

# Political Reforms and Public Policy: Evidence from Agricultural and Food Policies

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This paper studies the effect of political regime transitions on public policy using a new data set on global agricultural and food policies over a 50-year period (including data from 74 developing and developed countries over the 1955–2005 period). We find evidence that democratization leads to a reduction of agricultural taxation, an increase in agricultural subsidization, or both. The empirical findings are consistent with the predictions of the median voter model because political transitions occurred primarily in countries with a majority of farmers. The results are robust to different specifications, estimation approaches, and variable definitions. JEL codes: D72, F13, O13, P16, Q18

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Across the world, agricultural and food policies distort incentives for farmers and food consumers. Historically, governments in wealthy countries have subsidized farmers, whereas governments in poor countries have taxed farmers and subsidized food consumers (Anderson and Hayami 1986; Krueger et al. 1988; Anderson 1995). These observations have puzzled economists and other social scientists and triggered a series of studies in the 1980s and 1990s on “the political economy of agricultural policies” (see de Gorter and Swinnen 2002; Swinnen 2010 for reviews).

A recent global study on policy distortions in agriculture concludes that although policy distortions remain important, since the 1980s, the antiagricultural

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policy bias in developing countries and the proagricultural bias in high income countries have declined substantially (Anderson 2009). Interestingly, this was also the period during which important political reforms occurred in many countries. For example, the fall of the Berlin wall triggered democratic transitions across Eastern Europe in the early 1990s. Furthermore, several developing and emerging countries have become more democratic in recent decades. In Eastern Europe, political reforms induced radical economic liberalizations in the food system. In contrast, in the absence of major political reforms in East Asia, gradual economic liberalization was introduced, including the reduced taxation of farmers (Swinnen and Rozelle 2006).

Thus, the question arises whether and to what degree political reforms have affected agricultural and food policies worldwide. Political economy studies have demonstrated the importance of a variety of factors, such as economic structural factors and resource endowments, but the role of political institutions and reforms has received less attention. A few studies have attempted to analyze this issue, but the evidence on the impact of political reforms on agricultural and food policies is not clear because of problems with the data (see section II for references and details).

This paper employs a novel data set on agricultural distortions that was recently developed by the World Bank (see Anderson and Valenzuela 2008). This data set offers consistent and comparable protection indices for a large number of countries over a 50-year period. Employing this data set allows us to take advantage of not only cross-country variation but also within-country variation in the data and thus to overcome the strong identification assumptions that characterize previous cross-country studies.

Because the relationship between democracy and public policy may conceal potential feedback effects, we also study the reverse causality problem and exploit the timing of democratization. To control for potential nonlinearities and to better address unobserved heterogeneities, we estimate both linear specifications and semiparametric models. Specifically, we study the effects of democratic reform using the difference-in-difference (D-in-D) technique combined with propensity score matching methods, as in Persson and Tabellini (2008).

By studying agricultural and food policies, our analysis contributes to a broad body of literature on the impact of political reforms on economic policies (see Rodrik and Wacziarg 2005; Giavazzi and Tabellini 2005; Eichengreen and Leblang 2008).

The remainder of this paper is organized as follows. Section I presents stylized facts on trends in agricultural and food policies over time and across political regimes. Section II discusses conceptual issues and summarizes previous findings linking political reforms to public policies. Section III presents our empirical strategy. Section IV presents the data and key variables. In section V, the empirical results are presented and discussed, and in section VI, we test the robustness of our findings. Finally, section VII concludes.

## I. POLICY INDICATORS AND STYLIZED FACTS

We employ two different indicators of agricultural and food policies: the nominal rate of assistance (*NRA*) to agriculture and the relative rate of assistance (*RRA*), both from the World Bank's agricultural distortions database (see [Anderson and Valenzuela 2008](#), for calculation details). This database reports the most consistent and comparable estimates of agricultural protection across countries and over time. In our econometric analysis, we use a sample of 74 countries, comprising yearly data from 1955 to 2005 (see table S.1 in the supplemental appendix, available at <http://wber.oxfordjournals.org/>). The average number of observations per country is 35. We work with an unbalanced panel with more than 2,600 observations.

The *NRA* measures total transfers to agriculture as a percentage of the undistorted unit value. The *NRA* for agriculture is obtained as a weighted average of the *NRA* at the product level, using the undistorted value of production as a weight. The *NRA* is positive when agriculture is subsidized, negative when it is taxed, and zero when net transfers are zero. The *NRA* includes both the assistance provided by all tariff and nontariff trade measures applied to agricultural products and any domestic price-distorting measures.<sup>1</sup> The price equivalent of any direct intervention regarding inputs or outputs is also included.<sup>2</sup>

To account for the protection of manufacturing sectors, which is an important source of indirect taxation on agriculture, especially in developing countries, we use both the *NRA* and the *RRA*, which is calculated as the ratio of agricultural *NRA* to nonagricultural *NRA*.<sup>3</sup> The *RRA* is a useful indicator for international comparisons of anti- or proagricultural policy regimes. There are fewer observations for the *RRA* because the country and time-series coverage is smaller than for the *NRA*. Specifically, the *RRA* data contain five fewer countries (69 instead of 74).

Table 1 summarizes the *NRA* and *RRA* for democracies and autocracies (see below for definitions). The table indicates that autocracies are associated with negative levels for both *NRA* and *RRA*, whereas democracies have positive levels. Moreover, the differences are significant. The average *NRA* (*RRA*) is

1. This includes implicit trade taxes related to government intervention on the domestic market for foreign currency and support for public agricultural research ([Anderson et al. 2009](#)).

2. Note that the heterogeneous nature of agricultural protection in both developing and developed countries may cause an aggregation bias in measures such as the *NRA* (see [Aksoy 2005](#)). To attenuate this potential aggregation bias, in the empirical analysis presented below, we also work with data at the commodity level.

3. Specifically,  $RRA = 100[(1 + NRA_{ag}/100)/(1 + NRA_{nonag}/100) - 1]$ , where  $NRA_{ag}$  is the nominal assistance to agriculture, and  $NRA_{nonag}$  is the nominal assistance to nonagricultural sectors. Note that because of the computational complexity of this index, the *NRA* to nonagricultural sectors is only based on distortion owing to tariff protection at the border.

TABLE 1. NRA and RRA over Time and across Political Regimes

	Full sample		Autocracy		Democracy	
	<i>NRA</i>	<i>RRA</i>	<i>NRA</i>	<i>RRA</i>	<i>NRA</i>	<i>RRA</i>
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
<i>All years</i>	<i>0.21</i>	<i>0.11</i>	<i>–0.15</i>	<i>–0.26</i>	<i>0.45</i>	<i>0.31</i>
No. of countries	74	69	38	34	67	64

*Notes:* The figures report *NRA* and *RRA* averages for the full sample, autocracies, and democracies in different subperiods. The number of countries refers to the total number in each category in the 1955–2005 period and changes over time due to entry and exit.

