

Olper, Alessandro; Raimondi, Valentina

Working Paper

Electoral Rules, Forms of Government and Redistributive Policy: Evidence from Agriculture and Food Policies

LICOS Discussion Paper, No. 305

Provided in Cooperation with:

LICOS Centre for Institutions and Economic Performance, KU Leuven

Suggested Citation: Olper, Alessandro; Raimondi, Valentina (2012) : Electoral Rules, Forms of Government and Redistributive Policy: Evidence from Agriculture and Food Policies, LICOS Discussion Paper, No. 305, Katholieke Universiteit Leuven, LICOS Centre for Institutions and Economic Performance, Leuven

This Version is available at:

<https://hdl.handle.net/10419/74931>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



LICOS Centre for Institutions and Economic Performance

Centre of Excellence

LICOS Discussion Paper Series

Discussion Paper 305/2012

Electoral Rules, Forms of Government and Redistributive Policy: Evidence from Agriculture and Food Policies

Alessandro Olper and Valentina Raimondi



Katholieke Universiteit Leuven

LICOS Centre for Institutions and Economic
Performance
Waaistraat 6 – mailbox 3511
3000 Leuven

BELGIUM

TEL: +32-(0)16 32 65 98

FAX: +32-(0)16 32 65 99

<http://www.econ.kuleuven.be/licos>

Electoral Rules, Forms of Government and Redistributive Policy: Evidence from Agriculture and Food Policies*

Alessandro Olper and Valentina Raimondi

Version: 16 November 2011

Abstract

We investigate the effect of electoral rules and forms of government on public policy outcomes using a new dataset on agriculture and food policies from 74 countries over the 1960-2005 period. Using both cross-sectional and panel data analyses we find robust evidence that the specific nature of democratic institutions has important consequences for public policy. Proportional democracies and presidential democracies – compared to majoritarian and parliamentary democracies – give more public support to agriculture and less to food consumers. The magnitude of these constitutional effects are stronger for import-competitive sectors and staple food crops. The effects seem independent from the ideology orientation of governments.

Keywords: Electoral rules; forms of government; agriculture and food policy; political economy.

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

* We are grateful to Kym Anderson, Harry de Gorter, Kishore Gawande, Gordon Rausser, Annalisa Zezza and participants at conferences at the World Bank in Washington, the EAAE Conference in Ghent, the IATRC Meeting in Scottsdale, the ACE Conference in Adelaide, and the AEA Meeting in Atlanta for helpful comments on early drafts. A special thank to Jo Swinnen, for several constructive comments, and to Mauro Vigani who provided excellent research assistance. Financial support received from the World Bank, the Catholic University of Leuven (KUL), and the Italian Ministry of Education, University and Research (Scientific Research Program of National Relevance 2007 on “European Union policies, economic and trade integration processes and WTO negotiations”) is gratefully acknowledged.

Alessandro Olper is associated professor at the Università degli Studi di Milano, and associated researcher at LICOS - Centre for Institution and Economic Performance; Valentina Raimondi is researcher at the Università degli Studi di Milano.

Corresponding author: alessandro.olper@unimi.it, phone: +39 02 50316481; fax: +39 02 50316486; address: Università degli Studi di Milano, via Celoria 2 – 20133 Milano, Italy.

1. Introduction

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 (Barro, 1997; Rodrik and Wacziarg, 2005; Giavazzi and Tabellini, 2005). However, the evidence that democratization *per se*, systematically affects growth and public policy is weak (see Persson, 2005; Persson and Tabellini, 2008; Acemoglu *et al.* 2008).¹

As a consequence scholars have shifted their attention to more specific details of democratic constitutions, like electoral rules and forms of government. For example, Persson and Tabellini (2006) show that while a shift from autocracy to democracy does not have an 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-country evidence about the effect of constitutional rules on public policy outcomes (see Persson and Tabellini, 2003).

Theory predicts that proportional electoral systems and parliamentary regimes should be associated with a broad form of redistribution, like welfare programs, as well as with higher levels of government spending and redistribution, *vis-à-vis* majoritarian and presidential systems (see Persson and Tabellini, 2003). A related set of comparative politics models focus on trade policy, predicting a protectionism bias in majoritarian politics (see Grossman and Helpman 2005).

Until now, the evidence linking these constitutional features to public policy has been largely confined to macro-economic policies, like fiscal policy and trade policy.² Moreover, with the notable exceptions of Persson and Tabellini (2006), empirical evidence draws largely from cross-country variation. However, it is notoriously difficult

¹ The recent meta-analysis of Doucouliagos and Ulubasoglu (2008) concludes that democracy does not have a direct impact on economic growth. However, democracy has robust, significant, and positive indirect effects, through policies. The literature tends to be inconclusive especially when democratization episodes are considered against growth and trade policy. Differently, studies that contrast the *level* of democracy to different indices of economic liberalization (as in Giuliano *et al.* 2010), or by using instrumental variables techniques (as in Eichengreen and Leblang, 2008) are more supportive of a positive role played by democracy. See also the recent contributions of Murtin and Wacziarg (2011) and Grosjean and Senik (2011).

² A few empirical studies have investigated the effect of constitutions on agricultural policy, focusing especially on democracy. Cross-country studies displayed inconclusive results (see Swinnen *et al.* 2000; Olper 2001). Studies that exploit the panel dimension of the data find that democratization display a positive and robust effect on agricultural protection (see Swinnen *et al.* 2001; and Olper *et al.* 2011).

in comparative politics to extract causal inference from cross-sectional data (see Acemoglu, 2005). Constitutions are themselves political outcomes and their policy effects can be confounded with that of other factors, such as cultural, historical and institutional factors, if these are not properly controlled for.

The objective of this paper is to make a contribution to the literature by addressing two main issues. First, we focus on a specific redistributive policy which is widely used in developing and developed countries, i.e. policies to tax or support farmers and food consumers. This specific policy focus is interesting because of its policy relevance and because of the availability of new policy indicators. The agricultural and food sector is an ideal case for studying the political economy of public policies. The sector is subject to heavy-handed governmental interventions throughout the world. Despite decades (even centuries) of economists' arguments against agricultural subsidies and tariffs, political factors continue to dominate agricultural policy setting (including trade policy) in both rich and poor countries. The struggle of the World Trade Organization (WTO) to conclude the Doha round of multilateral trade negotiations brought again to the forefront the important role that agricultural policy continues to play in international trade relations. 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. For example in the EU, the Common Agricultural Policy (CAP) continues to absorb 45% of the entire EU budget in 2010. Despite a strong decline of agriculture in terms of employment and output, rich countries' unwillingness to reduce agriculture subsidies threatens the WTO negotiations.

Agricultural and food policies have changed dramatically over the course of long-term economic development and continue to vary widely among countries, across commodities, and in the choice of policy instruments used. Accordingly, the evolution of policies affecting this sector and the fundamental differences among countries provides an excellent empirical foundation for studying the determinants of policy choices, and in particular of political institutions.

We make use of a new dataset on agricultural and food policy recently developed by the World Bank (see Anderson and Valenzuela, 2008). The public policy indicators in this new dataset cover more countries and larger period of time than it was previously available. In addition the indicators capture the effect of various public policies, including

trade and fiscal policies, that affect agriculture and food consumers. Therefore, these data represent potentially an improvement compared to the standard practice of using trade openness indicators, like the Sachs and Warner (1995) index to measure trade policies (e.g. Giavazzi and Tabellini, 2005; Milner and Kubota, 2005; Persson, 2005; Persson and Tabellini, 2006, among others),³ or other measures like score indices of agricultural policy reforms in the export sectors, as in Giuliano *et al.* (2010). The new indicator measures directly the policy outcomes that we are interest in, and is likely less affected by measurement errors of subjective indicators. Second, and most important, the use of this large new dataset also allows us to include a mix of democracies and non-democracies and to measure political transition effects.

