perspective is trade policy. For example, Banerji and Ghanem (1997) show cross-country evidence supporting the view that authoritarian regimes are associated with higher trade protectionism (as well as greater labor market distortions). Similarly, Milner and Kubota (2005) and Giavazzi and Tabellini (2005) using panel data analysis find that democratization is associated with more liberal trade policy. The idea that democratization affect trade policy, however, has been questioned by both Persson (2005) and Persson and Tabellini (2006).[4] Indeed, these authors show that while democratization *per se* does not affect trade policy, transitions toward parliamentary and proportional democracies does, supporting the idea that what matters are the *details* of democracy.

A main limitation of actual evidence linking democratization to trade policy is related to how protection is measured. Due to the low data coverage of many developing countries, previous studies have not really examined the link between democracy and measures of trade policy, like ad valorem tariffs or the effective rate of protection. Instead, almost all the cited papers are based on weak measures of protection, like trade openness, and especially the Sachs and Warner (1995) openness index (see Giavazzi and Tabellini, 2005; Milner and Kubota 2005; Persson 2005; Persson and Tabellini, 2006, among others). In this respect it is important to note that, while the Sachs and Warner index have the merit of taking into consideration the multidimensionality nature of trade policy, at the same time it has been strongly criticized by Rodrìguez and Rodrik (2001) who stressed that it is a poor measures of trade barriers.[5] Not surprising, how trade policy is measured

[4] See also O'Rourke and Taylor (2007), Kono (2006) and Fehrs and Axelrod (2006).

[5] Whether the Sachs and Warner openness index measures exclusively trade openness remains an unanswered question precisely because trade openness is correlated with institutions (Rodrìguez and Rodrik,

matter for the final results. Tavares (2007), showed that the effect of democratization on trade protection is in fact sensitive on how openness is measured, finding significantly results especially when trade policy is proxy through the (criticized) Sachs and Warner index.

From this point of view our paper add to the existing literature by using a more comprehensive measures of (agricultural) trade protection, taking into account of both direct and indirect source of agricultural policy taxation and subsidization. Thus, our results are not driven by the subjectivity on how trade policies are measured.

The literature investigating the effect of political institutions within democracies, due to the low within country variation in constitutions, is largely confined to cross-countries empirical studies. Formal model and theoretical predictions rely especially on the differentiated policy effect of electoral rules and forms of government. Thus most of the papers have contrasted the effect on macroeconomic policies of these important institutional dimensions (see Persson and Tabellini, 2003; 2004 for reviews of the empirical findings).[6]

Across electoral rules, key findings show that a switch from proportional to majoritarian elections induce a significantly reduction in overall government spending, as well as a reduction of programmes directed to broad groups of population, like welfare programmes (see Persson and Tabellini, 1999; Kontopoulos and Perotti, 1999; Milesi-Ferretti et al. 2002; Persson and Tabellini, 2004; Persson et al. 2007). Similarly, across different forms of government the evidence support the notion of greater public spending and government redistribution in parliamentary systems, than in presidential ones (Persson et al 2000; Persson and Tabellini, 2004). A few studies have also explored the effect of these constitutional dimensions on trade policy, however the evidence are mixed in this respect.[7]

A main shortcoming of this literature come from its cross-sectional nature, as it is notoriously difficult in comparative politics to extract casual inference from cross-country

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2001). On the virtues and limits of Sachs and Warner index, see also the more optimistic view of Wacziarg and Welch (2008).

[6] The electoral rules and forms of government are not the only democratic details investigated by the actual empirical 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 matter are the 'number of access points' provide by institutions (see Ehrlich 2007).

[7] See Roelfsema (2004) for positive effect of majoritarian election on protection. By contrast, Rogowski and Kayser (2002) and Hatfield and Hauk (2003) obtain exactly the opposite, namely agricultural protection tend to be higher under proportional election.

data (see Acemoglu, 2005; Giavazzi and Tabelllini, 2005). From this point of view our paper try to go further by testing the policy effect of different forms of democracy exploiting the within country and across group variation in political institutions. Specifically, we adopt a before-after event study approach based on difference-in-difference regressions along the line recently introduced in the literature studying the effect of democratization on growth (see Giavazzi and Tabellini, 2005; Persson 2005; Papaioannou and Siourounis, 2008). Moreover, we depart from some limitations of difference-in-difference regressions by taking into consideration directly the (potential) heterogeneity effects of institutions on policy outcomes.