*Source:* Own calculations based on the data described in the text.

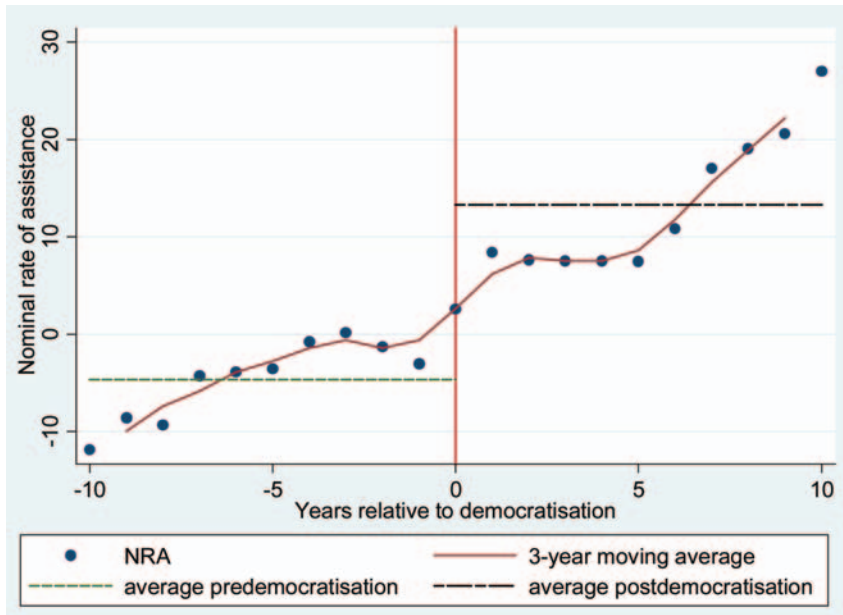
–0.15 (–0.26) for autocracies and +0.45 (+0.31) for democracies, a difference of 0.60 (0.57) or 60 (57) percentage points.

Although these statistics demonstrate that the average *NRA* and *RRA* values are much higher in democracies than in autocracies, they say nothing about the potential causal effect of democratization on agricultural policies. To obtain further insight on this issue, we examine the *NRA* and *RRA* values of countries in the data set that have experienced a political transition from autocracy to democracy. Specifically, figures 1 and 2 present the average *NRA* and *RRA* values in the predemocratization and postdemocratization periods for 23 countries that have experienced permanent democratization, as defined by Papaïannou and Siourounis (2008).<sup>4</sup>

The figures reveal interesting patterns. First, both the average *NRA* and *RRA* values are relatively stable during the decade prior to a democratic transition, approximately –5 percent for *NRA* and approximately –13 percent for *RRA*. Second, the average *NRA* and *RRA* values are significantly higher following democratic reform. The average *NRA* in the decade after democratization is 13 percent, or 18 percent higher than in the decade before democratization. For the *RRA*, the increase is 16 percent (from –13 percent to +3 percent). Third, the figures suggest that there is both an immediate effect

4. See section III for more detail on the definition and measures of “democratization.” Depending on the time period covered and the year of democratization, the average at each point in time in figures 1 and 2 is based on different samples of countries.

FIGURE 1. Average NRA and the Timing of Political Reforms



*Source:* Own calculations based on data from the World Bank's agricultural distortions database and the Polity IV database.

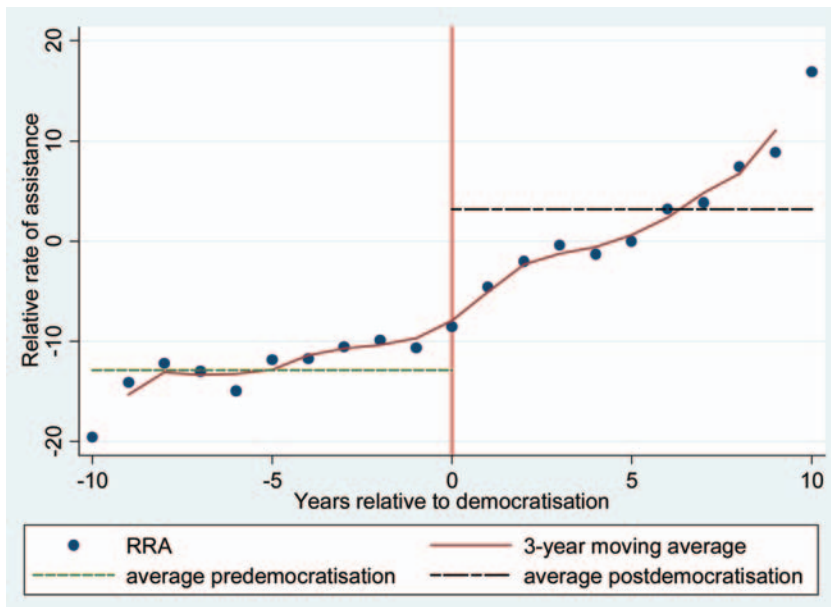
at about the time of democratization and an additional increase approximately five years later.

In summary, these descriptive statistics indicate interesting correlations between agricultural policies and political regimes, both across countries and over time. In the remainder of this paper, we use econometric methods to analyze whether there is a causal relationship.

## II. CONCEPTUAL ISSUES AND LITERATURE

There is a substantial body of literature on how political reforms influence government policies (Mulligan et al. 2004). However, theory does not provide a simple prediction of how democratic reforms affect agricultural protection. In democracies, the distribution of political power is typically more equal than the distributions of income and wealth. Consequently, median voter models predict that democracies tend to redistribute from the rich to the poor, and this effect is stronger with greater income inequality because the middle class has a greater incentive to form coalitions with the poor (Alesina and Rodrik 1994; Persson and Tabellini 1994). Similarly, democratic regimes may lead to

FIGURE 2. Average RRA and the Timing of Political Reforms



Source: Own calculations based on data from the World Bank's agricultural distortions database and the Polity IV database.

economic policy reforms if these reforms create more winners than losers (Giavazzi and Tabellini 2005).

Empirical studies have attempted to test this prediction using data on democracy and economic liberalization. An area that has attracted substantial interest is trade policy. Overall, the existing literature suggests a positive impact of democracy on economic (trade) liberalization (e.g., Banerji and Ghanem 1997; Milner and Kubota 2005; Giavazzi and Tabellini 2005; Eichengreen and Leblang 2008; Giuliano *et al.* 2011). Some studies, however, have argued that this effect is not generally true but depends on a country's resource endowment (e.g., O'Rourke and Taylor 2007; Kono 2006).