Our key 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). Thus, our main source of institutional variation comes from about sixty-five democratization episodes (political reforms) in the 1960-2005 period. Exploiting these *reforms*, we conduct a before-after analysis to disentangle the protection growth effect of democracy *per se*, from that of its constitutional details, in terms of electoral rules and forms of government.

Hence, by exploiting the within- and across-country variation in constitutions and public policies, our econometric approach is less susceptible to the endogeneity concerns of this type of analysis – as explained by Acemoglu (2005). However, as a robustness check, we complement our difference-in-difference analysis with cross-sectional inference also based on instrumental variables (IV) estimator.

We find a significant *positive* effect of political reforms on support and redistribution toward farmers. In particular, democratic reforms into proportional democracies (*vis-à-vis* majoritarian) induce a clear switch from taxation to subsidization of farmers. Reforms into presidential democracies (*vis-à-vis* parliamentary) point in the same direction, but have less effect. Moreover, our analysis suggests that the magnitude of the institutional reform effect on agricultural protection tends to be significantly stronger for import-competing sectors and staple food crops. Finally and interesting, we find a remarkable consistency between cross-sectional and panel data inference.

³ Whether the Sachs-Warner openness index measures exclusively trade openness remains an unanswered question, because trade openness is correlated with institutions (see Rodríguez and Rodrik, 2001). On virtues and limits of Sachs-Warner index, see also the more optimistic view of Wacziarg and Welch (2008).

The remainder of the paper is organised as follows. Section (2) summarizes recent theory and discuss the implication for agricultural and food policies. Section (3) explains how constitutional reforms are measured and classified, and gives some preliminary evidences on how agricultural protection maps across different political institutions. Section (4) introduces the econometric strategy and discusses identification issues. In Section (5) the results are presented and discussed, while Section (6) offers a battery of robustness checks. Finally, Section (7) concludes.

2. Main predictions and hypotheses

This paper builds on the recent political economy literature that have formalized how different forms of democracy translate to different policy outcomes. Persson and Tabellini (2004) survey the main literature. Our focus here is on electoral rules and types of government.

2.1 Forms of democracy and policy outcomes

We start with the policy effect of electoral rules. The conceptualization of electoral system typically contrast majoritarian election, with plurality rule and smaller districts, with proportional election, where the seats are attributed in proportion to votes in larger districts. The influence of electoral rules on public policy, can be both direct and indirect.⁴

First, models that focus on the direct effects of electoral rules give clear predictions about the *composition* of government spending (see Lizzeri and Persico, 2001; Persson and Tabellini, 2000; Milesi-Ferretti et al. 2002).⁵ Proportional elections stimulate government programs which benefit large groups in the population (like welfare programs), while majoritarian elections give politicians a greater incentive to implement programs for geographically smaller constituency groups. This is because in proportional elections policies are more likely to reflect party preferences, often reflecting the national perspective and favouring broad forms of redistribution. Differently, in majoritarian

⁴ A related distinction is about ‘pre-election’ and ‘post-election’ politics. Models of ‘pre-election politics’ emphasizes that policy promises made during the electoral campaign have a binding impact on policymaking. Differently, in ‘post-election politics’ electoral promises might not be binding, and the policymaking process depends in part on bargaining among the successful parties. However, as emphasized by Scartascini and Crain (2002), the ‘pre-election’ and the ‘post-election’ effects of electoral rules are not mutually exclusive.

⁵ These papers point to similar results, though they use a slight different frameworks. For example, Lizzeri and Persico (2001) study a model with binding electoral promises, where candidates can use tax revenue to provide either general public goods or targeted redistribution. Differently, Persson and Tabellini (2000) consider a *broad* or *narrow* policy choice by an incumbent policymaker trying to win re-election.

systems individual legislators have more incentives to support the interests of the represented district, thus favouring a more narrow and geographically targeted distribution.

Second, models that studied the indirect effect of electoral rules, namely their effect mediated by the parties structure, give predictions on the *size* of government spending and the *level* of redistribution. In a model of post-election bargaining, Austen-Smith (2000) showed that government coalition of two parties spends and taxes more compared to a single-party government. Persson *et al.* (2007) find a similar indirect effect of electoral rules on overall spending, however they go further by treating the party structure as endogenous.⁶ However, both Iversen and Soskice (2006) and Ticchi and Vindigni (2010), provide an alternative explanation of why proportional systems spend and redistribute more, focusing on the role played by partisan governments. They argue that proportional systems are ruled relatively more by centre-left government coalitions, that are more willing to tax and redistribute income.

A third prediction is that proportional systems cause less protection in trade policy than majoritarian systems (Grossman and Helpman 2005). This is because politicians in a majoritarian system maximize the welfare of their own districts, as opposed to the welfare of the overall country. Thus, the elected legislators will use tariffs to redistribute income to industries linked to their own districts, rather than maximize national welfare through free trade. Differently in a proportional system all regional interests will receive equal support, leading to welfare-maximizing policy of free trade.⁷

Moving to the forms of government, the classical distinction is between presidential and parliamentary systems. Persson *et al.* (1997, 2000) and Persson and Tabellini (2000, ch. 9) explain how differences in the separation of power between the executive and legislative branch of government (agenda setting), as well as the degree of legislative cohesion and party discipline, affect the political incentives in presidential and parliamentary governments.

Without going in the details, main predictions show that these different forms of government will affect the *level* and *composition* of government spending in a way quite

⁶ Kontopoulos and Perotti (1999) and Milesi-Ferretti *et al.* (2002) also find that proportional elections is associated with larger overall spending, rely on 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. Clearly this problem is exacerbated under proportional elections, as this rule makes coalition governments more likely.

⁷ Persson (2005) and Evans (2009) provide empirical support to this hypothesis.

similar to the effect of electoral rules. Thus, there is lower spending and taxation in presidential regimes, than in parliamentary regimes. Moreover, presidential systems are associated with more targeted programs, like local public good, whereas parliamentary systems are associated with broader spending programs, like national public goods.⁸

2.3 Implications for agricultural and food policy

What are the implications of these general predictions for the comparative politics of agricultural policy? Though the predictions are quite general and focused on macroeconomic policies, some hypotheses related to agricultural and food policy can be derived.

A first implication is related to the prediction about the direct effect of constitutional rules on the *composition* of government spending (narrow vs. broad). As stressed by Rausser and Roland (2010), this prediction can take different directions depending on the *nature* of agricultural policy. In a rich country, considering the relative dimension of the farmers and the consumers groups, the former is typically a special interest group while the latter represents the majority of the population. Thus, because in rich countries agricultural policy takes mainly the form of income distribution to special interest, we should observe relatively more support and transfers in presidential and majoritarian systems, than in parliamentary and proportional ones, *ceteris paribus*. By contrast in a developing country the situation is the opposite, with farmers representing the majority of the population, while (urban) consumers act as a special interest group. Thus, agricultural policy takes mainly the form of broad income distribution, and we should observe relatively more support and transfers in parliamentary and proportional systems than in presidential and majoritarian regimes.⁹

Taken together, the above considerations point to a conditional effect of electoral rules and forms of government on agricultural protection, mediated by the level of development.

Yet, if we consider the indirect effect of electoral rules on agricultural protection the implications are somewhat different. In fact, higher party fragmentation under

⁸ Overall, cross-country evidence on fiscal policies gives a substantial confirmation of these general predictions about the effect of forms of democracy (see Persson and Tabellini, 2003), although they are somewhat weak for the forms of government (see Blume *et al.* 2007).

⁹ A further consideration is linked to the probability of finding agricultural voters as pivotal voters. In a developed country this probability is higher under majoritarian than under proportional system, 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.

proportional electoral rule might lead to a higher frequency of parties in government representing rural interests. A consideration that points to more protection and support toward farmers in proportional (vs. majoritarian) electoral systems, irrespective of the level of development. Thus, overall, the effect of electoral rules on agricultural policy is of uncertain sign, as it depends from which of the two effects – direct vs. indirect – will prevail.