3. Data

The sample includes 74 countries, comprising yearly data from 1955 to 2005 over many agricultural products. 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 products groups called 'Grains and Tubers', 'Livestock Products', 'Oilseed' and 'Tropical Crops'.[8]

3.1 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.

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 this democracy indicator switch from 0 to 1 (and *vice versa*). In order to render the before-

[8] The groups composition is as follow. *Grains and Tubers*: rice, wheat, maize, cassava, barley, sorghum, millet, oat; *Oilseeds*: soybean, groundnut, palmoil, rapeseed, sunflower, sesame; *Livestock products*: pigment, milk, beef, poultry, egg, sheepmeat, wool; *Tropical crops*: sugar, cotton, coconut, coffee, rubber, tea, cocoa.

after analysis plausible it is also necessary that the outcome of interest, agricultural 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 (see Table A.1).

Next, following Persson (2005), we use four binary indicators to classify each country with respect to its specific *forms* of democracy. Among democracies, the 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).[9] 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. Moreover, countries are classified as *majoritarian* if their elections to the lower house rely 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 (see Beck et al. 2001), and the *Comparative Data Set on Political Institutions* (see Lundell and Karvonen, 2003)[10].

All the reform episodes discussed above with their specific classification are listed in Table A.2, panel (a). Differently, panel (b) reports the few (eleven) constitutional reforms that happen in permanent democracies. As it is clear from the figures, in a few countries we observe episodes of reversals in political reforms: countries that start as autocracy and then, after a democratization episode, return back into dictatorships. As some of these democratization episodes are very brief, following Giavazzi and Tabellini (2005) and Papaioannou and Siourounis (2008), we define treatment both considering *all* the reforms episodes and also considering only *permanent reforms*, namely those reforms that are not reversed in the sample within our sample period.

[9] 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.

[10] For our purposes, the main differences in these two data sets lie in the countries and time period covered. Specifically, the DPI cover a larger set of countries but it is limited to the 1975-2006 period. Instead the data set of Lundell and Karvonen covers only 'democracies' but the data start from 1960, at least for the more consolidated democracies.

3.2 Dependent variable 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 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's value share of each product. At 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 transfers to agriculture products (sector) as a percentage of the undistorted unit values, taking into account of 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.

To simplify the interpretation of the regression coefficients, we express *nra* (and *NRA*) as a percentage. Thus, the magnitude of the estimated coefficient on our political institution indicators measures the average percentage point changes in agricultural protection implied by a transition into democracy.

In the empirical specification we include additional structural controls that are likely to affect the level of agricultural protection, as suggested by many previous studies (e.g. Anderson, 1995; Beghin, and Kherallah, 2004; Swinnen et al 2000; Olper, 2001). Specifically, our basic specification always includes the following controls: 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. Moreover, given the high persistency of agricultural protection, we also include the lagged dependent variable. All these variables are computed starting from FAO and World Bank (WDI) sources, or from national statistics.

Finally, to take into account that the change in political regime could be related to the occurrence of conflicts (both domestic and international) we include also a variable conflict years (and its lagged value) equal to 1 (0 otherwise) if in that year(s) there has

been a conflict in the considered country. These data come from the UCD/PRIO Armed Conflict Dataset Version 4-2008 (see Gleditsch et al. 2002).

3.3 A preliminary look of the data

Figure 1 displays a test for unconditional differences in average *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 when exportable sectors are considered. Second, the differences across forms of democracies are impressive, especially when we contrast electoral systems. 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. Second, a similar pattern emerges across different forms of government. Parliamentary democracies consistently protect agriculture more than presidential democracies, although the magnitude of these differences are lower here, and decreasing 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. The next section will closely investigate whether these differences survive to a more robust empirical analysis.

4. Econometric method

Following Giavazzi and Tabellini (2005) we define a *reforms* into specific forms of democracy as a 'treatment' experienced by some countries but not others. Then we estimate the average effect of (multiple) treatments through a difference-in-difference regression exploiting the fact that, any democratic transition, could be characterized on the basis of its specific constitutional status in term of electoral rules and forms of government.

