Democracy and Agricultural Protection:
Parametric and Semi-parametric Matching Estimates

Alessandro Olper
University of Milano
alessandro.olper@unimi.it

Jan Fałkowski
University of Warsaw
jfalkowski@wne.uw.edu.pl

Jo Swinnen
Catholic University of Leuven
Jo.Swinnen@econ.kuleuven.be

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Jo Swinnen
Catholic University of Leuven
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Abstract

We estimate the effect of democratic transitions on agricultural protection in a sample of 74 developing and developed countries, observed in the 1955-2005 period. We employ both differences-in-differences regressions and semi-parametric matching methods, exploiting the time series and cross-sectional variation in the data. Our semi-parametric matching estimates show that parametric methods might underestimate the *true* effect of democracy on agricultural protection. We find a strong increase in agricultural protection (reduce in taxation) after a country transition to democracy. Specifically a democratic transition increases agricultural protection by about 9 percent points. However, the effect is asymmetric as the effect of leaving democracy on protection is close to zero. The evidence supports the redistributive nature of democratic institutions toward the majority and, therefore, it is not inconsistent with the median voter model of political behaviour.

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[*] Corresponding author.

1. Introduction

In the last decade the idea that political institutions matter for the economic policy making has gained growing emphasis in the economic literature. Institutions, by shaping the rules of the game, determine the context in which key functions of government, like redistribution of income and the provision of public goods, are undertaken (Persson and Tabellini, 2003). However, actual theory offers two contrasting views about the effect of democratic institutions on public policy outcomes. Indeed, moving from voting models to positive theories of public policy we reach a completely different view (see Mulligan et al. 2004).

Models based on the median voter theorem (Downs, 1957) emphasized that in democracies the distribution of political power is more equal than the distribution of income and wealth. As a consequence, voting models predict that democracies tend to redistribute from the rich to the poor, and this will be exacerbated by the level of income inequality as the middle-class have more incentive to form coalition with the poor (see Alesina and Rodrik, 1994; Persson and Tabellini, 1994). Influential political economy models based on this logic are those by Acemoglu and Robinson (2000; 2006) that indeed predict redistribution from the elite to the citizen after an extension of voting rights. A similar mechanisms that can induce different fiscal policy across democracy and autocracy are those proposed by Olson (1993) and McGuire and Olson and (1996). The idea is that in democracies the tax rates are lower because people can voice against it. Differently, the aim of the autocrat is to maximize the tax rate so that he can get highest amount of resource to devote to his private interest. Hence, autocracies in contraposition to democracies tend to tax more and spend less for general public goods.[1]

A completely different view can be found in the positive theory of public policy (e.g. Wittman, 1989) as well as in the 'Chicago school' of political economy (Stigler, 1971; Peltzman, 1976; Becker, 1983). Here the voting process represents only one, and often not the most important, mechanism that affects public policy. Indeed, what matters as a key determinant of policy is efficiency. Political factors such as elections and voting rules are presumed either to be unimportant or just reactions to efficiency considerations. These

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[1] Other models that emphasize a central role for political institutions in the economic policy-making are those developed in the recent comparative politics literature (see Persson and Tabellini, 2000). However, as their focus is on differences in the *forms* of democracy, they are less useful to understand differences in government policies between democracies and autocracies.

approaches stress that the key determinants of the policy-making are economic and demographic factors, such as interest group structure, urban location and the technology of tax collection, through their effects on both the public interest and the effectiveness of interest groups. Thus, when those factors are controlled for, differences in political regimes, if any, have only second order effects on policy outcomes (Mulligan et al. 2004).

Economic and policy consequences of democratic/autocratic institutions have recently received much attention in the literature. It is now well known that linkages between democracy and growth, when studied across-countries, are ambiguous and inconclusive (see Barro, 1997; Glaeser et al. 2004). However, a growing literature exploiting the within country variation in the data and difference-in-differences methodology (Papaioannou and Siourounis, 2008; Rodrik and Wacziarg, 2005; Giavazzi and Tabellini, 2005; Persson and Tabellini, 2006) also in combination with semi-parametric methods (Persson and Tabellini, 2007), shows that the effect of democracy on growth tends to be positive and large in magnitude[2]. Other important contributions to the strand of literature dealing with the impact of political regime on public policies include, among others, Besley et al. (2007), Acemoglu (2005), Banerji and Ghanem (1997) and Besley and Kudamatsu (2006). The former two papers, provide some theoretical considerations regarding public goods provision. The latter two, on the other hand, represent examples of a rich empirical literature studying the effect of democracy on public policy outcomes. In this context, Banerji and Ghanem (1997) show cross-country evidence supporting the view that authoritarian regimes are associated with higher trade protectionism and greater labor market distortions. Belsey and Kudamatsu (2006) on the other hand, using panel data find that health policy interventions are superior in democracies.

When dealing with the influence of political regimes on policy outcomes, it is important to note a close relationship between the emergence of democratic institutions, the structure of the economy, and the role of agricultural sector in particular. For example, Acemoglu and Robinson (2006) emphasize that democracy is more likely when the elites are industrialists rather than landowners.[3] Comparing this view with the one that perceives democracy as a concession from authoritarian ruler to raise taxation (Acemoglu and

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[2] On the positive effect of democracy and growth, see also the recent contribution of Aghion et al. (2007), that stresses how political rights induce positive growth, especially in more advanced sectors. Differently, on the long-run interplay between income and democracy see the contribution of Acemoglu et al. (2008) and of Gundlach and Paldam (2008).

[3] Already Moore (1966) and Dahl (1971) pointed that democracy was not feasible in agrarian societies and could emerge only where agricultural land commercialised and was no longer of feudal character.

Robinson, 2006) implies that transition to democracy may result in significant shift in agricultural policies.

The purpose of this paper is to explore these issues further by reconsidering the link between democracy and trade policies using a rich dataset on agricultural policy distortions recently developed by the World Bank (see Anderson and Valenzuela, 2008). The data cover a sample of 74 developing and developed countries in the period from 1955 to 2005. By exploiting the panel dimensions of the dataset we investigate whether the effect of regime changes – transitions from autocracy to democracy and *vice-versa* – systematically affect the taxation/subsidization patterns in agriculture protection. The key idea behind our empirical approach is to exploit both the cross-country and time series variation in the data (see Giavazzi and Tabellini, 2005). Specifically, we study the effect of democratic reforms using a difference-in-difference technique, as well as by combining it with propensity score matching methods along the line first introduced by Persson and Tabellini (2007).

Several recent papers have investigated how democracy affect economic (trade) liberalization at aggregated level. For example, de Haan and Sturm (2003) working on a developing country sample, show that greater political freedom furthers economic freedom. Similarly, Milner and Kubota (2005) show that regime change towards democracy is associated with more trade liberalization in developing countries. More general evidence on the relationship between regime changes and economic (trade) liberalization can be found in Giavazzi and Tabellini (2005) and Persson (2005). The former, using difference-in-difference estimation, show that economic liberalization tend to precede political liberalization, and not *vice-versa*. The latter, using a similar estimation strategy, shows that what matters is not the dichotomy between democracy and autocracy, but the *form* of democracy. The majority of this literature focused on how democracy affects policy (and *vice versa*) at the aggregated level. Differently, our focus here is at the industry level. This difference in focus could be important as one of the key transformation with economic development is through structural change. Thus, because democracy and development go hand in hand, focusing (only) at aggregate level could mask several potential sources of variation in the economic and political forces.