A critical aspect that need to be accounted for in the empirical analysis, is related to the extent to which the political reforms effect on agricultural policy moves toward more or less distortions and redistribution. This point is important, as governments in developing countries more often tax and do not support agriculture (see next). Our main source of variation to identify the effect of political institutions on agricultural policy come from political reforms that largely happen in developing countries. Thus, an increase in agricultural protection induced by a political reforms, could means less or more distortions and redistribution, depending on whether the main effect translate (only) toward a reduction in taxation or, differently, toward a switch from taxation to subsidization of the agricultural sector.

3. Data

The sample includes 74 countries, comprising yearly data from 1960 to 2005 and covering many agricultural products (see Table A.1 for countries and years coverage). Overall we worked with an unbalanced panel with more than 25,000 observations. In our estimation we used the full data set, sub-samples based on different commodity aggregations, as well as a cross-sectional sample averaged over the 1990-2004 period.

3.1 Political reforms

In classifying democratic reforms, we followed the literature (see Persson and Tabellini, 2003; Giavazzi and Tabellini, 2005; Persson, 2005). First, we classify countries into democracy or autocracy using the Polity2 index from the Polity IV data set (see Marshall and Jaggers, 2007). The Polity2 index assigns a value ranging from -10 to +10 to each country and year, with higher values associated with better democracies. 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 policy indicator variable is available for at least two years before and after each reform episode. Applying these criteria, the dataset includes 66 political transitions, 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 political regime. 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 $PRES = 0$).¹⁰ Moreover, countries are classified as *majoritarian* if 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 reforms are listed in Table A.2. The vast majority of reforms are where the choice of electoral rules and forms of governments coincide with the move from autocracy to democracy. This are listed in Panel (a). Panel (b) reports the few (eleven) political reforms in permanent democracies. A few countries experienced reversal episodes during political reforms: countries that start as autocracy and then, after a democratization episode, return back to dictatorships. As brief democratization episodes may have different effects than permanent reforms, we run separate regression models using *all* reform episodes and only *permanent reforms*, as in Giavazzi and Tabellini (2005) and Papaioannou and Siourounis, (2008).

3.2 *Dependent variables and stylized facts*

We test our hypotheses using as dependent variables the ‘nominal rate of assistance’ to agriculture (*nra*) measured at the product level. The variable and the data set are from the World Bank *Agdistortions Database* (see Anderson and Valenzuela, 2008). The *nra* is calculated 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-

¹⁰ 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.

market or exchange-rate intervention. Thus the *nra* is like an equivalent tariff measuring total transfer to agricultural products (sector) as a percentage of the undistorted unit values. The *nra* is positive when the product is subsidized, negative when it is taxed, and 0 when net transfers are zero.

A key advantage of using the *nra* is related to the wide range of policies captured by this indicator. The estimates include assistance provided by tariff and non-tariff trade measures applied to agricultural products, plus any domestic price-distorting measures, as well as an adjustment for output-price equivalent of direct interventions on inputs. Moreover, where multiple exchange rates operate, an estimate of the import or export tax equivalents of that distortion are included as well in the calculation. Thus, the *nra* is a more consistent index of overall support or taxation compared with previous measures used in similar exercises.

For some of the estimations we also use the aggregated nominal rate of assistance at the agricultural sector level (*NRA*), measured as the weighted average of the *nra*, using as weights the industry' value shares of each product. Working at both the agricultural and sector commodity level allows us to test whether our results are robust to potential aggregation bias, also because the weight used for aggregation, the undistorted values of production, may be measured with errors. Further, working at product level allows to investigate whether the political reform effect is heterogeneous across groups of commodities. Specifically, we will compare import-competing sectors *vis-à-vis* exportable sectors, as well as four different product aggregation.¹¹

Figures 1 and 2 compare the average nominal rate of assistance (*nra*) across different political regimes and commodities trade status. Figure 1 shows large differences between electoral rules. Proportional democracies have protection levels from 2 to 4 times higher than majoritarian democracies. Interestingly, these differences are increasing over time. Figure 2 shows that parliamentary democracies consistently protect agriculture more than presidential democracies, although the magnitude of these differences decreases over time. Moreover, these differences are particularly strong especially for import-competing products. Table 1, instead, reports the average level of *NRA* for countries that experience (permanent) reforms, averaging their values over 10 years before and after the democratization. Before democratizations, the average level of *NRA* of reforming

¹¹ 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.

countries was negative, and around -10.5% . After the reforms the *NRA* switches to positive, raising to $+5.8\%$. Thus, on average, a democratization episode clearly points to a redistribution toward farmers. Interesting, there is a strong heterogeneity across forms of democracy. In fact, a relevant change in policy orientation from taxation to subsidization is apparent especially for reforms to proportional and presidential democracies. Differently reforms to parliamentary and majoritarian democracies only appear to reduce the level of taxation.

3.3 Control variables

In the empirical specifications we include additional structural controls that are likely to affect the level of agricultural protection, as suggested by several previous studies (Anderson, 1995; Beghin, and Kherallah, 1994; Swinnen *et al.* 2000, 2001; Olper, 2001, 2007). In particular, our basic specification always includes the following covariates: the log of real per capita GDP (*lgdpc*), the share of agricultural employment (*empsh*), the log of agricultural land per capita (*landpc*), the log of total population (*lpop*); and, in regressions run at the product level, we always include the sector value share over total agricultural production (*prodsh*). All these variables are computed with data from FAO, World Bank (WDI), and the *Agdistortions database*, or from national statistics.

To take into account that a change in political regime could be related to the occurrence of conflict (both domestic and international) we also include current and lagged indicators for years of war (*war*), 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).

In cross-sectional regressions, following Persson and Tabellini (2003) we use two different set of variables as instruments for *MAJ* and *PRES*. First, three dummies dating the origin of the current constitution (*con2150*, *con5150*, and *con81*) and the age of democracy (*age*). Second, measures for the cultural influence of the West and Great Britain, proxy by the distance from the equator (*lat01*), the fraction of the country's population whose mother tongue in English (*engfrac*) or a European language (*eurfrac*) and a dummy for UK colonies (*col_uka*). For the majority of the countries in our sample (59), these variables come from the original data set of Persson and Tabellini (2003).

Differently, for the remaining countries (15) we have measured the same variables following strictly their online data appendix.¹²

We also include land Gini inequality (*landGini*) and government ideology (*left-orientation*), to check the robustness of our findings. Land inequality (circa 1980) is based on FAO data, and comes from Deininger and Olinto (2000). Government ideology is based on the World Bank DPI dataset, following Dutt and Mitra (2005) and Olper (2007). Specifically, starting from the DPI dataset we build an ideology index by attributing a score of 1, 2 and 3 to right, centre and left-wing government, respectively. Then the index is averaged over the time span covered by each country, which normally refers to the 1975-2005 period.

4. Econometric approach and identification

The existing literature studying the effect of political institutions on policy outcomes has exploited especially cross-country variation in the data. The well known problem with this method is that the estimated correlation could reflect an omitted variable bias or reverse causation. In theory, a potential solution to this problem is to find good instruments and run two-stage least squares regressions. However, this strategy is problematic in our context because good instruments for regime changes are not easily available (Persson and Tabellini 2008). Most importantly, though, cross-country regressions leave out important information from the time variation in the data, as recently suggested by Wacziarg and Welch (2008). Thus, following recent tendencies in comparative politics literature we estimate the average effect of constitutions on policy outcomes relying primarily on difference-in-difference regressions, and leaving cross-sectional inference as robustness check.

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-differences 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.

¹² See: <http://didattica.unibocconi.it/mypage/index.php?IdUte=48805&idr=4273>.

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*.

In our specific context we are interested in the (possible) heterogeneous effects induced by political reforms in *different* constitutional features. For this reason we implement a difference-in-differences approach using a multiple treatments specification (see Persson, 2005). Thus, our regression model can be expressed as follows

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

where $Y_{i,t}$ denotes our output 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-differences estimate of the reform effects. They are obtained by comparing average protection after political regime transition, minus protection before the 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.

