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Political Reforms and Public Policies
Evidence from Agricultural Protection

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Political Reforms and Public Policies:

Evidence from Agricultural Protection*

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Abstract

This paper studies the effect of political regime transitions on public policy using a dataset on global agricultural distortions over 50 years (including data from 74 developing and developed countries over the period 1955-2005). We employ both difference-in-differences regressions and semi-parametric matching methods, exploiting the time series and cross-sectional variation in the data. Our semi-parametric estimates show that parametric methods might underestimate the effect of democracy on public policy. In addition, we find that the effect is asymmetric: agricultural protection increases after a country's transition to democracy of about 9% points, but there is no effect when the political regime shifts from democracy to autocracy. Overall, the evidence supports the redistributive nature of democratic institutions toward the majority and, therefore, it is consistent with the median voter model of political behaviour.

JEL codes: D72, F13, O13, P16, Q18

Keywords: Democratic Reforms, Agricultural Distortions, Comparative Political Economics

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1. Introduction

The relationship between democracy and growth has received much attention in the recent literature. Cross-country studies on the impact of democratic institutions on growth yield ambiguous and inconclusive results (Barro, 1997; Glaeser et al. 2004). Even studies exploiting the within country variation in the data still show that transitions towards democracy are not necessarily associated with large improvements in economic outcomes (Rodrik and Wacziarg, 2005; Giavazzi and Tabellini, 2005; Persson and Tabellini, 2006, 2008). Furthermore, the direction of causation is hard to establish (see Acemoglu et al., 2008; Gundlach and Paldam, 2009).

Crucial questions in this debate of course are about the mechanism of how political institutions affect economic growth. In this respect, government policies should play a key role. Political institutions affect (economic) policy making by shaping the rules of the game and determine the context in which key policy decisions are made, such as redistribution of income and the provision of public goods (Persson and Tabellini, 2003). In the literature, two main contrasting views can be distinguished concerning the effect of democracy on public policy outcomes (Mulligan et al. 2004).

The first view, which largely derives from models based on the median voter theorem (Downs, 1957), emphasizes that in democracies the distribution of political power is typically 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 effect will be stronger with higher income inequality as the middle-class has more incentives to form coalitions with the poor (see Alesina and Rodrik, 1994; Persson and Tabellini, 1994). Based on this logic, Acemoglu and Robinson (2000; 2006) predict redistribution from the elite to the citizens after an extension of voting rights. Similar mechanisms that imply a different fiscal policy between a democracy and an

autocracy are proposed by Olson (1993) and McGuire and Olson (1996). They argue that autocracies tend to tax more and spend less for general public goods than democracies. The aim of the autocrat is to maximize the tax rate so that he can get highest amount of resources to devote to his private interest. Hence, in democracies the tax rates are lower because people can voice (vote) against it.

A different view can be found in the 'Chicago school' of political economy (Stigler, 1971; Peltzman, 1976; Becker, 1983) and studies such as Wittman (1989). Here the voting process represents only one, and often not the most important, mechanism that affects public policy. Indeed, what matters for political equilibrium is efficiency of a given group in producing pressure as well as the deadweight cost of taxes and subsidies. Political factors such as elections and voting rules are presumed either to be unimportant or just means to achieve the collective interests of pressure groups. Accordingly, these 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)[1].

A series of empirical studies have tried to test these predictions using data on democracy and policies. For example, Besley and Kudamatsu (2006), using panel data, find that health policy interventions are superior in democracies. Other papers have investigated how democracy affect economic liberalization. De Haan and Sturm (2003), using a developing country sample, show that greater political freedom furthers economic

[1] For similar conclusions based on a more structured model, see also Mulligan and Tsui (2008).

freedom. However, Giavazzi and Tabellini (2005) argue that economic liberalization often precedes political liberalization.

A policy that has attracted substantial interest in this perspective is trade policy. For example, Banerji and Ghanem (1997) show cross-country evidence supporting the view that authoritarian regimes are associated with higher trade protectionism (as well as greater labor market distortions). Giavazzi and Tabellini (2005) and Milner and Kubota (2005), exploiting the within country variation in trade policies, find that regime change towards democracy is associated with more trade liberalization in developing countries.

However, recent papers argue that this effect is not generally true but depends on the specific form of democracy and/or is conditional on the country's resource endowment. For example, both Persson (2005) and Persson and Tabellini (2006) show that while democratization *per se* does not affect structural (trade) policy, transitions toward parliamentary and proportional democracies, does. Differently, O'Rourke and Taylor (2007) find that democratization reduces trade protection, but only in countries where workers stand to gain from free trade (see also Fehrs and Axelrod, 2006; Kono, 2006; Tavares, 2007).[2]

Important criticisms on several of these studies relate to the policy indicators and the methodologies that are used. First, the Sachs and Warner (1995) 'openness index' to measure trade liberalization used by, among others, Giavazzi and Tabellini (2005), Milner and Kubota (2005) and Persson (2005) has been criticized by Rodriguez and Rodrik (2000)[3] who show that it is a poor measure of trade barriers. Second, cross-country studies investigating the relationship between democracy and public policy are typically based on strong identifying assumptions (e.g. Mulligan et al., 2004). Third, the existing empirical

[2] O'Rourke and Taylor (2007) applied the median-voter model in an Hecksher-Ohlin framework (see Dutt and Mitra, 2002) showing that the effect of democracy on protection is conditional to the country's relative resource endowment.
[3] On this point see also the discussion of Wacziarg and Welch (2008).

studies only rarely investigated the causal effect of democracy on public policy (for a discussion see Wacziarg and Welch, 2008; Giavazzi and Tabellini 2005).