Following this strategy, we are able to exploit both the time series and cross-sectional variation in the data, as well as the (potential) heterogenous effect of democratization across both different constitutional rules and agricultural sectors (see below). Indeed, in our sample we include both countries that experience a transitions in a specific forms of

democracy in the observed period, called the *treated*, and the countries that do not experience any reforms, called the *controls*.

More formally, this approach could be presented as follows. Denote by $S = \{0, 1\}$ the treatment indicator, equal to 1 for treated countries, i.e. those that experience a democratic reform, and equal to 0 for control countries, i.e. those that do not change political regime throughout the observed period for which data on agricultural protection are available. Let $Y_{i,t}^{S}$ the outcome of interest, namely the level of agricultural protection in country *i* in time *t* and constitutional state *S*. Let $t = 0$ corresponds to the period *before* the change in political institutions, and $t = 1$ corresponds to the period *after* transition. The individual treatment effect of a constitutional 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 take a switched in 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 follow[11]

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

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 had happen in the country's protection without any constitutional reform. Clearly, the key problem in estimating this average effect using the relation (1) is that we do not observe the counterfactual, $E\left(Y_{i,t}^{0} \middle| S_{i,1} = 1, X_i\right)$.

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 forms of democracy 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

[11] 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).

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

Relation (2) say that difference-in-difference estimate the average reform effect by comparing average protection after the constitutional transition, minus protection before the transition in the treated countries, to the change in protection in the control countries over the same period.

In our specific context, because we are interested in the heterogeneous effects induced by reforms into *different* constitutional features, the implementation of this difference-in-difference estimator require some additional qualification, as we need a multiple treatments specification (see Persson, 2005). Thus, our regression model can be expressed as follow

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

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 $S^f_{i,t}$ is a binary variable for a sub-set of different *forms* of democracy f = 1,...., 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 they are obtained by comparing average protection after a constitutional reform, minus protection before the transition in the 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 $S^f_{i,t}$ = 1 or $S^f_{i,t}$ = 0 over the entire sample period.

4.1 Identification

The difference-in-difference specification (3) address many limitation of standard cross-section regressions. Indeed, the specification account for time-invariant country characteristics, such as geography, resource endowment and historical events that may influence both economic and policy choices. At the same time it account for many 'structural' political economy determinants of protection, such as comparative advantage and lobbies structure, that typically move slowly over time and thus do not particularly affect the estimate on the reform variable. Moreover, the model also control 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 the specification (3) is based on two key restrictive assumptions (see Abadie, 2005; Persson and Tabellini, 2008). First of all, it is assumed that, absent any regime change, the average growth in protection in the treated countries should be the same as in control countries. This assumption could be quite restrictive in our specific context, as the protection dynamic between, e.g., developing and developed countries has been quite different at least starting from the half of the eighties.[12] We tackled this potential source of bias by adding several covariates in the vector *X*, with the aim to increase 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. Note that, this continent specific trends, will also control for the (potential) differences in the protection dynamics across reforming and control countries.

The second main restriction is that the specification (3) does not take into account the (potential) heterogeneity of regime change effects on agricultural protection. In that case the unexplained component of protection, $\varepsilon_{i,t}$, also includes the term $(\beta_{i,t} - \beta)S^f_{i,t}$, where $\beta_{i,t}$ is the country-specific effect of regime change in country *i* and year *t*.[13] However, as the main objective of the present paper is to understand the potential heterogenous effect of regime changes across different forms of democracy as well as across different agricultural sectors, our approach should be quite immune from this potential source of bias.

5. Regression results

5.1 Baseline results

Table 1 reports the baseline econometric results using the aggregated level of protection (*NRA*) as dependent variable. Columns 1-3 report 'unconditional' democracy effects by adding to the vector of covariates *X* only the level of development, to control for the well know positive correlation between *per capita* GDP and both democracy and agricultural protection. Then in columns 4-7 we analyze the democratization effect controlling for the

[12] Indeed, developing countries progressively dismantled their import-substitution policies, while developed countries start to reform their agricultural policies.
[13] See Ashenfelter (1978) and Ashenfelter and Card (1985) for a general discussion on this.

standard determinants of agricultural protection. As the dependent variable displays a strong positive autocorrelation, in all specifications the standard errors are clustered at the country level, allowing arbitrary country-specific serial correlation (see Bertrand et al. 2004).