The difference-in-differences estimator (1) addresses many limitations of standard cross-section regressions. For example, it accounts for time-invariant country characteristics such as geography, resource endowments and historical events, as well as for many ‘structural’ determinants of protection, such as comparative advantage and lobby structure, which typically move slowly over time. 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 political reform effect from equation (1) is based on two key assumptions (see Abadie, 2005; Persson and Tabellini, 2008). First, 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 problematic in our context as the protection dynamic between developing and developed countries, has been quite different, at least this was the case since the mid-eighties.¹³

We tackled this potential source of bias by adding several covariates in 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 , interactions between year fixed effects and time invariant continental dummies. The continent-year interaction effects control also for differences in regional protection dynamics.

The second key restriction is that equation (1) does not take into account the (potential) heterogeneity of reform 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 political regime in country i and year t (see Ashenfelter and Card 1985). However, because one of the objectives of our empirical exercise is to understand the potential heterogeneous effect of different constitutional features, as well as of different agricultural sectors, our approach should be quite immune to this potential source of bias.

5. Regression results

Table 2 reports regression results based on equation (1). In these regressions the dependent variable is the *NRA* (total support in agriculture). All the covariates discussed in Section (3) are included in the vector of controls X_{it} . Moreover, as agricultural protection is highly persistent, the reported standard errors are clustered at country level (see Bertrand *et al.* 2004).

Columns 1 and 2 replicate earlier evidence reported in Olper *et al.* (2011), 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 effect suggests that a transition from autocracy to democracy increases agricultural

¹³ Indeed, developing countries progressively dismantle their import-substitution policies, while developed countries start to reform their agricultural policies (see Anderson and Valenzuela, 2008).

protection, on average, by 7.8 percent points, a relevant effect from an economic point of view. Considering only *permanent reforms*, namely those reforms that are not reversed in our sample period, the estimated reform effect increases to 11.7 percent points, suggesting that democracy needs time to display its effect on agricultural policy. The above results highlight that democracy appear to be friendly of the agricultural sector, giving broad confirmation to the findings of Olper *et al.* (2011).

To disentangle the effect of constitutional rules, in the subsequent regressions of Table 4, the relevant variables are obtained by interacting the democracy dummy with the dummies for government systems, *PARL* and *PRES* (columns 3-4) and electoral rules, *PROP* and *MAJ* (columns 5-6). 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 percent points if only permanent reforms are considered. These effects are statistically significantly at the 5% level. Differently, a political reform from autocracy to a parliamentary democracy induces an increase in protection, which is still positive (about 4-5% points) but never significant at conventional statistical level. Thus, it appears that presidential democracies tend to support agriculture more than parliamentary democracies, *ceteris paribus*. However, performing an *F*-test to check their statistical difference, we cannot reject the equality of the two coefficients at the conventional statistical level. 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 seem to matter for agricultural policy.

Regressions in columns 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 it is clear from the magnitude of the estimated coefficients on *PROP* and *MAJ*, differences in electoral rules matter a lot 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, reforms from autocracy to a majoritarian democracy has an effect of about 2.5-5%, never statistically significant. Not surprising, the *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, 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. As the majority of countries experiencing democratic reforms have a negative agricultural protection before the transition, so they tax agriculture, but after the transition they switch to protecting it (see Table 1), then we have a clear indication that certainly a reform to proportional democracy and, to a lesser extent, a reform to presidential democracy increases redistribution toward farmers.

The regressions above are based on average *NRA* obtained by aggregating the level of protection across many agricultural commodities. This can rise potential aggregation bias. Thus, it is important to evaluate whether our results are robust to this issue, working also at the commodities level. This allows us to investigate the existence of possible heterogeneity in the constitutional policy effect across different groups of commodities. Indeed, as shown by Anderson and Valenzuela (2008), there exists a huge variation in sectoral protection levels.

A classical distinction in agriculture is between import-competing (Table 2) and export sectors (Table 3), the former traditionally more protected than the latter (see Krueger, 1990; Baldwin and Robert-Nicoud, 2007).¹⁴ In these additional regressions, the usable observations increase substantially. The specification now includes also country-*product* fixed effects, as well as each product value share on total agricultural production, to control for heterogeneity at the sectoral level.¹⁵

¹⁴ 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.

¹⁵ 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 Table 2. Finally, the war dummy (and its lagged value) is always positive but never significant.

Regressions at the product level broadly confirm the previous evidence, giving also new interesting insights.¹⁶ 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 (see Table 3) but it is only 6.8% points for export sectors (see Table 4). Across forms of government, what matters is again transitions 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. What is interesting from these additional results is the huge difference in the estimated effect between import-competing *vs.* export sectors. For import-competing sectors, a transition to presidential and proportional democracy, increases agricultural protection by about 22-23% points, whereas the effect is never higher than 10% points for export sectors.

Table 5 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, respectively. However, all these effects are precisely estimated only, or especially, in the grains-tubers sector that represent typical staple food crops.

6. Robustness checks and extensions

Our specification is more elaborate than previous similar studies of agricultural protection. Indeed, other than the key determinants found relevant in other studies, we also control for time invariant country-sector heterogeneity, unobserved common shocks, differences in continental trends in protection, and political crises. Furthermore, we performed a series of additional robustness checks adding to our specification measures of contemporaneous and lagged economic crisis (see Giuliano *et al.*, 2010); measures of government policy, like government consumption to GDP and trade openness (based on both trade over GDP

¹⁶ Working on the full sample (instead of splitting the sample in import-competing and export sectors) the estimated constitutional effects are normally higher in magnitude and more precisely estimated than regressions reported in Table 2, suggesting that aggregation bias could be a problem there.

and the Sachs-Warner openness index), as well as other agricultural specific controls, like the net export share. While some of these controls turn out to be significant in some regressions, they never change the quantitative and qualitative nature of the results.¹⁷

6.1 Dynamic panel model

A potential critique on the results is that they are based on a static model, while agricultural protection is highly persistent over time due to inertia and *status quo* bias. 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_{it} exhibit limited time variation, we attribute a large fraction of the dynamics in protection to unobserved common events, and this is particularly true for regressions based on sectoral *nra*.

To allow for such persistency, Table 6 presents a dynamic version of equation (1), 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.¹⁸ 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 reform effects, as now part of the dynamics in protection is captured by the lagged dependent variable. However, all the relevant political 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 increases significantly to 6.2% points on passing to proportional democratic transitions.¹⁹ 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

¹⁷ These additional results are available from the authors upon request.

¹⁸ Running regressions excluding the few countries with less than 20 years observations, as in Persson and Tabellini (2003) and Papaioannou and Siourounis (2008), or using System Generalized Method of Moment estimator, the results are even stronger. These additional results are available from the authors upon request.

¹⁹ Note that, these numbers are 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 are closed to the static models: i.e. the (short-run) democratization effect of 3.8% points reported in column 1 of Table 6, correspond to a long-run effect of 12.2% points, thus just a little higher to the 11.7% points of the static model reported in column 2 of Table 2.

higher sensitivity to institutionally induced policy changes, and the same pattern emerges considering the four sector groups (results not shown).

6.2 Cross-sectional inference and IV regressions

As a final check of our results we now move to cross-sectional regressions. Previous evidence based on difference-in-differences estimator offer more credible inference about the causal effect of political institutions on agricultural policy. However, a potential critique to our findings is related to the consideration that constitutions are endogenous (see Aghion *et al.* 2004; Acemoglu, 2005; Ticchi and Vindigni, 2010). As discussed in Persson and Tabellini (2003), it seems reasonable to assume that the endogeneity issues of electoral rules and forms of government on policy outcomes are mainly due to problems of selection bias. However, we cannot rule out *a priori* that the results are in fact also driven by simultaneity bias. This point has been clearly stated 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 constitutional choice’.