In the context of agricultural policy outcomes, all previous evidence but one, exploit especially the cross-country variation in the data showing mixed and often weak evidence on the effect of democracy on agricultural protection (see Beghin and Kherallah, 1994;

Swinnen et al. 2000; Olper, 2001)[4]. Differently, Swinnen et al. (2001), exploiting the within-country variation show that only those democratic reforms that determine a significant shift in the political balance towards agricultural interests – e.g. the extension of voting rights to small farmers in Belgium in the early 20th century – induced an increase in agricultural protection.[5]

Our approach has a number of attractive features. First, in contrast to cross-country studies the difference-in-difference approach allows us to overcome several strong identifying assumption typically made by previous literature investigating the relationship between democracy and public policy (e.g. Mulligan et al. 2004). Second, as the more recent micro-econometric literature have questioned the results obtained from the standard difference-in-difference procedure (see, e.g., Abadie, 2005), by combining it with propensity score matching we are able to investigate the causal effect of democracy on public policy, an issue rarely covered by existing empirical studies. Third, our endogenous variable, *agricultural distortions*, represent a comprehensive measure of *all* the distortions that affect the agricultural industry. From this point of view, it represents an improvement with respect to similar papers that have used as endogenous variable openness index (e.g. Giavazzi and Tabellini, 2005) largely criticized in the literature (see Rodriquez and Rodrik, 2001). The last point is important, as the more recent evidence on the interplay between democracy and trade policy have emphasized the sensitivity of the results on how openness is measured (see Tavares, 2007).

The evidence reported in this paper strongly suggests that democratization matters for the orientation of agricultural policies. Controlling for both observed and unobserved source of heterogeneity we find a robust positive effect of transition into democracy on agricultural protection. Our results show that standard difference-in-difference approach might underestimate the *true* effect of a democratic reform. In particular, the average positive effect of a transition to democracy on agricultural protection is about 9 percent points when semi-parametric methods are used, but it drops to about 5 percent points when standard difference-in-difference regression is applied. Moreover, the relationship is

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[4] Important precursors of this kind of analyses can be found in the works of Bates (1983; 1989) on agrarian development in African countries. Moreover, the relationship between democracy and agricultural protection was first highlighted by Lindert (1991), who in a cross-country analysis found a positive relationship when democracy was associated with rapid agricultural decline. Other relevant institutional dimensions like ideology, land inequality and electoral rules are investigated by Olper and Raimondi (2004), Olper (2007), Thies and Porche (2007) and Henning (2008). However, given our focus on democratic reforms, we do not further discuss these interesting lines of research.
[5] This result is important as it highlights the importance of drawing inferences from *regime changes* to more carefully capture the effect of democratization on protection (Swinnen, 2008).

asymmetric as the effect of leaving democracy on protection is close to zero. Overall, the results strongly confirm the redistributive nature of democratic institutions toward the majority and, therefore, it is not inconsistent with the median voter model of political behaviour. Moreover, our evidence rises an important question whether the strong taxation/subsidization switch induced on agricultural policy by democratization, could represent one of the channels through which democracy exerts its positive effect on growth.

The reminder of the paper is organized as follows. Section (2) presents and justifies our empirical strategy summarizing the pros and cons of difference-in-difference regressions and the potential advantages of propensity score matching techniques. Section (3) carefully describes and motivates how we measure *reforms* into and out of democracy, and presents the data and the basic empirical specification. In Section (4) the empirical results are presented and discussed. Finally, Section (5) concludes.

2. Methodology

This section presents our empirical strategy finalized to estimate the causal effect of democracy on agricultural protection. As summarized before, the existing literature studying the effect of political institutions on agricultural policy has exploited especially the cross-country variation in the data. The well know problem with this approach 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 last square regressions. However, this strategy is problematic in our context because good instruments for regime changes are not easily available (see Persson and Tabellini, 2007). Most importantly, though, cross-country regressions leaves out important information from the time variation in the data (see Wacziarg and Welch, 2008). Indeed, as it is clear from figure 1 that maps the dynamics of agricultural protection averaged across democracy and autocracy, there exists considerable variation over time in our sample.

A second possibility is to estimate panel regressions. Here the advantage is to exploit also the time dimension in the data. However, as stressed by Giavazzi and Tabellini (2005) and Persson and Tabellini (2007) traditional panel data approach relies on too restrictive and untestable identifying assumptions when studying the effect of regime changes on economic or policy outcomes, taking the form of exclusion restrictions (see below). Thus, following recent tendencies in the comparative political economy literature we estimate the average effect of democracy on policy outcomes relying on micro-econometric

approach, using difference-in-difference regressions also in combination with semi-parametric matching methods.

2.1 Differences-in-differences regressions

Following Giavazzi and Tabellini (2005) we define regime transitions or democratic reforms as a 'treatment' experienced by some countries but not others. Then we estimate the effect of the treatment through a difference-in-difference regression. In this way we are able to exploit both the time series and cross-sectional variation in the data. Indeed, in our sample we include both countries that experience a regime transitions in the observed period, called the *treated*, and the countries that do not experience any reforms, called the *controls*. In the regression we compare agricultural protection in the treated countries, before and after the treatment, with the agricultural protection of the control group of countries that do not experience any reforms in the period of analysis. In this way we exploit both the within country variation and the comparison between groups (treated and control) of countries.

More formally, this means running panel regressions with the following specification:

$$Y_{i,t} = \beta D_{i,t} + \rho \mathbf{X}_{i,t} + \alpha_i + \theta_t + \varepsilon_{i,t} \qquad (1)$$

where $Y_{i,t}$ denotes our measure of interest, namely agricultural protection, α_i and θ_t are respectively the country and year fixed effects, $\mathbf{X}_{i,t}$ is a set of control variables, and $D_{i,t}$ is a dummy variable taking the value 1 under democracy and 0 otherwise. The parameter β is the difference-in-difference estimate of the reform effect. It is obtained by comparing average protection after democratic transition, minus protection before the transition in the treated countries, to the change in protection in the control countries over the same period (Persson and Tabellini, 2007). Here the control countries are those that do not experience a transition into or out of democracy, thus those that have either $D_{i,t} = 1$ or $D_{i,t} = 0$ over the entire sample period. We use regression (1) to estimate the average effect of democratization on agricultural protection.

As stressed by Persson (2005), one problem with the interpretation of the specification (1) is the correct econometric identification. Specifically, the coefficient β identifies the causal effect of democratic transitions, only if countries in the various reform groups do not have trends in Y which are different from those in the control group but unrelated to reforms. As we will show later, the frequency of transitions into democracy (autocracy) change quite a lot across continents. Thus, to avoid confounding such non-random

incidence with continents-specific trends in agricultural protection, we ensure that the estimates of β are robust to the inclusion of a set of continent-time interaction effects.

A final econometric problem arises when the dependent variable displays a strong positive autocorrelation. In that circumstance, Bertrand et al. (2004) show that the estimated standard errors with difference-in-difference approach are strongly underestimated. To overcome this issue, we follow the most conservative method of estimating standard error by clustering at the country level, allowing arbitrary country-specific serial correlation. Moreover, we also estimate regressions with added the lagged dependent variable. This transforms specifications (1) into a dynamic panel model where the lagged dependent variable allows for the strong persistence in agricultural protection.