The objective of this paper is to address some of these criticisms, specifically by using better policy indicators and improved methodologies to measure the impact of political reforms, i.e. the shift between democracy and autocracy, on government policy making. First, in terms of better policy indicators we make use of a new dataset covering both trade and fiscal policies, which is the dataset on agricultural policy distortions recently developed by the World Bank (see Anderson and Valenzuela, 2008). The dataset includes annual data for 74 countries from 1955 to 2005. Second, we investigate the effect of political regime changes – transitions from autocracy to democracy and *vice-versa* – on taxation and subsidization in agriculture, by exploiting both the cross-country and time series variation in the data. Specifically, we study the democratic reforms effects using a difference-in-differences technique, as well as by combining it with propensity score matching methods as in Persson and Tabellini (2008). Finally, while the majority of the literature focused on how democracy affects taxation or trade policy at an aggregate level, we focus our attention to a specific sectoral (agricultural) policy. This difference in focus could be important since concentrating on aggregated levels may be misleading as different (possibly offsetting) effects may occur at a more disaggregated level. Thus, looking at more disaggregated policies could yield additional insights.

A few previous studies have investigated the impact of democracy on agricultural policy outcomes. All studies but one, exploit the cross-country variation in the data and find 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]. Swinnen et al.

[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.

(2001), using long-run data for a single country exploit the within-country variation and show that only those democratic reforms that caused a significant shift in the political balance towards agricultural interests – in particular the extension of voting rights to small farmers in Belgium in the early 20th century – induced an increase in agricultural protection. Our paper will make use of a much larger dataset and better econometric techniques to test the impact of regime changes on agricultural protection.

The reminder of the paper is organized as follows. Section (2) presents our empirical strategy of difference-in-difference regressions and propensity score matching techniques. Section (3) 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

Several studies in the literature studying the effect of political institutions on policy outcomes have focused on cross-country variation in the data. The well known problem with this approach is that the results 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, as in Persson and Tabellini (2003). However, this strategy is also problematic because good instruments for political regime changes are not easily available[5]. Most importantly, though, cross-country regressions leave out important information from the time variation in the data (Giavazzi and Tabellini, 2005; Wacziarg and Welch, 2008).

Thus, following the recent comparative political economics literature (e.g. Giavazzi and Tabellini, 2005; Persson and Tabellini, 2008) we estimate the causal effect of regime

[5] As explained further in more detail, with the term 'regime change' or 'regime transition' we mean the change from autocracy to democracy or *vice-versa*.

transitions on policy outcomes relying on a micro-econometric approach, using difference-in-difference regressions in combination with semi-parametric matching methods.

2.1 Differences-in-differences regressions

Following Giavazzi and Tabellini (2005) we define regime changes as a 'treatment' experienced by some countries but not others. Then we estimate the effect of the treatment through a difference-in-differences regression. In this way we are able to exploit both the time series and cross-sectional variation in the data. Countries that experience a regime change in the observed period we refer to as *treated* countries, and countries that do not experience regime change we refer to as *control* countries. In the regressions we compare agricultural protection in the treated countries, before and after the treatment, with agricultural protection in the control countries over the same period.

More formally, we run panel regressions with the following specification:

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

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, $X_{i,t}$ 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 regime change effect. It is obtained by comparing average protection after regime change, minus protection before the transition in the treated countries, to the change in protection in the control countries over the same period. Here the control countries are those that do not experience a transition into or out of democracy, i.e. those that have either $D_{i,t} = 1$ or $D_{i,t} = 0$ over the entire sample period.

2.2 Propensity score matching

Estimates obtained from the standard difference-in-differences procedure are based on two main restrictive assumptions (see Abadie, 2005, Persson and Tabellini, 2008). First, it is assumed that, absent any regime change, the average growth in protection in the treated countries should be the same as in control countries.[6] Second it does not take into account the (potential) heterogeneity of regime change effects on agricultural protection. In that case the unexplained component of protection, $\varepsilon_{i,t}$, also includes the term $(\beta_{i,t} - \beta)D_{i,t}$, where $\beta_{i,t}$ is the country-specific effect of regime change in country i and year t.[7]

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

Hence we combine a difference-in-differences methodology with a 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 (2008). Propensity score matching allows to rule out the impact of unobservable factors and to relax linearity assumptions (Rosenbaum and Rubin, 1983). To avoid confounding the effect of political regime transition with that of factors determining this shift, and since one does not observe what would have happened if a democratic country had remained in autocracy (or *vice versa*), an estimate of the counterfactual is constructed. Conditional on

