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April 2006

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ABSTRACT

Reform Redux: Measurement, Determinants and Reversals*

We construct objective measures of privatization, internal and external liberalization reform efforts, across countries over time, and investigate their determinants, reversals and macroeconomic impacts. We find that GDP growth determines external liberalization and privatization, concentration of political power drives internal liberalization, and democracy underpins all three. We find that FDI inflows reduce the probability of privatization reversals, labour strikes increase that of internal liberalization reversals, and OECD growth increase that of external liberalization reversals. We replicate previous studies and find that the macroeconomic effects of reform (when measured objectively) tend to be larger and more precisely estimated.

JEL Classification: E23, D72, H26, O17

Keywords: reform, liberalization, privatization, political economy, transition

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1. Introduction

In the last two decades, profound changes in economic policy took place around the world culminating with the almost simultaneous pronouncement of the "Washington Consensus" and the fall of communism. Although the literature on political economy of reform is large, prominent and fast growing, it is still essentially theoretical. Empirical testing of the various (sometimes conflicting) hypotheses is rare. According to two authoritative reviews (Drazen, 2000, and Persson and Tabellini, 2000)[1], the empirical evidence has yet to materialise in large part because the reforms we focus on here (e.g., privatization) share elements of both "stroke of the pen national policies" (Easterly, 2006) and harder to change "institutions" (Acemoglu, Johnson and Robinson, 2006). One would expect that research on the former communist countries (that is, on those countries that during the 1990s have implemented large-scale economic reforms) would provide such empirical evidence at once, but that has not happened either.[2]

The theoretical literature on economic reform started out examining positive issues (such as, how can we explain that socially beneficial reforms are not implemented?) and has evolved towards a more difficult task, namely tackling normative questions, such as how can reform packages be designed so that they overcome political resistance.[3] Fernandez and Rodrik (1991) present a seminal model of status quo bias. Their basic intuition is that individual- specific uncertainty over the outcome of a reform (that is, who will be the winners and who will be the losers) leads a majority of voters to expect to loose from reform ex ante,

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[1] These authors identify one single empirical study for developing countries, namely Lora (1997) for Latin American countries in the 1980s and 1990s.

[2] Campos and Coricelli conclude their survey noting that: "more emphasis should be placed upon a better understanding of the role of economic reforms and reform strategies in dictating the path of the transition process (…) There are a number of theoretical models that stress the role of reform strategies. Yet the data for discriminating among these models is lacking. The few indicators available are *unnecessarily* subjective (…)" (2002, p. 831, italics added).

[3] For surveys of the literature, see Asilis and Milesi-Ferreti (1994), Rodrik (1996) Bhattacharya (1997), Drazen (2000), Persson and Tabellini (2000) and Kuczynski and Williamson (2003).

even though everybody knows that a majority will gain from reform ex post. If reform is implemented, the losers will be a minority so they do not have the political power to reverse the reform while the winners (from reform) cannot credibly promise to compensate the losers ex post. Another seminal paper is Alesina and Drazen (1991), which treat implementation delays in a war of attrition model. This generates an important hypothesis (for which we find support below), regarding political fractionalization. Governments will not need to negotiate how to allocate the short term cost of reforms if they have a stable majority in parliament. Dewatripont and Roland provide various influential models of reform dynamics inspired by the transition from communism (1992a, 1992b, 1995a, 1995b). They stress the role of uncertainty about the outcome of reforms in terms of the government choice of implementation sequence. Comparing big-bang with gradualist reform strategies, they argue the latter is easier to implement because it involves learning and experimentation.[4] Although the theoretical arguments for sequencing seem to have been vindicated by the emergence of the so-called second-generation reforms, a number of authors have spelled out the case for a big bang strategy. The main arguments for the latter include the costs of partial reforms, time-consistency issues, and the advantages of a political honeymoon in which credibility provide an opportunity to implement painful measures.[5] One central element in the Dewatripont-Roland models is the role of reform reversals: reformers try to design reform packages that incorporate costs of reversal that are high enough to deter political resistance (see also

[4] On the role of learning in reform dynamics see also Goodhue, Rausser and Simon (1998) and Schroder (2001). Correctly sequenced reforms also create constituencies for further reforms. Collier and Gunning (1999) argue that a main reason for the poor performance of IMF-supported structural adjustment programs is inattention to sequencing.