2.2 Propensity score matching

As noted in the literature, estimates obtained from the standard difference-in-difference procedure are based on two main restrictive assumptions (see, e.g., Abadie, 2005; Persson and Tabellini, 2007). First, it is assumed that in the control and treated countries, absent any regime change, the average growth in protection in the treated countries should be the same as in control countries. This is obviously a strong assumption.[6]

The second restriction is related to (potential) heterogeneity in the democracy effect on agricultural protection. In that case the unexplained component of protection, $\varepsilon_{i,t}$, also includes the term $(\beta_{i,t} - \beta)D_{i,t}$, where $\beta_{i,t}$ is the country-specific effect of democracy in country i and year t. This occurs any time a reform into democracy is, among other relevant factors, due to the protection effect of the democratic transition itself, namely when $D_{i,t} = 1$ is more likely when $\beta_{i,t} > \beta$ (see, e.g., Ashenfelter, 1978; Ashenfelter and Card, 1985).

To circumvent this issue the existing literature often interacts the democracy dummy with other specific characteristics of the reforms, such as the specific electoral rules or forms of government implemented by the new democracy (see Persson, 2005; Olper and Raimondi, 2009). However, as suggested by Persson and Tabellini (2007), the problem here is that the potential interactions or non-linearity are too numerous, relative to the few democratic transitions. In these circumstances, semi-parametric methods could provide an elegant solution to these problems.

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[6] This restriction is obviously partially tackled by adding several covariates in the vector $X_{i,t}$, with the aim to increase the 'similarity' between treated and control countries.

Accordingly, to have better insights on the effect of political regime transition on the level of agricultural protection we combine difference-in-differences with propensity score matching method. As such we follow the approach discussed by Smith and Todd (2005) and Abadie (2005) and applied by Blundell et al. (2004) and Persson and Tabellini (2007). The focus by the propensity score matching is to employ methods that would allow to rule out the impact of unobservable factors and to relax linearity (Rosenbaum and Rubin, 1983; Persson and Tabellini, 2003). Otherwise the effect of political regime transition may be easily confounded with that of the factors determining this shift. Since one does not observe what would have happened if the democratic country had remained in autocracy (or the converse), an estimate of the counterfactual is constructed. Conditional on number of observable characteristics the probability of regime change is calculated for each country, the propensity score. Based on this estimate, the next step involves evaluating the difference in the evolution of agricultural protection between the countries with and without a regime change. Since matching relies on comparing countries with similar values of propensity score the inferences are not distorted by counterfactuals very different from the treated observations.

More formally this approach could be presented as follows. Denote by $D = \{0, 1\}$ the treatment indicator, equal to 1 for treated countries, i.e. those that made transition to democracy (autocracy); and equal to 0 for control countries, i.e. those that remained autocracies (democracies) throughout the period for which data on agricultural protection are available. Let $Y_{i,t}^D$ represents the level of agricultural protection in country i in time t and democratic state D. Let $t = 0$ corresponds to the period before the change in political regime, and $t = 1$ corresponds to the period after transition. Finally denote by $\mathbf{X}_{i,t}$ a set of observable characteristics. Following Heckman et al. (1997), Abadie (2005) and Smith and Todd (2005) our outcome of interest could be represented as follows:

$$E\left(Y_{i,1}^0 - Y_{i,0}^0 \middle| \mathbf{X}_i, D_{i,1} = 1\right) = E\left(Y_{i,1}^0 - Y_{i,0}^0 \middle| \mathbf{X}_i, D_{i,1} = 0\right). \tag{2}$$

The left-hand side is the *unobserved* average change in agricultural protection in control countries had they change their political regime. The right-hand side is the actual change in agricultural protection in those countries. This formula represents the so-called 'conditional mean independence', which states that, conditional on the vector $\mathbf{X}$, the outcomes are independent of the selection process. In other words, if countries from the control group ($D = 0$) were treated, their outcome in terms of change in agricultural

protection, once conditioned on **X**, would not differ from the expected value of outcomes in the treated group.

Rosenbaum and Rubin (1983) showed that instead of conditioning on the **X** vector one can condition on propensity score $P(\mathbf{X})$ which is the probability of selection conditioned on **X**,

$$p(\mathbf{X}_i) = \text{Prob } (D_{i,1} = 1 | \mathbf{X}_i).$$

Provided that the probability of treatment is strictly greater than 0 and less than 1, that is, $0 < \text{Prob}(D_{i,1} = 1 | \mathbf{X}_i) < 1$, each treated observation have the potential of an analogue in the control group. This assures that the impact of treatment is only valid for observations not violating the common support assumption. In these circumstances our outcome of interest becomes:

$$E\left(Y_{i,1}^0 - Y_{i,0}^0 \big| p(\mathbf{X}_i), D_{i,1} = 1\right) = E\left(Y_{i,1}^0 - Y_{i,0}^0 \big| p(\mathbf{X}_i), D_{i,1} = 0\right). \tag{3}$$

The set of covariates **X** is chosen to reduce the bias attributable to unobserved factors and thus is crucial for the quality of matching (see Becker and Ichino, 2002). Basically, reducing the bias should be accomplished by using diversity of the conditioning variables. However, in macroeconomic setting, i.e. in our context, where the sample is relatively small dropping too many observations due to violation of the so-called common support assumption (when the treatment is predicted too well) would not be desirable.

In this context, following Persson and Tabellini (2007), we decided to use limited number of covariates that are likely to influence both regime change and level of agricultural protection. The same variables are used when estimating the effect of democratic transitions as well as when estimating the effect of autocratic transitions (more on this below).

We use several control countries, to act as the matches for a treated country. The idea is to calculate the average propensity score from a neighbourhood of propensity scores of several control countries, match this average propensity score to the propensity score of a treated country, and then obtain the *average treatment effect*. Two matching estimators were used, namely Epanechnikov kernel and Gaussian kernel (Fan, 1992; Heckman et al., 1997; 1998). The average estimated effect of regime transitions that we compute could be presented as follows:

$$ATT = \frac{1}{I}\sum_i \left(a_i - \sum_j w_{ij} a_i^j\right) \tag{4}$$

where *I* stands for number of treated observations within the common support; a_i is the difference between average level of agricultural protection after and before the transition in the treated country *i*; a_i^j is the difference between average level of agricultural protection in the control country *j* over the periods before and after the transition date in the treated country it is matched with; and w_{ij} ($w_{ij} > 0$ and $\sum_j w_{ij} = 1$) are weights based on the propensity score and depend on the matching estimator (Sianesi, 2001).

We allow the effect of democratic transition to differ from the effect of autocratic transition and therefore we treat transitions from autocracy to democracy separately from transitions from democracy to autocracy. Countries that experience transitions in both directions are used in both these estimations however with different time coverage. For instance, Philippines enter the autocratic transition sample for the period 1962-1985 (with the shift occurring in 1972) and the democratic transition sample for the period 1972-2005 (with the shift occurring in 1986).

3. Data, sample and basic specification

The sample refers to 74 countries, comprising yearly data from 1955 to 2005 (see Table A.1). Not every country fulfils the whole time period, and the average number of years of observation per country is 35. Overall we worked with an unbalanced panel with more than 2,500 observations.