The pooled OLS specification in column 1 yields an estimate of the democracy coefficient positive but barely significant. This result is primarily driven by cross country variation in the data and, given the existing evidence showing a weak effect of democracy on agricultural protection, comes as no surprise. In column 2 we control for global shocks adding time fixed effects. The democracy coefficient is only slight affected but it is now insignificant at the conventional statistical level, suggesting that democratization effects should be especially driven by within country variations in the data. This is largely confirmed by the results in column 3, where we isolate the within country democracy effect by adding a vector of country fixed effects. The coefficient on democracy is about two times larger than in the pooled OLS estimate, and statistically significant at 1% level.

Regression in column 4, shows the results of our first 'unconditional' difference-in-difference regression. The magnitude of the democracy variable suggests that a transition from autocracy to democracy, on average, increase agricultural protection of 12.7% points, a magnitude that represent a relevant effect from an economic point of view. Column 5 tests the robustness of our finding by including a set of covariates normally found significant determinants of agricultural protection. Taken individually these variables display the expected signs but they are often insignificant, although they are significant when considered jointly. More interesting is the fact that controlling for development, comparative advantage and other structural variables, the democracy effect is still significant (p-value < 0.05), experiencing only a slight reduction to 9.5%. In regression 6 we add to the specification the continent years interaction effect to control for both differences in regional protection dynamics and the non-stationary nature of the democracy dummy.[14] This amendment induce a slight reduction of the democracy coefficient to 8%, that is still significant at 5% level.

Finally, column 7 presents a dynamic version of equation (3), estimating autoregressive specifications that control for the persistence in agricultural protection. Note that, although the joint presence of fixed effects and the lagged dependent variable

[14] 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.

could yield inconsistent estimates, our large 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. Note surprising, this version of the model shows a smaller magnitude of the democracy effect, as now part of the dynamics in protection is captured by the lagged dependent variable. The increase in protection after a democratization episode is about 3%, with a significance levels close to the static versions.[15]

5.2 Results at product-group level

Table 2 displays the results of equation (3) estimated across different samples, using as dependent variable the *nra* at product levels. The specification now include country-sector, instead of country, fixed effects as well as each sector value share. Regressions 1 and 2 consider the full sample, so they are 'equivalent' to specification 7 of Table 1.[16] In column 1 the democracy coefficient is positive and strongly significant (p-value < 0.01), meaning that a reform into democracy induces, on average, an increase in agricultural protection of about 4.4% points. In column 2, controlling for continent year interaction effects, the estimated democracy coefficient is only slightly affected by this manipulation and is still strongly significant. Taken together, these results are very close to the previous findings which are based on country (weighted) average protection level.

Regressions 3 to 6 run the same specifications, but this time splitting the sample into import-competing (3-4) and exportable (5-6) sectors. The democracy coefficient is always positive but shows strong heterogeneity. The reform effect jumps to about 6-7% points for importable sectors, whereas for exportable sectors the increase in protection after democratization is low and not precisely estimated, confirming the impressions emerged from the analysis of figure 1.

In table 3 the sample is split by considering four different product groups. The increase in protection after democratization is strongly positive and significant for Grains

[15] The magnitude of this democracy effect is just slight lower than that estimated in Olper and Raimondi (2010) using a very close specification. This is because in the present study we use the last version of the World Bank *Agdistortions_database 0609*, that present some minor corrections in the *NRA* (and *nra*) series.

[16] The sign and significance of other covariates in this specification are as follows. First, and not surprisingly, agriculture protection displayed strong persistency; it is strongly positively associated with the level of development and 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 'aggregate' *NRA* regressions of Table 7. Finally, the conflict dummy (as its lagged value) is always positive but never significant.

and Tubers, with a magnitude of about 6% points, is about 5% but barely significant for Oilseeds, and never significant for either Livestock or Tropical products. Thus, there is strong evidence that democratization increases protection especially in staple food crops.