[6] This restriction is partially tackled by adding several covariates in the vector $X_{i,t}$, with the aim to increase the 'similarity' between treated and control countries.
[7] See Ashenfelter (1978) and Ashenfelter and Card (1985) for a general discussion on this.

the number of observable characteristics the probability of regime change is calculated for each country, i.e. 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, denote $D = \{0, 1\}$ as the treatment indicator, equal to 1 for treated countries, i.e. those that experienced a regime transition; and equal to 0 for control countries, i.e. those that had no transition throughout the period for which data on agricultural protection are available.[8] Let $Y_{i,t}^{D}$ represents the level of agricultural protection in country i in time t and democratic state D, where $t = 0$ corresponds to the period before the change in political regime, and $t = 1$ to the period after transition. Finally $X_{i,t}$ represents a set of additional observable characteristics.

Following Heckman et al. (1998), Abadie (2005) and Smith and Todd (2005) our outcome of interest could be represented as follows:

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

The left-hand side is the *unobserved* average change in agricultural protection in control countries had they changed 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 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

[8] Since we do not want to impose a symmetry constraint we treat the effect of transitions to democracy separately from the effect of transitions to autocracy. In the latter case the treatment indicator distinguishes countries that made transition to autocracy whereas the reference group (controls) includes countries that always remained democracies.

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 $P(X)$ which is the probability of selection conditioned on X,

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

Provided that the probability of treatment is strictly greater than 0 and less than 1, that is, $0 < \text{Prob}(D_{i,1} = 1|X_i) < 1$, each treated observation has 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:

$$E\left(Y_{i,1}^0 - Y_{i,0}^0 \middle| p(X_i), D_{i,1} = 1\right) = E\left(Y_{i,1}^0 - Y_{i,0}^0 \middle| p(X_i), D_{i,1} = 0\right). \quad (3)$$

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

We follow the approach of Persson and Tabellini (2008) and use a limited number of covariates that are likely to influence both the regime change and the 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 (see further).

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 estimators (Fan, 1992; Heckman et al., 1998).

The average estimated effect of regime transitions that we compute can be presented as follows:

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

where I stands for number of treated observations within the common support; a_i is the difference between the average level of agricultural protection after and before the transition in the treated country i; a_i^j is the difference between the 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).

3. Data, sample and basic specification

The two methods summarized above are applied to a sample of 74 countries, comprising yearly data from 1955 to 2005 (see Table A.1). 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 Dependent variables

We test the effect of regime transitions on agricultural distortions using two different indicators of government transfers to the agricultural sector as 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* measures the total transfer to agriculture as a percentage of the undistorted unit value. The *NRA* is positive when agriculture is subsidized, negative when it is taxed and 0 when net transfers are zero. The *RRA* is calculated as the ratio between the agricultural and non-agricultural *NRA*.[9] One potential 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 useful indicator for international comparisons of anti- or pro-agricultural policy regimes (see Anderson and Valenzuela, 2008). There are fewer observations with *RRA* as the country and time-series coverage is lower than for *NRA*. Specifically, we lose 5 countries from the dataset (from 74 to 69) and also some regime changes (see Table A1). Therefore in the econometric analyses we will use both indicators.

Figures 1 and 2 and Table 1 display the evolution of average levels of the relative and nominal rates of assistance in the full sample (dotted line), and split the sample in autocracies and democracies as explained below. Several interesting patterns emerge. First, autocratic countries tax agriculture: they have, on average, in each year a *negative* level of agricultural assistance (both relative and nominal), implying taxation. The average across the period is –26% for *RRA* and –15% for *NRA*. Democratic countries subsidize agriculture: the average *RRA* is 31% and *NRA* 45%. However, the gap is decreasing since the mid-eighties, because the average protection in democracies has been reduced and average taxation in autocracies has fallen. Part of this convergence in protection across democracies and autocracies could be due to autocratic countries that have experienced a transition toward democracy. This argument is consistent with the observation that in Figures 1 and 2 the average protection level (dotted line) moves closer to the level of protection in the democracies' sample over time.

[9] Specifically, *RRA* is calculated as $100[(1 + NRA_{ag}/100)/(1 + NRA_{nonag}/100)-1]$, where NRA_{ag} is the nominal assistance to agriculture and NRA_{nonag} is the nominal assistance to non-agricultural sectors.

3.2 Political reforms

In classifying regime transitions and countries as democracies or autocracies, we follow the most recent literature which largely relies on the composite *Polity2* index from the Polity IV database (Marshall and Jaeggers, 2007). 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 (or from 1 to 0). While other democracy databases, like the Freedom House data, could be used, we follow the common practise of using the Polity2 index for both comparability and practical reasons. It has a longer time series and therefore includes more usable political reforms. 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 starts only in 1972.[10]

Overall, applying these criteria to the dataset, we obtain 67 regime changes, of which 42 are transition into democracy and 25 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.