3.1 Democratic reforms and their determinants

In classifying reform episodes, the most recent literature is followed (see Giavazzi and Tabellini, 2005; Persson, 2005; Persson and Tabellini, 2007; Papaioannou and Siourounis, 2008). Recent studies investigating the effect of democracy largely rely on the composite Polity2 index from Polity IV database (Marshall and Jaeggers, 2007).

Countries are classified into democracy or autocracy using the Polity2 index. This index assigns a value ranging from -10 to +10 to each country and year, with higher values associated with better democracies on the basis of several institutional characteristics like the openness of elections, or constraints on the executive. We code a country as democratic in each year that the Polity2 index is strictly positive, setting a binary indicator called *democracy* = 1 (0 otherwise). A reform into (or out of) democracy occurs in a country-year when this democracy indicator switches from 0 to 1 (and *vice versa*). While in theory other democracy database, like the Freedom House data, could be used, we follow the common practise of using the Polity2 index for both comparability

and practical reasons. Indeed, it is the only data source that allows to maximise the number of usable political reforms in our dataset. For example, using the Freedom House data, quite apart from its shortcomings due to classification bias (see Papaioannou and Siourounis, 2008), strongly limits the number of usable transitions because the information start only in 1972.[7]

Overall, applying these criteria to the dataset, we reach 67 transitions into or out of democracy, of which 41 are transition into democracy and 27 are into autocracy (see Table A.1). The distribution of these reforms is quite uniform over time (53% before 1985) but not across continents: about 50% of the reforms are in Africa, 28% in Asia, 18% in Latin America.

To start, following Persson and Tabellini (2007), we introduce the criteria that the outcome of interest, agricultural protection, be observed for at least four years before and after each reform episode. Thus, by treating the first and last four year observations as missing values, the effective number of reform episodes used in the empirical analysis will be lower, and around to 40.[8]

In order to implement our matching strategy we need variables determining the shift in political regimes. These are as follows. Variable *initial polity2* takes the value of our democracy index *polity2* at the beginning of the sample. This variable is included to take into account that countries with *polity2* taking values close to zero are more likely to change the regime. To control for the fact that the sample period varies in length across countries and that the length of sample may be (positively) correlated with the probability of changes in the political regime, we include also a variable *length of sample* (measured in years). To capture the level of economic development a variable *relative gdp* is included. It measures each country's per capita income at the beginning of the sample relative to US per capita income in the same year.

Finally, to take into account that change in political regime could be closely related to the occurrence of conflicts (both internal as well as inter-state) we include also a variable *conflict years* which measures the fraction of conflict years over the total period length for

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[7] The other usable data source on democratic transitions with a good overlapping with our sample, is that of Papaioannou and Siourounis (2008), that indeed cover the period from 1960 to 2005. However, as shown by the same authors, the differences between their transition data and those based on the Polity index are minor, and do not affect the regression results and conclusion. For a critical discussion of democracy indices, see Munck and Verkuilen (2002).

[8] It is important to note that by relaxing this criteria to only two years of observable outcomes, and using almost all the reform episodes reported in Table A.1, the regression results are quantitatively and qualitatively the same. The only notable difference is a slight lower magnitude of the democracy effect, as now the sample include several doubtful or partial reform episodes.

which data on agricultural protection are available. These data come from the UCD/PRIO Armed Conflict Dataset Version 4-2008 (see Gleditsch et al. 2002).

3.2 Dependent variable and other covariates

We test our hypotheses using two different dependent variables: the agricultural nominal rate of assistance (*NRA*) and the relative rate of assistance (*RRA*), both from the World Bank *Agdistortions Database* (see Anderson and Valenzuela, 2008 for calculation details). The *NRA* to agriculture is measured as the weighted average of the nominal assistance at the product level, using as a weight the industry's value share of each product. Differently, the *RRA* is calculated as the ratio between the agricultural and non-agricultural *NRA*.[9] One advantage of using also the *RRA*, is that especially in developing countries, one important source of indirect taxation to agriculture comes from protection of manufacturing sectors. Thus, the *RRA* is a more useful indicator in undertaking international comparison over time of the extent to which a country's policy regime has an anti- or pro-agricultural bias (see Anderson and Valenzuela, 2008). Moreover, by comparing the differentiated effect induces by democratic reforms on *NRA* instead on *RRA*, allows us to better understand which kind of liberalization/protectionism effect dominate in driving the results.

Figure 1 (and Table 1), displays the evolution of average levels of the nominal rate of assistance in the full sample (dotted line), and splits the sample across autocracy and democracy. Several interesting patterns emerge. First, autocratic countries have, on average, as well as in each year considered, a *negative* level of agricultural protection. Thus agriculture in these countries is always taxed at an average period rate of −15%. The opposite applies to democratic countries, that are always strongly protected at an average rate of 45%, though at a decreasing rate starting from the mid-eighties. Moreover, part of the reduction in democracy protection in the last twenty years, other than to external constraints like WTO, appears potentially determined by countries that have experienced a transition toward democracy. Indeed, as emerges from an inspection of Figure 1, the yearly average protection level (dotted line) is progressively closer to the level of protection in the democracies sample.

In the empirical specification we also include additional controls that are likely to affect the level of agricultural protection, as suggested by many previous studies (e.g.

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[9] Specifically, *RRA* is calculated as $[(1 - NRA_{ag})/(1 - NRA_{nonag})-1]$, where NRA_{ag} is the nominal assistance to agriculture and NRA_{nonag} is the nominal assistance to non-agricultural sectors.

Anderson, 1995; Beghin, and Kherallah, 2004; Swinnen et al 2000; Olper, 2001). Specifically, our basic difference-in-difference specification always includes the following structural controls: the level of development *gdppc*, measured by the log of real per capita GDP; the share of agricultural employment in total employment, *empsh*; the log of agricultural land per capita, *landpc*; the log of total population, *lpop*; and, finally, given the high persistency of agricultural protection and for reasons discussed above, in some specification we also include the lagged dependent variable. All these variables are computed starting from FAO and World Bank (WDI) sources, or from national statistics.

4. Estimation results

4.1 Differences-in-differences estimates

Table 2 displays the results of specification (1) estimated across different samples. This corresponds to a standard difference-in-differences estimation on yearly data. The specification, except for country and year fixed effects and the covariates defined above, always includes interaction effects between continent and year dummies to control for both differences in regional protection dynamics and the non-stationary nature of the *democracy* dummy. Indeed, as emphasized by Papaioannou and Siourounis, (2008), our *democracy* indicator tend to exhibit a trending behaviour, because when a country experience a (successful) transition to democracy, the probability of a reversion to autocracy is low. Following Persson and Tabellini (2007), regressions of Table 2 experiment with different assumptions about the *treatments* and the *control group*, testing the effect of a democratic transition on different samples. Thanks to this we also test the robustness of the results.[10] To simplify the interpretation of the results, we express *NRA* (and *RRA*) as a percentage. Thus, the magnitude of the estimated coefficient on our democracy dummy measures the average percentage point changes in agricultural protection implied by a transition into (or out of) democracy.