To account for simultaneity problems we move to cross-sectional inference, averaging the level of protection across fifteen years (from 1990 to 2004) and running IV regressions. Instruments for the forms of government indicators (*MAJ* and *PRES*) came from Persson and Tabellini (2003), the only difference been in the first stage regression where we include also all the right hand-side variable of the second stage.²⁰

Table 7, columns 1 and 2, shows the benchmark OLS results across two different samples: a *broad* sample, which considers all the countries with data on our constitutional indicators; 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 4 plus ‘regional’ dummies for African, Asia, Latin American and OECD countries.

The estimated coefficients on *MAJ* and *PRES*, are consistent with our previous findings. A country with a majoritarian electoral rule has, on average, a level of agricultural protection from 26% to 33% lower than a country with a proportional system, a result strongly significantly. On the contrary, different forms of government do not

²⁰ As discussed in Acemoglu (2005), this indeed should be the preferable strategy to account for simultaneity bias.

affect the level of protection significantly, although the effect points in the same direction than before, namely presidential countries tend to protect more agriculture. Thus, there is a remarkable consistency of the form of democracy effect between cross-country and panel regressions.

Columns 3 and 4 present results from IV regressions. Moving to 2SLS the results are even stronger, in the sense that, if any, OLS regressions is likely to be biased down-ward in absolute value. Once again electoral rules affect significantly agricultural protection while forms of government do not, although using IV the magnitude of the estimated effect on *PRES* increases substantially.²¹ The bottom of the table reports the *p*-values for the Sargan's overidentification test of the instruments, and the Wooldridge's robust score test for the exogeneity of *MAJ* and *PRES*. Both tests are insignificant meaning that, on the one hand the suitability of the instruments cannot be rejected and the regressions in columns 3 and 4 are thus consistent. On the other hand, the hypothesis that our forms of government indicators are exogenous to agricultural protection cannot be rejected, suggesting that OLS regression is the correct estimator.

As a final check of our results we consider the potential effect of two further exogenous variables founded relevant in previous studies (Olper, 2007): inequality and government ideology. Indeed, though the overidentification test suggests that our results should be robust to one or more (excluded) exogenous variables, it is also well know that this test might have low power.

Consider first inequality. Countries with a more equal distribution of assets, in particular land for agricultural issues, may be more likely to make transitions to proportional democracy that, as discussed in Section 2, should better preserve an egalitarian distribution of income. 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 La Ferrara, 2002; Olper 2007).

Second, there is evidence that left-wing governments are more likely in a proportional democracy (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

²¹ However, in these cross-country regressions the magnitude of the estimated effect on *PRES* is sensitive to a small change in the classification. More specifically, removing Switzerland from the sample, a potentially outlier with a very high *NRA* coded as presidential country by Persson and Tabellini (2003), the estimated *PRES* coefficient reduce substantially and it is never significant. Note moreover that, using the two steps Heckman selection correction, we reach results similar to IV regressions.

with those of a left-wing government.²² Thus, Columns 5 and 6 of Table 9, include both the land Gini and the left-orientation variables in our baseline OLS specification. The electoral rule effect is totally unaffected by the inclusion of these additional controls. In columns 7 and 8 we test a specification that includes also an interaction effect between land Gini and government ideology, as a left-wing government could have a strong rationale for redistribution in an unequal society, potentially affecting the ideology-protection relationship (see Olper, 2007). The results support the above hypothesis: the interaction term is positive and significant and, at the same time, the (absolute) linear coefficients of both land Gini and the ideology variables increase in magnitude, and are now significant at 5% or 10% 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 our main results are robust to the use of IV regressions and that the electoral rules effect on public policies is not driven by the partisan orientation of governments.

7. Discussion and conclusions

The results presented above point to a robust positive effect of proportional electoral rule on agricultural protection and support, an effect that is particular strong for import-competing sectors and staple food crops. A similar positive effect is detected for presidential countries, although in that case the effect is smaller in magnitude and less robust. The key question is now how can we interpret these results in the light of the political economy predictions summarized in Section 2.

Existing theory on the direct effect of electoral rules on the composition of government spending, when applied to agricultural policy, suggests that proportional democracies (*vis-à-vis* majoritarian ones) should have larger transfers and redistribution toward farmers, when this group represents the majority of the population. According, our difference-in-differences estimates show that reforms to proportional democracy increases protection, on average, by about 17% points, an effect consistent with the idea that

²² The two hypotheses find some support in the data. Specifically, running a Probit regression where the dependent variable is equal to 1 for majoritarian countries (0 otherwise), on the land Gini coefficient and the level of development, we find that inequality, indeed, increases weakly the probability of having a majoritarian electoral system, at least in OECD countries. Similarly, if we regress our indicator of left-wing orientation on the majoritarian dummy, controlling for the level of development, we find that countries with proportional electoral rules have a higher frequency of left-wing government, although this effect is never statistically significant.

proportional systems tend to redistribute toward farmers. This result is consistent with our hypothesis about the electoral rules effect on the *composition* of government spending (broad vs. narrow). Indeed in our context, redistribution towards agriculture has to be considered a *broad* form of redistribution, as countries undergoing democratic transition have an average share of agricultural population higher than 50%. However, at the same time, our results on electoral rules are contrary to the Grossman and Helpman (2005) model who predicts a protectionism bias in majoritarian democracies: for the agricultural sector we find exactly the opposite.

The findings about the effect of forms of government are less clear. Indeed, theory predicts that presidential countries should have lower government spending, 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 not confirmed by cross-sectional regressions. From this point of view, given the structure of our reforming countries, our econometric evidence tends to be contrary to theory.²³

Our analysis also suggests 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, their weight in the government objective function increase.

A final outcome of our results is related to the channel through which electoral rules exert its effect on agricultural policy. A first result suggests that neither inequality, nor government ideology, is at the root of the agricultural protectionism bias of proportional democracies. However, it is important to note that our cross-sectional evidence is consistent with prediction about the *indirect* effect of electoral rules. Thus, on the one hand the difference-in-differences evidence supports the prediction about the *compositions* of government spending. On the other hand, from the cross-sectional inference one can argue that *indirect* political incentive effects appear the most plausible

²³ 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.

interpretation of our findings, *ceteris paribus*. Thus, how can we interpret this apparent puzzle results? In our view there are two possible interpretations.

First, as emphasized by Scartascini and Crain (2001), for ‘pre-election’ and the ‘post-election’ effects of electoral rules, the *direct* effect on the composition of public spending, and the *indirect* effect on the overall level of spending, are not mutually exclusive.

A second interpretation is linked to 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 for the population. This view fits with the idea that agricultural protection persists also because the population at large, and thus government, tends to give high national priority to food security related issues. Future investigations of this idea appear important to better understand the extent to which actual comparative politics models are able to explain patterns of agricultural protection.

References

- Abadie, A. (2005). Semiparametric difference-in-difference estimators. *Review of Economic Studies* 72, 1-19.
- Acemoglu, D. (2005). Constitutions, politics and economics: A review essay on Persson and Tabellini's 'The Economic Effect of Constitutions'. *Journal of Economic Literature*, December 2005, 1025-1048.
- Acemoglu, D., S. Johnson, J. A. Robinson, and P. Yared (2008). Income and democracy, *American Economic Review* 98(3), 808-842.
- Aghion, P., Alesina, A. and Trebbi, F. (2004). Endogenous Political Institutions, *Quarterly Journal of Economics*, 119(2), 565-611.
- Anderson, K. (1995). Lobbying incentives and the pattern of protection in rich and poor countries. *Economic Development and Cultural Change*, 43(2), 401-23.
- Anderson, K. and Valenzuela E. (2008). Estimates of global distortions to agricultural incentives, 1955 to 2007. World Bank, Washington DC, October. www.worldbank.org/agdistortions.
- Ashenfelter, O. and Card, D. (1985). Using the Longitudinal Structure of Earnings to Estimate the Effects of Training Programs. *Review of Economics and Statistics*, 67 (1985), 648-660.
- Austen-Smith, D. (2000). Redistributing income under proportional representation, *Journal of Political Economy*, 108:1235-1269.
- Baldwin, R.E. and Robert-Nicoud, F. (2007). Entry and asymmetric lobbying: Why governments picks losers. CEP Discussion Paper No 791, May 2007.
- Barro, Robert J. (1997). *Determinants of Economic Growth: A Cross-country Empirical Study* (MIT Press, Cambridge, 1997).
- Beck, T., Clarke, G., Groff, A. and Keefer, P. (2001). New tools in comparative political economy: The database of political institutions. *World Bank Economic Review*, 15, 165-176.
- Beghin, J.C. and Kherallah, M. (1994). Political institutions and international patterns of agricultural protection. *Review of Economics and Statistics*, LXXVI, 482-489.
- Bertrand, M., Duflo, E. and Mullainathan, S. (2004). How much should we trust difference-in-difference estimates? *The Quarterly Journal of Economics*, 119, 249-275.

