ECONOMIC AND POLITICAL LIBERALIZATIONS

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Abstract

This paper studies empirically the effects of and the interactions amongst economic and political liberalizations. Economic liberalizations are measured by a widely used indicator that captures the scope of the market in the economy, and in particular of policies towards freer international trade (cf. Sachs and Werner 1995, Wacziarg and Welch 2003). Political liberalizations correspond to the event of becoming a democracy. Using a difference-in-difference estimation, we ask what are the effects of liberalizations on economic performance, on macroeconomic policy and on structural policies. The main results concern the quantitative relevance of the feedback and interaction effects between the two kinds of reforms. First, we find positive feedback effects between economic and political reforms. The timing of events indicates that causality is more likely to run from political to economic liberalizations, rather than viceversa, but we cannot rule out feedback effects in both directions. Second, the sequence of reforms matters. Countries that first liberalize and then become democracies do much better than countries that pursue the opposite sequence, in almost all dimensions.

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1. Introduction

In an assessment of the recent research on the effects of institutions on growth, the IMF concludes: "While the association between institutional quality and economic performance appears strong and robust, much more unsettled is the question of what lies behind these findings. (WEO, April 2003, chapter 3)". Contrasting the view that institutions are mainly determined by a country's geography, that is by its location on the earth, or by its history, for instance by the origin of the Europeans who first settled in the country, the IMF further observes: "The evidence that greater openness to trade and stronger competition are conducive to institutional improvement, and thus to growth, suggests that countries are not 'predestined', say by geography or history: the 'right' policies may shape institutions and through this cannel affect growth."

But if economic liberalization affects growth and institutions, what determines a country's decision to liberalize its economy? *Economic* liberalization, moreover, is just one dimension along which a country may open up, the other, and perhaps the most important one, being *political* liberalization, that is becoming a democracy. What are the relationships between these two forms of liberalization? Does one appear to "cause" the other? Do both affect growth and other economic policies? Are there positive interaction effects—that is, do the benefits from adopting economic *and* political liberalization exceed the individual effect that each of them would produce if adopted in isolation? These are the questions motivating this paper.

More precisely, the paper addresses four separate questions: (i) How do economic and political liberalizations affect economic outcomes such as growth and investment, macroeconomic policies, such as inflation and the budget surplus, and structural policies, such as indicators of protection of property rights and control of corruption ? (ii) Does economic liberalization "induce" political liberalization, is the causality running the other way, or are the two forms of liberalization unrelated? (iii) How do economic and political liberalizations interact, that is are the effects of adopting both forms of liberalization greater than the sum of the individual effects of the two, when adopted in isolation? (iv) Does the 'sequencing' matter ? That is: if a country that was originally closed and non democratic decides to open up in both areas, does where it starts from make a difference?

It is obviously not the first time these issues are addressed. Parts of the first question--the effects of economic and political liberalizations on growth and investment--have been addressed in the literature. Sachs and Werner (1995), and more recently Wacziarg and Welch (2003), have studied the effects of economic liberalization. A large literature, that includes Barro (1995), Prezworsky and Limongi (1993) and (2000), Roll and Talbott (2003) and Persson (2004) among others, has studied the economic effects of political liberalizations. However, with the exception of

Persson (2004), who focuses on the policy effects of different types of democratizations, economic and political liberalizations have been studied separately, thus missing the possibility that the two might interact. The main contribution of this paper is to study the interaction between the two types of liberalizations, focusing not only on the economic outcomes (growth and investment), but also on the effects on the quality of institutions that accompany or are induced by liberalizations.

We address these questions using data from a sample of about 140 countries, over the period 1960-2000. The variables we look at are the traditional ones considered in the literature on economic and political liberalizations, and are described in section 2. Our empirical methodology is adapted from the microeconometric literature on the effects of various treatments. Specifically, following Persson (2004), we estimate the effect of reforms using a difference-in-difference technique: this exploits both the cross country and the time series variation in the data, but with arguably weaker identifying assumptions than the typical exclusion restrictions employed by most of the macroeconomic literature on this topic. In this respect, our results provide new information even when we consider issues that have been studied before in the literature. The empirical methodology is illustrated in section 3.

Our empirical results are described in section 4. We start by studying the effects of each liberalization separately. Here we confirm the finding that economic liberalization is good for growth and investment; but this effect cannot be entirely attributed to international trade: economic liberalizations tend to be accompanied or followed by a host of other policy improvements, including an improvement in the budget surplus, better protection of property rights and lower corruption. The main effect of a transition to democracy, on the other hand, is to improve the quality of institutions (protection of property rights and control of corruption), but to deteriorate the macroeconomic environment, with only small positive effects on economic growth.

Studying the effects of each reform separately can be misleading, however, because it conceals possible feedback and interaction effects between the two kinds of reforms. The main results of this paper concern the quantitative relevance of these feedback and interaction effects. First, the data strongly suggest that indeed there are positive feedback effects between economic and political reforms. The timing of events indicates that causality is more likely to run from political to economic liberalizations, rather than viceversa: many economic liberalizations are preceded by political liberalizations, while the converse is observed less frequently--although we cannot rule out feedback effects in both directions. Second, the data also suggest that there are interaction effects between the two kinds of reforms: countries that enact both reforms have better economic performance compared to countries that enact only one kind of reform, and the effects are not additive. More importantly, the sequencing of reforms matters. Countries that first liberalize and then become democracies do much better than countries that pursue the opposite sequence. Section 5 briefly discusses our interpretation of this finding.

Thus, the main practical but tentative lesson of this paper can be summarized as follows. Consider a country that is closed both economically and politically, like China or Russia in the late 1980s. This country can follow two paths to economic and political liberalism. The "easy path" is to do what Russia did: first become a democracy and then try open up the economy. This route is "easy" in the sense that democratic governments are more likely to pursue economic liberalizations compared to dictatorships. But the economic payoffs are much higher for countries that do it the "hard way", namely who open up the economy while still being autocracies, and only then become democracies. In some sense, this is what China is trying to do. This route is harder in the sense that very few autocracies have pursued economic liberalizations; but those who did performed much better than the rest. The comparison between China and Russia, of course, fits this lesson very well.

2. The data

The sample consists of yearly data for about 140 advanced and developing countries included in the analysis of Persson (2004) and selected on the basis of data availability during the period 1960-2000.

2.1 Economic and political liberalizations

Our indicator of economic liberalizations is taken from Wacziarg and Welch (2003), who in turn have updated the earlier indicators compiled by Sachs and Werner (1995). A country is considered as closed to international trade if one of the following conditions is satisfied: (i) average tariffs exceed 40%; (ii) non-tariff barriers cover more than 40% of its imports; (iii) it has a socialist economic system; (iv) the black market premium on the exchange rate exceeds 20%; (v) much of its exports are controlled by a state monopoly. A country is open if none of these conditions applies. Throughout the paper we refer to an economic *liberalization* as the event of becoming open, given that a country was closed in the previous year. Thus, this measure of economic liberalization seeks to capture discrete and comprehensive policy changes that increase the scope of the market in allocating goods and services. Freer international trade is an important component, though not the only one, of economic liberalizations as defined here. Since we are less interested in the specific problems raised by transitions away from a socialist economic system, throughout the analysis we control for formerly socialist countries, as described below.

Sachs and Werner (1995) find that this indicator of openness is positively correlated with economic growth in the period 1970-89. The effect is very large and robust: economic liberalization increases average growth by as much as 2%. Following Rodriguez and Rodrik (2000) [1], Wacziarg and Welch (2003) update the Sachs and Werner index of economic liberalizations for the 1990s. The cross-sectional correlations are weaker in the 1990s: they find that an updated dummy for the 1990s is conditionally uncorrelated with economic growth across countries, so that the results in SW appear to be specific to their chosen time period. However, using the country-specific dates of liberalization—the same we use in this paper--and studying the within-country effects of liberalization, Wacziarg and Welch (2003) confirm that episodes of ecnomic liberalizations are followed by an increased trade volume, faster growth and an acceleration of investment. The effects on trade are significant over the entire sample (1950-1998), though weaker in the most recent period (1990-1998). This last finding suggests that announced trade reforms are not always associated with increases in trade: this will happen if, for instance, tariffs are replaced by other trade barriers, as was the case in India in 2000-01. Why "liberalizations" as defined by the Wacziarg and Welch (2003) dummy may not be accompanied by increases in trade volumes is one of the facts addressed in this paper.

Following Persson and Tabellini (2003), Persson (2004) and a large literature on the topic, we define a country as a democracy if it has strictly positive values of the indicator *POLITY2* in the POLITY IV database[2]. Throughout this paper, we refer to a *democratization* as the event of becoming a democracy, given that a country was not a democracy the previous year. The choice of 0 as the dividing line between democratic and non democratic regimes is suggested by the observation that *POLITY2* tends to jump discretely around zero. The standard deviation of this variable is 0.2 over the entire range (-10, +10 where the mean is 7.6) and 0.5 in the range (-3, +3 where the mean is 1.7). A cursory look at the time series data indicates that indeed crossing 0 is often associated with large and discrete improvements in institutions that take place over one or two years, while subsequent improvements in this indicator tend to be much more gradual. The same definition of democracy was used in previous studies, such as Persson and Tabellini (2003) and in Persson (2004).

[1] Rodriguez and Rodrik (2000) point out that the Sachs and Werner (1995) definition of being closed is dominated by the last two conditions (state monopoly in exports and black market premia). But see the reply in Werner (2003).

[2] *POLITY2* codes transition years by interpolating the variable *POLITY* from the years before to the years at the end of the transition. The variable *POLITY* in turn seeks to measure the quality of democratic institutions, on the basis of freedom of active and passive participation in elections, checks and balances on the executive, freedom of political association and respect of other basic political rights. It has been coded in the POLITY project (http://www.cidcm.umd.edu/inscr/polity/index.htm) precisely with the purpose of detecting changes in political institutions over time.

2.2 Performance measures

We consider three types of indicators of performance: (i) general economic outcomes; (ii) macroeconomic policies; (iii) governance indicators.

Our first and main question is whether economic and political liberalizations have an effect on general economic outcomes. Perhaps the ultimate indicator of economic performance is real per capita income, but for reasons that we discuss below, it is difficult to draw inferences about the causal effect of reforms on the level of income. Moreover, the time period we consider only lasts 40 years, and many reforms take place in the second half of this period. Hence, rather than studying the effect of reforms on the level of per capita income, we focus on its growth rate, defined as the first difference of the log of GDP per capita (*growth*). In addition, we also consider the investment rate, defined as the ratio of total investment to GDP (*investment*), and in some cases we also look at a measure of the relative size of international trade, defined as import plus exports over GDP (*trade*). The source for these three variables are the Penn World tables. For most countries, these variables are available for the whole period 1960-2000.

Our second question is whether economic and political liberalizations induce governments to choose (or are accompanied by) better macroeconomic policies. As indicators of macroeconomic policy, we consider the yearly rate of inflation, expressed in logs (*inflation*), and the central government surplus as a fraction of GDP (*surplus*). The source of these variables is the IMF. Inflation is available for the whole period for many countries, although for quite a few countries the series contains some non-contiguous years of missing observations. The variable *surplus* is available from the early 1970s onwards only, and for a few countries for a shorter period.

Finally, we ask whether economic and political liberalizations also induce governments to introduce new institutions or improve existing institutions, with the results of enhancing the protection of property rights or the protection from abuse by government. For this purpose, we include among our measures of performance two widely studied indicators of perception of good governance. The first, called *gadp,* summarizes perceptions of structural policies and institutional environments encouraging the production of output rather than its diversion (through theft, corruption, litigation or expropriation). This variable has been compiled by Knack and Keefer (1995) using ICRG data. It is available over the period 1982-97 and consists of a simple average of five indicators: two relate to the role of the government in protecting property rights against private diversion (law and order, and bureaucratic quality); the other three to the role of the government itself as a source of diversion (corruption, risk of expropriation and government repudiation of contracts). The variable *gadp* varies from 0 to 10, with higher values indicating better policies (more protection of property rights). As we are particularly interested in the role of regime changes in preventing abuse of power by government officials, we also consider one specific component of *gadp*, namely, perceptions of the control of corruption (*corruption*). This indicator (unlike *gadp*) varies from 0 to 6, again with higher values denoting better policies – i.e. less corruption. This variable too is only available from 1982 -1997, and its source is the same as for *gadp*.