Regression (1) imposes the assumption that the effect on protection of a transition to democracy is the same as the negativity of the effect of a transition to autocracy, thus exploiting the full sample. The coefficient on democracy is positive and significant at 1%

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[10] The sign and significance of other covariates used in these specifications (results not shown) are as follow. First, and not surprisingly, agricultural protection is positively and significantly associated with the level of development (GDP per capita). Moreover, protection is positively related to the log of population, and negatively to both the land per capita and the employment share of agriculture. However, it is important to note that the last variables are insignificant in several specifications, suggesting that in the previous analyses they especially capture the cross-country variation in protection, here subsumed in the fixed effects.

level, meaning that a transition into democracy induces an increase in agricultural protection of about 14 percent points. Thus the effect is not only statistically significant, but also important from an economic point of view.

Regression (2) estimates only the effect of a transition into democracy, removing reforms to autocracy from the sample, and using as control group only permanent autocracies. The democracy coefficient is again positive, but drops somewhat in magnitude and it is now significant at only 10 percent level by using clustered standard errors. In regression (3), by adding also permanent democracies to the control group, the coefficient on the democracy dummy increases and turns out to be significant at 1 percent level. Now the estimate implies that a democratic transition induces a protection growth effect of about 15 percent points. Regression (4) estimates the effect of a transition out of democracy (or into autocracy), using permanent democracies as the control group. Here the democracy coefficient is negative, thus going in an opposite direction with respect to our prior,[11] but it is statistically insignificant. Thus, we have evidence that the reform effect is asymmetric, indeed leaving democracy do not affect the level of protection.

Columns (5) to (9), replicate the same battery of regressions using as dependent variable the *RRA*. In general term, the pattern of the results are quite comparable, but with some notable exceptions. First of all, the magnitude of the positive effect on protection of reforms into democracy is significantly lower with a range from about 5 to 9 percent points. In same cases the democracy coefficient is not statistically significant when clustered standard errors are used. The democracy coefficient is always positive and significant when the treatment measures transitions toward democracy, and the control group also includes the permanent democracies. However, when the control group includes only permanent autocracies, we observe a reduction in the magnitude of the democracy effect that is now significant only using not clustered standard errors. Similarly as before, a transition out of democracy never affects significantly the level of agricultural protection. Finally and interesting, note that the lower magnitude of democracy coefficient on *RRA* with respect to *NRA* regressions, suggests that what matters after a regime change is a genuine agricultural policy reforms, instead of policy reforms into non agricultural industry. Otherwise, we would find exactly the opposite result.

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[11] It should be noted that here we are measuring the *negative* protection effect of a transition away from democracy.

Summarizing, this preliminary evidence suggests that the effect of transition to democracy induces an increase in agricultural protection that vary from about 10-14% using *NRA*, to about 5-9% using *RRA*. Thus, agriculture, that is discriminated and taxed in an autocratic country, after a democratic transition will take advantage of a redistribution process, a result consistent with historical evidence reported in Lindert (1994), Acemoglu and Robinson (2000), and Swinnen et al. (2001). An important implication of these results come by the fact that the average and the median values of the share of agricultural population in countries undergoing democratic transitions is higher than 50%. Thus, the evidence supports the notion that democratic institutions tend to redistribute toward the majority and, therefore, it is consistent with the median voter model of political behavior. Moreover, the fact that the same relation does not hold for transition out of democracy, appears consistent with several stylized facts suggesting that, once implemented, agricultural policies tend to persist for some time, even if changes in (external) conditions made them 'inefficient', or not politically justifiable (see Olper and Swinnen, 2008).

Finally, to check the robustness of our evidence, Table 3 presents results of the dynamic version of the equation (1), estimating autoregressive specifications that control for the persistency in agricultural protection. Note that, although the joint presence of fixed effects and the lagged protection level could yield inconsistent estimates, our large time period (35 years for the average countries) strongly reduces this potential source of bias (see Papaioannou and Siourounis, 2008).[12]

As expected, agricultural protection is highly persistent over time, namely actual protection is an important predictor of future protection. This version of the model shows an expected smaller magnitude of the democracy effect, as now the large part of the dynamics in protection is captured by the lagged depended variable. The increase in protection after a democratization episode is about 4-5% for *NRA* and 3-4% for *RRA*, and their significance levels are very close, and sometime higher, than in the static versions. Once again, we have clear evidence that the reform effect is asymmetric: a transition toward autocracy do not affect the level of protection. Thus, overall, these additional results give a strong confirmation of our conclusions.

4.2 Matching estimates

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[12] Following Giavazzi and Tabellini (2005), to reduce this potential source of bias, we have also run regressions by excluding the few countries with less than 20 annual observations. These additional results are qualitatively and quantitatively closed with those reported in Table 3.

The results presented above already give important insights on the role that change in political regime may have on the level of agricultural protection. However, having in mind caveats with respect to difference-in-difference methods we proceed to results obtained from semi-parametric methods.

The coefficients of the probit models that were used to calculate propensity scores are presented in Table 4. The first two columns give the results for democratic transition sample whereas the two latter ones give the results for the autocratic transition sample. The presented results indicate that the probability of shifting towards democracy (autocracy) increases (decreases) with the level of economic development. In addition, transitions from democracy to autocracy are the more likely the lower the initial value of *polity2* variable. In general the chosen covariates predict shift towards autocracy much better than transition in opposite direction, as suggested by the pseudo R^2 equal to 0.51-0.52 and 0.23-0.24, respectively.

In order to check the goodness of matching we compared the distribution of observed covariates between the countries in the treated group and the control group. The results are shown in Tables 5 and 6. Clearly, matching did well in terms of removing significant differences between treated and control countries especially in autocratic sample. As regards transitions to democracy on the other hand, the treated and control groups already before matching are not so different. Nevertheless, it seems that matching may still be relevant here since it removes the difference in means of dummy for *Africa*, and reduces the differences in means of *relative GDP* and *conflict years*.

The results of matching are displayed in Table 7. Before analysing them it is important to keep in mind the main advantage (and some caveats) of implementing matching over difference-in-difference regressions (Persson and Tabellini, 2007). Basically, with matching we relax linearity, allowing for any heterogeneity in the effect of democracy on protection. However, there is a cost: the estimates are less efficient and, due to the few usable observations in our hand, the loss in precision is a matter of fact. However and interestingly, it seems that matching estimates are fully in line with the results obtained from the standard difference-in-difference method. That is, the effect of transition to democracy on the level of agricultural protection is strongly positive and statistically significant whereas the effect of transition to autocracy does not differ from zero. Moreover, the effect of democracy in *NRA* sample is larger than that observed when using *RRA* sample. What is worth noting is the fact that point estimates obtained by matching are larger than that estimated using difference-in-difference method. To see this, compare

matching results with the difference-in-difference benchmark reported in Table 2 (column 2 for *NRA*, and column 7 for *RRA*). Matching estimates oscillate in the region of 14% and 9% whereas the parametric ones are 10% and 4.8% for *NRA* and *RRA* respectively. This suggests that previous parametric methods could have underestimated the true effect of democracy on the level of agricultural protection.

What might be also noted is that, contrary to difference-in-difference method, using matching and *NRA* sample, point estimates of the impact of transition towards autocracy are positive, although insignificant. This again rises the question whether the former approach does not underestimate the true effect of regime transition. Possible explanation for positive effect of autocratic transition could draw on the so-called 'divide-and-rule' strategy adopted by the autocratic ruler who bribes politically pivotal groups so to ensure he can stay in power (see, e.g., Acemoglu et al., 2004). Important to note here is that, limited by the macrosetting, our matching estimates are based on relatively small sample.