There are several methodological critiques of these studies, such as the problem of spurious correlation between democracy and economic reforms (Eichengreen and Leblang 2008) or the existence of potential feedback effects (Giavazzi and Tabellini 2005; Milner and Mukherjee 2009). An additional problem is that most existing studies have examined the relationship between democracy and trade policy using aggregate trade indices, such as the trade to GDP ratio or the Sachs and Warner (1995) openness index (e.g., Giavazzi and Tabellini 2005; Milner and Kubota 2005; Persson 2005; Eichengreen and Leblang 2008; Tavares 2007). Studies have only rarely employed direct indicators of trade policy, such as tariffs. Moreover, aggregated trade policy

indicators may be misleading because different (and possibly offsetting) effects may occur at a disaggregated level (Anderson and Martin 2006). Thus, an examination of disaggregated policies, such as agricultural and food policies, could yield additional insights.

Empirical studies have estimated the impact of political institutions on agricultural policies. Lindert (1991) was the first to document a positive impact of democracy on agricultural protection. Beghin and Kherallah (1994) examine the impact of different political systems (no-party, one-party, dominant party, and multiparty systems) on agricultural protection. They find that political institutions are important and that their effect is nonmonotonic: protection peaks with dominant party systems and then becomes nonincreasing despite further democratization. A nonmonotonic relationship between democracy and protection is also found by Swinnen et al. (2000), who uses the Gastil index of political rights. Specifically, they demonstrate that moving from low to medium levels of political rights reduces protection, but any further increase in democratization does not necessarily result in substantial effects on agricultural protection. However, this nonlinear behavior runs in the opposite direction of that found by Beghin and Kherallah. Olper (2001) finds that the level of democracy *per se* does not seem to matter, but the quality of institutions that protect and enforce property rights is important.

Although these studies highlight a number of interesting aspects, they should be interpreted with caution. The studies all have potential problems of reverse causality and omitted variable bias because they rely predominantly on the between-country variation in the data. Their data sets do not allow for the exploitation of time series variation. To date, the only study to investigate the relationship between democracy and agricultural protection by employing a long time series is the study by Swinnen et al. (2001), which examines agricultural protection patterns in Belgium between 1877 and 1990 and uses detailed indicators of political reforms. Their paper demonstrates that only those political reforms that generate a significant shift in the political balance toward agricultural interests (e.g., the extension of voting rights to small farmers in the early 20th century) induce an increase in agricultural protection. This result provides a logical interpretation of the democracy-protection nonlinearity discussed above and highlights the importance of drawing inferences from autocratic-democratic *regime changes* to improve understanding of the impact of democratization on agricultural protection.

An additional problem is that the absence of representative information on the preferences of autocratic rulers complicates predictions of the effect of democratization. The insulation of decision makers means that they can follow their private preferences to a large extent when selecting policies. However, this argument has little predictive power in the absence of information on autocrats' preferences. The preference of rulers is a key variable, but there are

major data and measurement problems. For example, quantitative data exist on ideologies, but these data are limited to democracies.<sup>5,6</sup>

Assuming that rulers' preferences are randomly distributed,<sup>7</sup> the median voter model predicts that the impact of democratization is conditional on the structure of the economy. The share of farmers (or the rural population) in the economy differs significantly between rich and poor countries. The factors that make it difficult for farmers to organize politically in poor countries (such as their large number and substantial geographic dispersion; see Olson 1965) render them potentially powerful in electoral settings because they represent a large share of the votes (Bates and Block 2010; Varshney 1995). Therefore, *ceteris paribus*, one would expect that democratization is more likely to benefit farmers in poor countries.

In our data set, the vast majority of transitions from autocracy to democracy occur in poorer countries with a large number of farmers.<sup>8</sup> In fact, the average share of agriculture in total employment at the time of political transition is 65 percent, whereas the average share for all countries and time periods in the data set is 25 percent. This finding implies that the measured effect of

5. Olper's (2007) study of a cross-section of countries found that, on average, right-wing governments are more protectionist with respect to agriculture than left-wing governments. Furthermore, although left-wing governments support agriculture to a lesser extent, they tend to support farmers more in unequal societies. This finding is consistent with qualitative evidence from Bates (1983), who argues that socialist rulers in Africa tax farmers (by imposing low commodity prices), and from Tracy (1989), who found that right-wing governments in Europe (such as those dominated by Catholic and conservative parties) tend to support farm interests and protectionism.

6. There are other problems in empirically assessing the impact of rulers' preferences. First, applying a simple left-wing/right-wing model to agricultural policy is not straightforward because increases to food costs through agricultural protection hurts both urban workers (left-wing interests) and industrial capitalists (right-wing interests). Thus, rulers who support either labor or capital should oppose agricultural protection, as they did historically in Europe (Kindleberger 1975; Schonhardt-Bailey 1998; Findlay and O'Rourke 2007). Second, economic development may change rulers' preferences. As their economies developed, Communist autocracies shifted from taxing to subsidizing agriculture, as was the case in democracies. Communist dictators of poor countries, such as Stalin in Russia, Mao in China, and Hoxha in Albania, heavily taxed agriculture. However, farmers were subsidized at higher incomes, such as in the Soviet Union under Brezhnev and in most Eastern European Communist countries in the 1970s and 1980s (Swinnen and Rozelle 2009). Third, rulers' preferences are not restricted to ideology; they may also reflect regional interests. Bates and Block (2010) show that the regional backgrounds of leaders in Africa significantly affected their policy preferences. Leaders who drew political support from cities and semiarid regions (as in Tanzania and Ghana) seized a major portion of revenues generated by the export of cash crops (coffee and cocoa), whereas in countries where leaders came from regions where cash crops were important sources of income (such as in Kenya and Ivory Coast), they imposed few taxes on coffee and cocoa exports.

7. Olper (2007) finds more variation in policy choices, *ceteris paribus*, under dictatorial regimes than under democracies. This result is consistent with the argument that dictatorial leaders are less constrained in setting policies and that government responses to pressure from interest groups are stronger in democracies.

8. Of the 42 democratic transitions (see table S.1 and the discussion below) included in the data set, only five occurred in countries that are currently members of the OECD (Spain, Portugal, Mexico, South Korea, and Turkey), and these transitions occurred at times when they had considerably lower incomes than at present.



democratization on agricultural policies in our data set should be in favor of farmers (i.e., a positive impact on *NRA* and *RRA*) because of the structural “bias” of political reforms. The move from autocracy to democracy primarily occurs in countries where farmers constitute the majority of the population, and the median voter model predicts that this situation should induce a pro-farmer policy effect.

### III. EMPIRICAL METHODOLOGY

To address the problems of omitted variable bias and reverse causation in the analysis of the effect of political institutions on policies and to make use of both cross-country variations and time variations in the data, we use a D-in-D strategy, as in recent studies (e.g., [Giavazzi and Tabellini 2005](#); [Rodrik and Wacziarg 2005](#)). To analyze the robustness of our results, we combine the standard D-in-D approach with semiparametric matching methods, as in [Blundell et al. \(2004\)](#) and [Persson and Tabellini \(2008\)](#).