3. Methodology

3.1 General econometric strategy

How can we estimate the causal effect of economic and political reforms on economic performance? Most existing macroeconomic literature has focused on one of two approaches. The simplest one is to estimate cross country regressions. Economic performance, or economic policies, are regressed on indicators of the political or trade regime.[3] The obvious problem here is that the estimated correlation could reflect an omitted variable or reverse causation. The typical solution is to find an instrument for the political or trade regime, as in Hall and Jones (1999). But good instruments are not easily available, particularly when it comes to democracy. Moreover, as discussed in Wacziarg and Welch (2003), cross-sectional regressions mask useful information from the time variation in the data. The second approach is to estimate panel regressions.[4] While exploiting also the time variation in the data, this approach too relies on restrictive and untestable identifying assumptions taking the form of exclusion restrictions.

In this paper, we follow the microeconometric approach. We define reforms as a "treatment" administered to some countries but not others, and estimate the causal effect of the treatment through a difference-in-difference estimation. This methodology, used in this context also by Persson (2004), allows us to exploit both the time series and the cross sectional variation in the data.[5] Specifically, we include in the analysis as many countries as possible: some experienced a reform during the period of observation, and are called "treated"; others had no reform during this period, and are called "controls". For instance, when studying the effect of economic liberalizations, the control countries are those that were always open or always closed during the relevant time period. We then compare economic performance in the treated countries, before and after the treatment, with the economic performance of the control group over the same time period. The estimation method thus exploits both the within-country variation as well as the comparison between countries. This has clear advantages relative to the simpler comparisons in isolation:

[3] Examples of this approach are Mulligan, Gil and Sala-i-Martin (2004) on the effects of democracy, Alesina, Spolaore and Wacziarg (2003) on the effect on trade volumes.

[4] Examples of this approach are Sachs and Werner (1995) on economic liberalizations, Barro (1996) or Prezworski and Limongi (1993) on democracy.

[5] In this respect, our methodology differs from both Wacziarg and Welch (2003) and Roll and Talbott (2003), who estimate the effect of economic and political liberalizations, respectively, only from within-country (i.e. before-after) comparisons.

exploiting the within-country variation only, risks confounding the effect of a treatment with that of unobserved variables that move all countries at the same time--a relevant possibility in our context because many economic and political liberalizations are clustered in the 1990s. Exploiting the cross-sectional comparisons only, can be even more misleading, because the omitted variable problem is daunting in this context.

Since reforms do not take place in all countries at the same time, to implement the difference-in-difference approach we estimate the following regressions in the whole sample of treated and control countries, where *i* subscripts refer to countries and *t* subscripts refer to years:

$$y_{it} = a_i + b_t + \gamma x_{it} + \delta \, reform_{it} + e_{it} \tag{1}$$

where y_{it} denotes the measure of performance, *a* and *b* are country and year fixed effects respectively, x_{it} is a set of other control variables, $reform_{it}$ is a dummy variable taking a value of 1 in the years after the reform in the treated countries and 0 otherwise (i.e , in the treated countries before the reform and in the control countries) and *e* is an unobserved error term. The coefficient δ measures the effect of the reform on the variable of interest *y*.

3.2 Identification

As explained for instance in Besley and Case (2000) or in Blundell and McCurdy (2000), the crucial identifying assumption in this difference-in-difference estimation is that there is no unobserved variable affecting performance that moves systematically over time in a different way between the treated and control groups. A violation of this assumption is more likely if the treated and control countries are very different from each other, because in this case any omitted time-varying variable, such as technological progress or increased globalization, could affect treated and control countries in very different ways. The identifying assumption could also be violated if reforms are not random and whatever triggers the reform also has a causal effect on performance; for instance, economic liberalizations might be systematically enacted by far sighted political leaders, who also promote sound economic performance in many other ways.

Both identifying assumptions are clearly restrictive, as is always the case in macroeconomics. Nevertheless, there are a number of steps we can take to reduce the likelihood of violation and to check their validity. First, by including in the control groups countries that are always open or always closed economically, or always democratic or non-democratic, we insure that the average control country is not very different from the average treated country. [6] Second, we

[6] To check this, we have estimated the probability of treatment (i.e. of undergoing economic or political liberalizations) as a function of some time invariant country features, namely continental location (being in Africa, Asia and Latin America) and socialist legal origin. Figure A1 in the appendix displays the histograms of the estimated probability of

always include in the vector *x* of additional controls a dummy variable for socialist legal origin interacted with the economic or political reform that we are studying. This makes sure that the estimated effects of reforms do not reflect the very special circumstances of the transition in formerly socialist countries. Moreover, we also always check that the results are robust to including in the vector *x* of additional controls the interaction between year fixed effects and time invariant variables that classify countries according to their continent (Africa, Latin America and Asia) and to socialist legal origin. Conditioning on this time varying variable makes countries more similar and thus reduces the likelihood of a violation of our identifying assumption – see also footnote 6 and Figure A1. Third, we check the estimated residuals of the control group (over the whole period) and of the treated group before the reform; a violation of the assumption that reforms are random is likely to result in systematically different time patterns of the estimated residuals between these two groups of countries. If we do not find clearly different patterns over time, we are reassured about the validity of our identifying assumption.

3.3 Implementation

Implementing this estimation strategy in our context requires addressing a few other problems. First, some reforms take place very close to the end of the sample for which we have available measures of performance. Since we expect that it takes some time for reforms to influence performance, we discard the reforms that took place in the last three years of the available sample. Specifically, we set to missing the observations of the dependent variables after a reform, if the reform is not followed by at least three additional years of data on performance. For instance, Burkina Faso liberalized its economy in 1998 and *growth* is only available until 2000. We have thus set *growth* to missing for Burkina Faso from 1998 onwards, and this country is thus considered a control (since it did not experience any liberalization before 1998). Since the pattern of available data differs depending on the measures of performance, this also implies that the groups of control and treated countries vary with our definition of performance. With regard to the beginning of the sample, we only require one available observation of performance before the reform took place, for a country to be classified as treated (since here delayed effects are not a problem).

Second, in a few countries we observe episodes of reversals in economic and political liberalizations. Reversals are more frequent for democratizations, particularly in a few African

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treatment (i.e. of having at least one reform) for different groups of countries: those who had no reforms, those who had only one reform, and those who had both. We find controls and treated countries close to both extremes of the estimated probabilities of treatment (the so called "propensity score"); that is we find a few control countries that were likely to experience some reforms but did not, such as Haiti, as well as several treated countries that were not very likely to receive treatment, such as Ireland with regard to economic liberalization, or Iran towards the end of the sample with regard to political liberalization. This reassures us that the two groups of countries are not too different from each other.

countries that start out as democracies upon becoming independent and then, after a few years, collapse into dictatorships. Some of these episodes of reversals or of democratization are very brief and last only a few years. To cope with this problem, we define treatment in two different ways. First, we only consider *permanent reforms*, that is uninterrupted reforms that are not reversed in the sample up to the year 2000. In this case we ignore temporary reforms that are subsequently reversed. The reason for doing this is that reversed liberalizations are in some sense incomplete reforms that failed in some important yet unobserved dimension. Here we are interested in the effects of the reforms that lasted. Of course, this might create a selection problem for the reforms that happen towards the end of the sample, for which a reversal might take place in the future but cannot be observed. Next, we define the treatment to include all reform episodes that last at least four years, *irrespective of whether they are temporary or permanent*. The restriction to at least four years of reform is imposed in light of the observation that the effects of the reform on performance do not occur suddenly.[7]

Last, some of our measures of performance, such as the rate of investment or corruption, move slowly over time. Despite the inclusion of year dummy variables, the residuals of our regressions for these measures of performance are likely to be serially correlated. Although this does not bias the estimated treatment effect, it could lead us to underestimate the true standard errors (see Bertrand, Duflo and Mullainathan, 2004). To cope with this problem, we always report also standard errors estimated with clustered regressions, that allow residuals to be correlated within each country block. In some specifications we also control for lagged per capita income or the lagged dependent variable, or we estimate by averaging the data over longer periods. We discuss these specification and estimation issues more in detail in the next section.

Finally, *Table 1* lists the sample of countries for which we have data on growth and on at least one of the reform indicators (democracy, and being economically open or closed). The table is split in three panels: panel A lists the control countries (those that were always open or always closed during the period in which data on growth are available); panel B lists the treated countries that had only one reform during the period in which data on growth are available--either political or economic liberalization; panel C lists all treated countries that experienced both reforms during the relevant time period. In each panel, the second and third columns report the date of their last

[7] In a few countries, reforms are enacted, then are interrupted for just a few years, and then are enacted again. If the reversal lasts three years or less, we neglect it and when coding all reform years (permanent and temporary) we code the reversal period as if it did not occur. Again, this is suggested by the logic that reforms (and reversals) need to last some time to show their effects. For instance, Albania became a democracy with available data on *growth* in 1992, and remained a democracy until the end, except for a one-year, 1996, during which democracy was interrupted. When we define treatment as a permanent reform, we code the treatment as having started in 1997 (the year of permanent democratization). When we consider all instances of democratization, we neglect the reversal of 1996 that lasted only one year, and we classify Albania as a democracy throughout this period (and hence we consider it a control country).

liberalization and of their last democratization (i.e. a permanent liberalization or democratization as defined above). A missing date means that no change in the relevant dimension was observed during this period.[8] About 85 countries had at least one episode of trade liberalization during 1960-2000 that was not subsequently reversed, while there are about 50 countries that have become democratic and had not reverted to autocracy by the year 2000. 32 countries experienced both reforms.

4. Results

First we study the effects of liberalizations and of democratizations in isolation. Then we study the feedbacks and the interactions between the two types of reform.

4.1 The effects of economic liberalizations

Table 2 reports the effects of economic liberalization on growth an investment. The control group consists of all the countries that, in our sample, did not go through a regime change as far as economic liberalization is concerned: that is, as explained in the previous section, the controls are the countries that remained either always closed or always open throughout the sample--or, more precisely, in the portion of our sample for which the dependent variable exists, here growth and investment.

Table 2 should be read as follows (the same holds for Tables 3 through 7). The variable *lib* is a dummy variable equal to 1 in the post-liberalization years for the treated countries only. Its estimated coefficient captures the average effect of the reform. The first columns, labelled "permanent" in the fourth-but-last row, only consider permanent liberalizations, that is liberalizations that last until the end of our sample. The columns labelled "all" consider instead all liberalization episodes, including those that were eventually reversed, provided they last longer than 3 years. For each regression we report two standard errors, those from the OLS regression (above) and those for the clustered regressions (below). As explained in the previous section, all regressions include country fixed effects and year dummy variables, as well as the dummy variable for socialist legal origin interacted with the reform dummy variable. In columns 2 and 5, as well as columns 7 and 10, we also control for year dummy variables interacted with dummy variables for continental location (Africa, Asia and Latina America) and for socialist legal origin.

Table 2 shows that economic liberalizations speed up growth by about 1% and raise the share of investment by almost 2% of GDP. The effects of permanent and temporary liberalizations are not very different—if anything, temporary liberalizations seem to have a larger effect on growth and investment than those that are not reversed. These estimates are similar to those obtained by

[8] For a few countries only, a missing observation means that the economic or political regime could not be classified based on available data.

Wacziarg and Welch (2003), who only consider treated countries and compare the periods before and after the reform.

Columns 3 and 8 investigate the timing of these effects, by replacing the variable *lib* with a dummy variable equal to 1 in the three years preceding the reform (*3y_pre_lib*), a dummy variable equal to 1 in the year of the reform and in the three following years (*3y_post_lib*), and a dummy variable equal to 1 from year 4 after the reform and onwards (*4yon_post_lib*). Liberalizations seem to be triggered by crisis: they occur at the end of a period during which the economy grows less than usual (about 1 percent below trend growth), and investment is unusually low. Moreover, the positive effects of liberalization take at least 4 years to show up. Note that the estimated coefficient of the variable (*4yon_post_lib*) captures the difference between average economic performance four years after the reform and the default years (i.e. the control countries and the treated countries in the years that precede the reform by more than three years). Thus, after four years or more, not only is the crisis overcome, but economic performance is significantly better than before the crisis.