[5] Martinelli and Tommasi (1997) argue that even when gradualism is the choice of an unconstrained social planner, time-consistency considerations may force simultaneous implementation of all possible reforms. In their model, the time-inconsistency of the "optimal reform sequences" arises because winners from early reforms will oppose any later reforms that may hurt them. Knowing that, losers from early reforms will oppose the earlier measures and require additional compensation. Lipton and Sachs (1990) advocate that reformers should introduce simultaneously and in a comprehensive way all elements of a market economy, taking advantage of the political honeymoon to implement painful reforms on a stroke. Murphy et al (1992) argue that introducing partial reforms would eliminate their positive effects and disorganize the economy.

Cukierman and Tommasi, 1998). Finally, a powerful idea in this literature is that crises trigger economic reforms (see Drazen and Grilli, 1993). The argument is that the political, or informational, impediments to reform may be so large that reform will not be implemented unless a crisis occurs because a larger share of the population benefits from reform in the aftermath of a crisis (Drazen and Easterly, 2001).

There have been few efforts to test empirically the main propositions from the theoretical literature. One reason being the lack of comprehensive reform measures.[6] In other words, there are a number of studies that focus on one reform and/or on one country but few that focus on multiple reforms in more than one country over time. For instance, two reform areas that have received a great deal of attention across countries and over time are financial liberalization and privatization.[7]

Note that measures of reform efforts during the transition from communism have been constructed by international organizations, such as the World Bank and the European Bank for Reconstruction and Development (EBRD). In our view, these indicators are unsatisfactory for at least five reasons. One is that it is rather difficult to know which are the exact variables underlying each reform measure.[8] Second, it is not disclosed how the reform scores are generated (that is, how the potential set of underlying variables translate into the overall reform measures). Third, among the listed (potential) underlying variables one finds policy inputs as well as outcomes (for instance, for external liberalization, one can find tariff levels as well as trade openness). Fourth, there are various instances in which the overall reform

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[6] As noted, the measurement of economic reforms was pioneered by Lora (1997, 2001), which consider five reforms: trade, tax, financial, privatization and labor market regulation. These are aggregated in a structural policy index for 20 Latin American countries yearly from 1985 to 1995.

[7] On privatization, Megginson and Netter (2001) provide an extensive review of the evidence, while on financial liberalization, important recent contributions are those by Kaminsky and Schmuckler (2003) and Abiad and Mody (2005).

[8] The World Bank produced three reform indexes for the transition economies for the period 1989 to 1997, while the EBRD has a set of nine indicators starting in 1991. These indexes and their potential problems are discussed in greater detail in section 2.

score was revised despite the "underlying data" remaining unchanged, which suggests that the algorithm may well have changed. Fifth and finally, the existing reform measures are benchmarked against an imprecisely defined reference point (for instance, an "advance industrial economy"). One main reason for these potential problems is that these existing reform indicators are subjective. They are based on the judgement of country specialists at the World Bank (de Melo et al., 1996) and at the EBRD.[9] Expert opinion might be swayed by ex post reports of favourable or unfavourable performance. However, differently from some measures of institutions (e.g., rule of law), measures of reform need not be subjective.[10]

The period after the collapse of communism in Central Europe and the former Soviet Union provides for what is arguably the largest natural experiment on economic reform in recent history and it is paradoxical that objective indicators of reform are still unavailable.[11] This paper tries to address this gap. More precisely, the objective of this paper is two-fold. One is to construct objective measures of privatisation, external and internal liberalisation reform efforts for up to 25 Eastern European and former Soviet Union economies between 1989 and 2001. The second is to use these new measures to shed light on various hypotheses from the theoretical literature on the political economy of reform.

How are these reform measures constructed? Firstly, we compile an extensive set of underlying variables, yielding almost 30 variables for external liberalization, 3 for internal liberalization and about 12 for privatization.[12] Secondly, in terms of ways to normalize and aggregate these data, we investigate, *inter alia*, simple averages, principal components and

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[9] For instance, "The transition indicators scores in Chapter 1 reflect the *judgement* of the EBRD's Office of the Chief Economist about country-specific progress in transition" (EBRD, 2004, p. 119, italics added).