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

More formally, we run panel regressions<sup>9</sup> with the following specification:

$$(1) \quad y_{it} = \beta D_{it} + \rho X_{it} + \alpha_i + \theta_t + \varepsilon_{it}$$

where  $y_{it}$  denotes our measure of interest, namely, agricultural policies measured by *NRA* and *RRA*;  $\alpha_i$  and  $\theta_t$  are country and year fixed effects, respectively;  $X_{it}$  is a set of control variables; and  $D_{it}$  is a dummy variable that takes the value one for democracy and zero otherwise (see section IV). The parameter  $\beta$  is the D-in-D estimate of the regime change effect. It is obtained by comparing the average protection after a 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—that is, those that have either  $D_{it} = 1$  or  $D_{it} = 0$  over the entire sample period.

Estimates obtained from the standard D-in-D procedure are based on several restrictive assumptions (see [Abadie 2005](#); [Persson and Tabellini 2008](#)). First, it

9. For a discussion of the relationships among various estimators in the context of panel data, see [Mundlak \(1978\)](#) and [Mundlak and Larson \(1992\)](#).

is assumed that, absent any regime change, the average growth in protection in the treated countries should be the same as in the control countries.<sup>10</sup> Second, the estimates do not take into account the (potential) heterogeneity of regime change effects on agricultural policies.<sup>11</sup> Finally, the estimates may suffer from omitted variable bias due to time-varying (country-specific) covariates correlated with both democracy and policies.

To address the latter problem, in addition to the traditional controls, we include in our specifications several time-varying, country-specific variables. Furthermore, given our specific concern for (omitted) time-varying factors, we add continent-year interaction effects in some specifications. This process takes into account that changes in agricultural policies may be due to general developments in geographical clusters. Finally, we check the robustness of our results by running dynamic panel models.

To circumvent the heterogeneity of regime change effects, the existing literature interacts the political reform dummy with other characteristics of reforms, such as specific electoral rules or forms of government implemented by the new democracy (see [Persson 2005](#); [Olper and Raimondi Forthcoming](#)). However, the problem with this approach is that the potential interactions or nonlinearities are too numerous compared with the regime transitions in the data. Therefore, we use semiparametric methods to address these problems; that is, we combine a D-in-D methodology with a propensity score matching method, following the approach discussed by [Smith and Todd \(2005\)](#) and [Abadie \(2005\)](#) and applied by [Blundell et al. \(2004\)](#) and [Persson and Tabellini \(2008\)](#). This method has two main advantages over the standard D-in-D estimator. First, it ensures that the pretreatment characteristics that are thought to determine the outcome variable are balanced between the treated and untreated countries. Thus, this method relaxes the strong identifying restriction of the standard approach ([Abadie 2005](#)). Second, it relaxes linearity assumptions by allowing for heterogeneous impacts of democratic transitions on agricultural policies.

Our matching *cum* D-in-D strategy is implemented in two steps. First, to avoid confounding the effect of political regime transition with that of factors that determine this shift and because we cannot observe what would have happened if a democratic country had remained an autocracy, an estimate of the counterfactual is constructed. Conditional on 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 an evaluation of the difference in the evolution of agricultural policies between countries with and without a regime change. Because matching relies on comparing

10. This restriction is partially addressed by adding several covariates in the vector  $X_{i,t}$  to increase the similarity between treated and control countries.

11. See [Ashenfelter \(1978\)](#) and [Ashenfelter and Card \(1985\)](#) for a general discussion of this subject.

countries with similar propensity score values, the inferences are not distorted by counterfactuals that differ substantially from the treated observations.

The average estimated effect of regime transitions that we compute (the so-called average treatment on treated, *ATT*) can be presented as follows:

$$(2) \quad ATT = \frac{1}{I} \sum_i \left( a_i - \sum_j w_{ij} a_i^j \right)$$

where  $I$  stands for the number of treated observations within the common support;  $a_i$  is the difference between the average level of agricultural protection before and after 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 in the treated country with which it is matched; and  $w_{ij}$  ( $w_{ij} > 0$  and  $\sum_j w_{ij} = 1$ ) are weights based on the propensity score that depend on the matching estimator (Sianesi 2001). We use Epanechnikov kernel and Gaussian kernel estimators (Fan 1992; Heckman *et al.* 1998).

#### IV. POLITICAL REFORM INDICATORS AND CONTROL VARIABLES

To study how a regime transition toward democracy affects agricultural and food policies, we need data on democratization episodes. Unfortunately, although various democracy data sets exist, none of these data sets provides a specific coding of regime transitions. Therefore, we follow the same strategy as recent studies that have investigated similar questions at the aggregate level by relying on the Polity2 index from the Polity IV data (Marshall and Jaggers 2007).<sup>12</sup> The composite Polity2 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, such as the openness of elections or constraints on the executive branch. Following Persson (2005) and Giavazzi and Tabellini (2005), we code a country as “democratic” in each year that the Polity2 index is strictly positive, setting a binary indicator called *democracy* to one (zero otherwise). A reform into (or out of) democracy occurs in a country-year when this democracy indicator switches from zero to one (from one to zero).

A potential shortcoming of this definition of *political reform* is that being near any particular divide may differ from being far from the divide.<sup>13</sup> Indeed, the threshold of zero for Polity2 corresponds to a generous definition of

12. Polity IV has a longer time series and therefore includes more usable political reforms than other existing democracy indices. For example, in addition to its shortcomings due to classification bias (see Papaioannou and Siourounis 2008), the use of the Freedom House data strongly limits the number of usable transitions because the information only begins in 1972. For a critical discussion of democracy indices, see Munck and Verkuilen (2002).

13. We thank an anonymous referee for focusing our attention on this issue.

democracy. However, as emphasized by [Persson and Tabellini \(2008\)](#) and others, this definition has the important advantage that many large changes in the Polity2 score are clustered around zero, an important property given our identification strategy based on the within-country variation in the data. Consequently, using a higher threshold for the definition of democracy has the shortcoming of including very (small) gradual changes that are only poorly related to significant regime changes in democratic transitions.<sup>14</sup>

Applying these criteria to our 74-country data set, we obtain 67 regime changes, of which 42 are transitions into democracy and 25 are transitions into autocracy (see table S.1). The distribution of these reforms is uniform over time (53 percent before 1985) but not across continents: approximately 50 percent of the reforms are in Africa, 28 percent are in Asia, and 18 percent are in Latin America.

To avoid the use of very brief reform episodes, we introduce the criterion that the dependent variable must be observed for at least four years before and after each regime transition. Under this rule, the effective number of reform episodes decreases to approximately 40. As a robustness check, we relax this criterion to only two years of observable outcomes; this period includes nearly all of the reform episodes reported in table S.1.