If reforms are preceded by a crisis, is our identification assumption at risk? Not necessarily, unless one believes that something else happened during or after the crisis (other than the economic reform itself), which in turn is responsible for the observed improvement in economic performance four years or more down the line. On the contrary, this time pattern suggests that the improvement in economic performance certainly did not start before the reform was implemented, and thus if anything it reinforces a causal interpretation of the estimates. We return to a discussion of the identifying assumptions in subsection 4.3 below.

The finding that reforms are preceded by crisis raises yet another concern: could the growth and investment acceleration after the reform simply reflect economic convergence once the crisis is overcome? To answer this question we re-estimated the equation including lagged per-capita income among the regressors. If the growth or investment acceleration four years after the reform was just due to the income loss suffered during the crisis years, it would be captured by this new variable. To avoid the bias due to the inclusion of lagged per-capita income in a panel regression with country fixed effects, we discarded all countries for which less than 21 years of data are available – this left us with 100 countries and an average panel length of about 30 years per country. The estimated effect of liberalization on growth and investment was very similar to that reported in Table 2, for all specifications.

As a final check against spurious dynamic effects, we also re-estimated the model with a two-step procedure suggested by Bertrand, Duflo and Mullainathan (2004) to cope with serially correlated residuals. First, we estimated the residuals of a panel regression of economic performance (growth or investment) against country and year fixed effects (in some specifications we also included year dummy variables interacted with continental location and socialist legal origin), for the whole sample of countries (treated and controls). Then we retained only the treated countries and computed the average of the residuals before and after the last unreversed reform. To have a long enough time average, we discarded the spells (before or after the reform) that lasted less than 10 years. Under the null hypothesis that economic liberalizations have no effect on economic performance, the averaged residuals should be the same before and after the reform. We could always reject this null hypothesis, finding that economic liberalizations improve economic performance.

Table 3 documents the effect of economic liberalization on *gadp* and corruption. Remember that *gadp* is an index ranging between 0 and 10, while corruption ranges between 0 and 6. Liberalizations appear to be associated with improvements in the quality of these structural policies. The estimated effect is generally significant, particularly for *gadp*, but it is relatively small, never exceeding 0.6. Again, we find that the effects are delayed by at least 3 years. But since the dependent variables measure *perceptions* of good policies, these delayed effects cannot be interpreted as causal. Rather, a more natural interpretation is that economic liberalizations are simultaneously accompanied by improvement in structural policies, and the perceptions improve a few years after new and better structural policies are in place. These episodes of economic reforms probably correspond to the implementation of a cluster of good policies, of which opening up to international trade is but one aspect.

This general interpretation is also suggested by the estimates in Table 4, that look at the effects of economic liberalizations on macro policies. Following an economic liberalization the budget surplus improves by some 1.5 per cent of GDP - - here too the effects seem somewhat delayed. Inflation however, does not appear to be affected by economic liberalizations, although these tend to happen at the end of a period during which inflation was unusually high.[9]

We further discuss our identifying assumptions in section 4.3 below. But before doing that, we study the effects of political reforms.

4.2 The effects of democratizations

Tables 5-7 repeat the analysis for the same dependent variables and with exactly the same structure, but defining the reform as the event of becoming a democracy. Here the control group includes all the countries that were either always democratic or always non-democratic.

[9] In Tables 3 and 4 we generally do not have a long enough time period to estimate dynamic equations with lagged dependent variables or with the two-step procedure suggested by Bertrand, Duflo and Mullainathan (2004). The only exception is inflation, for which we have 91 countries with 21 years of data or more. Including a lagged dependent variable and estimating the effect of liberalization on inflation yields a negative and significant estimated coefficient, suggesting that inflation goes down after economic liberalization.

In Tables 5 the dependent variables are growth and the investment rate. Democratic transitions are associated with small improvements in economic performance. The effects are generally too small to be statistically significant, however, except when we consider all political reforms (rather than permanent reforms only) – cf. columns 4, 5 and 9. Columns 3 and 8 study the timing of these effects. As for economic liberalization, the event of becoming a democracy is preceded by a slowdown in growth and investment--though the estimated coefficients are not statistically significant. The results are very similar if we include lagged per-capita income among the regressors (disregarding the countries for which less than 21 years of data are available): the estimated effect of becoming a democracy is positive and about the same order of magnitude as in Table 5, but it is statistically significant only when considering all democratizations. The two step procedure described above and suggested by Bertrand, Duflo and Mullainathan (2004) yields statistically insignificant estimates.

Overall, these estimates tend to confirm previous results in the literature, that found no robust effect of becoming a democracy on economic performance, although they point to small positive effects of democratizations, leaving some room for an optimistic assessment about the effects of becoming a democracy.

Tables 6 shows that political liberalizations, improve *gadp* and corruption with a lag, though again by relatively small amounts. The effects on corruption are typically stronger than those for *gadp*. The order of magnitude is about the same as for economic liberalizations.

Finally, Table 7 shows that democratizations are associated with ambiguous effects on macroeconomic policy: inflation rises but so does the budget surplus. The timing of these effects, illustrated in columns 3 and 8, is puzzling however: both inflation and the budget surplus are already higher up to three years before democratization, relative to the default observations. This suggests that the identifying assumption might be violated, since the policy changes might precede the political reform.[10]

4.3 Discussion

The results up to this point can be summarized as follows. Economic liberalization is good along all dimensions: it is accompanied by better structural policies and better macroeconomic policies, and it is followed by improved economic performance. This timing suggests a causal interpretation, at least with regard to economic outcomes. Political liberalization, on the contrary, do not have strong and robust effects on growth and investment, though they appear to improve structural policies and

[10] Adding a lagged dependent variable to the inflation regressions, or estimating with the two step procedure discussed above, yields small positive coefficients of democratization on inflation, which are significant in some but not all specifications.

they yield mixed results on macroeconomic policies. These findings confirm with a new methodology previous results in the literature about the effects of economic and political liberalizations on growth and investment, and add some new insights on other policy variables.

As anticipated in section 3, the identifying assumption behind these estimates is that there is no unobserved time varying variable that affects performance in the treated and control groups differently. To check that this assumption is not clearly inconsistent with the data, Figures 1 and 2 plot the average estimated residuals in each year, for the control group and for the treated group before the corresponding reform (permanent liberalization in Figure 1, permanent democratization in Figure 2).[11] The specification is the more comprehensive one, inclusive also of year fixed effects interacted with continental location and socialist legal origin. Under the identifying assumption, the residuals for these two groups of countries ought to be similar, up until the time of the reform. But this is not what we find. Only in one case (the growth regression when the treatment is economic liberalization) the two groups of countries exhibit very similar time patterns. In all other cases the dependent variable for the group of treated countries before the 'treatment' appears to behave somewhat differently from that for the control group. The difference is particularly pronounced towards the end of the sample, when the number of treated countries becomes very small because more and more countries have taken the treatment.

These figures suggest two possible sources of bias in these 'single treatment' regressions. The 'treatment', that is economic or political liberalization, did not happen randomly, but at the end of a period during which a country that eventually opened up, along one or the other dimension, behaved in a systematically different way from the control group—for instance was investing more, or less, than the controls. If the reform does not happen randomly, then our results could be affected by a selection bias—for instance we could find larger investment after economic liberalizations simply because the countries that opened up were already investing more than the group of control countries. Alternatively, the bias could be the result of having omitted one or more variables correlated with both performance and treatment. This second problem is particularly relevant if both reforms tend to be undertaken simultaneously, or if one type of reform induces the other. If so, omitting one of the two treatment variables biases the estimated effect of the included one—for instance we may attribute an improvement in *gadp* to economic liberalization, while it is really the effect of the transition to a democratic regime which accompanies economic liberalization.

[11] In interpreting these figures, one should bear in mind that the treated and control groups vary in each diagram, and that the treatment date is different for different countries. Moreover, as time progresses, the group of treated countries becomes smaller (because more and more countries have taken the treatment), while the size of the control group in each diagram remains constant over time. For each dependent variable, the residuals in Figure 1 are estimated from the *second* column in each of the panels in Tables 2-4, while the residuals in Figure 2 are estimated from the *second* column in each of the panels in Tables 5-7.

Motivated by these concerns, we now consider the feedback effects between economic and political liberalizations, as well as possible interactions in their effects on the performance indicators.

4.4 Effects of economic liberalizations on democracy, and viceversa

We start by studying the feedback effects between economic and political liberalizations. That is, we first ask whether one reform appears to 'cause' the other.

A priori, the feedback effects could go in both directions and are likely to reinforce each other. Trade tends to benefit many, and hurt a few: it thus seems more likely that a democratic regime shifts the balance in favour of freer trade. It is also possible, however, that a liberalized economic regime fosters a transition towards democracy, for instance because it increases the economic well being and the economic power of the middle classes (see for instance Acemoglou and Robinson, 2004 and Rajan and Zingales, 2003).

The results are displayed in Table 8. Here the dependent variables are, respectively, the continuous variable *POLITY2,* that varies from -10 to +10 and measures the democratic quality of the political regime (higher values being better democracies), and the 0-1 index of economic liberalization. In the regression in which the dependent variable is the quality of democracy, the treatment is defined as the economic reform and the control group includes all the countries that never changed their economic regime. Viceversa, when the dependent variable is being economically open, the treatment is democratization and the control groups consists of all countries that never changed their political regime.[12]

The first lesson from Table 8 is that feedback effects are generally important. The estimated coefficients are often positive and significant both when we ask whether economic liberalization affects political liberalization, or the other way around. Investigating the effects of these two reforms in isolation, as commonly done in the literature, may thus result in biased estimates of their effects.

The timing of these feedback effects is very different for the two reforms, however, and suggests that causality is more likely to run from political to economic liberalizations rather than viceversa. Economic liberalizations (the left-hand-side panel of Table 8) do not appear to lead the transition to a democracy: as shown in columns 3 and 5, the quality of democracy is higher *both before and after* the date of economic liberalization. In particular, there is no evidence that *POLITY2* is higher in the years following economic liberalization, compared to the 5 preceding

[12] In columns 6-8, where we consider the effects of permanent democratizations, the dependent variable is defined as being permanently open ; in columns 9 and 10, where we consider the effect all democratizations (permanent and temporary), the dependent variable is being open (irrespective of whether or not there has been a reversal).

years. Note that this result is obtained both for permanent democratizations (column 3) as well as for temporary ones (column 5).

Democratizations, on the contrary, appear to lead economic liberalization (see the right-hand-side panel of Table 8). The index of economic liberalization rises over time in the years following the transition to a democratic regime, thus suggesting that political liberalization 'induces' economic liberalizations much more than the other way around. Since the dependent variable here is either 0 or 1, the coefficients in the right hand panel of Table 8 can be interpreted as effects on probabilities—that is, for instance, the coefficient 0.32 in column 8 means that over 4 years after the transition to a democracy the probability that a country will open up has increased by 32 per cent – a large effect indeed.

A cursory look at the data in Table 1 also suggests that the direction of causality is more likely to go from political to economic liberalizations rather than viceversa. As shown in panel C of Table 1, among the countries that undertook both reforms in the period 1960-2000, as many as 23 countries first became democracies and then opened up the economy, while the opposite sequence is observed in only 9 countries. Moreover, countries that first became democracies opened up the economy after about 4 years on average, while for the opposite sequence the average distance between the two reforms exceeds 9 years, suggesting that these two reforms are less closely related in this second group of countries.

Despite these remarks, other features of the data suggest that the feedback effects could go in both directions. Figure 3 displays the estimated residuals for the control countries and for the treated countries before the reform. In the top panel, the dependent variable is economic liberalization, and the treatment is becoming a (permanent) democracy; in the bottom panel, the dependent variable is the quality of democracy as measured by *POLITY2*, and the treatment is (permanent) economic liberalization.[13] If the direction of causality ran exclusively from democracy to economic liberalization, in the top panel the residuals for the control countries and the treated countries before the political reform should display similar patterns. But this is not what we find. In particular, with reference to the upper panel of Figure 3, where the dependent variable is economic liberalization, the residuals from the treated group before political reform display a positive trend towards the end of the 1990s. This suggests that this group of countries was more likely to open up in the 1990s, quite independently of the prior transition to a democracy. Indeed, most cases of economic liberalization that are preceded by political liberalization happen in the 1990's; when we exclude this decade from the sample the estimated coefficients in Table 8 drop and become negative or

[13] Here too, as in the previous figures, the specification includes also year fixed effects interacted with dummy variables for continental location and for socialist legal origin. For each dependent variable the residuals are thus estimated from the *second* column in each of the panels in Table 8.

statistically insignificant, and the evidence of a 'causal' link between the two forms of liberalizations disappears.