- Blume, L., Muller, J., Voigt, S., & Wolf, C. (2007). The economic effects of constitutions: Replicating and extending Persson and Tabellini. CESifo Working Paper.
- Deininger, K. and Olinto, P., 2000. Asset distribution, inequality, and growth. Policy Research Working Paper no. 2375. The World Bank.
- Doucouliafos, H. and Ulubasoglu, M. (2008). Democracy and growth: A meta-analysis, Working Paper, School of Accounting, Economics and Finance Faculty of Business and Law, Deakin University.
- Dutt, P., Mitra, D. (2005). Political ideology and endogenous trade policy: an empirical investigation. *Review of Economic and Statistics*, 87 (1), 59-72.
- Eichengreen, B. and Leblang, D. (2008). Democracy and globalization, *Economics & Politics*, vol. 20(3), 289-334.
- Evans, Carolyn. (2009). A protectionist bias in majoritarian politics: An empirical investigation. *Economics & Politics*, 21(2), 278-307.
- Giavazzi, F. and Tabellini, G. (2005). Economic and political liberalization. *Journal of Monetary Economics*, 52, 1297-1330.
- Giuliano, P. Mishra, P. and Spilimbergo, A. (2010). Democracy and reforms: Evidence from a new dataset, IMF Working Paper No. 10/173.
- Gleditsch, N.P., Wallensteen, P., Eriksson, M., Sollenberg, M. and Strand, H. (2002). Armed conflict 1946–2001: A new dataset. *Journal of Peace Research* 39(5), 615–637.
- Grosjean, P. and Senik, C. (2011). Democracy, market liberalization and political preferences, *Review of Economic and Statistics*, 93 (1), 365–381.
- Grossman, G., and Helpman, E. (2005). A protectionist bias in majoritarian politics, *Quarterly Journal of Economics*, 120 (4), 1239-1282.
- Iversen, T and D. Soskice (2006). Electoral institutions and the politics of coalitions: Why some democracies redistribute more than others, *American Political Science Review*, 100(2), 165-181.
- Kontopoulos, Y. and Perotti, R. (1999). Government fragmentation and fiscal policy outcomes: Evidence from the OECD countries, in Poterba, J. and von Hagen, J. (eds.) *Fiscal Institutions and Fiscal Preference*. (Chicago: University of Chicago Press).
- Krueger, A. (1990). Asymmetries in policy between exportable and import-competing goods. In Roland Jones and Anne Krueger (Eds), *The Political Economy of International Trade*. (Cambridge MA, Basil Blackwell).

- La Ferrara, E. (2002). Inequality and group participation: theory and evidence from rural Tanzania. *Journal of Public Economics*, 85 (2), 235–273.
- Lizzeri, A. and Persico, N. (2001). The provision of public goods under alternative electoral incentives. *American Economic Review*, 91 (1), 225-239.
- Lundell, K. and Karvonen, L. (2003). A comparative data set on political institutions. Department of Political Science, Occasional Papers Series Nr. 19/2003, Abo Akademi University.
- Marshall, M. G., and Jaggers, K. (2007). *Polity IV Project: Dataset Users' Manual*. (Arlington: Polity IV Project, 2007).
- Milesi-Ferretti, G-M., Perotti, R. and Rostagno, M. (2002). Electoral systems and the composition of public spending. *Quarterly Journal of Economics*, 117, 609-657.
- Milner, H.V. and Kubota, K. (2005). Why the move to free trade? Democracy and trade policy in the developing countries. *International Organization*, 59, 107-143.
- Murtin, F. and Wacziarg R. (2011). The democratic transition, NBER Working Paper nr. 17432.
- Olper, A. (2001). Determinants of agricultural protection: the role of democracy and institutional setting. *Journal of Agricultural Economics*, 52 (2), 75-92.
- Olper, A. (2007). Land inequality, government ideology and agricultural protection. *Food policy*, 32, 67-83.
- Olper, A., Falkowsky, J. and Swinnen, J. (2009). Political reforms and public policies: Evidence from agricultural protection. LICOS Discussion Paper Series 251/2009.
- Papaioannou, E., and Siourounis, G. (2008). Democratization and growth. *The Economic Journal*, 118, 1520-1551.
- Persson, T. (2005). Forms of democracy, policy and economic development. Working Paper, Institute for International Economic Studies, Stockholm University, January, 2005.
- Persson, T. and Tabellini, G. (2000). *Political Economics: Explaining Economic Policy*. (Cambridge, The MIT Press).
- Persson, T. and Tabellini, G. (2003). *The Economic Effect of Constitutions*. (Cambridge, The MIT Press).
- Persson, T. and Tabellini, G. (2004). Constitutions and economic policy. *Journal of Economic Perspectives*, 18(1), 75-98.

- Persson, T. and Tabellini, G. (2006). Democracy and development: The devil in the details. *American Economic Review*, 96, 319-324.
- Persson, T. and Tabellini, G. (2006). Electoral systems and economic policy. In B. Weingast and D. Wittman eds. *Oxford Handbook of Political Economy*, (Oxford University Press).
- Persson, T., and Tabellini, G. (2008). The growth effect of democracy: Is it heterogeneous and how can it be estimated?. in: Helpman, E. (ed.), *Institutions and Economic Performance* (Harvard, Harvard University Press).
- Persson, T., Roland, G. and Tabellini, G. (1997). Separation of power and political accountability. *Quarterly Journal of Economics*, 112, 310-327.
- Persson, T., Roland, G. and Tabellini, G. (2000). Comparative politics and public finance. *Journal of Political Economy*, 108, 1121-1161.
- Persson, T., Roland, G. and Tabellini, G. (2007). Electoral rules and government spending in parliamentary democracies. *Quarterly Journal of Political Science*, 2, 155-188.
- Rausser, G., and, G. Roland (2010). Special interests versus the public interest: The determination of policy, in Kym Anderson (ed.) *Political Economy of Agricultural Price Distortions*, Cambridge and New York: (Cambridge University Press) (forthcoming).
- Rodriguez, F., and Rodrik, D. (2001). Trade policy and economic growth: A skeptic's guide to the cross-national evidence. in *Macroeconomics Annual 2000*, eds. Ben Bernanke and Kenneth S. Rogoff (MIT Press for NBER, Cambridge, MA, 2001) 15: 261–325.
- Rodrik, D. and R. Wacziarg (2005). Do democratic transitions produce bad economic outcomes? *American Economic Review*, 95(2), 50-55.
- Sachs, J. and Warner, A. (1995). Economic reform and the process of global integration. *Brookings Papers on Economic Activity*, 1, 1–95.
- Scartascini, C.G. and Crain, W.M. (2002). The Size and Composition of Government Spending in Multi-Party Systems, Working Paper, April 2002.
- Swinnen, J., de Gorter, H., Rausser, G.C. and Banerjee, A. N. (2000). The political economy of public research investment and commodity policy in agriculture: An empirical study. *Agricultural Economics*, 22, 111-122.
- Swinnen, J.F., Banerjee, A.N. and de Gorter, H. (2001). Economic development, institutional change, and the political economy of agricultural protection: An

econometric study of Belgium since the 19th century. *Agricultural Economics*, 26 (1), 25-43.