[10] See also Glaeser, La Porta, Lopez de Silanes and Scheilfer (2004).

[11] Campos, Hsiao and Nugent (2005) find that large cross-country samples do not pass standard poolability tests and on this basis argue for analyses at the regional level. In light of the abundance of case studies on the subject of reform, this provides another reason to focus on one region only as we do here.

[12] By focusing on internal and external liberalization efforts we try to go beyond price and trade liberalization. By internal liberalization we mean price and wage liberalization, while external liberalization is defined here to include the liberalization of trade as well as of capital flows.

the one proposed by Lora (1997) and decided for the latter on the basis of it being the simplest, most transparent as well as the one method that has been used in the reform literature (see also Loayza, Oviedo and Serven, 2005). Thirdly, we classify these underlying objective indicators into "input" and "outcome" indicators of reform in order to generate input-only measures. This last point is crucial as we believe it is one way of addressing the Rodrik critique (2005) according to which we learn little from regressions of growth on policies because existing measures seldom isolate effort from reform outcomes. If outcomes receive a high weight in the index (this should be expected if they are subjective), then regressing indexes heavily weighted towards outcomes on outcomes themselves clearly should not be very informative. Fourthly, and finally, we subject our indexes to various robustness tests by (a) excluding outcome indicators (or conversely, by examining our preferred input-only measures of reform), (b) assessing reform dynamics across countries for various sub-periods and (c) comparing our objective indexes with those from the EBRD and World Bank.

Our main findings are as follows. Compared to the existing subjective measures, ours generate a less optimistic assessment of the reform process, depicting it as much less smooth than previously thought (in other words, we find that reform reversals abound). Among the main determinants of reform, we find domestic GDP growth for external liberalization and privatisation, concentration of political power for internal liberalisation, and democracy for the three of them. We also find that inflows of foreign direct investment reduce the probability of privatization reversals, labour strikes increase that of internal liberalization reversals, and negative terms of trade shocks increase the likelihood of an external liberalization reversal. Finally, we replicate various econometric studies on the effects of reform on growth and find that those effects, using our objective measures of reform, are larger and more precisely estimated.

We note at the outset that we leave one major topic for future research. The two central issues in the political economy of reform literature are reversals and optimal sequencing. We have dealt extensively with the former in this paper, but have strong reasons to leave the latter for future efforts. This is despite our results being somewhat clear in this respect: our set of countries seems to have implemented reform by first advancing internal liberalization, then external liberalization and finally privatization. This sequence is observed even in the countries that went the farthest in privatizing and opening up their economies. Because we have focused on only three reform areas, the number of potential sequences of reform is limited. We are convinced that disaggregating our three indexes is vital for a deeper examination of sequencing issues. For instance, from the raw data we can identify that wage liberalization was preceded by price liberalization in most countries (these are two components of our internal liberalization index).

The paper is organized as follows. In the next section we briefly review the existing measures of reform. Section 3 present our new objective reform indicators and benchmark them against the subjective indicators used in most of the literature. Section 4 compares the performance of our indicators with that of the existing indicators in terms of the determinants of reform and in terms of growth implications. Section 5 concludes and presents some suggestions for future research.

2. Potential Drawbacks of Existing Measures

International organizations are the main source of indicators of reform for our sample of countries. Such indexes have been constructed by The World Bank and the EBRD. The World Bank started this work in the early 1990s by putting forward three reform indicators, covering privatization and internal and external liberalization efforts. Later on, the EBRD took over this task and improved upon the early set by offering many more (nine) indicators,

covering finer, more detailed aspects of reform. The two sets of indexes are constructed in a similar manner, namely in three steps: (1) a comprehensive set of underlying objective variables is collected, (2) a common scale and weighting scheme is agreed upon, and (3) country and sector specialists study these data, judge them and agree on individual scores on each reform item for each country in each year (the top score is set to reflect the standards and performance typical of those in advanced industrial countries.)

One main advantage of this approach is the ability to quality-weight the data. Consider, for instance, a government that chooses to manipulate the data because it believes that if more favourable figures are presented this would increase the likelihood of receiving a loan from an international organization or to improve the terms of that loan. Subjective indexes can to some extent discount, or give a lower weight to, such information. Another main advantage is that these indexes are available in a balanced panel format for all transition economies and for all years since 1991.