Overall, we are thus led to conclude that the positive feedback between economic and political liberalizations could run in both directions, and that it is difficult to ascertain a precise direction of causality between economic and political reforms. This suggests that we ought to study the effects of the two liberalizations jointly; this is what we do in the next section.

4.5 Interactions between political and economic liberalizations

The control group now consists of all countries that have never changed either political or economic regime, and we allow for multiple treatments: only economic liberalization, only democratization, or both.

The question of whether there are complementarities or other interactions between different types of reforms is of independent interest, beyond addressing the identification problem discussed in the previous section. We would like to know if the *joint* adoption of both reforms enhances the sum of the individual effects, and if the sequence of reforms matters. But there is also a more technical reason for allowing the estimated coefficients to differ depending on the number and sequence of reforms. If we imposed a priori the same coefficients on the reform dummy variables for all countries irrespective of the number and sequence of reforms--while the true effects are heterogeneous--the error term would pick up part of the heterogeneous treatment effect; with multiple treatments (and hence multiple dummy variables for economic reform in the same country), this would create a correlation between the error term and the reform dummy variables in the countries that experienced both treatments, leading to biased estimates.

For these reasons, in this subsection we partition the countries in mutually exclusive groups and we estimate a specification that includes the following dummy variables for reforms:

- two dummy variables equal to one after political (economic) liberalization in the countries that only changed their political (economic) regime, leaving the other unchanged throughout the sample. These variable are labelled *dem_1t*, for democratization-1-treatment-only, (*lib_1t*, for liberalization -1-treatment-only).
- two dummy variables equal to one after political (economic) liberalization in the countries that enacted both reforms, that is liberalized the economy and also introduced democratic institutions. These variable are labelled *dem_2t*, for democratization-2-treatments, (*lib_2t*, for liberalization -2-treatments).
- two dummy variables equal to one after the *second* reform only, depending on the sequence of the reform. The variable *lib_after_dem* is equal to one after the second reform only,

and only for countries that first became a democracy and then liberalized the economy; it is zero in all other cases. Likewise, the variable *dem_after_lib*, is equal to one after the second reform for countries that first liberalized the economy and then became a democracy, otherwise it is zero. If the estimated coefficients of both variables are zero, then it means that there are no interaction effects (i.e. the effect of reforms as captured by the dummy variables *dem_2t* and *lib_2t* are additive), and sequencing does not matter.[14]

Thus, the effect of reforms in countries that undertook both reforms should be read as follows. Consider a country like Mexico, that first opened up the economy and then became a democracy. When it liberalizes the economy, the effect on economic performance is given by the estimated coefficient of the variable *lib_2t*. When it then becomes a democracy, the effect is captured by the algebraic sum of the coefficients of *dem_2t* and *dem_after_lib*. Conversely, consider a country like Argentina, that followed the opposite sequence: first it became a democracy and then it opened up the economy. The effect of the first (political) reform is captured by the estimated coefficient of the variable *dem_2t*. The effect of the second (economic) reform, instead, is captured by the algebraic sum of the coefficients of *lib_2t* and *lib_after_dem*.

As in the previous subsections, we always include a dummy variable for socialist legal origin interacted with a dummy variable for political liberalization and with a dummy variable for economic liberalization, to isolate the effects that are due to the special case of transition economies. Given that here we seek to extract more information from the data, we pay more attention to the other conditioning variables; in particular, we always include year dummy variables interacted with dummy variables for continental location and socialist legal origin. We also report the estimates for a variety of alternative specifications and estimation methods.

Figure 4 illustrates the estimated residuals in the usual way, for the control countries and for the treated countries before the first (permanent) reform.[15] With this richer specification and this definition of multiple treatments, the control and the treated groups now display a very similar behaviour, confirming that here the identifying assumption seems consistent with the data.

Consider Table 9 first, where the dependent variables are growth and the investment rate. The first two columns of each panel (columns 1-2 and 6-7) report our basic estimates for permanent and all reforms respectively. Columns 3-4 and 8-9 add lagged income (also for permanent and all reforms respectively), to control for possible convergence dynamics; to reduce the impact of the lagged dependent variable bias in fixed effects estimation, here we discard all countries with less

[14] One country, Paraguay, undertook both reforms in the same year; we thus set both variables, *lib_after_dem* and *dem_after_lib*, equal to 1 after both reforms for Paraguay.

[15] For each dependent variable, the residuals are estimated from the *first* column in each of the panels in Tables 9-11.

than 21 years of data. Since serial correlation in the residuals is less likely to be a problem in these regressions, we only report standard errors estimated by OLS.

In the first two rows we report the estimated coefficients of the variables *dem_1t* and *lib_1t*, referring to the countries that opened up in only one dimension. Here we see that becoming a democracy either has no effect on economic performance, or if anything it has a negative effect. Economic liberalization instead has a positive effect on economic performance, except in columns 1 and 3 where we confine attention to growth and to permanent reforms and where the effect is insignificant. These estimates thus roughly confirm the findings already discussed in the previous subsections, when considering each reform in isolation, although the effects here are generally weaker.

In rows three and four we report the estimated coefficients of the variables *dem_2t* and *lib_2t*, referring to the countries that undertook both reforms. As explained above, these coefficients capture the effect of the reform that came first (democracy or economic liberalization, depending on the sequence). Once more, economic liberalization has strong positive effects on growth and (when all reforms are included) on investment. Becoming a democracy has no effects on investment, but leads to growth accelerations. Compared to the countries that opened up in only one dimension, the effects here are generally stronger and more likely to be positive, particularly for democracy. Compared to the results described in the previous section where we considered each reform in isolation, we confirm that economic liberalizations induce economic improvements, but we now find stronger positive effects from becoming a democracy.

Finally, rows six and seven report the estimated coefficients of the variables *lib_after_dem* and *dem_after_lib.* As explained above, these variables capture possible interaction effects between the two reforms and discriminate among countries on the basis of the sequencing. These estimated coefficients are generally different from zero, suggesting the presence of interaction effects, although with opposite signs on growth and investment.

The overall effect of the last reform (democracy or economic liberalization, depending on the sequence) can be obtained by the algebraic sum of the estimated coefficient of *dem_2t* and *dem_after_lib* (if the sequence was first economic liberalization and then democracy), or by the sum of *lib_2t* and *lib_after_dem* (under the reverse sequence). These algebraic sums indicate that, when the second reform is enacted, investment accelerates further while growth is not affected or might even fall. But here the effects are stronger for democracy than for economic liberalization. When the second reform is democracy, growth is not affected but investment accelerates by 2-3% of GDP, depending on the specification. When instead the second reform is liberalization, growth either falls or remains unaffected and investment rises but by less (about 1.5% of GDP).

Thus, the central new lesson from Table 9 is that the sequence of reforms matters a lot. Opening up the economy first and then becoming a democracy gives better results than the opposite sequence. This can be seen directly by comparing the estimated coefficients of *lib_after_dem* and *dem_after_lib*. In the growth regressions, the estimated coefficient of *lib_after_dem* is always negative and significant, while the estimated coefficient of *dem_after_lib*, is not significantly different from zero (and sometimes it is even positive). In the investment regressions, the estimated coefficient of *dem_after_lib* is always positive and statistically significant, while the estimated coefficient of *lib_after_dem* is generally not significant and sometimes it is even negative. Thus, although the sign of the interaction effects is different on growth vs investment, both regressions imply that countries that open up the economy first perform better compared to countries that enact the opposite sequence. Opening up the economy first gives two boosts to economic performance: the first one at the time of economic liberalization; and then a second one, on investment, when the country becomes a democracy. Becoming a democracy first, instead, gives more disappointing results: there is some acceleration of growth (but not of investment) at the time of democratization; but later on, when the economy is liberalized, the positive effects of liberalizations tend to vanish or are smaller compared to the countries that enacted the two reforms in reverse order.

As a final robustness check, columns 5 and 10 of Table 9 report the two step estimates obtained with the procedure suggested by Bertrand, Duflo and Mullainathan (2004) and described above. First we estimate the residuals of a panel regression of economic performance (growth or investment) against country and years fixed effects and the year dummy variables interacted with continental location and socialist legal origin, for the whole sample of countries (treated and controls). Then we retained only the treated countries and computed the country average of the residuals under three sub-periods: before any reform, after the first reform and before the second reform, and after the second reform (whenever it took place).[16] To have a long enough time average, we discarded the sub-periods lasting less than 10 years; we were left with 110 observations corresponding to an unbalanced panel of at most three periods for the treated countries. We then regressed theses remaining averaged estimated residuals on the same set of dummy variables used on yearly data and described above.[17] As shown in columns 5 and 10, the resulting estimates confirm the importance of the sequence of reforms.

Table 10 reports the same set of estimates for two other dependent variables: international trade (defined as the volume of trade in percent of GDP) and the rate of inflation. The structure of Table 10 and the estimation procedures are identical to those of Table 9, except that in columns 3-4

[16] Here we always refer to permanent (i.e. unreversed) reforms.

[17] As for the yearly data, in this second step we also controlled for socialist legal origin interacted with the political and economic reforms.

and 8-9 we now include the lagged dependent variable (rather than lagged per-capita income); thus, the estimated coefficients in these columns capture the short run effects of the reforms.

The left hand panel of Table 10, on international trade, helps to understand why the sequence might be important. Under the “good” sequence, economic liberalization gives a big boost to trade (the estimated coefficient of *lib_2t* is always positive and generally highly significant), with a second smaller boost once the country becomes a democracy (the algebraic sum of *dem_2t* and *dem_after_lib* is positive and significant in the first two columns). Under the “bad” sequence, becoming a democracy reduces, if anything, trade volumes (the estimated coefficient of *dem_2t* is often negative and generally insignificant), and economic liberalization has negligible effects on trade (the algebraic sum of *lib_2t* and *lib_after_dem* is close to zero). The right hand panel of Table 10, on inflation, suggests a second way in which the sequence seems to matter. Repeating the same steps, we see that economic liberalizations induce a fall in inflation in the countries that open up the economy first, but this does not happen if economic liberalizations follow democratizations. Table 10 thus suggest that there are two types of economic liberalizations: those that are associated with improvements in trade and better macroeconomic policy, and those that are not. Economic liberalizations that are enacted after a country has become a democracy are less effective at boosting trade volume and are accompanied by worse macroeconomic policies. This might be one channel through which the sequence of reforms matters.

Table 11 repeats the analysis for the budget surplus and structural policies (*gadp* and corruption). Since a shorter time series is available for these dependent variables, here we do not attempt to also control for a lagged dependent variable or to estimate via the two-step procedure. Thus, we only report the usual set of estimates on yearly data. The estimates on the budget surplus provide yet more evidence that the sequence matters: economic liberalizations enacted after becoming a democracy are associated with smaller improvements in the budget surplus, compared to economic liberalizations that come first. The results on *gadp* and corruption instead suggest that economic and political reforms seem to have additive effects, confirming the results obtained when considering each reform in isolation. Here the sequence seems unimportant, although the countries that enact both reforms do better than the countries that enact only one of them.

Finally, we address one last question. Consider countries that had only one reform in our sample period, 1960-2000. Some of them were closed in the other (non-reformed) dimension, others were open. If the sequence of reforms matters, could the effect of the observed reform differ depending on whether the country was open or closed in the other dimension? To answer this question, we split the dummy variables *lib_1t* and *dem_1t* into a finer partition, allowing the effect of *lib_1t* to differ between democracies and non-democracies, and the effect of *dem_1t* to differ between countries that were economically open or closed. Here the comparison did not yield conclusive results. One reason could simply be lack of data: among the countries experiencing only one reform, only three countries (Central African Republic, Iran and Malawi) became democracies in closed economic environments, and relatively few opened up the economy while remaining dictatorships throughout. In most other cases, the reform took place in countries that were already open in the other dimension. A second possibility is that the distance between the two reforms in this group of countries was so large to make them incomparable to the countries undertaking both reforms in a closer sequence. Whatever the reason, the inference that the sequence of reforms matters is only supported by the sample of countries that undertook both reforms during the observed sample period.