Ticchi, D. and Vindigni, A. (2010). Endogenous constitutions. *The Economic Journal*, 120(543), 1-39.

Wacziarg, R. and Welch, K. H. (2008). Trade liberalization and growth: New evidence. *The World Bank Economic Review*, 22(2), 187-231.

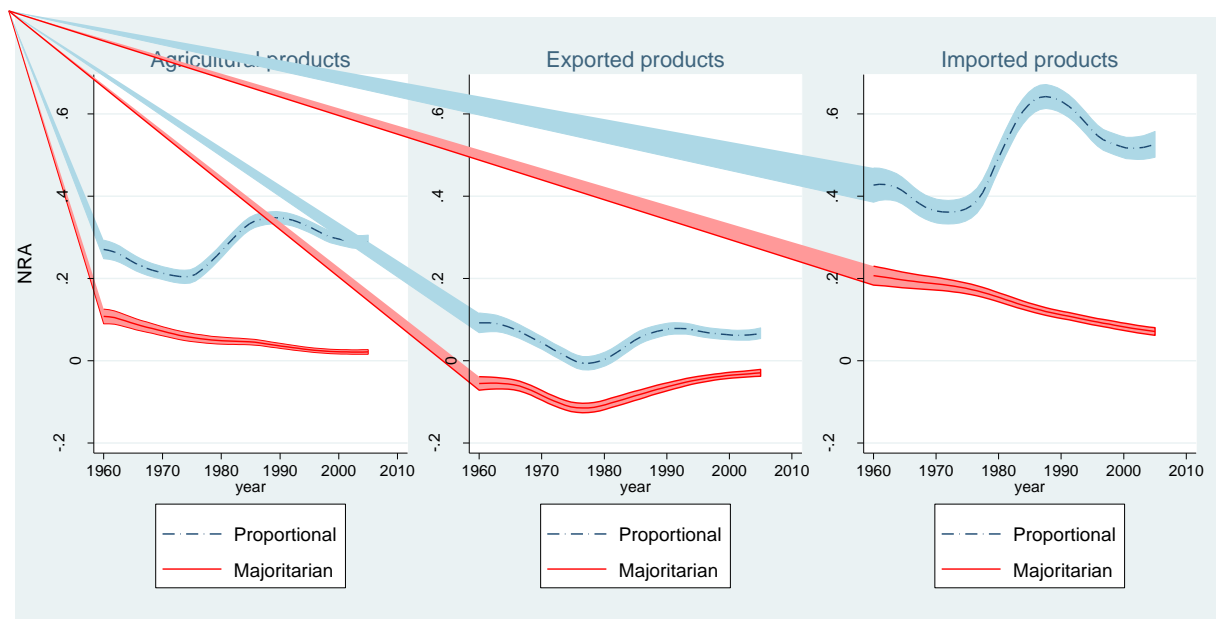


Figure 1. Average nominal rate of assistance to agriculture over electoral systems, 1960 to 2005

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).

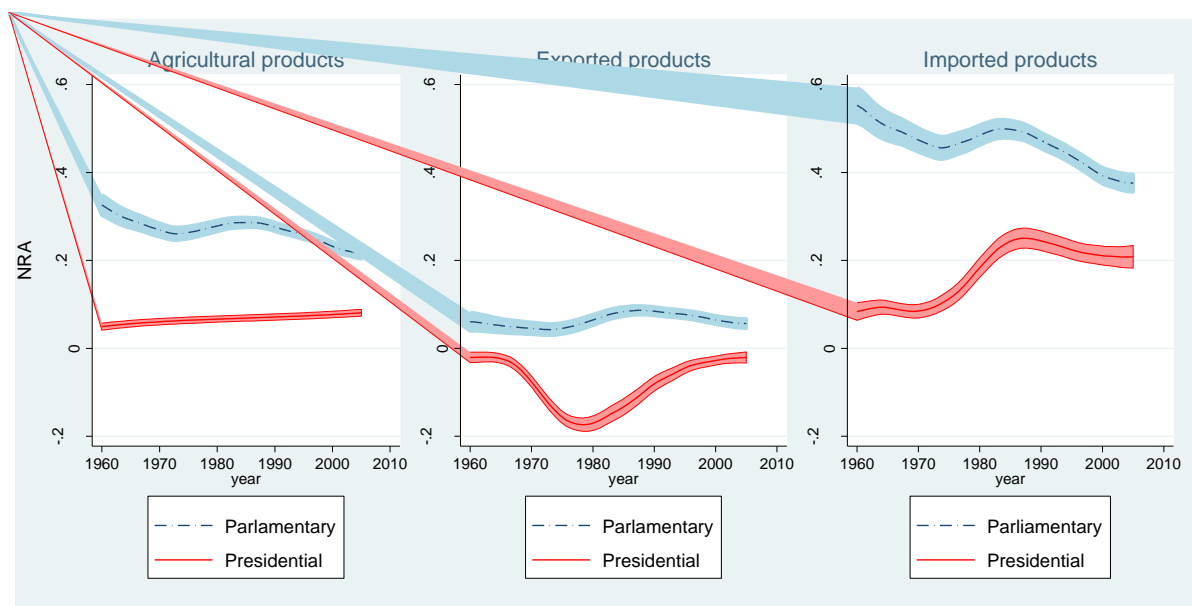


Figure 2. Average nominal rate of assistance to agriculture over government types, 1960 to 2005

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. Average nominal rate of assistance (NRA) before and after political reforms

	Reforms				
	<i>Democracy</i>	<i>PRES</i>	<i>PARL</i>	<i>PROP</i>	<i>MAJ</i>
Avg NRA before reforms (1)	-0.11	-0.10	-0.11	-0.09	-0.13
Avg NRA after reforms (2)	0.06	0.08	-0.01	0.13	-0.06
Difference in means (2) - (1)	0.16	0.19	0.10	0.22	0.08

Notes: the first two lines report the level of *NRA*, averaged over 10 years before and after reforms, for countries experiencing permanent democratization episodes, and across different types of reforms. The last line reports the difference in means effect, calculated as the average *NRA* after the reforms minus the average *NRA* before reforms (see text).

Table 2. Political regimes 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 3. Political regimes and agricultural protection: 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 4. Political regimes and agricultural protection: export sectors

Dependent variable: country-sectors nominal rate of assistance (<i>nra</i>)						
Estimation	Difference-in-difference estimates					
Sectors	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 5. Political regimes and protection across 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 (0.000)			0.087 (0.322)			0.150 (0.106)			0.059 (0.284)		
<i>PARL</i>		0.041 (0.611)			0.256 (0.036)			0.047 (0.628)			0.039 (0.759)	
<i>PRES</i>		0.150 (0.000)			0.004 (0.969)			0.186 (0.138)			0.064 (0.179)	
<i>PROP</i>			0.201 (0.001)			0.106 (0.311)			0.359 (0.091)			0.146 (0.159)
<i>MAJ</i>			0.064 (0.047)			0.041 (0.655)			0.030 (0.648)			0.004 (0.924)
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 6. 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 (0.000)			0.061 (0.001)			0.023 (0.048)		
<i>PARL</i>		0.027 (0.113)			0.052 (0.206)			0.003 (0.860)	
<i>PRES</i>		0.040 (0.000)			0.062 (0.002)			0.031 (0.013)	
<i>PROP</i>			0.062 (0.000)			0.092 (0.000)			0.038 (0.035)
<i>MAJ</i>			0.018 (0.046)			0.028 (0.205)			0.014 (0.241)
<i>Lagged NRA</i>	0.693 (0.000)	0.692 (0.000)	0.692 (0.000)	0.669 (0.000)	0.669 (0.000)	0.668 (0.000)	0.580 (0.000)	0.579 (0.000)	0.579 (0.000)
Wald test									
<i>F-statistic</i>		0.45	7.50		0.06	4.54		2.08	2.05
<i>p-value</i>		0.504	0.006		0.808	0.034		0.150	0.153
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 7. 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)
<i>MAJ</i>	-0.265 (0.005)	-0.332 (0.016)	-0.501 (0.013)	-0.444 (0.036)	-0.268 (0.007)	-0.305 (0.030)	-0.247 (0.009)	-0.258 (0.062)
<i>PRES</i>	0.219 (0.354)	0.374 (0.195)	0.743 (0.213)	0.704 (0.141)	0.253 (0.338)	0.443 (0.175)	0.211 (0.439)	0.388 (0.253)
<i>Land Gini</i>					-0.709 (0.114)	-0.596 (0.305)	-2.916 (0.041)	-3.623 (0.036)
<i>Left-orientation</i>					-0.132 (0.137)	-0.093 (0.506)	-0.672 (0.070)	-0.961 (0.073)
<i>Left * Land Gini</i>							0.926 (0.095)	1.411 (0.063)
Sample	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Method of estimation	OLS	OLS	2SLS	2SLS	OLS	OLS	OLS	OLS
Chi ² over-id (<i>p-value</i>)			0.770	0.580				
Chi ² endogeneity (<i>p-value</i>)			0.176	0.440				
R ²	0.59	0.60	0.42	0.55	0.64	0.64	0.66	0.67
Observations	65	48	65	48	50	39	50	39