The data effort carried out at the World Bank is presented in the *World Development Report 1996* as well as in de Melo et al. (1997). Their overall liberalization index is a weighted average of scores from three areas: (1) internal markets (liberalization of domestic prices and the abolition of state trading monopolies), (2) external markets (liberalization of the foreign trade regime, including elimination of export controls and taxes, and substitution of low-to-moderate import duties for import quotas and current account convertibility), and (3) private sector entry (privatization of small-scale and large-scale enterprises and banking reform.) The weights for this overall liberalization index are determined by expert judgment and set as follows: 0.3 for internal, 0.3 for external liberalization and 0.4 for privatization.

The nine EBRD (2004) reform indicators are as follows: large-scale privatization, small-scale privatization, governance and enterprise restructuring, price liberalization, trade and foreign exchange system, competition policy, banking reform and interest rate

liberalization, securities markets and non-bank financial institutions, and infrastructure reform. For this paper, the EBRD indexes on price and external liberalization and privatization are of particular interest. Regarding price liberalization, the EBRD surveys national authorities and also uses IMF country reports to determine the share of administered (i.e., regulated by the government) prices in the Consumer Price Index as well as the number of goods with administered prices in the so-called EBRD-15 basket.[13] The EBRD also provides information on whether or not wages are regulated. Concerning external liberalization, the EBRD reports on the share of trade in GDP, share of trade with non-transition economies and tariff revenues (in percentage of imports, it includes all revenues from international trade and imports are those of merchandise trade). With respect to privatization, the EBRD surveys national authorities for data on, *inter alia*, the share of privatized enterprises and the estimated share of private sector output and employment to GDP and total employment, respectively. The EBRD then creates aggregate indexes, one for price liberalization, for foreign exchange and trade liberalization, and for small- and large-scale privatization.[14]

These indexes take values from "1" to "4+." For example, regarding price liberalization, higher values of the index are associated with a smaller extent of regulated prices. Thus a score of 1 is obtained when most prices are controlled by the government. A score of 2 stands for some lifting of price administration, yet the state still sets the majority of prices. A score of 3 is reserved for significant progress in price liberalization, but still some involvement of the state in price regulation. A score of 4 stands for comprehensive price liberalization when only a small number of administered prices remain. A score of 4+ means

[13] The basket consists of following 15 goods and services: flour/bread, meat, milk, gasoline, cotton textiles, shoes, paper, cars, TV sets, cement, steel, coal, wood, housing rents and intercity bus service.

[14] Note that the price liberalization index was revised in 2003. The revision stressed the distinction between state price controls and utility price regulation. The improved index focuses solely on state price controls (see EBRD Transition report 2003, p. 18).

that standards and performance are typical to those of advanced industrial countries with no price control outside housing, transport and natural monopolies.

What are the main problems we identify with these efforts? We identify five potential problems: (1) lack of information regarding which are the variables that make up each index; (2) lack of information regarding how the underlying variables are combined into the aggregate indexes; (3) the fact that in the lists of potential underlying variables presented in the above mentioned reports one finds policy inputs as well as policy outcomes; (4) that the indexes change without attendant changes in the underlying data; and (5) that these indexes maximum score refers to an ill-defined reference point such as a "well-functioning market economy" or an "advance industrial economy."

Let us now expand a bit on each one of these potential problems. One first problem we identify is the difficulty in knowing the exact variables underlying each reform indicator. More precisely, accompanying each index one invariably finds a (sometimes large) number of related variables. Yet, a statement indicating exactly which one of these variables is used in computing each reform index is not provided.

A second potential problem is that it is very difficult to know exactly how the reform scores are generated. In other words, we were not able to find a description of how the set of underlying variables are translated into the overall scores. Notice that in the World Bank case, we know how each individual reform indicator is weighted in an overall reform index. But this is not what we have in mind here. In this case, we know how an aggregate reform index is constructed (that is, we do not know which variables are taken into account and what are the weights attached to each one of them) but we do not know how each of the three individual components are constructed (that is, an exact list of underlying variables and set of weights are not provided). The same issue holds with respect to the EBRD indexes.