Finally in order to avoid the use of very short reform episodes, we introduce the criteria that the dependent variable is observed for at least four years before and after each

[10] 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).

political regime change. Thus, by treating the first and last four year observations as missing values, the effective number of reform episodes used in the empirical analysis is lower, around 40.[11]

3.3 Other explanatory variables

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. Anderson, 1995; Beghin, and Kherallah, 2004; Swinnen et al 2000; Olper, 2001). Specifically, our basic difference-in-differences 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 below, in some specifications we also include the lagged dependent variable. All these variables are computed from World Bank (WDI), and FAO sources, or from national statistics.

As noted before, for our matching strategy we need variables determining both the level of agricultural protection and the shift in political regimes. Accordingly, the following variables were chosen. Variable *initial polity2* takes the value of our democracy index 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 the sample may be correlated with the probability of changes in the political regime, we include also a variable *length of sample* (measured in years). To capture the

[11] 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.

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 the change in political regime could be related to the occurrence of conflicts (both domestic and international) we include also a variable *conflict years* which measures the share of conflict years over the total period length for 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).

4. Estimation results

4.1 Difference-in-differences estimates

Table 2 displays the results of specification (1) estimated across different samples. This corresponds to a standard difference-in-differences estimation. The specification, in addition to 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[12]. Moreover, as the dependent variable displays a strong positive autocorrelation, we follow the most conservative method of estimating standard errors also by clustering at the country level, allowing arbitrary country-specific serial correlation (see Bertrand et al. 2004).

The regressions of Table 2 experiment with different assumptions about the *treatments* and the *control group* to test the robustness of the results, by testing the effect of a regime transition on different samples. The magnitude of the estimated coefficient on our

[12] Indeed, as emphasized by Papaioannou and Siourounis, (2008), the *democracy* indicator tends to exhibit a trending behaviour which is illustrated in Table A.1 where countries experiencing a (successful) transition to democracy hardly ever switched back into autocracy.

democracy dummy measures the average percentage point changes in agricultural protection implied by a transition into (or out of) democracy. First we present the results based on *NRA* estimations.

Regression (1) exploits the full sample and imposes the assumption that the effect of political regime change on protection is symmetric, i.e. the absolute values of the effect of a transition to democracy is the same as of the effect of a transition to autocracy (the treated countries include transitions to both democracy and autocracy whereas control countries include both permanent democracies as well as permanent autocracies). The coefficient on democracy is positive and significant at 1% level also using the clustered standard errors that, not surprising, they are more then twice as higher as the conventional standard errors. A transition into democracy induces an increase in agricultural protection of about 14 percent points.[13] 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 is somewhat lower in magnitude (10 percentage points), and it is now only barely significant using the more conservative 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.

[13] The sign and significance of other covariates used in this and the following specifications (results not shown) are as follows. 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.

Regression (4) estimates the effect of a transition from democracy to autocracy, using permanent democracies as the control group. As such, contrary to the previous regressions, it estimates the effect of a transition away from democracy. Note that when estimating this effect, we still display the results in the same mode as in columns (1)-(3), namely computed as the negative protection effect of transitions away from democracy. Accordingly, the coefficient of interest points to positive relationship between transition to autocracy and agricultural protection. However, the estimated coefficient is statistically insignificant at conventional levels. This suggests that the reform effect may be asymmetric: the transition from democracy to autocracy does not affect the level of protection significantly.

Columns (5) to (9) replicate the same set of regressions using as dependent variable the *RRA*. The results are similar but the sizes of the effects are smaller. The magnitude of the effect of reforms into democracy on protection is significantly lower with a range from about 5 to 9 percent points.[14] 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. Consistent with the previous results on *NRA*, a transition out of democracy never significantly affects the *RRA*.

To check the robustness of our evidence, in Table 3 we present results of a dynamic version of equation (1), estimating autoregressive specifications that control for the persistence in agricultural protection. Note that, although the joint presence of fixed

[14] Obviously, this lower effect is due to the fact that *RRA* is a relative measure taking into account also the indirect agricultural protection coming from protection in non-agricultural sector. Thus, *RRA* is always lower than *NRA* provided that *NRA* for non-agricultural sector is positive. Note that average *NRA* for non-agricultural sectors, although low, is always positive for all regions and time periods over 1955-2005 (see Table 3 of Anderson et al. 2009).

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.

As expected, agricultural protection is highly persistent over time, implying that actual protection is an important predictor of future protection. As expected, this version of the model shows a smaller magnitude of the democracy effect, as now part of the dynamics in protection is captured by the lagged dependent variable. The increase in protection after a democratization episode is about 4-5% for *NRA* and 3-4% for *RRA*, but their significance levels are close to the static versions. Once again, we find evidence that the reform effect is asymmetric: a transition toward autocracy does not affect the level of protection. Overall, these additional results confirm our previous findings.