5. Why sequencing might matter?

The main lesson we learn from the joint study of economic and political liberalizations is that the sequence matters. Countries that first liberalize the economy, and then make the transition to a democracy, do better, in terms of growth, investment, trade volume and macro policies, than those that adopt the two reforms in the reverse order. This finding can be interpreted in two alternative and non-mutually exclusive ways.

One possibility is that economic liberalizations enacted first are more effective. This interpretation is suggested by the findings on trade volume and inflation: economic liberalization first is associated with a sharp increase in trade volumes, both at the time of economic liberalizations and then again later on, when the country becomes a democracy; instead, economic liberalizations that are preceded by transitions to democracy have much smaller effects on trade. Similarly, the reduction in inflation is only observed after an economic liberalization, if this comes first. The type of economic liberalization a country adopts thus seems different depending on whether it is, or it is not, a democracy. Democracies do tend to liberalize the economy, but trade does not expand, suggesting that although the economy is formally open, protection remains pervasive, or new non-tariff barriers are introduced to replace formal tariffs. This is not the case when the liberalization takes place in a dictatorship. "Dictators" are less likely to open up the economy, as suggested by the fewer cases of economic liberalizations under dictatorships. But those who do--for instance Chile in 1976, or Guyana in 1988, or Mexico in 1986--if they decide to open up, it is because they are able to crush the interest groups that oppose free trade and a market system. Hence liberalization is more pervasive and effective, and less bogged down by compromises.

The other possibility is that democratizations, when enacted in an open economic environment, produce "better" democracies. There are several reasons why this might be the case. One reason is that liberalization speeds up growth and introduces a more competitive environment. Eventually, when the country gets rid of the dictator and becomes a democracy, it is in some sense, a better democracy. First, because it is now open to trade and competition; second because, having grown faster for some time, it now has the resources for the redistribution that a democracy requires. A young democracy in a closed economic environment, instead, is more likely to bogged down in redistributive conflicts and be more unrestrained in the pursuit of populist and inefficient policies. A second reason might be that the sequence economic liberalization followed by political liberalization might indicate the presence of a controlled and pre-planned liberalization enacted by a far sighted leader. When democratization comes first, instead, it is more likely to be unexpected and result from violent struggles or collapses of state authority. As such, it is more likely to be associated with economic disruptions and redistributive struggles. The data lend some support to this interpretation as well. Democratizations that follow liberalizations seem to give an additional boost to investment and trade volumes, perhaps because they give more confidence that the open economic environment will last over time. This does not happen when democratization comes first. Moreover, when democratization comes second, we tend to observe a more gradual improvement in the quality of democracy (as measured by the variable *POLITY*) in between the two liberalization episodes – a sign of a more controlled reform process.

What does all of this imply for a country that is closed economically and politically and that is contemplating economic and political reforms, or for a new-born country e.g. like Iraq? If reforms could be administered like medical treatments, then the answer would be clear cut. Economic liberalization should come first and receive the strongest priority; only afterwards should the country worry about political reform. But reforms are not ordered by a doctor and the data do indicate that autocrats are unlikely to open up the economy. Indeed, most economic liberalizations tend to be preceded by political reforms, perhaps imposed by a struggling population on an unwilling leader. In this case, the sequence of reforms cannot be chosen ex-ante and the path to reform might be less effective from an economic point of view.

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Table 1: Countries and years of permanent democratizations and liberalizations

(Treatment refers to growth regressions)

A. Countries that received no treatments (controls).

In this table some countries are defined as controls even if a liberalization or democratization year appears: it means that the dependent variable was not available in the year of permanent liberalization or democratization, or that the reform took place so late in the sample that the last few observations of the dependent variable were discarded.

Country	*year of perm. liberalization*	*year of perm. democratization*	*Country*	*year of perm. liberalization*	*year of perm. democratization*
Algeria	.	.	Lesotho	.	.
Angola	.	.	Lithuania	1993	.
Austria	1960	.	Luxembourg	.	.
Azerbaijan	1995	.	Malta	.	.
Belarus	.	.	Moldova	1994	.
Belgium	.	.	Namibia	.	.
Bulgaria	1991	1990	Netherlands	.	.
Burkina Faso	1998	.	Nigeria	.	1999
Burundi	1999	.	Norway	.	.
Canada	.	.	Pakistan	.	.
Chad	.	.	Papua New G.	.	.
China	.	.	Russia	.	.
Comoros	.	.	Rwanda	.	.
Congo	.	.	Senegal	.	2000
Croatia	.	1999	Sierra Leone	.	.
Cuba	.	.	Slovak Rep.	1991	.
Czech Republic	1991	.	Slovenia	1991	.
Denmark	.	.	Swaziland	.	.
Eq. Guinea	.	.	Sweden	1960	.
Estonia	.	.	Switzerland	.	.
Finland	1960	.	Syria	.	.
France	.	.	Tajikistan	1996	.
Gabon	.	.	Togo	.	.
Georgia	1996	.	Ukraine	.	.
Germany	.	.	United Kingd.	.	.
Haiti	.	.	United States	.	.
Hong Kong	.	.	Uzbekistan	.	.
Iceland	.	.	Vietnam	.	.
India	.	.	Yemen	.	.
Italy	.	.	Zaire	.	.
Kazakhstan	.	.	Zimbabwe	.	.
Kyrgyzstan	1994	.			

B. Countries that received one treatment only: democratization

In this table, as in the previous one, some countries are defined as having received only one treatment even if a year appears for both liberalization and democratization: it means that the dependent variable was not available in one of those years, or that the reform took place so late in the sample that the last few observations of the dependent variable were discarded.. The same applies for the following table.

Country	*year of perm. l iberalization*	*year of perm .democratization*
Albania	1992	1997
Cambodia	.	1998
Central African Rep.	.	1993
Cyprus	1960	1968
Fiji	.	1990
Greece	.	1974
Iran	.	1997
Malawi	.	1994
Portugal	.	1975
Spain	.	1976
Thailand	.	1992

Countries that received one treatment only: liberalization

Country	*year of perm. liberalization*	*year of perm. democratization*
Armenia	1995	1998
Australia	1964	.
Barbados	1966	.
Botswana	1979	.
Cameroon	1993	.
Cape Verde	1991	.
Colombia	1986	.
Costa Rica	1986	.
Egypt	1995	.
Gambia	1985	.
Guinea	1986	.
Guinea-Bissau	1987	1999
Indonesia	1970	1999
Ireland	1966	.
Israel	1985	.
Ivory Coast	1994	2000
Jamaica	1989	.
Japan	1964	.
Jordan	1965	.
Kenya	1993	.
Latvia	1993	.
Macedonia	1994	.
Malaysia	1963	.
Mauritania	1995	.
Mauritius	1968	.
Morocco	1984	.
New Zealand	1986	.
Niger	1994	1999
Singapore	1965	.
South Africa	1991	.
Sri Lanka	1991	.
Tanzania	1995	.
Trin. & Tobago	1992	.
Tunisia	1989	.
Uganda	1988	.
Venezuela	1996	.

C. Countries that received 2 treatments

Democratization first

Country	*year of perm. liberalization*	*year of perm. democratization*
Argentina	1991	1983
Panama	1996	1989
Paraguay	1989	1989
Uruguay	1990	1985
Bolivia	1985	1982
Brazil	1991	1985
Dominican Rep.	1992	1978
Ecuador	1991	1979
El Salvador	1989	1982
Nicaragua	1991	1990
Guatemala	1988	1986
Honduras	1991	1980
Poland	1990	1989
Romania	1992	1990
Hungary	1990	1989
Turkey	1989	1983
Nepal	1991	1990
Bangladesh	1996	1991
Philippines	1988	1986
Zambia	1993	1991
Ethiopia	1996	1993
Mozambique	1995	1994
Madagascar	1996	1991

Liberalization first

Country	*year of perm. liberalization*	*year of perm. democratization*
Chile	1976	1989
Guyana	1988	1992
Peru	1991	1993
Mexico	1986	1994
Ghana	1985	1996
Benin	1990	1991
Mali	1988	1992
South Korea	1968	1987
Taiwan	1963	1992

Table 2: Effects of liberalizations on growth and investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var			*growth*					*investment*		
lib	1.01 (0.36)*** (0.38)**	0.93 (0.41)** (0.47)*		1.42 (0.34)*** (0.36)***	1.32 (0.38)*** (0.43)***	1.59 (0.31)*** (0.86)*	1.98 (0.34)*** (1.11)*		2.08 (0.29)*** (0.81)**	2.32 (0.31)*** (0.98)**
3y_pre_lib			-0.95 (0.49)* (0.60)					-2.36 (0.42)*** (0.88)***		
3y_post_lib			0.48 (0.46) (0.44)					-0.55 (0.39) (1.00)		
4yon_post_lib			0.95 (0.44)** (0.42)**					2.12 (0.38)*** (1.14)*		
Treatment	permanent	permanent	permanent	all	all	permanent	permanent	permanent	all	all
Y*conts	No	Yes	No	No	Yes	No	yes	No	No	Yes
Obs.(countries)	4492(134)	4492(134)	4492(134)	4492(134)	4492(134)	4640(135)	4640(135)	4640(135)	4640(135)	4640(135)
Adj.R2(within)	0.01	0.03	0.01	0.01	0.03	0.02	0.08	0.04	0.03	0.08

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%;
*** significant at 1%.
lib = 1 after liberalization
3y_pre_lib = 1 in the 3 years preceding liberalizations
3y_post_lib = 1 in the year of liberalization and in the 3 following years
4yon_post_lib = 1 from the 4^{th} year and onwards after liberalization
*Y*conts:* Y are dummy variables for years; conts are dummy variables for Asia, Africa, Latin America and for socialist legal origin
Regressions always include country and year fixed effects, as well as a dummy variable for *socialist legal origin* interacted with *lib*

Table 3: Effects of liberalizations on gadp and corruption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var			*gadp*					*corruption*		
lib	0.33 (0.07)*** (0.16)**	0.16 (0.08)* (0.14)		0.33 (0.07)*** (0.15)**	0.15 (0.08)* (0.14)	0.21 (0.06)*** (0.13)	0.15 (0.07)** (0.16)		0.21 (0.06)*** (0.13)	0.15 (0.07)** (0.16)
3y_pre_lib			-0.24 (0.09)*** (0.14)					0.03 (0.08) (0.14)		
3y_post_lib			-0.03 (0.09) (0.21)					0.10 (0.08) (0.18)		
4yon_post_lib			0.59 (0.10)*** (0.26)**					0.45 (0.09)*** (0.22)**		
treatment	permanent	permanent	permanent	all	all	permanent	permanent	permanent	all	all
Y*conts	No	Yes	No	No	Yes	No	yes	No	No	Yes
Obs.(countries)	1559(106)	1559(106)	1559(106)	1561(106)	1561(106)	1593(106)	1593(106)	1593(106)	1595(106)	1595(106)
Adj.R2(within)	0.45	0.47	0.47	0.45	0.47	-0.01	0.02	0.00	-0.01	0.02

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%;
*** significant at 1%.
lib = 1 after liberalization
3y_pre_lib = 1 in the 3 years preceding liberalizations
3y_post_lib = 1 in the year of liberalization and in the 3 following years
4yon_post_lib = 1 from the 4^{th} year and onwards after liberalization
*Y*conts:* Y are dummy variables for years; conts are dummy variables for Asia, Africa, Latin America and for socialist legal origin
Regressions always include country and year fixed effects, as well as a dummy variable for *socialist legal origin* interacted with *lib*