Third, and in our view the most important issue, is that in the list of underlying variables provided, one finds policy inputs as well as outcomes. For example, in the list of potential underlying variables often presented for external liberalization, one can find tariff levels as well as trade openness. As noted, Rodrik (2005) argues that we learn little from cross-country regressions of growth on reform because, *inter alia*, the literature does not isolate effort from outcomes when measuring reform. Loayza and Soto (2004) and Glaeser et al. (2004) also make this important point.

Fourth, there are many instances in which the overall reform score have been revised despite the fact that the "underlying data" remained unchanged, which suggests that the algorithm may well have changed.[15] This is rather surprising. It is well-known that statistical offices in the former communist countries were excellent in measuring output and employment (in physical terms) but were unprepared to deal with say inflation and unemployment (Bartholdy, 1997). One would expect the underlying data to be revised first, not the indexes.

Fifth and finally, existing reform indicators are not continuous and are also benchmarked against an imprecisely defined reference point. They are categorical variables taking values from 1 to 4+, the latter reflecting the level of liberalization achieved in an "advance industrial economy." In an important paper, Nicoletti and Scarpetta (2003) show that "advance industrial economies" are highly heterogeneous with respect to the implementation of economic reforms which diminishes the usefulness of this comparator.

[15] Another possibility is that learning occurred. There are many examples of such changes, but arguably none less pronounced than the revisions one can observe in the scores for the Baltic countries in subsequent versions of the World Bank papers and of the EBRD's Transition Reports.

3. Constructing New Measures of Reform

The objective of this section is to present our new objective indexes of reform for 25 former communist economies for all years between 1989 and 2001. We constructed three indexes of reform. The first captures internal liberalization efforts and thus reflects the extent of price and wage liberalization. The second captures external liberalization efforts and reflects the severity of trade barriers and capital controls. The third index captures privatization reform efforts. In addition to reporting on the construction of these indexes, we also examine their robustness by (a) excluding outcome indicators (or conversely, by examining input-only measures of reform), (b) assessing reform dynamics across countries for various sub-periods, and (c) comparing our measures with those from the EBRD and World Bank.

In constructing our indexes, we of course want to address each of the major drawbacks we identify in the existing measures. More specifically (and referring to the individual potential problems discussed in the previous section), our goal is to be as transparent and explicit as possible regarding (1) what are the underlying variables that make up each of our three reform indexes, (2) how the underlying variables are combined into each of the reform indexes, (3) how we separate out reform efforts inputs from reform outcomes, (4) how our indexes change over time and relate these changes to changes in the underlying variables, and (5) how we use the in-sample maximum value of each index as a reference point (as opposed to an ideal "well-functioning market economy").

We set out to construct objective indexes of reform for 25 countries for all years between 1989 and 2001.[16] This time window covers the period immediately following the collapse of communism as well as the late transition period (that is, the years following the Russian crisis of August 1998).

[16] These are: Albania, Armenia, Azerbaijan, Belarus, Bulgaria, Croatia, Czech Rep., Estonia, Georgia, Hungary, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Macedonia, Moldavia, Poland, Romania, Russia, Slovakia, Slovenia, Tajikistan, Turkmenistan, Ukraine and Uzbekistan. Bosnia and Herzegovina, and Serbia and Montenegro were excluded for lack of data.

What are the underlying variables that make up each of our three reform indexes? Altogether, we collect data on 44 underlying variables. Three of them assess the extent of internal liberalization; twenty-nine correspond to the measure of external liberalization and the remaining twelve variables capture the extent of privatization. These underlying variables for each of our three reform indexes are listed in Table 1 (which also shows how we choose to separate reform inputs from outcomes, more on this issue below).[17]

Regarding internal liberalization, we collected data for the following indicators: the number of goods subject to price regulation in the 15 goods EBRD basket, the share of administered prices in the consumer price index (CPI), and wage regulation. The latter is a dummy variable reported in the EBRD *Transition Report* and is admittedly a very rough measure of labour market liberalization. The other two underlying variables also originate from the EBRD *Transition Report*, although we have contacted all the 25 national statistical offices and Ministries of Finance (and/or Economy) for assistance in double-checking and filling any remaining data gaps. Out of our three reform indexes, the internal liberalization measure is clearly the closest to the existing indexes (except in that it also includes wage regulation). As discussed below in more detail, despite this resemblance when we examine the correlation coefficients between ours and the existing reforms measures, those for internal liberalization turn out surprisingly to be lower than those for external liberalization and privatization.