4.2 Matching estimates

We now proceed with results from our semi-parametric analysis. In implementing the first step of this approach, the estimation of the probability of treatment (the propensity score), we follow Persson and Tabellini (2008), allowing the effect of democratic transition to differ from the effect of autocratic transition. Therefore we treat transitions from autocracy to democracy separately from transitions from democracy to autocracy[15].

The coefficients of the probit models that were used to calculate propensity scores are presented in Table 4. The first two columns present the results for the democratic transition sample and columns (3) and (4) present the results for the autocratic transition sample. The 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*

[15] Note that we have countries with more than one transition. Thus, 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).

variable. In general the chosen covariates predict the 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 the autocratic sample. As regards transitions to democracy on the other hand, the treated and control groups were not so different already before matching. Nevertheless, matching may still be relevant since it reduces the difference in means for several variables such as the dummy for *Africa*, *relative GDP* and *conflict years*.

The results of matching are displayed in Table 7. Recall that the main advantage of implementing matching over difference-in-difference regressions is that with matching we relax linearity assumptions and allow for any heterogeneity in the effect of democracy on protection. However, this has a cost as the estimates are less efficient and less precise due to fewer usable observations. Keeping this in mind, the matching estimates are fully in line with the results obtained from the standard difference-in-differences 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 the *NRA* sample is larger than that observed when using the *RRA* sample.

Note that the point estimates obtained by matching are larger than those estimated using difference-in-differences method. To see this, compare the matching results with the difference-in-differences evidence reported in Table 2, particularly with the regressions in columns (2) for *NRA*, and (7) for *RRA*, which represent the natural benchmarks in terms of

the used sample.[16] Matching estimates are around 14% and 9% for *NRA* and *RRA*, respectively, whereas the parametric ones are 10% and 5% – in fact the matching estimates are close to those obtained in equation (1) and (3) for *NRA* and (6) and (8) for *RRA* in Table 2. This suggests that the parametric methods may underestimate the true effect of democracy on the level of agricultural protection.

Overall, the matching evidence reinforces our finding of the existence of a positive effect of democratic transitions on agricultural protection and that agricultural distortions are not affected by autocratic transitions.

4.3 Discussion and interpretation

The results reported above indicate a significant positive, and economically important, causal effect of a democratic transition on agricultural protection. An important question is how to interpret this result in the light of political economy theory. In this perspective it is important to point out that, in our sample, the average and the median values of the agricultural employment share in countries undergoing democratic transitions, is higher than 50%[17]. In these countries it is thus probable that average farm incomes are close to the median income level. In such environment, the median voter model of political behavior would predict that a democratization process will be followed by a redistribution toward the majority, which would be consistent with our empirical results.

In this context it is also worth noting that our results importantly complement previous studies which find that democratization reduces trade protection measured at the aggregated level (e.g. Milner and Kubota, 2005; Giavazzi and Tabellini, 2005). Given

[16] Indeed, these regressions estimate the average treatment effect by comparing democratic transitions using as control group permanent autocracy, thus they are the parametric counterpart of our semi-parametric results.

[17] More specifically, the average (median) share of agricultural employment in countries that made a transition to democracy is 55% (56%). For the transition to autocracy sub-sample the relevant number is 62% (60%). See table A1.

these results, our study seems to suggest that democratic institutions, while strengthening support for the agricultural sector, lead to a reduction in the level of assistance given to non-agricultural sectors. Hence, higher protection given to agriculture emerges through two channels. On the one hand, democracy leads to higher nominal support for the agricultural sector. On the other hand, the agricultural sector gains also in relative terms compared to non-agricultural industries. This is consistent with our finding that democratic transition is positively related not only to the *nominal* rate of assistance, but also to the *relative* rate of assistance.

A puzzling result of our study is the asymmetric effect of regime change on the level of protection: why should a regime change be relevant only for a transition to democracy, and not *vice versa* ? One possible explanation of such asymmetric effect of transitions to democracy and autocracy could be based on theories explaining (lack of) leadership turnovers and economic performance under autocracies (see Besley and Kudamatsu, 2008; Acemoglu *et al*. 2004). These studies emphasise the importance of some institutional features of autocracy, in particular political stability. The objective of staying in power could stop a potential policy reversal against the agrarian population by preventing rural unrest. Our data preclude a deeper investigation of this issue but further analysis would certainly constitute a fruitful line of inquiry.

5. Conclusions

Motivated by the recent developments in comparative political economics about the effect of political institutions on public policy outcomes, we have investigated how democratization affects the level of protection at sector level. Based on the unique data set recently collected by the World Bank, we studied the impact of political regime transitions

on agricultural taxation and subsidization. The empirical results highlight the important role played by political regime transitions in affecting agricultural policy distortions.

Using panel data analysis and difference-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 a transition to democracy increases (relative) agricultural protection by roughly 9% when propensity score matching methods were used and by roughly 5% when difference-in-differences approach was employed. These results suggest that parametric methods might underestimate 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, this evidence supports the notion that political change may have a significant effect on *specific* policies, but that it depends on the nature of the regime change. 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 in line with the median voter model of political behavior.