Table 4 displays the political reform effect considering different forms of democracy, by interacting the democracy dummy with the indicators for government type or electoral system. The results suggest that what matters are reforms into proportional democracies and, to a lesser extent, reforms into presidential democracies. A country that experiences a reform into proportional democracy, from an autocratic status or from a majoritarian democracy, increases agricultural protection by about 6% points, whereas a reform into majoritarian elections does not. Moreover, this effect jumps to more than 9% points for import-competing sectors, but it is only 3.7% for exportable sectors. Differently, introducing a presidential democracy in autocratic countries, raises overall protection by about 3.9% points (6.2% for import-competing, and 3% for exportable sectors, respectively), whereas reform into proportional systems does not. However, the magnitude of the effects of a transition to presidential democracies, although positive and significant, is not statistically different from transition to parliamentary democracies.[17]

Finally, table 5 studies the protection effect of different forms of democracy across product groups. The results on Grains-tubers and Oilseeds broadly mimic the protection effect of presidential and proportional democracies shown before. However, in that case, the differences between parliamentary and presidential reforms are more stark than before, although still insignificant from a statistical point of view. What appears puzzling here is the result of Livestock products. Contrary to the above results, in this important sector a reform into parliamentary democracy displays a protection growth effect of about 8.7% points, significant at 10% level. Finally, a democratic transition does not affect to any degree the level of protection for Tropical crops, no matter which kinds of forms of democracy are considered.

5.3 Discussion and interpretation

A fundamental question now is how to interpret the above results in the light of actual political economics theory. First of all, in the data we find a robust protection growth effect of democratization, a result in line with historical findings showing that the

[17] We perform an *F*-test to check the statistical difference between parliamentary and presidential coefficients, and we fail to reject the null of $H_0 : (\beta^1 = \beta^2)$ at conventional statistical level in every regression of table 4.

extension of voting rights is often followed by a redistributive process (Lindert, 1994; Acemoglu and Robinson, 2000; Swinnen, Banerjee and de Gorter, 2001). From a theoretical point of view, this is consistent with the median voter model of political behaviour if the redistributive process triggered by democratization goes toward the majority. This is exactly what we find in the data. Indeed, in countries undergoing democratic transition the average and the median share of agricultural population is higher than 50% (see Olper, Falkowski and Swinnen, 2009).

Second, one of the key findings of the above results is the strong effect of electoral rules on agricultural protection. Existing theoretical predictions (and evidence) suggest that proportional democracies (vis-à-vis majoritarian ones) have larger government spending, larger budget deficits and larger transfers to broad population groups, like welfare programmes (see Kontopoulos and Perotti, 1999; Milesi-Ferretti, Perotti, Rostagno, 2002; Persson and Tabellini, 2003; Persson, Roland and Tabellini, 2007). According to our estimate, a reform into proportional democracy increases protection by about 6-8% points. If we consider agricultural protection a broad form of redistribution – and this could be the case in our reforming countries – then our results are fully consistent with theoretical predictions.

Third, our findings on the effect of forms of government on agricultural protection are less clear. Theory predicts that presidential countries should have lower government spending, less budget deficit and smaller transfers to broad population groups (Persson, Roland and Tabellini, 1997; 2000). Econometric evidence shows that agricultural protection tends to increase more after a transition to a presidential democracy, though this effect is sometimes weak, and even reverse in some circumstances. From this point of view, given the structure of our reforming countries, our econometric evidence tends to be contrary to theory. Unconditionally 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 lower protection in presidential democracies can be attributed to other country features.

A final outcome of our analysis suggests that the magnitude of the constitutional effects on agricultural protection tend to be ‘sectors’ 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, then it is not surprising to find that, after a democratization process, they are more able to influence the government behavior. Of course, the higher sensitivity of import-competing and staple food sectors to regime changes calls for a deeper interpretation.

6. Conclusion

Motivated by recent developments in political economy theory about the effect of rule-based political institutions on public policy outcomes, we have investigated how transitions into different forms of democracy affect agricultural protection. The results strongly suggest that democratic institutions tend to be friendly to agriculture: a shift from autocracy or a majoritarian democracy to a proportional democracy induce a strong increase in agricultural protection. A similar but weak effect are detected for transition to presidential system. Interesting, the magnitude of these effects tend to be 'sectors' specific, with import-competing sectors and staple food crops being significantly more sensitive to the institutionally induced policy changes.

These findings reinforce the idea that institutions matter to redistributive outcomes and, most interesting, in a direction that is quite consistent with theory. Obviously, we have 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 comes from panel data analyses using difference-in-difference regressions focusing on changes in agricultural protection following transitions in specific political regimes.