Overall, the matching evidence reinforce our finding of the existence of a positive effect of democratic transitions on agricultural protection. On the other hand, we found no evidence that agricultural distortions are affected by autocratic transitions.

5. Conclusions

Motivated by the recent developments in comparative politics about the effect of political institutions on public policy outcomes, we have investigated how transitions into democracy affect agricultural protection and support. The empirical results highlight the important role played by political regime transitions in affecting agricultural policy distortions. In particular, using panel data analysis and differences-in-differences estimation, alone and combined with semi-parametric matching methods, we documented a significant positive effect of a democratic transition on agricultural protection. More specifically, we showed that transition to democracy increases agricultural protection by roughly 9% when propensity score matching methods were used and by roughly 5% when difference-in-difference approach was employed. These results point to an important conclusion that parametric methods might have underestimated the *true* effect of democratic transitions on the level of agricultural distortions. Interestingly, no effect of transition to autocracy was found regardless of the specification and approach used.

Overall, these results support the notion that democracy does matter in affecting the adoption of structural policies. Furthermore, because in countries undergoing democratic transitions it is probable to find many farmer incomes at the median income level, our

evidence gives support to the idea that a process of democratization is followed by redistribution toward the majority and, therefore, it is not inconsistent with the median voter model of political behavior. Finally, our evidence rises the important question whether the strong taxation/subsidization switch induced on agricultural policy by democratization, could represent one of the channels through which democracy exerts its positive effect on growth.

Several further improvements should be made to better understand the interaction between institutions and agricultural policy distortions. For example, just to cite one potential direction, this paper has focused on the role of democracy in general. Therefore, possible extension could explore the potential importance of various forms of democracy, i.e. importance of various electoral rules or various government structures. Extension into these and other directions could significantly improve our understanding of the interlink between constitutions and public policies

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Figure 1. *Average agricultural protection (NRA) over time and constitutional features*
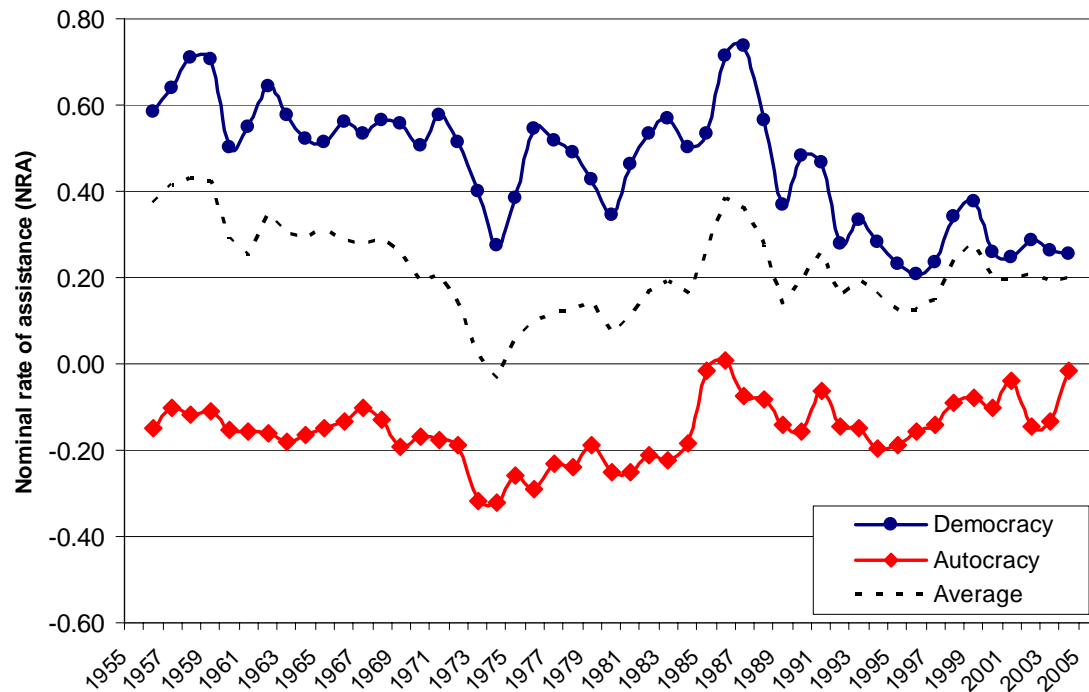
Notes: The figure shows the evolution of the (simple) yearly average of *NRA*, calculated across democratic and autocratic countries. A country in a given year is classified as a democracy if variable Polity2 in the Polity IV data set is greater than zero (see text).

Table 1. *NRA over time and political regimes*

	Full sample	*Autocracy*	*Democracy*
1955-1959	0.41	-0.13	0.66
1960-1964	0.28	-0.16	0.54
1965-1969	0.27	-0.13	0.51
1970-1974	0.10	-0.24	0.46
1975-1979	0.10	-0.23	0.44
1980-1984	0.09	-0.22	0.38
1985-1989	0.29	-0.06	0.59
1990-1994	0.23	-0.14	0.41
1995-1999	0.19	-0.13	0.28
2000-2005	0.20	-0.08	0.26
All years	0.21	-0.15	0.45
N. Countries	74	39	67

Notes: The figures report simple *NRA* average across autocracies and democracies in different sub-periods. The number of countries refers to 'total presences' in each category in 1955-2005, and changes over time due to entry and exit.

Table 2. *Democracy and agricultural protection, difference-in-difference estimates*

	(1)	(2)	(3)	(4)	(6)	(7)	(8)	(9)
Dependent variable	NRA	NRA	NRA	NRA	RRA	RRA	RRA	RRA
Democracy	14.00	10.17	15.73	-2.53	9.51	4.80	9.62	0.71
	(2.09)***	(2.34)***	(2.60)***	(4.95)	(1.99)***	(2.52)*	(2.51)***	(4.41)
	[4.62]***	[5.37]*	[5.66]***	[5.66]	[4.43]**	[5.53]	[5.37]*	[6.38]
Treatment (transition to)	*Democracy and autocracy*	*Democracy*	*Democracy*	*Autocracy*	*Democracy and autocracy*	*Democracy*	*Democracy*	*Autocracy*
Control group (permanent)	*Autocracy and democracy*	*Autocracy*	*Autocracy and democracy*	*Democracy*	*Autocracy and democracy*	*Autocracy*	*Autocracy and democracy*	*Democracy*
Continent-year mummie	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2565	1176	2290	1506	2314	987	2065	1444
Number of countries	74	38	73	51	69	33	68	50
R^2 (within)	0.34	0.59	0.35	0.28	0.35	0.56	0.35	0.31

Notes: Standard errors in parentheses; in bracket robust standard errors clustered by country. All regressions include: Log of GDP per capita, Log of population, agricultural employment share, land per capita, year and country fixed effects, and interaction effects between continents (Africa, Asia, and Latin America) and year dummies. (See text).
***$p < .01$; **$p < .05$; *$p < .10$.