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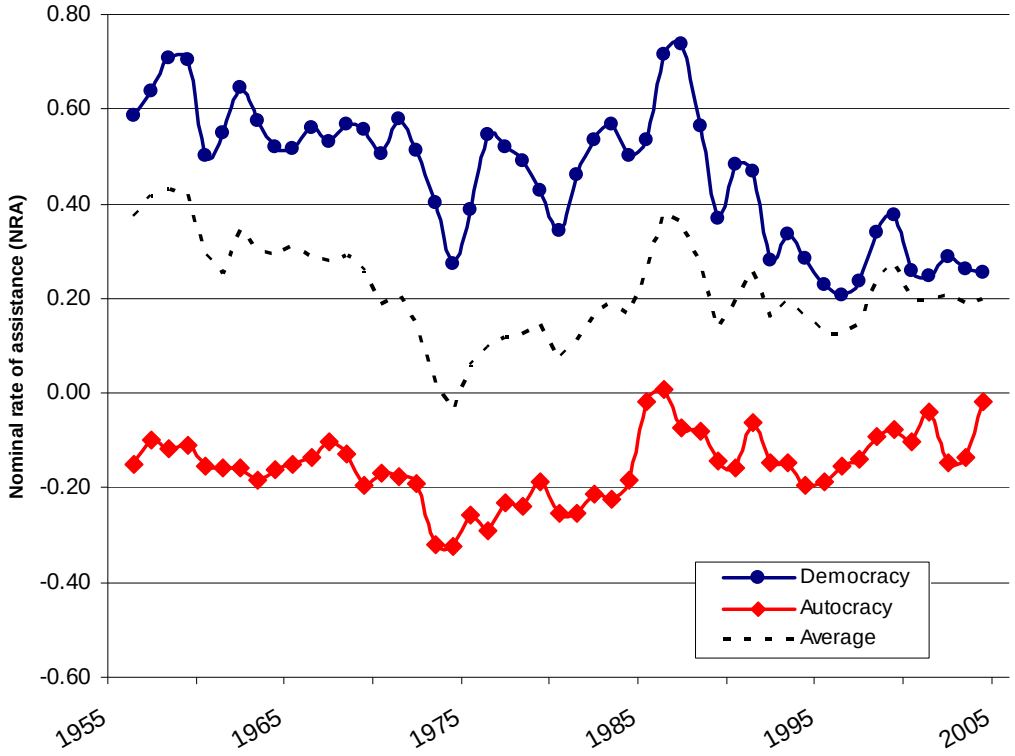

Figure 1. *Nominal Rates of Assistance (NRA) over time and regime type*

Notes: The figure shows the evolution of the (simple) yearly average of *NRA* calculated across democracies and autocracies. 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).

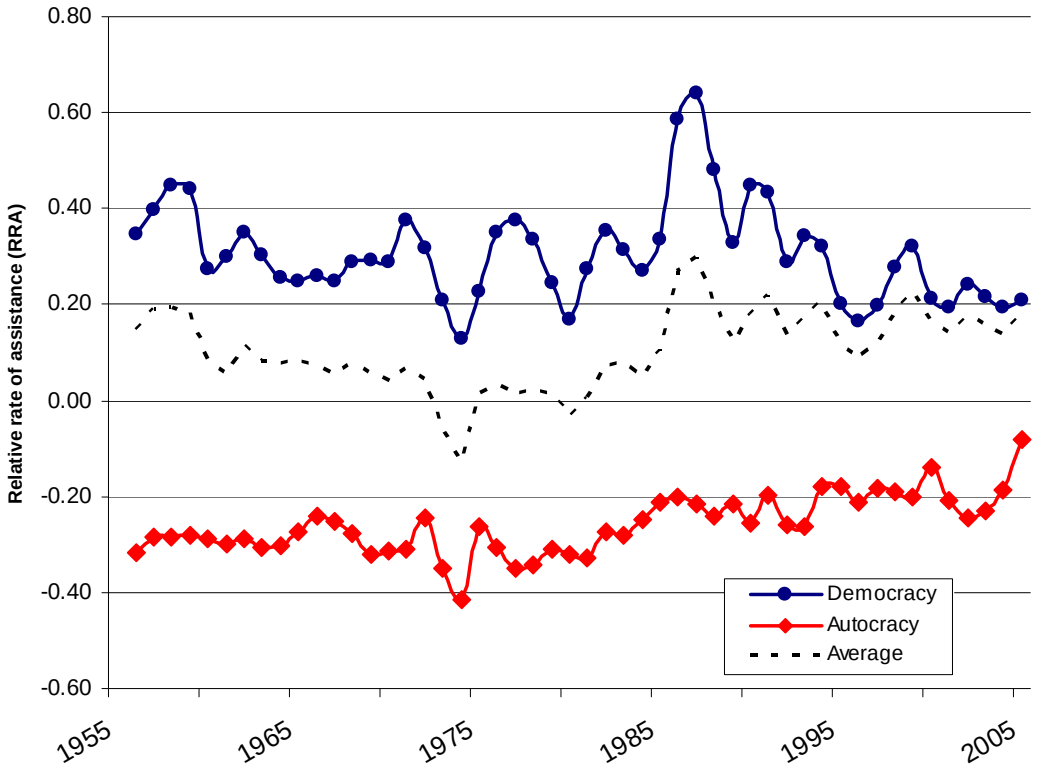

Figure 2. *Relative Rates of Assistance (RRA) over time and regime type*

Notes: The figure shows the evolution of the (simple) yearly average of *RRA* calculated across democracies and autocracies. 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 and RRA over time and political regimes*