Several further improvements should be made to better understand the interaction between institutions and agricultural policy. For example, this paper assumed that electoral rules directly affect political incentives. 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). Moreover, a major limitation of this paper, common to the actual evidence linking institutions to public policy, is that we have treated political institutions as an exogenous accident. However, if different constitutional provisions lead to different public policy, generating different benefits for 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 constitutional choice. Thus, further empirical exercise need to take into account more carefully of the (potential) subtle effect due to the endogenous nature of constitutions. Extension in these and other directions could significantly improve our understanding of the interlink between political institutions and public policies.

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Figure 1. Average nominal rate of assistance to the agricultural sector over constitutional features, 1955 to 2005

Notes: The figures show the evolution of the (smoothed) average *NRA*s, and their 95 percent confidence interval (computed using Stata's lpolyci), calculated across constitutional features and product trade status (see text).

Table 1. Democracy and Agricultural Protection: Baseline Results

Estimation	OLS	Time FE	Country FE	Difference in Difference estimates			
	(1)	(2)	(3)	(4)	(5)	(6)	(5)
Democracy	6.994*	6.649	11.696***	12.724***	9.493**	8.135**	3.184**
	(4.188)	(4.528)	(3.329)	(4.174)	(4.127)	(3.738)	(1.295)
Log GDP per capita	19.151***	19.268***	21.069**	30.640***	35.160***	29.724***	12.951***
	(3.463)	(3.446)	(9.318)	(12.241)	(11.945)	(9.426)	(3.234)
Employment share					-75.92	-58.89	-13.17
					(58.779)	(48.596)	(14.222)
Land per-capita					-0.669	-1.865	-0.482
					(1.013)	(1.134)	(0.304)
Log population					29.407*	-50.11	-2.86
					(15.343)	(31.604)	(8.524)
Conflicts					0.272	0.287	1.685
					(2.374)	(2.191)	(1.120)
Lagged NRA							0.717***
							(0.026)
Years fixed effects	No	Yes	No	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Continental trends	No	No	No	No	No	Yes	Yes
Observations	2671	2671	2671	2671	2535	2535	2535
Countries	74	74	74	74	74	74	74
R squared (within)	0.371	0.398	0.105	0.206	0.263	0.391	0.704

Notes: Robust standard errors clustered by country in parentheses. Interaction between years and continent dummies (Africa, Asia, and Latin America) included as indicated (see text).
****p < .01, **p < .05, *p < .10.*

Table 2. Democracy and Agricultural Protection: Country-sectors Results

	All sectors		Import-competing		Exportables	
Regression	(1)	(2)	(3)	(4)	(5)	(6)
Democracy	4.430***	3.761***	7.013***	6.058***	1.794	2.288**
	(0.870)	(0.818)	(1.814)	(1.887)	(1.215)	(1.180)
Lagged NRA	0.696***	0.693***	0.671***	0.669***	0.588***	0.580***
	(0.015)	(0.016)	(0.017)	(0.017)	(0.037)	(0.036)
Log GDP per-capita	12.44***	12.06***	19.59***	16.56***	8.897***	12.79***
	(2.540)	(2.146)	(5.522)	(5.007)	(2.869)	(2.729)
Continent-year dummies	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25301	25301	13045	13045	9355	9355
Number of countries	74	74	74	74	74	74
R^2 (within)	0.560	0.572	0.593	0.605	0.422	0.434

Notes: Robust standard errors clustered by country-sector in parentheses. All regressions also include the log of population, agricultural employment share, land per capita, sector share on total agricultural production value and conflict year dummy. Interaction between years and continent dummies (Africa, Asia, and Latin America) included as indicated (see text).
****p* < .01, ***p* < .05, **p* < .10.