Notes: *p*-values based on robust standard errors in parentheses; controls always included in every regression: *lgdpc*, *empsh*, *landpc*, *lpop* plus *Africa*, *Asia*, *Latin American* and *OECD* regional dummies. First stage specification of 2SLS (columns 3-4) includes: all second stage controls plus *con2150*, *con5180*, *con81*, *age*, *engfrac*, *eurfrac*, *lat01* and *col_uka* (see text). Left-orientation is equal to 1, 2 and 3 for countries with right, center and left-wing government ideology, respectively (see text). Samples: *Broad* include all country with data; *Narrow*, considers country with *Polity2*>5. Chi2 Over-id. reports the Sargan's test statistic for overidentifying restriction, with *H*₀ testing whether the instruments are uncorrelated with the error term and that the equation is misspecified. Chi2 endogeneity reports the Wooldridge's score test, with *H*₀ testing whether *MAJ* and *PRES* are exogenous. Figures in **bold (italics)** when the significant level is higher than 95% (90%).

Table A1. Country sample, average *NRA* and agricultural employment share (*empsh*)

#	Country	<i>Years coverage</i>		<i>Average</i>	<i>Average</i>	#	Country	<i>Years coverage</i>		<i>Average</i>	<i>Average</i>
		Start	End	<i>NRA%</i>	<i>empsh</i>			Start	End	<i>NRA</i>	<i>empsh</i>
1	Argentina	1960	2005	-18.5	0.13	38	Malaysia	1960	2005	-4.5	0.38
2	Australia	1955	2005	5.9	0.07	39	Mali	1970	2005	-33.8	0.88
3	Austria	1956	2005	32.2	0.11	40	Mexico	1979	2005	7.6	0.35
4	Bangladesh	1974	2004	-0.01	0.71	41	Morocco	1961	2004	-8.6	0.53
5	Benin	1970	2005	-27.8	0.68	42	Mozambique	1975	2005	-28.9	0.84
6	Brazil	1966	2005	-15.6	0.33	43	Netherlands	1956	2005	78.5	0.06
7	Bulgaria	1992	2005	-10.2	0.23	44	New Zealand	1955	2005	6.6	0.11
8	Burkina Faso	1970	2005	-28.3	0.92	45	Nicaragua	1991	2004	-11.4	0.38
9	Cameroon	1961	2005	-9.1	0.74	46	Nigeria	1961	2004	7.2	0.53
10	Canada	1961	2005	15.8	0.06	47	Norway	1956	2005	242.5	0.09
11	Chad	1970	2005	-26.5	0.86	48	Pakistan	1962	2005	-3.2	0.58
12	Chile	1960	2005	5.7	0.21	49	Philippines	1962	2005	11.7	0.50
13	China	1981	2005	-19.3	0.74	50	Poland	1992	2005	12.3	0.31
14	Colombia	1960	2005	1.4	0.35	51	Portugal	1956	2005	-0.2	0.24
15	Cote d'Ivoire	1961	2005	-31.9	0.64	52	Romania	1992	2005	31.3	0.34
16	Czech Republic	1992	2005	14.1	0.09	53	Rep. of South Africa	1961	2005	9.2	0.20
17	Denmark	1956	2005	52.9	0.08	54	Russia	1992	2005	3.2	0.11
18	Dominican Republic	1955	2005	-10.0	0.34	55	Senegal	1961	2005	-14.2	0.79
19	Ecuador	1970	2003	-6.1	0.40	56	Slovakia	1992	2005	18.6	0.09
20	Egypt	1955	2005	-14.7	0.50	57	Slovenia	1992	2005	66.0	0.03
21	Estonia	1992	2005	7.0	0.12	58	Spain	1955	2005	16.1	0.19
22	Ethiopia	1981	2005	-11.9	0.88	59	Srilanka	1955	2004	-13.6	0.51
23	Finland	1956	2005	74.7	0.13	60	Sudan	1958	2004	-33.3	0.72
24	France	1956	2005	62.2	0.09	61	Sweden	1956	2005	85.3	0.06
25	Germany	1955	2005	70.2	0.07	62	Switzerland	1956	2005	324.7	0.06
26	Ghana	1960	2004	-16.0	0.60	63	Taiwan	1955	2002	53.2	0.24
27	Hungary	1992	2005	16.2	0.19	64	Tanzania	1976	2004	-43.7	0.86
28	India	1960	2005	6.2	0.67	65	Thailand	1978	2004	-6.3	0.69
29	Indonesia	1970	2005	3.0	0.59	66	Togo	1970	2005	-31.3	0.68
30	Ireland	1956	2005	70.0	0.19	67	Turkey	1961	2005	4.1	0.60
31	Italy	1956	2005	44.7	0.13	68	Uganda	1961	2004	-7.9	0.86
32	Japan	1955	2005	110.1	0.13	69	UK	1956	2005	66.1	0.03
33	Kenya	1966	2001	-14.8	0.81	70	Ukraine	1992	2005	-11.4	0.16
34	Korea South	1955	2005	103.5	0.32	71	USA	1955	2005	8.4	0.04
35	Latvia	1992	2005	13.6	0.13	72	Vietnam	1986	2005	-3.1	0.73
36	Lithuania	1992	2005	10.5	0.13	73	Zambia	1964	2005	-40.1	0.76
37	Madagascar	1960	2005	-20.5	0.80	74	Zimbabwe	1970	2005	-48.1	0.71

Notes: numbers in bold refer to the average level of *NRA* and agricultural employment share (*empsh*), in the respective observed period, for countries experiencing political reforms.

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	Parliamentary	Majoritarian
Sudan	1970	Out	Parliamentary	Majoritarian
Sudan	1986	Into	Presidential	Majoritarian
Sudan	1989	Out	Presidential	Majoritarian
Senegal	2000	Into	Presidential	Proportional
Thailand	1974	Into	Parliamentary	Majoritarian
Thailand	1976	Out	Parliamentary	Majoritarian
Thailand	1978	Into	Parliamentary	Majoritarian
Turkey	1971	Out	Parliamentary	Proportional
Turkey	1973	Into	Parliamentary	Proportional
Turkey	1980	Out	Parliamentary	Proportional
Turkey	1983	Into	Parliamentary	Proportional
Taiwan	1992	Into	Parliamentary	Proportional
Tanzania	2000	Into	Presidential	Majoritarian
Uganda	1966	Out	Parliamentary	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

Notes: The table reports reform episodes related to exits and entries in different forms of democracy, based on Polity2 index, Persson and Tabellini (2003), 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). (See text).