	Full sample		Autocracy		Democracy	
	NRA	*RRA*	*NRA*	*RRA*	*NRA*	*RRA*
1956-1959	0.41	0.18	-0.13	-0.29	0.66	0.41
1960-1964	0.28	0.08	-0.16	-0.30	0.54	0.30
1965-1969	0.27	0.07	-0.13	-0.27	0.51	0.27
1970-1974	0.10	-0.01	-0.24	-0.33	0.46	0.26
1975-1979	0.10	0.02	-0.23	-0.31	0.44	0.31
1980-1984	0.09	0.03	-0.22	-0.29	0.38	0.28
1985-1989	0.29	0.20	-0.06	-0.22	0.59	0.47
1990-1994	0.23	0.18	-0.14	-0.23	0.41	0.37
1995-1999	0.19	0.15	-0.13	-0.19	0.28	0.23
2000-2005	0.20	0.16	-0.08	-0.20	0.26	0.21
All years	*0.21*	*0.11*	*-0.15*	*-0.26*	*0.45*	*0.31*
Nr. Countries	74	69	38	34	67	64

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[a]	9.51	4.80	9.62	0.71[a]
	*(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*. [a]Note that when estimating this effect, we still display the results in the same mode as in columns (1)-(3), namely computed as the negative protection effect of transitions away from democracy.

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	0.87[a]	3.75	3.26	4.02	0.00[a]
	*(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). [a]Note that when estimating this effect, we still display the results in the same mode as in columns (1)-(3), namely computed as the negative protection effect of transitions away from democracy.
****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		***RRA***				***NRA***			
		Mean		*t-test*		*Mean*		*t-test*	
	Sample	Treated	Control	t-value	p > |t|	Treated	Control	t-value	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	0.100	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	-0.14	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	2.90	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, 2008 for details). ***$p < .01$; **$p < .05$; *$p < .10$.

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

	Country	*Years coverage*		*Democratic reforms*		*Polity2*	*Protection average*		*Empsh*
		Start	End	Into	Out	Average	RRA	NRA	Average
1	Argentina	1960	2005	1973; **1983**	1976	1.0	-30.8	-18.5	**0.13**
2	Australia	1955	2005	Always democracy		10.0	-5.1	5.9	0.07
3	Austria	1956	2005	Always democracy		10.0	27.3	32.2	0.11
4	Bangladesh	1974	2004	**1991**		0.4	-21.0	0.0	**0.71**
5	Benin	1970	2005	**1991**		-1.6		-27.8	**0.68**
6	Brazil	1966	2005	**1985**		1.7	-26.6	-15.6	**0.33**
7	Bulgaria	1992	2005	Always democracy		-2.2	-6.1	-10.2	0.23
8	Burkina Faso	1970	2005	1977	1980	-4.5		-28.3	0.92
9	Cameroon	1961	2005	Always autocracy		-6.4	-30.8	-9.1	0.74
10	Canada	1961	2005	Always democracy		10.0	16.9	15.8	0.06
11	Chad	1970	2005	Always autocracy		-5.3		-26.5	0.86
12	Chile	1960	2005	**1989**	**1973**	2.7	-5.8	5.7	**0.21**
13	China	1981	2005	Always autocracy		-7.5	-26.2	-19.3	0.74
14	Colombia	1960	2005	Always democracy		7.0	-10.7	1.4	0.35
15	Cote d'Ivoire	1961	2005	2000	2002	-7.0	-42.5	-31.9	0.64
16	Czech Republic	1992	2005	Always democracy		-1.6	12.9	14.1	0.09
17	Denmark	1956	2005	Always democracy		10.0	48.0	52.9	0.08
18	Dominican Republic	1955	2005	**1978**		2.0	-16.9	-10.0	**0.34**
19	Ecuador	1970	2003	1968**; 1979**	1970	3.9	-11.4	-6.1	**0.40**
20	Egypt	1955	2005	Always autocracy		-6.4	-32.3	-14.7	0.50
21	Estonia	1992	2005	Always democracy		6.0	17.6	7.0	0.12
22	Ethiopia	1981	2005	**1994**		-5.5	-50.0	-11.9	**0.88**
23	Finland	1956	2005	Always democracy		10.0	64.5	74.7	0.13
24	France	1956	2005	Always democracy		7.9	47.3	62.2	0.09
25	Germany	1955	2005	Always democracy		10.0	61.2	70.2	0.07
26	Ghana	1960	2004	1970; 1979; **1996**	1972; 1981	-2.9	-23.6	-16.0	**0.60**
27	Hungary	1992	2005	Always democracy		-1.2	14.8	16.2	0.19
28	India	1960	2005	Always democracy		8.6	-29.4	6.2	0.67
29	Indonesia	1970	2005	**1999**		-4.5	-15.5	3.0	**0.59**
30	Ireland	1956	2005	Always democracy		10.0	59.7	70.0	0.19
31	Italy	1956	2005	Always democracy		10.0	41.1	44.7	0.13
32	Japan	1955	2005	Always democracy		10.0	85.4	110.1	0.13
33	Kenya	1966	2001	2002	1966	-3.6	-14.6	-14.8	0.81
34	Korea South	1955	2005	**1963**; **1987**	**1972**	0.5	81.6	103.5	**0.32**
35	Latvia	1992	2005	Always democracy		8.0	26.8	13.6	0.13
36	Lithuania	1992	2005	Always democracy		10.0	23.2	10.5	0.13
37	Madagascar	1960	2005	**1991**		-0.1	-32.6	-20.5	**0.80**
38	Malaysia	1960	2005	Always democracy		5.1	-8.5	-4.5	0.38
39	Mali	1970	2005	**1992**		-2.8		-33.8	**0.88**
40	Mexico	1979	2005	**1994**		-1.7	4.2	7.6	**0.35**
41	Morocco	1961	2004	Always autocracy		-7.1	-25.4	-8.6	0.53
42	Mozambique	1975	2005	**1994**		-2.2	-43.9	-28.9	**0.84**
43	Netherlands	1956	2005	Always democracy		10.0	67.0	78.5	0.06
44	New Zealand	1955	2005	Always democracy		10.0	-9.6	6.6	0.11
45	Nicaragua	1991	2004	1990		-1.9	-11.9	-11.4	0.38
46	Nigeria	1961	2004	**1979; 1999**	**1966; 1984**	-1.4	26.7	7.2	**0.53**