Table 4: Effects of liberalizations on inflation and surplus

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var			*inflation*					*surplus*		
lib	0.12 (0.07)* (0.18)	-0.15 (0.07)** (0.19)		0.00 (0.06) (0.18)	-0.17 (0.06)*** (0.16)	1.82 (0.40)*** (1.01)*	2.01 (0.48)*** (1.57)		1.47 (0.40)*** (1.02)	1.53 (0.48)*** (1.65)
3y_pre_lib			1.16 (0.08)*** (0.18)***					-0.79 (0.50) (0.77)		
3y_post_lib			0.71 (0.08)*** (0.17)***					1.34 (0.49)*** (1.07)		
4yon_post_lib			0.22 (0.08)*** (0.23)					1.90 (0.53)*** (1.22)		
treatment	permanent	permanent	permanent	all	all	permanent	permanent	permanent	all	all
Y*conts	No	Yes	No	No	Yes	No	yes	No	No	Yes
Obs.(countries)	3594(131)	3594(131)	3594(131)	3594(131)	3594(131)	1907(103)	1907(103)	1907(103)	1907(103)	1907(103)
Adj.R2(within)	0.21	0.33	0.26	0.21	0.33	0.04	0.06	0.04	0.04	0.06

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%; *** significant at 1%.
lib = 1 after liberalization
3y_pre_lib = 1 in the 3 years preceding liberalizations
3y_post_lib = 1 in the year of liberalization and in the 3 following years
4yon_post_lib = 1 from the 4th year and onwards after liberalization
*Y*conts:* Y are dummy variables for years; conts are dummy variables for Asia, Africa, Latin America and for socialist legal origin
Regressions always include country and year fixed effects, as well as a dummy variable for *socialist legal origin* interacted with *lib*

Table 5: Effects of democratizations on growth and investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var			*growth*					*investment*		
dem	0.34	0.44		0.64	0.78	0.29	-0.07		0.58	0.48
	(0.41)	(0.45)		(0.35)*	(0.37)**	(0.35)	(0.37)		(0.29)**	(0.30)
	(0.45)	(0.52)		(0.40)	(0.38)**	(0.99)	(0.98)		(0.78)	(0.79)
3y_pre_dem			-0.67					-0.37		
			(0.61)					(0.51)		
			(0.68)					(1.00)		
3y_post_dem			0.39					0.02		
			(0.56)					(0.47)		
			(0.55)					(1.02)		
4yon_post_dem			0.10					0.34		
			(0.49)					(0.41)		
			(0.51)					(1.30)		
treatment	permanent	permanent	permanent	all	all	permanent	permanent	permanent	all	all
Y*conts	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Obs.(countries)	4397(138)	4397(138)	4397(138)	4388(138)	4388(138)	4530(150)	4530(150)	4530(150)	4518(150)	4518(150)
Adj.R2(within)	0.01	0.03	0.01	0.01	0.03	0.01	0.07	0.01	0.01	0.06

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%; *** significant at 1%
dem = 1 after democratizations
3y_pre_dem = 1 in the 3 years preceding democratizations
3y_post_dem = 1 in the year of democratizations and in the 3 following years
4yon_post_dem = 1 from the 4^{th} year and onwards after democratizations
*Y*conts:* Y are dummy variables for years; conts are dummy variables for Asia, Africa, Latin America and socialist legal origin
Regressions always include country and year fixed effects, as well as a dummy variable for *socialist legal origin* interacted with *dem*

Table 6: Effects of democratizations on gadp and corruption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var			*gadp*					*Corruption*		
dem	0.29 (0.09)*** (0.20)	0.20 (0.09)** (0.20)		0.03 (0.08) (0.19)	-0.03 (0.08) (0.18)	0.33 (0.07)*** (0.19)*	0.30 (0.08)*** (0.21)		0.19 (0.07)*** (0.18)	0.19 (0.07)*** (0.20)
3y_pre_dem			-0.19 (0.11)* (0.17)					-0.09 (0.08) (0.16)		
3y_post_dem			0.04 (0.11) (0.23)					0.17 (0.09)* (0.22)		
4yon_post_dem			0.49 (0.12)*** (0.30)					0.50 (0.10)*** (0.27)*		
treatment	permanent	permanent	permanent	all	all	permanent	permanent	permanent	all	all
Y*conts	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Obs.(countries)	1790(122)	1790(122)	1790(122)	1791(122)	1791(122)	1828(122)	1828(122)	1828(122)	1825(122)	1825(122)
Adj.R2(within)	0.45	0.47	0.45	0.44	0.46	-0.02	0.00	-0.01	-0.03	-0.00

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%;
*** significant at 1%.
dem = 1 after democratizations
3y_pre_dem = 1 in the 3 years preceding democratizations
3y_post_dem = 1 in the year of democratizations and in the 3 following years
4yon_post_dem = 1 from the 4th year and onwards after democratizations
*Y*conts:* Y are dummy variables for years; conts are dummy variables for Asia, Africa, Latin America and socialist legal origin
Regressions always include country and year fixed effects, as well as a dummy variable for *socialist legal origin* interacted with *dem*

Table 7: Effects of democratizations on inflation and surplus

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var			*inflation*					*surplus*		
dem	0.40 (0.07)*** (0.17)**	0.17 (0.07)** (0.21)		0.35 (0.06)*** (0.13)***	0.18 (0.06)*** (0.13)	0.89 (0.53)* (1.02)	0.04 (0.62) (1.00)		1.51 (0.46)*** (1.16)	0.99 (0.52)* (1.28)
3y_pre_dem			0.31 (0.11)*** (0.21)					2.06 (0.69)*** (0.94)**		
3y_post_dem			0.53 (0.10)*** (0.23)**					1.97 (0.66)*** (1.15)*		
4yon_post_dem			0.43 (0.09)*** (0.22)*					1.21 (0.67)* (1.24)		
treatment	permanent	permanent	permanent	all	all	permanent	permanent	permanent	all	all
Y*conts	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Obs.(countries)	3739(141)	3739(141)	3739(141)	3740(141)	3740(141)	1996(110)	1996(110)	1996(110)	1995(110)	1995(110)
Adj.R2(within)	0.21	0.32	0.21	0.21	0.32	-0.00	-0.01	-0.00	-0.00	-0.01

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%; *** significant at 1%.
dem = 1 after democratizations
3y_pre_dem = 1 in the 3 years preceding democratizations
3y_post_dem = 1 in the year of democratizations and in the 3 following years
4yon_post_dem = 1 from the 4th year and onwards after democratizations
*Y*conts:* Y are dummy variables for years; conts are dummy variables for Asia, Africa, Latin America and socialist legal origin
Regressions always include country and year fixed effects, as well as a dummy variable for *socialist legal origin* interacted with *dem*

Table 8: Effects of liberalizations on democracy and viceversa

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var			*Polity2*			*permanently open*	*permanently open*	*permanently open*	*open*	*open*
Lib	1.14 (0.22)*** (0.75)	-0.15 (0.24) (0.81)		0.79 (0.21)*** (0.67)						
Dem						0.25 (0.02)*** (0.06)***	0.12 (0.02)*** (0.07)*		0.12 (0.01)*** (0.05)**	0.02 (0.01) (0.05)
5y_pre_treat			2.15 (0.35)*** (0.60)***		1.86 (0.33)*** (0.61)***					
3y_pre_treat			3.08 (0.30)*** (0.75)***		2.78 (0.27)*** (0.55)***			0.05 (0.02)** (0.05)		
3y_post_treat			2.38 (0.28)*** (0.89)***		2.00 (0.27)*** (0.75)***			0.15 (0.02)*** (0.06)**		
4yon_post_treat			2.21 (0.27)*** (1.01)**		1.32 (0.23)*** (0.79)*			0.32 (0.02)*** (0.07)***		
treatment	permanent	permanent	permanent	all	all	permanent	permanent	permanent	all	all
Y*conts	No	Yes	No	No	No	No	yes	No	No	Yes
Obs.(countries)	4603(132)	4603(132)	4603(132)	4603(132)	4603(132)	4593(132)	4593(132)	4593(132)	4581(132)	4581(132)
Adj.R2(within)	0.23	0.29	0.25	0.23	0.25	0.40	0.50	0.41	0.38	0.50

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%;
*** significant at 1%.
lib (dem)= 1 after liberalization (democratization)
ny_pre_treat = 1 in the n years preceding treatment (liberalizations or democratizations)
3y_post_treat = 1 in the 3 years following treatment (liberalizations or democratizations)
4yon_post_treat = 1 from the 4th year and onwards after treatment (liberalizations or democratizations)
*Y*conts:* Y are dummy variables for years; conts are dummy variables for Asia, Africa, Latin America and for socialist legal origin
Regressions always include country and year fixed effects, as well as a dummy variable for *socialist legal origin* interacted with *lib* (columns 1-5) and with *dem* (columns 6-10)

Table 9 – Effects of democratizations and liberalizations on growth and investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			growth					*Investment*		
dem_1t	-1.72	0.47	-1.22	0.86	-0.78	-1.88	0.70	-2.03	0.55	-0.56
	(0.82)**	(0.61)	(0.81)	(0.60)	(0.52)	(0.68)***	(0.50)	(0.69)***	(0.53)	(1.02)
	(0.69)**	(1.06)				(1.62)	(1.45)			
lib_1t	0.04	0.86	0.34	1.05	-0.00	1.55	1.42	1.51	1.45	0.49
	(0.50)	(0.49)*	(0.49)	(0.47)**	(0.25)	(0.41)***	(0.40)***	(0.42)***	(0.40)***	(0.49)
	(0.56)	(0.46)*				(1.19)	(1.05)			
dem_2t	1.66	1.53	1.07	1.00	0.59	0.42	0.31	0.46	0.49	1.16
	(0.71)**	(0.52)***	(0.70)	(0.51)**	(0.54)	(0.59)	(0.42)	(0.60)	(0.43)	(1.06)
	(0.58)***	(0.39)***				(1.05)	(1.11)			
lib_2t	2.29	2.20	1.44	1.71	1.00	-0.47	2.64	-0.43	2.79	-1.11
	(0.90)**	(0.66)***	(0.88)	(0.64)***	(0.48)**	(0.73)	(0.54)***	(0.75)	(0.55)***	(0.95)
	(0.71)***	(0.71)***				(1.54)	(1.63)			
dem_after_lib	-1.23	-1.02	0.84	0.99	-0.46	3.61	1.49	3.47	1.41	3.67
	(1.09)	(0.93)	(1.08)	(0.92)	(0.71)	(0.89)***	(0.76)*	(0.91)***	(0.78)*	(1.40)***
	(0.75)	(0.87)				(1.45)**	(2.22)			
lib_after_dem	-3.01	-1.88	-2.51	-2.07	-1.89	1.80	-0.35	1.81	-0.40	-0.14
	(1.13)***	(0.86)**	(1.11)**	(0.85)**	(0.67)***	(0.92)*	(0.71)	(0.94)*	(0.72)	(1.33)
	(0.94)***	(0.91)**				(1.53)	(1.95)			
Lagged income	No	No	Yes	Yes	No	No	No	Yes	Yes	No
Estimation	OLS, FE	OLS, FE	OLS, FE	OLS, FE	2 step	OLS, FE	OLS, FE	OLS, FE	OLS, FE	2 step
Treatment	Permanent	All	Permanent	All	Permanent	Permanent	All	Permanent	All	Permanent
Obs.(countries)	4243(130)	4229(130)	4079(107)	4065(107)	110	4361(131)	4230(130)	4044(106)	4030(106)	113
Adj.R2 within	0.04	0.04	0.07	0.07	0.05	0.11	0.10	0.11	0.11	0.08

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%;
*** significant at 1%.
*dem*_1t (*dem_2t*) = 1 after democratizations for countries that did not (did) liberalize
*lib*_1t (*lib_2t*) = 1 after liberalizations for countries that did not (did) democratize
dem_after_lib (*lib_after_dem*) = 1 after the second treatment for countries that liberalized first (became dem. first)
Controls always included: country and year fixed effects, dummy variables for years interacted with dummy variables for *Asia, Africa, Latin America* and *socialist legal origin*; dummy variable for *socialist legal origin* interacted with *lib* and *dem* (as defined in the previous tables).