To check the robustness of our results, we use a distinct definition of regime transitions. Specifically, we use the recently developed data set by [Papaioannou and Siourounis \(2008\)](#) to define regime changes. These data are based on a more complex procedure than that applied above. Specifically, to identify the precise timing of each regime change, [Papaioannou and Siourounis \(2008\)](#) rely not only on the Polity2 index and the Freedom House democracy index but also historical evidence derived from numerous political archives and election databases. Using this procedure, the authors identify “full” or “partial” democratization episodes. However, because their analysis focuses solely on permanent democratization, the use of their coding applies to a lower number of transition episodes (23) in our data (see table S.1).

### *Control Variables*

In the empirical specification, we include additional controls that are likely to affect agricultural and food policies, as suggested by many previous studies (e.g., [Anderson 1995](#); [Beghin and Kherallah 1994](#); [Swinnen et al. 2000](#); [Olper 2007](#)). Specifically, our basic D-in-D specification always includes the following structural controls: the level of development, measured by the log of real per capita GDP; the share of agricultural employment in total employment; the log

14. It is important to note that the use of the “continuous” Polity2 index, instead of a discrete index, does not affect our qualitative conclusions; a higher Polity2 score increases the level of agricultural protection. These additional results are not reported to conserve space, but they may be obtained from the authors upon request.

of agricultural land per capita; and the log of total population. All of these variables are computed from World Bank (*WDI*), FAO, or national statistics.

We also test the robustness of our findings by controlling for several other (macro) covariates, such as different indicators of (aggregate) openness, government expenditures, and economic and political crises (wars and conflicts). Openness indicators (the trade to GDP ratio and the [Sachs and Warner \(1995\)](#) index) and government expenditures are obtained from [Wacziarg and Welch \(2008\)](#)<sup>15</sup> and the Penn World Table, respectively. War and conflict year dummies are based on the UCDP/PRIO Armed Conflict Dataset Version 4-2008 (see [Gleditsch et al. 2002](#)).

For our matching strategy, we use a limited number of covariates that are likely to influence both regime change and agricultural and food policies. As discussed previously, a shift in agricultural policy may require political reforms of sufficient size ([Swinnen et al. 2001](#)). Therefore, in our model, we include a variable, *initial polity2*, that 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 values close to zero are more likely to have a political regime change.

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 the variable *length of sample* (measured in years). This variable is designed to account for the possibility that democratization may require time to have an impact. Furthermore, to control for the fact that changes in both agricultural policy and political regime may be related to economic development, we include the variable *relative gdp*, which measures each country's per capita income at the beginning of the sample relative to U.S. per capita income in the same year. Finally, to control for the possibility that the change in political regime may be related to the occurrence of conflicts (both domestic and international), we include the variable *conflict years*, which measures the share of conflict years over the total length of the period for which policy data are available.

## V. ESTIMATION RESULTS

Table 2 reports D-in-D econometric results with the *NRA* and *RRA* as dependent variables. Columns 1 and 5 report “unconditional” democracy effects by adding only the level of development to the vector of covariates *X* to control for the well-known positive correlations between per capita GDP and both democracy and agricultural protection. In columns 2–4 and 6–8, we analyze

15. The Sachs and Warner index, based on the recent update by [Wacziarg and Welch \(2008\)](#), is equal to one when a country is considered open and zero otherwise on the basis on the following criteria: an aggregate tariff rate greater than 40 percent, a nontariff barrier covering more than 40 percent of trade, a black market exchange rate of less than 20 percent relative to the official exchange rate, and a state monopoly in major exports.

TABLE 2. Effect of Democratic Reforms on Agricultural Protection

Estimation Regression	D-in-D regressions							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	<i>NRA</i>	<i>NRA</i>	<i>NRA</i>	<i>NRA</i>	<i>RRA</i>	<i>RRA</i>	<i>RRA</i>	<i>RRA</i>
<i>Democratic reform</i>	18.560 (.001)	16.272 (.001)	13.997 (.003)	13.274 (.005)	13.183 (.006)	11.206 (.016)	9.506 (.036)	10.378 (.016)
<i>Log GDP per capita</i>	32.919 (.011)	48.717 (.000)	42.461 (.002)	45.935 (.001)	34.518 (.004)	39.014 (.008)	35.076 (.005)	41.540 (.000)
<i>Employment share</i>			-88.857 (.107)	-94.180 (.082)			-61.086 (.324)	-65.281 (.276)
<i>Land per capita</i>			-2.392 (.097)	-2.484 (.125)			-1.180 (.371)	-1.292 (.348)
<i>Log population</i>			-28.825 (.410)	-32.349 (.340)			-3.249 (.931)	-16.952 (.622)
<i>Trade policy reform (Sachs-Warner)</i>				(.002)				(.000)
<i>Trade openness</i>				-0.065 (.278)				-0.053 (.390)
<i>Government consumption</i>				-0.213 (.583)				0.616 (.176)
Treatment	All	All	All	All	All	All	All	All
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Continental trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Countries	74	74	74	72	69	69	69	67
Observations	2,664	2,664	2,565	2,502	2,394	2,394	2,314	2,253
$R^2$ (within)	0.184	0.323	0.338	0.359	0.230	0.339	0.351	0.387

Notes:  $p$  values based on clustered standard errors at the country level in parentheses. Year and country fixed effects as well as interaction effects between continents (Africa, Asia, and Latin America) and year dummies are included as indicated. The democracy variable is based on the Polity2 index (see text).

Source: Own calculations based on the data described in the text.

the democratization effect using regressions controlling for both the standard determinants of agricultural protection and macroeconomic and trade policy. In all regressions, the standard errors are clustered at the country level, allowing for arbitrary, country-specific serial correlation (see [Bertrand \*et al.\* 2004](#)).<sup>16</sup> Because the fixed effects and other covariates are correlated, we only report the fixed effects results ([Mundlak 1978](#); [Mundlak and Larson 1992](#)).<sup>17</sup>

All specifications yield positive estimates of the democracy coefficient. The significance varies between the 1 percent and 5 percent levels. The magnitude of the democracy variable in column 1 suggests that a transition from autocracy to democracy induces a strong effect: the *NRA* increases, on average, by 18.6 percentage points. In column 2, we add a set of continent-year interaction effects to control for both differences in regional protection dynamics and the nonstationary nature of the democracy dummy.<sup>18</sup> Although their inclusion slightly reduces the democracy coefficient, it remains significant at the 1 percent level. Columns 3 and 4 test the robustness of our findings by including a set of covariates normally found to be significant determinants of agricultural protection (in column 3) and the share of government consumption expenditures in GDP<sup>19</sup> and two different openness variables: the trade to GDP ratio and a trade policy reform index based on [Sachs and Warner \(1995\)](#) (in column 4). The democracy effect is still estimated with strong precision ( $p < .01$ ). The magnitude of the estimated effect is very similar in both equations and slightly lower than in columns 1–2. The effect on *NRA* is now approximately 14 percentage points. These results suggest that the positive effect of a regime change on the *NRA* is very robust. The estimated coefficients of the other variables are consistent with expectations from the agricultural protection literature.<sup>20</sup>

16. An alternative means of correcting for the potential problem of inconsistent standard errors would be to follow a residual-aggregation procedure, as suggested by [Bertrand \*et al.\* \(2004\)](#). In our case, where we consider approximately 40 reform episodes, this could be problematic because the power of this procedure is quite low and diminishes rapidly with sample size.