47	Norway	1956	2005	Always democracy		10.0	196.7	242.5	0.09
48	Pakistan	1962	2005	1972; **1988**	1969; **1977; 1999**	0.3	-37.9	-3.2	**0.58**
49	Philippines	1962	2005	**1987**	**1972**	2.3	-2.2	11.7	**0.50**
50	Poland	1992	2005	Always democracy		-1.8	8.3	12.3	0.31
51	Portugal	1956	2005	**1975**		2.4	-1.2	-0.2	**0.24**
52	Romania	1992	2005	Always democracy		-2.7	26.8	31.3	0.34
53	Rep. of South Africa	1961	2005	Always democracy		5.3	6.2	9.2	0.20
54	Russia	1992	2005	Always democracy		5.4	1.3	3.2	0.11
55	Senegal	1961	2005	**2000**		-1.8	-21.3	-14.2	**0.79**
56	Slovakia	1992	2005	Always democracy		8.1	8.8	18.6	0.09
57	Slovenia	1992	2005	Always democracy		10.0	65.3	66.0	0.03
58	Spain	1955	2005	**1976**		2.7	13.7	16.1	**0.19**
59	Srilanka	1955	2004	Always democracy		6.2	-44.2	-13.6	0.51
60	Sudan	1958	2004	**1965**; 1986	1958; **1970**; 1989	-3.6	-32.5	-33.3	**0.72**
61	Sweden	1956	2005	Always democracy		10.0	76.4	85.3	0.06
62	Switzerland	1956	2005	Always democracy		10.0	283.5	324.7	0.06
63	Taiwan	1955	2002	**1992**		-2.5	15.8	53.2	**0.24**
64	Tanzania	1976	2004	**2000**		-5.1	-57.1	-43.7	**0.86**
65	Thailand	1978	2004	1974; 1978	1976	1.1	-12.9	-6.3	0.69
66	Togo	1970	2005	Always autocracy		-5.3		-31.3	0.68
67	Turkey	1961	2005	1973; 1983	1971; 1980	6.4	-13.9	4.1	0.60
68	Uganda	1961	2004	**1980**	**1966**; **1985**	-3.0	-35.9	-7.9	**0.86**
69	UK	1956	2005	Always democracy		10.0	49.0	66.1	0.03
70	Ukraine	1992	2005	Always democracy		6.3	-13.6	-11.4	0.16
71	USA	1955	2005	Always democracy		10.0	7.2	8.4	0.04
72	Vietnam	1986	2005	Always autocracy		-6.4	-8.5	-3.1	0.73
73	Zambia	1964	2005	**1991**	**1968**	-2.5	-51.9	-40.1	**0.76**
74	Zimbabwe	1970	2005		**1987**	-1.7	-62.5	-48.1	**0.71**

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. Instead, countries like Burkina Faso, Cote d'Ivoire and Kenya are used as permanent autocracies, whereas countries like Nicaragua, Thailand and Turkey are used as permanent democracies. (See text).