Table 10 – Effects of democratizations and liberalizations on inflation and international trade

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Trade					*inflation*		
dem_1t	-1.56	-5.22	0.80	-1.09	0.16	0.66	0.16	0.05	-0.03	0.22
	(1.97)	(1.45)***	(1.14)	(0.81)	(3.17)	(0.14)***	(0.10)	(0.12)	(0.09)	(0.19)
	(4.38)	(3.53)				(0.31)**	(0.22)			
lib_1t	0.38	0.23	0.93	0.01	0.04	-0.19	-0.01	-0.08	0.01	-0.03
	(1.20)	(1.15)	(0.69)	(0.64)	(1.53)	(0.08)**	(0.08)	(0.07)	(0.06)	(0.10)
	(3.11)	(2.88)				(0.19)	(0.21)			
dem_2t	-2.81	-1.56	-0.24	0.32	-4.31	0.08	0.11	0.07	0.11	-0.32
	(1.71)*	(1.22)	(0.97)	(0.67)	(3.30)	(0.11)	(0.09)	(0.09)	(0.07)	(0.20)
	(3.70)	(2.28)				(0.32)	(0.20)			
lib_2t	10.76	8.45	2.61	2.28	1.14	-0.59	-0.84	-0.39	-0.43	-0.11
	(2.16)***	(1.56)***	(1.23)**	(0.86)***	(2.95)	(0.16)***	(0.12)***	(0.13)***	(0.10)***	(0.18)
	(5.98)*	(4.42)*				(0.42)	(0.36)**			
dem_after_lib	8.98	10.52	1.16	1.23	4.72	-0.43	-0.16	-0.17	-0.07	-0.24
	(2.61)***	(2.21)***	(1.47)	(1.21)	(4.35)	(0.18)**	(0.16)	(0.14)	(0.13)	(0.27)
	(9.16)	(9.21)				(0.35)	(0.40)			
lib_after_dem	-7.55	-7.22	-2.18	-2.45	2.94	0.57	0.64	0.21	0.16	0.78
	(2.72)***	(2.05)***	(1.55)	(1.13)**	(4.13)	(0.19)***	(0.14)***	(0.16)	(0.12)	(0.26)***
	(8.74)	(6.14)				(0.50)	(0.52)			
Lagged dep var	No	No	Yes	Yes	No	No	No	Yes	Yes	No
Estimation	OLS, FE	OLS, FE	OLS, FE	OLS, FE	2 step	OLS, FE	OLS, FE	OLS, FE	OLS, FE	2 step
Treatment	Permanent	All	Permanent	All	Permanent	Permanent	All	Permanent	All	Permanent
Obs.(countries)	4243(130)	4229(130)	3961(106)	3946(106)	110	3466(127)	3371(126)	2928(88)	2876(88)	81
Adj.R2 within	0.29	0.30	0.78	0.80	-0.04	0.35	0.34	0.57	0.58	0.11

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%;
*** significant at 1%.
*dem_*1t (*dem_2t*) = 1 after democratizations for countries that did not (did) liberalize
*lib_*1t (*lib_2t*) = 1 after liberalizations for countries that did not (did) democratize
dem_after_lib (*lib_after_dem*) = 1 after the second treatment for countries that liberalized first (became dem. first)
Controls always included: country and year fixed effects, dummy variables for years interacted with dummy variables for *Asia*, *Africa*, *Latin America* and *socialist legal origin*; dummy variable for *socialist legal origin* interacted with *lib* and *dem* (as defined in the previous tables).

Table 11 – Effects of democratizations and liberalizations on surplus, gadp and corruption

	(1)	(2)	(3)	(4)	(5)	(6)
	surplus		*gadp*		*corruption*	
lib_1t	-1.92	2.55	-0.03	-0.11	-0.06	0.06
	(0.85)**	(0.61)***	(0.12)	(0.11)	(0.11)	(0.10)
	(0.92)**	(2.06)	(0.32)	(0.22)	(0.35)	(0.25)
dem_1t	2.04	1.24	0.02	0.06	0.10	0.14
	(0.58)***	(0.57)**	(0.09)	(0.10)	(0.08)	(0.08)*
	(1.92)	(2.05)	(0.16)	(0.17)	(0.15)	(0.13)
dem_2t	1.40	-0.73	0.35	-0.06	0.60	0.30
	(0.86)	(0.81)	(0.18)*	(0.14)	(0.14)***	(0.12)**
	(2.29)	(1.99)	(0.17)**	(0.29)	(0.34)*	(0.36)
lib_2t	3.58	4.04	0.34	0.14	0.28	0.27
	(1.13)***	(1.10)***	(0.22)	(0.14)	(0.19)	(0.12)**
	(2.45)	(2.40)*	(0.16)**	(0.21)	(0.20)	(0.28)
dem_after_lib	-0.29	-0.42	-0.13	0.06	-0.21	0.06
	(1.23)	(1.01)	(0.25)	(0.15)	(0.21)	(0.13)
	(2.38)	(1.98)	(0.30)	(0.35)	(0.43)	(0.33)
lib_after_dem	-2.95	-1.83	-0.10	0.12	-0.31	-0.16
	(1.30)**	(1.12)	(0.24)	(0.12)	(0.21)	(0.10)
	(2.15)	(1.65)	(0.24)	(0.19)	(0.21)	(0.15)
Treatment	Permanent	All	Permanent	All	Permanent	All
Obs.(countries)	1861(101)	1802(100)	1535(104)	1500(103)	1569(104)	1534(103)
Adj.R2 within	0.07	0.07	0.49	0.51	0.04	0.05

Standard errors in parentheses (above: OLS; below: clustered); * significant at 10%; ** significant at 5%; *** significant at 1%.
*dem*_1t (*dem_2t*) = 1 after democratizations for countries that did not (did) liberalize
*lib*_1t (*lib_2t*) = 1 after liberalizations for countries that did not (did) democratize
dem_after_lib (*lib_after_dem*) = 1 after the second treatment for countries that liberalized first (became dem. first)
Controls always included: country and year fixed effects, dummy variables for years interacted with dummy variables for *Asia, Africa, Latin America* and *socialist legal origin*; dummy variable for *socialist legal origin* interacted with *lib* and *dem* (as defined in the previous tables).
No lagged dependent variable included; estimation by OLS, FE

Figure 1 Estimated residuals for controls (always) and treated (before liberalization)

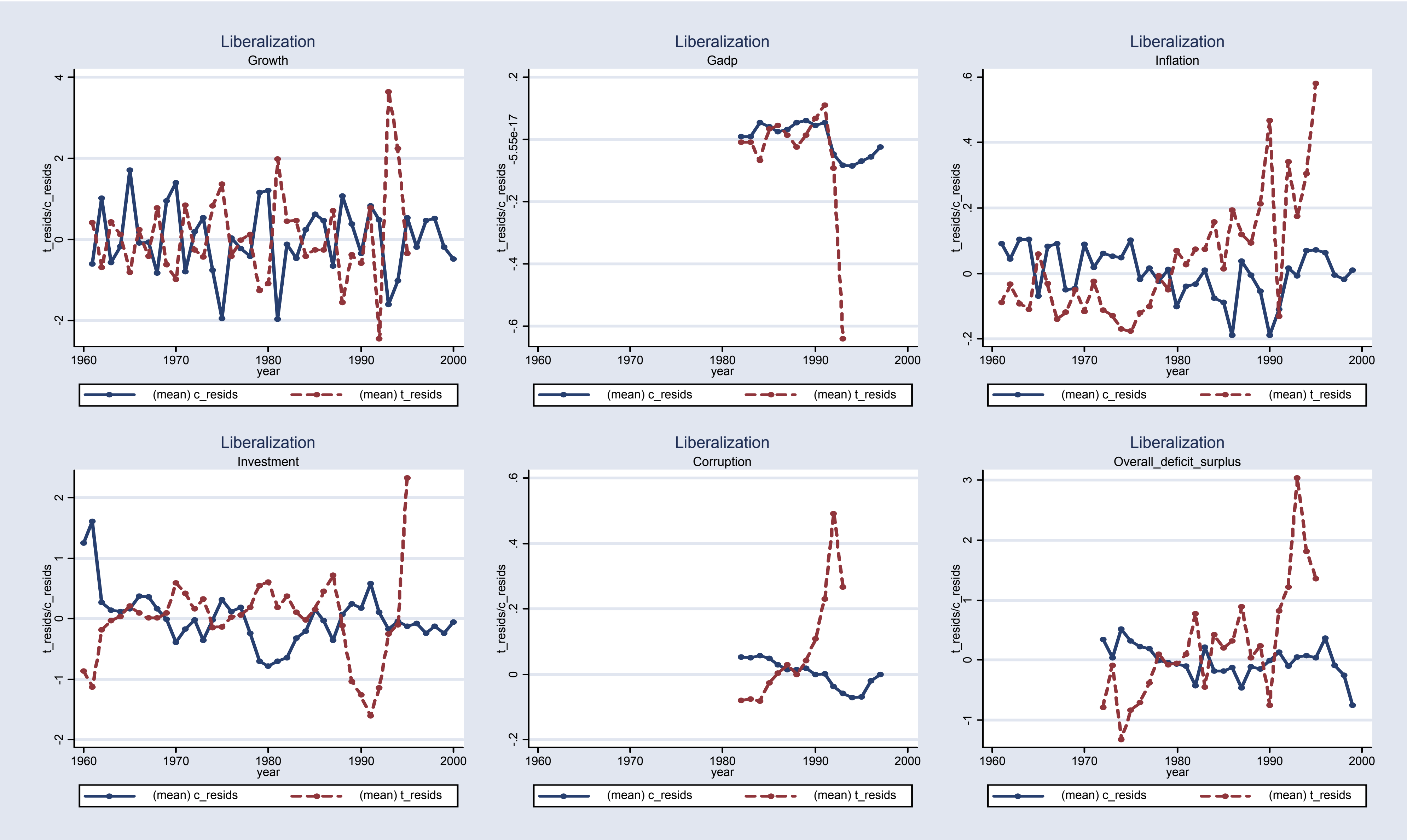


Figure 2 Estimated residuals for controls (always) and treated (before democracy)

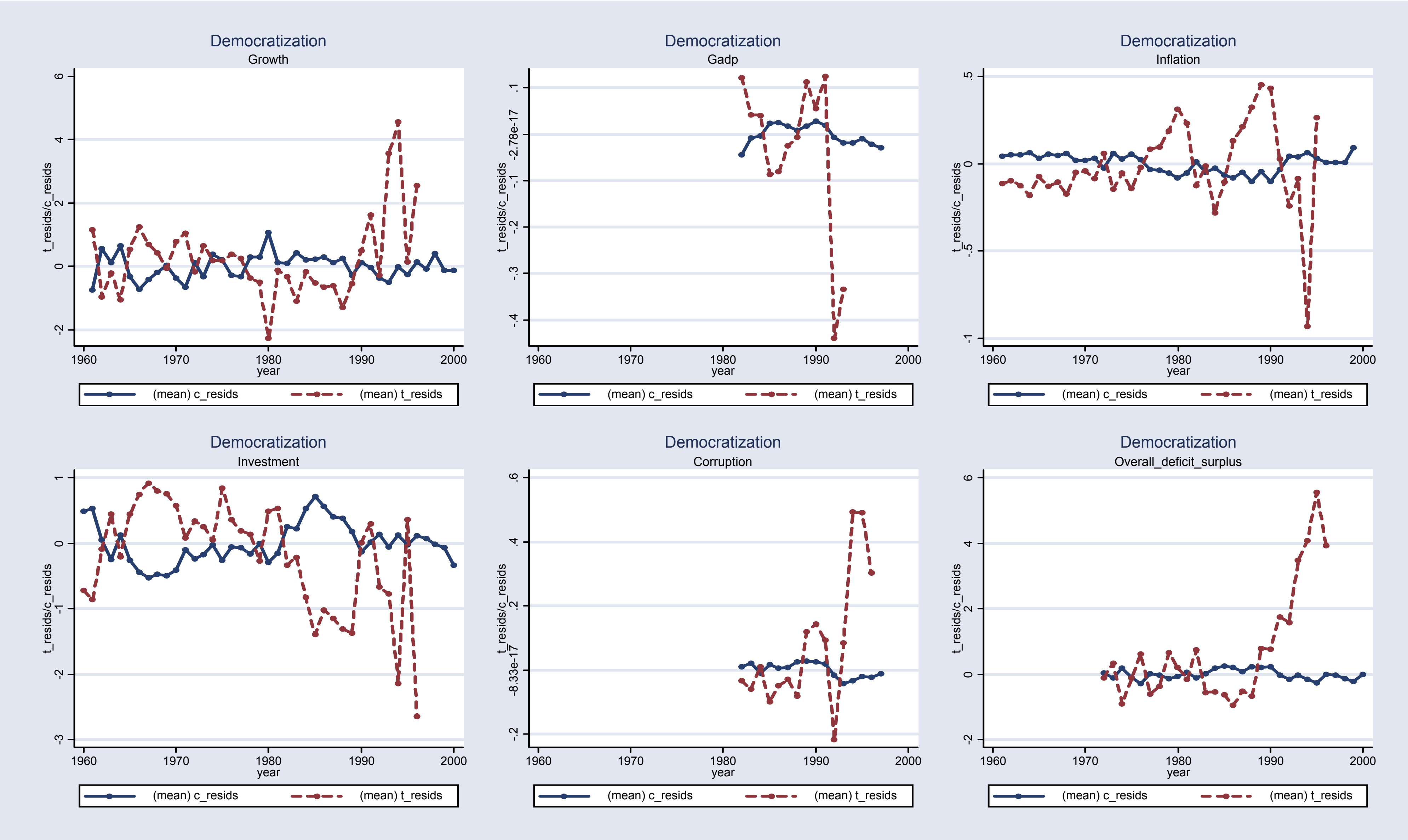


Figure 3 Estimated residuals for controls (always) and treated (top panel: before democracy; bottom panel: before liberalization))