Table 3. *Democracy and agricultural protection, difference-in-difference estimates: dynamic model*

Regression	(1)	(2)	(3)	(4)	(6)	(7)	(8)	(9)
Dependent variable	*NRA*	*NRA*	*NRA*	*NRA*	*RRA*	*RRA*	*RRA*	*RRA*
Democracy	4.70	4.70	5.18	0.875	3.75	3.26	4.02	0.00
	*(1.34)***	*(1.78)***	*(1.64)***	*(3.02)*	*(1.27)***	*(1.79)**	*(1.55)***	*(2.87)*
	*[1.34]***	*[2.11]***	*[1.57]***	*[1.83]*	*[1.34]***	*[2.00]*	*[1.56]***	*(1.76)*
Lagged protection	0.77	0.66	0.77	0.80	0.78	0.70	0.79	0.77
	*(0.01)***	*(0.02)***	*(0.01)***	*(0.02)***	*(0.01)***	*(0.02)***	*(0.01)***	*(0.02)***
	*[0.03]***	*[0.05]***	*[0.03]***	*[0.03]***	*[0.03]***	*[0.06]***	*[0.02]***	*[0.04]***
Treatment (transition to)	*Democracy and autocracy*	*Democracy*	*Democracy*	*Autocracy*	*Democracy and autocracy*	*Democracy*	*Democracy*	*Autocracy*
Control group (permanent)	*Autocracy and democracy*	*Autocracy*	*Autocracy and democracy*	*Democracy*	*Autocracy and democracy*	*Autocracy*	*Autocracy and democracy*	*Democracy*
Continent-year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2529	1158	2258	1488	2278	972	2034	1423
Number of countries	74	38	73	51	69	33	68	50
R^2 (within)	0.73	0.77	0.75	0.74	0.74	0.78	0.75	0.75

Notes: Standard errors in parentheses; in bracket robust standard errors clustered by country. All regressions, other than variables reported in the table, include the following controls: Log of GDP per capita, Log of population, agricultural employment share, land per capita, year and country fixed effects, and interaction effects between continents (Africa, Asia, and Latin America) and year dummies. (See text).
***p < .01; **p < .05; *p < .10.

Table 4. *Estimates of the propensity score*

	Transitions to democracy		Transitions to autocracy	
	RRA	*NRA*	*RRA*	*NRA*
Initial polity2	0.07	0.09	-0.25	-0.25
	(0.77)	(1.03)	(1.99)**	(1.91)*
Relative GDP	25.17	35.77	-17.27	-17.30
	(1.84)*	(1.73)*	(2.28)**	(2.30)**
Sample length	0.01	-0.06	-0.01	-0.01
	(0.13)	(1.75)*	(.65)	(.53)
Conflict years	3.30	0.85	0.18	0.11
	(1.77)*	(0.84)	(0.17)	(0.11)
Constant	-0.38	2.17	2.70	2.61
	(0.36)	(1.72)*	(2.34)**	(2.28)**
Observations	33	38	49	49
Pseudo R^2	0.23	0.24	0.52	0.51

Notes: t-values in parentheses: ****p < .01*; ***p < .05; *p < .10.*

Table 5. *Transitions to democracies: balancing properties*

Variable	Sample	RRA Mean Treated	RRA Mean Control	RRA t-test t-value	RRA t-test p > |t|	NRA Mean Treated	NRA Mean Control	NRA t-test t-value	NRA t-test p > |t|
Relative GDP	Unmatched	0.065	0.026	1.19	0.242	0.062	0.022	1.47	0.150
	Matched	0.031	0.035	-0.58	0.577	0.021	0.023	-0.29	0.776
Initial polity2	Unmatched	-5.538	-5.714	0.13	0.899	-5.464	-6.000	0.46	0.645
	Matched	-4.000	-5.694	0.90	0.395	-5.700	-6.342	0.51	0.619
Sample length	Unmatched	33.23	32.00	0.24	0.813	33.79	36.90	-0.80	0.426
	Matched	35.80	42.29	-0.84	0.423	37.00	37.18	-0.04	0.967
Conflict years	Unmatched	0.246	0.127	1.01	0.322	0.229	0.202	0.26	0.799
	Matched	0.089	0.091	-0.02	0.985	0.176	0.189	-0.12	0.905
Latin America	Unmatched	0.230	0.000	1.40	0.170	0.214	0.000	1.61	0.117
	Matched	0.400	0.000	1.63	0.141	0.100	0.000	1.00	0.331
Asia	Unmatched	0.269	0.285	-0.08	0.933	0.250	0.200	0.31	0.757
	Matched	0.000	0.000	.	.	0.100	0.206	-0.63	0.535
Africa	Unmatched	0.423	0.714	-1.37	0.182	0.464	0.800	-1.87	0.070
	Matched	0.600	1.000	-1.63	0.141	0.800	0.794	0.03	0.974

See text

Table 6. *Transitions to autocracy: balancing properties*

Variable	Sample	RRA				NRA			
		Mean		*t-test*		*Mean*		*t-test*	
		Treated	Control	t-value	p > |t|	Treated	Control	t-value	p > |t|
Relative GDP	Unmatched	0.035	0.366	-3.40	0.001	0.035	0.366	-3.40	0.001
	Matched	0.056	0.056	0.01	0.991	0.056	0.055	0.04	0.969
Initial polity2	Unmatched	5.000	8.675	-5.14	0.000	5.000	8.540	-4.94	0.000
	Matched	5.000	5.661	-0.42	0.684	5.000	5.688	-0.44	0.672
Sample length	Unmatched	25.00	34.87	-1.76	0.085	25.08	35.76	-2.08	0.043
	Matched	27.00	18.13	1.54	0.162	27.00	18.50	1.58	0.153
Conflict years	Unmatched	0.368	0.141	2.40	0.020	0.368	0.140	2.40	0.021
	Matched	0.213	0.1	0.67	0.522	0.213	0.100	0.67	0.521
Latin America	Unmatched	0.083	0.054	0.36	0.720	0.083	0.054	0.36	0.720
	Matched	0.200	0.000	1.00	0.347	0.200	0.000	1.00	0.347
Asia	Unmatched	0.333	0.135	1.55	0.129	0.333	0.135	1.55	0.129
	Matched	0.200	0.240	-0.140	0.893	0.200	0.243	-0.15	0.885
Africa	Unmatched	0.583	0.027	5.82	0.000	0.583	0.027	5.82	0.000
	Matched	0.600	0.006	2.39	0.044	0.600	0.005	2.40	0.043

See text

Table 7. *Matching estimates of the protection effect of becoming a democracy (autocracy)*

	NRA				RRA			
	Going to democracy		Going to autocracy		Going to democracy		Going to autocracy	
	(5)	(6)	(7)	(8)	(1)	(2)	(3)	(4)
Growth in agric. protection	14.63	13.95	3.18	2.9	9.72	9.25	0.17	0.76
Std. Error lower bound	(7.84)*	(7.88)*	(16.67)	(17.49)	(5.67)*	(5.79)	(18.30)	(18.91)
Std. error upper bound	(8.04)*	(8.08)*	(18.77)	(20.45)	(6.05)	(6.48)	(19.92)	(23.21)
Matching estimator	Kernel Epanechnikov	Kernel Gaussian	Kernel Epanechnikov	Kernel Gaussian	Kernel Epanechnikov	Kernel Gaussian	Kernel Epanechnikov	Kernel Gaussian
No. of treated countries	10	10	5	5	5	5	5	5
No. of control countries	10	10	37	37	7	7	37	37
No. of controls with repetitions	79	100	48	185	32	35	48	185

Notes: Standard errors (in parentheses), in the upper raw are estimated assuming independent observations, whereas standard errors in the lower raw are estimated assuming perfect correlations of repeated observations in control countries (see Persson and Tabellini, 2007 for details).
*** $p < .01$; ** $p < .05$; * $p < .10$.