Table 3. Democracy and Agricultural Protection: Products-groups results

	Grains and tubers		Livestock products		Oilseeds		Tropical Crops	
Regression	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy	6.473***	5.692***	4.037	1.102	5.117*	4.772*	0.449	2.513
	(1.283)	(1.238)	(3.742)	(3.717)	(3.212)	(2.739)	(2.008)	(1.943)
Lagged NRA	0.653***	0.653***	0.704***	0.698***	0.716***	0.698***	0.675***	0.662***
	(0.032)	(0.033)	(0.021)	(0.023)	(0.043)	(0.047)	(0.025)	(0.033)
Log GDP per-capita	17.82***	15.97***	12.34**	10.33*	27.26**	25.61**	-1.202	7.555*
	(4.824)	(3.913)	(6.275)	(6.265)	(12.034)	(10.032)	(3.899)	(4.484)
Continent-year dummies	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8758	8758	6785	6785	2456	2456	3791	3791
R^2 (within)	0.541	0.563	0.652	0.662	0.650	0.675	0.564	0.676

Notes: Robust standard errors clustered by country-sector in parentheses. All regressions also include the log of population, agricultural employment share, land per capita, sector share on total agricultural production value and conflict year dummy. Interaction between years and continent dummies (Africa, Asia, and Latin America) included as indicated (see text).
****p* < .01, ***p* < .05, **p* < .10.

Table 4. Forms of Democracy and Agricultural Protection: Country-sectors Results

	All sectors		Import-competing		Exportables	
Regression	(1)	(2)	(3)	(4)	(5)	(6)
Parliamentary (PARL)	2.73		5.15		0.33	
	(1.75)		(4.16)		(1.91)	
Presidential (PRES)	3.99***		6.23***		3.11**	
	(0.89)		(2.02)		(1.28)	
Proportional (PROP)		6.18***		9.24***		3.78**
		(1.42)		(2.66)		(1.83)
Majoritarian (MAJ)		1.82**		2.82		1.35
		(0.92)		(2.27)		(1.18)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24976	24976	12978	12978	9229	9229
R^2 (within)	0.57	0.57	0.61	0.61	0.44	0.44

Notes: Robust standard errors clustered by country-sector in parentheses. All regressions include: the lagged dependent variable, the log of per-capita GDP, the log of population, agricultural employment share, land per capita, sectoral shares, conflict year dummy, and interaction between years and continent dummies (Africa, Asia, and Latin America).
****p < .01, **p < .05, *p < .10.*

Table 5. Forms of Democracy and Agricultural Protection: Product-Groups Results

	Grains and tubers		Livestock products		Oilseeds		Tropical Crops	
Regression	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parliamentary (PARL)	2.26		8.75*		-0.93		5.09	
	(3.00)		(5.19)		(3.34)		(4.55)	
Presidential (PRES)	6.67***		-2.53		6.59*		1.71	
	(1.43)		(3.85)		(3.63)		(1.74)	
Proportional (PROP)		8.25***		1.31		12.70**		3.71
		(2.37)		(4.36)		(6.52)		(3.35)
Majoritarian (MAJ)		3.64**		0.41		0.28		1.70
		(1.64)		(4.40)		(2.79)		(1.73)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8608	8608	6759	6759	2456	2456	3708	3708
R^2 (within)	0.57	0.57	0.67	0.67	0.70	0.70	0.69	0.69

Notes: Robust standard errors clustered by country-sector in parentheses. All regressions include: the lagged dependent variable, the log of per-capita GDP, the log of population, agricultural employment share, land per capita, sectoral shares, conflict year dummy, and interaction between years and continent dummies (Africa, Asia, and Latin America).

****p < .01, **p < .05, *p < .10.*

Table A1. Reform episodes and their distribution under different forms of democracy

	All	*Into*	*Out*	*PARL*	*PRES*	*PROP*	*MAJ*
Changes < 1985	0.62	0.49	0.84	0.83	0.53	0.67	0.59
Latin America	0.15	0.17	0.12	0.00	0.21	0.33	0.05
Africa	0.50	0.46	0.56	0.39	0.53	0.25	0.63
Asia	0.24	0.24	0.24	0.28	0.23	0.13	0.32
Number of transitions	66	41	25	18	47	24	41

Notes: The figures report the number of democratic transitions classified by forms of democracy and their relative distribution across time and continents. (See text). The sum across different forms of government doesn't not give the total number of transitions because one transition (Sudan, 1958) is unclassifiable.

Table A2. Reform episodes (1955-2005)

(a) Exits and entries in different forms of democracy

Country	*Year*	*Into or Out of Democracy*	*Form of government*	*Electoral rule*
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

Country	*Reform*	*Type of reform*
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