17. Hausman tests also confirm this correlation. Please note that in the random effects model the key result (i.e., the effect of democratization on the *NRA* and *RRA*) is positive and strongly significant and is virtually identical in magnitude to the results reported in table 2 (additional results are available upon request).

18. As emphasized by [Papaioannou and Siourounis \(2008\)](#), the *democracy* indicator behaves as a trend because countries that switch to democracy seldom revert to autocracy.

19. We use total government consumption instead of government spending owing to data limitations. For our broad country sample and the 1955–2005 time period, this is the most widely available measure of government spending.

20. See [de Gorter and Swinnen \(2002\)](#) for a survey. A positive impact of GDP per capita is in line with the so-called development paradox. A negative impact of agricultural employment is in line with Olson's (small) interest group story and the reduced per capita tax costs of subsidizing a declining sector. A negative impact of land per capita is in accordance with the notion that countries with a comparative advantage in agriculture are less protected ([Anderson 1995](#); [Swinnen 1994](#)). Moreover, this variable may capture collective action problems due to the heterogeneity of the farm group. This latter interpretation draws on the observation that countries with more abundant land tend to consistently have a more unequal distribution of land ([Olper 2007](#)).

Columns 5–8 are analogous using the *RRA* as the dependent variable. The results are similar, but the sizes of the effects and the precision of the estimates are somewhat smaller. The magnitude of the estimated effect of reforms into democracy on the *RRA* is 10–13 percentage points, depending on the model. The small difference (4 percentage points) between the *NRA* and *RRA* regressions suggests that the bulk of the democracy effect comes from changes in agricultural policies.<sup>21</sup>

An interesting hypothetical question is what the level of agricultural protection would be if all countries were democracies.<sup>22</sup> In our sample, autocracies are only present in Africa and Asia at the end of the data period. The issue is most relevant for Africa because 10 out of 22 countries were still autocracies, whereas only 3 out of 11 were still autocracies in Asia. A simple prediction based on average effects (an increase from 14 percent to 18 percent for *NRA*) and the use of 2000 as the base year (the year for which we have the largest recent country sample) yields the following: with an average *NRA* of –15 percent in Africa in 2000, *ceteris paribus*, a hypothetical democratization wave would induce a reduction in the average level of taxation of 6 to 8 percentage points, resulting in an average *NRA* of –7 percent to –9 percent and effectively halving agricultural taxation. In Asia, the average effect of hypothetical democratization is smaller because fewer autocracies remain. The effect depends on whether a simple average or a weighted average is used; China is one of three remaining autocracies in the data set. The simple average effect on *NRA* is an increase of between 4 and 5 percentage points for Asia, whereas the population weighted average protection effect is an increase of 7 to 9 percentage points (from a weighted average *NRA* of approximately 9 percent).

## VI. EXTENSIONS AND ROBUSTNESS CHECKS

To further test whether our results capture a causal effect of democratization on agricultural policies, we run several extensions of the model and robustness checks. Specifically, in this section, we analyze how the results are affected by considering or using (a) disaggregated commodities, (b) different indicators to capture the timing of political reforms, (c) alternative estimation models (matching, dynamic panels, feedback effects), (d) alternative definitions of regime changes, and (e) additional indicators of economic and political crises. For brevity, some of these additional regression results are reported in the supplemental appendix.

21. This is consistent with the fact that running a regression using the nominal rate of assistance to nonagricultural products,  $NRA_{nonag}$  (i.e., the denominator of the *RRA*), as the dependent variable means that the democracy reform dummy is never significant, irrespective of specification. These additional results are available from the authors upon request.

22. We thank a referee for this suggestion.



### *Disaggregated Commodities*

The results reported in table 2 are based on an aggregated measure of protection. However, various sectors are taxed and subsidized differently for a number of reasons, including differences in demand and supply conditions and because these sectors are characterized by different market structures (e.g., small vs. large farms), which influences rent-seeking behavior. The heterogeneous nature of agricultural protection may also cause an aggregation bias in measures such as the *NRA* (see Aksoy 2005). To investigate potential heterogeneity in the political reform effects across different groups of commodities, table 3 reports regression results by separating importing and exporting sectors (columns 1 and 2) and four commodity groups (columns 3–7).<sup>23</sup>

The disaggregated regressions demonstrate that democratization increases the *NRA* for all subsectors, but the magnitude of the estimated effect differs: it is higher (approximately 17.4 percentage points) for import-competing sectors than for exporting sectors (6.8 percentage points). Similarly, the democratization effect is positive for the four different product groups, but it is much higher for grains and tubers (17 percent) and oilseeds (31 percent) than for livestock products (5 percent) and tropical crops (7 percent).

### *Timing of Political Reforms*

A potential shortcoming of our findings is that we have constrained the democratization effect to be monotonic (Papaioannou and Siourounis 2008). Relaxing this assumption could yield additional insights into the dynamics of this effect. Following Giavazzi and Tabellini (2005) and Wacziarg and Welch (2008), we investigate these issues by studying the timing of the reform effects. To do so, we replace the variable *democracy* with three nonoverlapping dummies: a dummy equal to one in the three years preceding the regime change (*3 years before*), a dummy equal to one in the year of the reform and in the three following years (*years 0–3*), and a dummy equal to one from the fourth year after the regime change and onward (*years 4 and after*). The *3 years before* dummy aims to account for potential positive changes in agricultural protection before the democratic transition. For example, it is possible that an autocratic government may implement protectionist policies to gain legitimacy and remain in power.

Table 4 presents the results for the *NRA* and *RRA*. The estimated effect of the *3 years before* dummy is negative, except in column (2), but it is never significant. This finding suggests that agricultural policies do not change prior to democratization. Thus, our results do not support the hypothesis that the

23. The compositions of the groups are as follows: grains and tubers (e.g., rice, wheat, maize, cassava, barley, sorghum, millet, and oats), oilseeds (e.g., soybean, groundnut, palm oil, rapeseed, sunflower, and sesame), livestock products (e.g., pigment, milk, beef, poultry, egg, sheep meat, and wool), and tropical crops (e.g., sugar, cotton, coconut, coffee, rubber, tea, and cocoa).