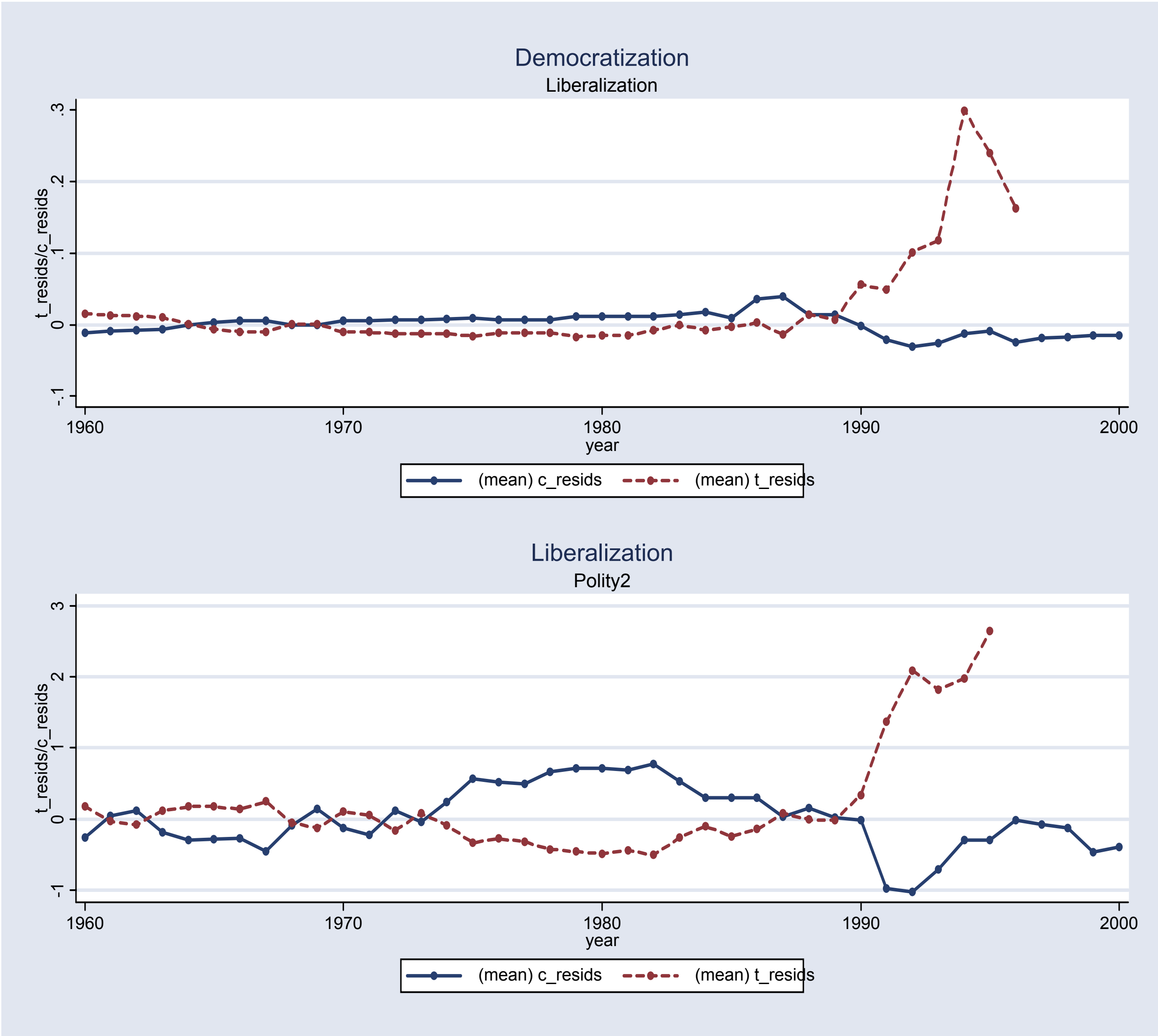
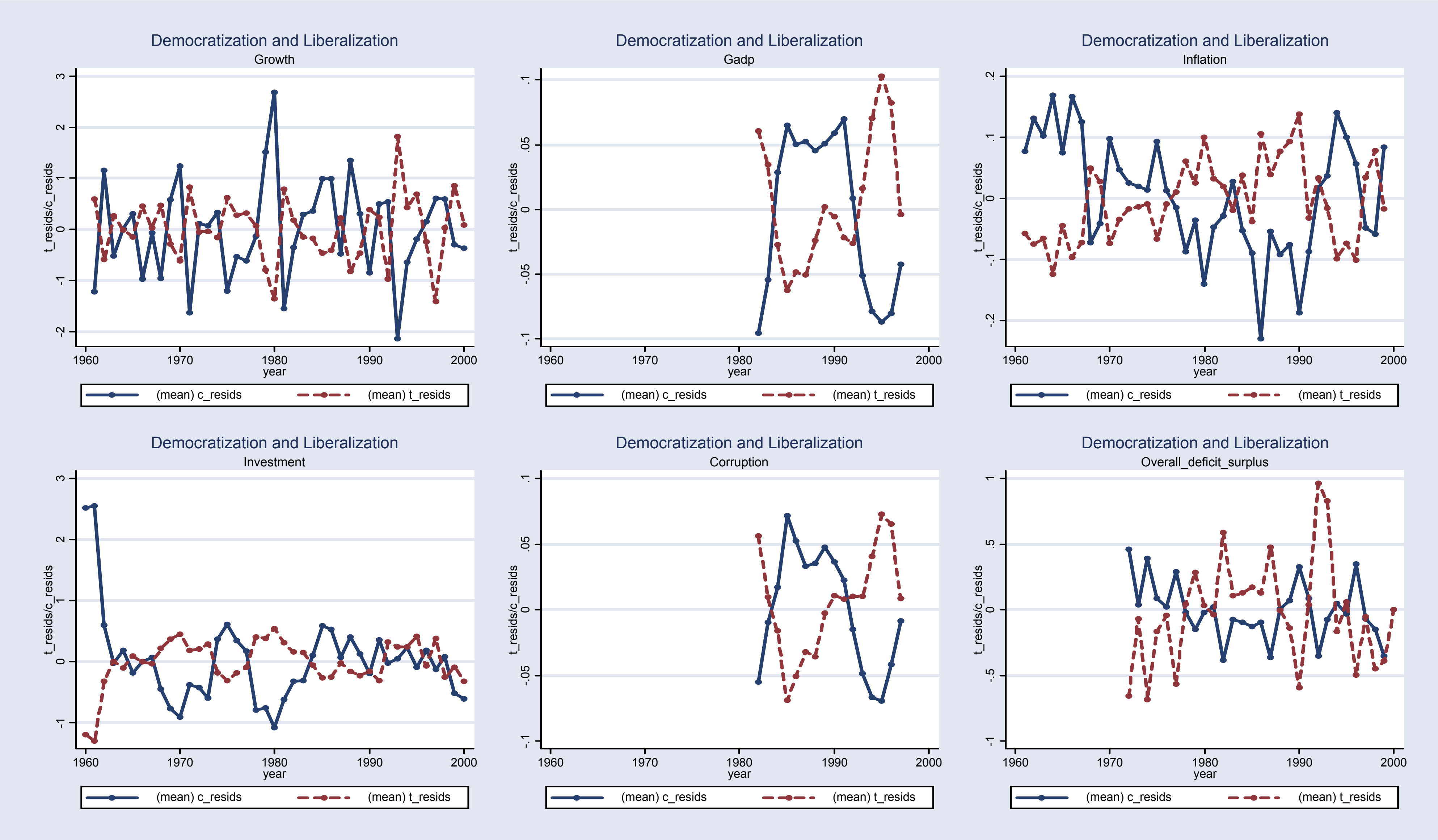
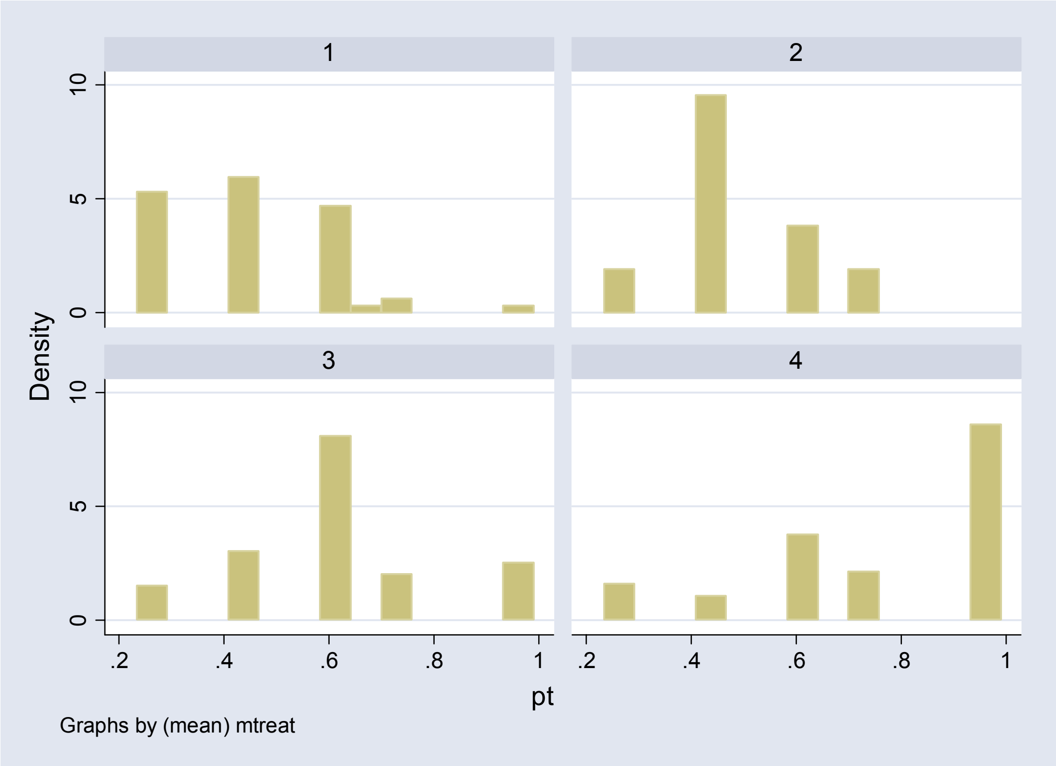


Figure 4 Estimated residuals with multiple treatments - controls (always) and treated (before first reform)



APPENDIX

Figure A1. Probability of having at least one reform (either political or economic liberalization)



The horizontal axis measures the estimated probability of treatment – i.e. of having at least one reform (either economic or political liberalization) - conditional upon being in Africa, Asia, Latin America and on having a socialist legal origin. The vertical axis measures the number of countries having this estimated probability., by group of countries:

- Group 1 are the countries that had no reform at all (ie they are the control countries)
- Group 2 are the countries that had political liberalization only
- Group 3 are the countries that had economic liberalization only
- Group 4 are the countries that had both economic and political liberalization

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