Table A1. *Country sample and democratic (autocratic) reform episodes*

		Coverage		Democratic reforms		Polity2	Protection average	
	Country	Start	End	Into	Out	Average	RRA	NRA
1	Argentina	1960	2005	1973; **1983**	1976	1.0	-30.8	-18.5
2	Australia	1955	2005	Permanent democracy		10.0	-5.1	5.9
3	Austria	1956	2005	Permanent democracy		10.0	27.3	32.2
4	Bangladesh	1974	2004	**1991**		0.4	-21.0	0.0
5	Benin	1970	2005	**1991**		-1.6		-27.8
6	Brazil	1966	2005	**1985**		1.7	-26.6	-15.6
7	Bulgaria	1992	2005	Permanent democracy		-2.2	-6.1	-10.2
8	Burkina Faso	1970	2005	1977	1980	-4.5		-28.3
9	Cameroon	1961	2005	Permanent autocracy		-6.4	-30.8	-9.1
10	Canada	1961	2005	Permanent democracy		10.0	16.9	15.8
11	Chad	1970	2005	Permanent autocracy		-5.3		-26.5
12	Chile	1960	2005	**1989**	1973	2.7	-5.8	5.7
13	China	1981	2005	Permanent autocracy		-7.5	-26.2	-19.3
14	Colombia	1960	2005	Permanent democracy		7.0	-10.7	1.4
15	Cote d'Ivoire	1961	2005	2000	2002	-7.0	-42.5	-31.9
16	Czech Republic	1992	2005	Permanent democracy		-1.6	12.9	14.1
17	Denmark	1956	2005	Permanent democracy		10.0	48.0	52.9
18	Dominican Rep.	1955	2005	**1978**		2.0	-16.9	-10.0
19	Ecuador	1970	2003	1968; **1979**	1970	3.9	-11.4	-6.1
20	Egypt	1955	2005	Permanent autocracy		-6.4	-32.3	-14.7
21	Estonia	1992	2005	Permanent democracy		6.0	17.6	7.0
22	Ethiopia	1981	2005	**1994**		-5.5	-50.0	-11.9
23	Finland	1956	2005	Permanent democracy		10.0	64.5	74.7
24	France	1956	2005	Permanent democracy		7.9	47.3	62.2
25	Germany	1955	2005	Permanent democracy		10.0	61.2	70.2
26	Ghana	1960	2004	1970; 1979; **1996**	1972; 1981	-2.9	-23.6	-16.0
27	Hungary	1992	2005	Permanent democracy		-1.2	14.8	16.2
28	India	1960	2005	Permanent democracy		8.6	-29.4	6.2
29	Indonesia	1970	2005	**1999**		-4.5	-15.5	3.0
30	Ireland	1956	2005	Permanent democracy		10.0	59.7	70.0
31	Italy	1956	2005	Permanent democracy		10.0	41.1	44.7
32	Japan	1955	2005	Permanent democracy		10.0	85.4	110.1
33	Kenya	1966	2001	2002	1966	-3.6	-14.6	-14.8
34	Korea South	1955	2005	**1963**; **1987**	**1972**	0.5	81.6	103.5
35	Latvia	1992	2005	Permanent democracy		8.0	26.8	13.6
36	Lithuania	1992	2005	Permanent democracy		10.0	23.2	10.5
37	Madagascar	1960	2005	**1991**		-0.1	-32.6	-20.5
38	Malaysia	1960	2005	Permanent democracy		5.1	-8.5	-4.5
39	Mali	1970	2005	**1992**		-2.8		-33.8
40	Mexico	1979	2005	**1994**		-1.7	4.2	7.6
41	Morocco	1961	2004	Permanent autocracy		-7.1	-25.4	-8.6
42	Mozambique	1975	2005	**1994**		-2.2	-43.9	-28.9
43	Netherlands	1956	2005	Permanent democracy		10.0	67.0	78.5
44	New Zealand	1955	2005	Permanent democracy		10.0	-9.6	6.6
45	Nicaragua	1991	2004	1990		-1.9	-11.9	-11.4

46	Nigeria	1961	2004	**1979; 1999**	**1966; 1984**	-1.4	26.7	7.2
47	Norway	1956	2005	Permanent democracy		10.0	196.7	242.5
48	Pakistan	1962	2005	1972; **1988**	1969; **1977; 1999**	0.3	-37.9	-3.2
49	Philippines	1962	2005	**1987**	**1972**	2.3	-2.2	11.7
50	Poland	1992	2005	Permanent democracy		-1.8	8.3	12.3
51	Portugal	1956	2005	**1975**		2.4	-1.2	-0.2
52	Romania	1992	2005	Permanent democracy		-2.7	26.8	31.3
53	R. South Africa	1961	2005	Permanent democracy		5.3	6.2	9.2
54	Russia	1992	2005	Permanent democracy		5.4	1.3	3.2
55	Senegal	1961	2005	**2000**		-1.8	-21.3	-14.2
56	Slovakia	1992	2005	Permanent democracy		8.1	8.8	18.6
57	Slovenia	1992	2005	Permanent democracy		10.0	65.3	66.0
58	Spain	1955	2005	**1976**		2.7	13.7	16.1
59	Srilanka	1955	2004	Permanent democracy		6.2	-44.2	-13.6
60	Sudan	1958	2004	**1965**; 1986	1958; **1970**; 1989	-3.6	-32.5	-33.3
61	Sweden	1956	2005	Permanent democracy		10.0	76.4	85.3
62	Switzerland	1956	2005	Permanent democracy		10.0	283.5	324.7
63	Taiwan	1955	2002	**1992**		-2.5	15.8	53.2
64	Tanzania	1976	2004	**2000**		-5.1	-57.1	-43.7
65	Thailand	1978	2004	1974; 1978	1976	1.1	-12.9	-6.3
66	Togo	1970	2005	Permanent autocracy		-5.3		-31.3
67	Turkey	1961	2005	1973; 1983	1971; 1980	6.4	-13.9	4.1
68	Uganda	1961	2004	**1980**	**1966**; **1985**	-3.0	-35.9	-7.9
69	UK	1956	2005	Permanent democracy		10.0	49.0	66.1
70	Ukraine	1992	2005	Permanent democracy		6.3	-13.6	-11.4
71	USA	1955	2005	Permanent democracy		10.0	7.2	8.4
72	Vietnam	1986	2005	Permanent autocracy		-6.4	-8.5	-3.1
73	Zambia	1964	2005	**1991**	**1968**	-2.5	-51.9	-40.1
74	Zimbabwe	1970	2005		**1987**	-1.7	-62.5	-48.1

Notes: The Table reports sample characteristics (columns 1-2); the classification of democratic (autocratic) reform episodes and political regimes (columns 3-4); the average Polity2 index of democracy in the respective observed period (column 5); and finally the overage level of the two protection indices (columns 6-7). Bold numbers in columns 3-4 refer to reform episodes that satisfy the criteria requested and thus are those used in the estimation of the democracy effect. (See text).