TABLE 3. Effect of Democratic Reforms on Agricultural Protection at the Sector Level

Estimation	D-in-D regressions					
	Import sectors (1)	Export sectors (3)	Grains and tubers (4)	Livestock products (5)	Oilseeds (6)	Tropical crops (7)
Dependent variable	<i>NRA</i>	<i>NRA</i>	<i>NRA</i>	<i>NRA</i>	<i>NRA</i>	<i>NRA</i>
<i>Democracy</i>	17.4 (.007)	6.8 (.026)	17.4 (.000)	5.1 (.632)	31.0 (.050)	7.1 (.367)
Treatment	All	All	All	All	All	All
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Continental trends	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$ (within)	0.239	0.146	0.228	0.279	0.391	0.424
Country-sectors	519	440	269	238	80	112
Observations	13,278	9,558	8,932	6,920	2,510	3,869

Notes:  $p$  values based on robust standard errors clustered at country-sectors in parentheses. Additional controls include the *log of per capita GDP*, the *log of population*, *agricultural employment share*, *land per capita*, and *product value shares*. All regressions include time and country fixed effects and interactions between years and continental dummies (for African, Asian, and Latin American countries).

Source: Own calculations based on the data described in the text.

anticipation of the democratization process is reflected in changes in agricultural protection.

The estimated coefficient of the variable *years 4 and after*, which captures the long-term effect of regime change, is positive and strongly significant for both the *NRA* and *RRA*. The estimated values are similar to the values in columns 4 and 8 of table 2. The results imply a long-run democratization effect of approximately 16–19 percentage points for *NRA* and 13 percentage points for *RRA*.<sup>24</sup> For both the *NRA* and *RRA*, the short-term effect, captured by the *year 0–3* dummy, is always positive but is smaller in magnitude than the long-term effect. For the *RRA*, in particular, the short-term effect is small and not significant.

The results in table 4 are consistent with the descriptive evidence reported in figures 1 and 2. After a democratization episode, there is an immediate increase in agricultural protection. Then, policies appear to be stable for some years. After a few years of democracy, we observe an additional increase in agricultural protection. Thus, it appears that it takes time for democratization to fully exert its influence on agricultural policy.

24. Not surprisingly, the magnitudes of these reform effects are similar to those in the regressions that consider only permanent reforms. See the regressions in columns 3 and 6 in table S.5 in the supplemental appendix.

TABLE 4. Timing of Political and Agricultural Policy Reforms

Estimation Regression	D-in-D regressions			
	(1)	(2)	(3)	(4)
Dependent variable	NRA	NRA	RRA	RRA
3 years before democratic reform	-1.371 (.640) (.736)	0.954 (.744) (.776)	-5.505 (.047) (.131)	-4.100 (.145) (.222)
years 0–3 after democratic reform	6.962 (.028) (.191)	8.478 (.008) (.067)	1.156 (.696) (.775)	1.841 (.544) (.637)
years 4 or more after democratic reform	16.360 (.000) (.019)	19.455 (.000) (.004)	13.629 (.000) (.032)	13.675 (.000) (.034)
Time fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Continent-year dummies	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2,565	2,565	2,314	2,314
Number of countries	74	74	69	69
R <sup>2</sup> (within)	0.246	0.342	0.269	0.356

Notes: *p* values in parentheses based on robust and clustered standard errors, respectively. Controls include *log per capita GDP*, *employment share*, *land per capita*, *log of population*, and year and country fixed effects included in every regression.

Source: Own calculations based on the data described in the text.

### Matching

We now use a semiparametric analysis (i.e., a matching approach) to at least partly relax the strong identifying assumptions in the D-in-D approach.<sup>25</sup> The results of the matching procedure are presented in table 5. Given that the estimates are less efficient and less precise owing to fewer usable observations, the matching results are consistent with the results obtained from the standard D-in-D method. The effect of a transition to democracy is strongly positive and statistically significant and is of the same order of magnitude. As in the D-in-D regressions, the effect of democracy is larger on the *NRA* than on the *RRA*.

### Dynamic Panel Methods

Next, we estimate the effect of democratization on protection using dynamic panel models. Specifically, we employ a dynamic D-in-D regression to control

25. Table S.2 in the supplemental appendix presents the coefficients of the Probit models that were used to calculate propensity scores. Although our model is not ideal for the prediction of shifts toward democracy, the selected covariates provide some explanation for a regime change (pseudo  $R^2$  equal to 0.23 – 0.24). Table S.3 in the supplemental appendix compares the distribution of observed covariates between the countries in the treatment and control groups. The matching performed well in terms of removing significant differences between the treatment and control countries, although the treatment and control groups were not particularly different prior to matching. Matching reduces the difference in means for several variables, such as the dummy for *Africa*, *relative GDP*, and *conflict years*.

TABLE 5. Robustness Check: Matching Estimates of the Democratization Effect

	NRA		RRA	
	(1)	(2)	(3)	(4)
<i>Growth in agric. Protection</i>	14.63	13.95	9.72	9.25
<i>Std. error lower bound</i>	(.062)	.076)	(.087)	(.109)
<i>Std. error upper bound</i>	(.070)	(.085)	(.107)	(.152)
<i>Estimation technique</i>	Matching Kernel Epanechnikov	Matching Kernel Gaussian	Matching Kernel Epanechnikov	Matching Kernel Gaussian
No. of treated countries	10	10	5	5
No. of control countries	10	10	7	7
No. of controls with repetitions	79	100	32	35

*Notes:*  $p$  values in parentheses. In the upper row, they are estimated assuming independent observations, whereas in the lower row, they are estimated assuming perfect correlations of repeated observations in control countries.

*Source:* Own calculations based on the data described in the text.

TABLE 6. Robustness Check: Dynamic Panel Model of the Effect of Democratization on Policy Reforms

Estimator	D-in-D regression with $T > 20$ years		GMM difference	
	(1)	(2)	(3)	(4)
Regression				
Dependent variable	NRA	RRA	NRA	RRA
<i>Democratic reform</i>	4.972 (.001)	4.002 (.002)	7.798 (.000)	5.213 (.000)
<i>Lagged NRA (RRA)</i>	0.771 (.000)	0.775 (.000)	0.852 (.000)	0.878 (.000)
<i>Log per capita GDP</i>	13.655 (.000)	11.308 (.001)	50.915 (.002)	47.153 (.001)
Controls	Yes	Yes	Yes	Yes
Number of countries	61	55	74	68
Observations	2,364	2,151	2,439	2,194
$R^2$ (within)	0.695	0.707		
No. of GMM Instruments			52	52
Hansen test for over-id. ( <i>p value</i> )			.858	.920
AR2 test ( <i>p value</i> )			.279	.605

*Notes:* *p* values based on clustered standard errors in parentheses. Controls include *log per capita GDP*, *employment share*, *land per capita*, *log of population*, and year fixed effects included in every regression. GMM first difference based on `xtabond2` in Stata, with instrument lag structure (2 4) and collapse option to control for instrument proliferation and using forward orthogonal deviations instead of first differencing (see [Arellano and Bover 1995](#)).

*Source:* Own calculations based on the data described in the text.

for the well-known persistence of agricultural protection. However, because the lagged dependent variables in a fixed effects specification are mechanically correlated with the error term for  $N > T$ , we also use a first difference Generalized Method of Moments (GMM) estimator (see [Arellano and Bond 1991](#)). The inclusion of a lagged protection variable on the right-hand side may help to attenuate omitted variables bias because it captures accumulated (unobserved) factors that affect actual protection.

To reduce bias due to the contemporaneous presence of both fixed effects and the lagged dependent variable, we do not include countries for which fewer than 20 years of data are available in the dynamic D-in-D regressions. In addition, to render the regressions more comparable across dynamic estimators, the dynamic D-in-D specification does not include the continental-year interaction terms used in the static D-in-D regressions.<sup>26</sup>

The results of these additional regressions are reported in table 6. They are consistent with our previous findings. The democratization dummies are

26. Adding these continent-year interaction terms in the GMM equations induces a strong increase in the number of instruments, rendering it difficult, if not impossible, to have fewer instruments than groups and thus to respect the “rule of the thumb” when running GMM models (see [Roodman 2009](#)). However, note that cross-country differences in protection dynamics are now largely subsumed in the autoregressive coefficient.

consistently estimated with strong precision ( $p < .01$ ). As expected, the magnitudes of the estimated effects are lower than with the static model because we are now capturing only the short-term effect of democratization on agricultural protection. Moreover, the magnitudes of the democratization effects in the GMM first-difference regressions are even higher than those using the least-squares dynamic estimator.

### *Feedback Effects*

To further assess the problem of potential simultaneity bias, we regress the Polity2 democracy index on the level of protection in period  $t - 1$ . Specifically, we estimate the following democracy regression:

$$(3) \quad d_{it} = \alpha d_{it-1} + \varphi NRA_{it-1} + X_{it-1}\beta + \mu_t + \delta_i + \varepsilon_{it}$$

where  $d_{it}$  is the Polity2 democracy index of country  $i$  in period  $t$ . The lagged value of this variable on the right-hand side is included to capture the persistence of democracy. The parameter  $NRA_{it-1}$  is the lagged value of the protection level in agriculture. Other covariates are included in the vector  $X_{it-1}$ . The parameters  $\mu_t$  and  $\delta_i$  denote full sets of year and country fixed effects, respectively, and  $\varepsilon_{it}$  is an error term capturing all other omitted factors.

For the same reason given above, the model in equation 3 is estimated using D-in-D<sup>27</sup> and first-difference GMM estimators. Moreover, because democracy is a persistent variable, we run a system GMM regression (see [Arellano and Bover 1995](#)). We find that the lagged protection coefficient is always insignificant in these additional regressions (see the results in table S.4 in the supplemental appendix). Thus, these results suggest that there is no feedback effect of agricultural protection on the transition to democracy.

### *Definition of Regime Change*

The evidence presented thus far has been obtained from approximately 40 political reform episodes based on the Polity2 index. We have checked whether our results are driven by the specific definition of our political reform variable. We have employed three alternative approaches: (a) defining a democracy variable using *all* of the 67 reform episodes from Polity2; (b) using the data from [Papaioannou and Siourounis \(2008\)](#) and including only 23 (permanent) democratization episodes; and (c) only considering permanent transitions from Polity2, namely, those that lasted at least eight years. These democratization dummies differ in terms of not only the number of regime transitions considered but also the timing of the reform episodes (see table S.1).

27. We use a sample that excludes countries for which fewer than 20 years of data are available to reduce bias resulting from the contemporaneous presence of both fixed effects and the lagged dependent variable.

The results are presented in table S.5 in the supplemental appendix. The results remain robust using these different measures of democratic transitions. Moreover, the additional regression results suggest that permanent transitions are most important. In line with the dynamic results discussed above, temporary democratization episodes (i.e., in countries that revert to dictatorships after a brief democratization episode) have a significantly lower effect on agricultural protection.

### *Economic and Political Crises*

As noted in the recent political economy literature, the implemented policies may be related to both economic and political (in)stability (see [North et al. 2009](#); [Besley and Persson 2009](#) among others). Therefore, we complement our earlier specifications with three variables designed to capture the effect of economic and political crises. An economic crisis is measured with a dummy equal to one for every year that the real GDP per capita growth rate from the Penn World Table is negative (zero otherwise). A political crisis is measured with two dummies equal to one in every year a country is involved in a domestic war or international conflict (zero otherwise). All three variables are used in the regressions with several lags.<sup>28</sup> The effect of democratic reform on policy outcomes is very robust to the inclusion of these additional covariates (see table S.6).

## VII. CONCLUSIONS

In this paper, we investigate how democratization affects agricultural and food policies. On the basis of the unique data set collected by the World Bank, we empirically analyze the impact of political regime transitions on agricultural taxation and subsidization.

We find a significant positive (negative) effect of a democratic transition on agricultural protection (taxation). The transition to democracy increases agricultural protection by 10 to 18 percentage points, depending on the indicator and the model employed. This measured effect primarily reflects changes in poor countries, where the vast majority of the transitions from autocracy to democracy occurred and where farmers constitute a large share of the population. In the data set we used, the average share of agriculture in total employment at the time of the transition from autocracy to democracy was 65 percent (whereas the average share for all countries and time periods was 25 percent).

Our results are consistent with the predictions of the median voter model suggesting that the impact of democratization is conditional on the structure of

28. As correctly noted by a referee, these variables can serve as imperfect proxies, at best, for shocks in policy or world markets or variations in world prices. Nevertheless, they seem to be the best proxies available. Note that by using a dynamic panel model, as in table 6, we implicitly account for potential spurious correlations between democratization and protection due to (unobserved) policy shocks.

the economy, which determines the share of votes of farmers among all voters. The median voter model predicts that in poor countries where a large share of the population is involved in farming, democratic reforms induce a profarmer policy effect. The factors that make it difficult for farmers to organize politically in poor countries (such as their large number and substantial geographic dispersion) render them potentially powerful in electoral settings. Thus, our results suggest that democratization has benefited farmers in poor countries.

We also find that the short-term effects are smaller than the long-term effects. The effect of democratization on agricultural policies is strongest four to five years after a change in political regime. This finding suggests that time is needed to arrive at a new equilibrium in economic and political institutions.

An important question related to an empirical analysis such as ours is whether the relationship that we document is causal. We cannot rule out the possibility of spurious correlation due to various shocks that may have occurred over the past 50 years. We ran a number of extensions of the model and robustness checks to account for this possibility to the greatest extent possible. Our tests demonstrate that the results are robust to using different levels of commodity aggregation, different indicators to capture the timing of political reforms, alternative estimation methods, alternative definitions of regime changes, and additional variables.

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