Regarding external liberalization, the variables underlying our index are more numerous and it is thus very different from the other (subjective) indexes. Ours contain 29 measures of capital controls and trade barriers. Capital controls indicators are as follows: controls on commercial credit, controls on foreign direct investment, controls on the

[17] A detailed appendix with information on the definition, coding and source for each one of these variables is available from the authors upon request.

liquidation of foreign direct investment, documentation requirements for the release of foreign exchange for imports, exchange rate taxes, interest rate liberalization, investment liberalization, multiple exchange rates, permission requirements for foreign exchange accounts held abroad by residents, permission requirements for foreign exchange accounts held domestically by residents, permission requirements for foreign exchange accounts for non-residents, repatriation requirements, repatriation requirements for invisible transactions, surrender requirements and surrender requirements for invisible transactions. Data on trade barriers include the following: compatibility with Article VIII (current account convertibility), export duties as percentage of tax revenues, export licences, export taxes, import duties as percentage of tax revenue, import licences and quotas, import tariff rate, OECD and WTO membership, trade openness, share of trade with non-transition countries, tariff code lines, tariff revenues as percentage of imports, and tax revenues on international trade (as percentage of revenue).

The underlying variables for external liberalization come from various sources. One main source of data is the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* (Bodenstein, Plumper and Schneider, 2003). Additional data sources are the World Bank's *World Development Indicators,* UNCTAD's *Handbook of Trade Statistics,* EBRD's *Transition Reports,* Penn World Tables 6.1 and IMF, OECD and WTO web-sites. We have again contacted all 25 national statistical offices and Ministries of Finance (and/or Economy) for assistance in filling data gaps.

Our privatization index is based upon the following variables: privatization revenues as percent of GDP, the share of small firms privatized, the ratio of assets of private-owned banks to assets of all banks, total number of enterprises privatized, total cumulative number of private enterprises, total number of small and medium enterprises privatized, total number of large enterprises privatized, share of foreign-owned banks over total number of banks,

private sector share in GDP, credit to private sector, private sector share of employment and private sector investment. The sources of the underlying variables for our privatization measure come from various sources: IMF's *International Financial Statistics*, WB's *ECSPF database*, EBRD's *Transition Reports*, and EBRD survey to Central Banks and national authorities. We have also in this case contacted all 25 national statistical offices and Ministries of Finance (and/or Economy) for assistance in filling data gaps.

How are these underlying variables combined into each of the reform indexes? There are many aggregation methods to generate indexes of privatization, internal and external liberalization. Two obvious candidates are simple arithmetic averages and principal components. One major drawback of simple averages is that when we have so many different underlying variables in different units and scales, the ensuing values of the indexes would be difficult to interpret. One major drawback of principal components is that maximum and minimum values of the resulting indexes are entirely determined by the data and have no clear economic meaning.[18] For these two reasons, we choose to apply the methodology developed by Lora (1997). One important advantage of Lora's method is that it has been developed and used previously for similar purposes (that is, to capture various reform efforts across countries and over time). Our overall index *I* for *i*-th country is constructed as follows:

$$I_i = \frac{1}{m_i}\frac{1}{n_i}\sum_{j=1}^{m_i}\sum_{t=1}^{n_i}\left[\frac{V_{j\max} - V_{jit}}{V_{j\max} - V_{j\min}}\right] \qquad (1)$$

where *V* is a value of *j*-th variable in *i*-th country in time *t*. *n* stands for the number of the years (typically 13 years) and *m* for the number of variables (maximum of 35). Also note that $V \in \langle 0,1 \rangle$ for all *i, t*, which is because we normalize the 'raw' *V* value of the *j*-th variable by the maximal value observed in all countries in time *t*. Notice that, firstly, in the case of year-by-year indexes, we do not average over time and thus all terms containing *n* drop out.

[18] Simple averages and principal components versions of our indexes are available upon request.

Secondly, the equation holds when the higher values of underlying variable indicate less reform (for the opposite case, the numerator become the difference between the actual value and the observed minimum).

A major advantage of the Lora transformation is that the reference point is within the sample. In other words, this method does not require to benchmark reform efforts against an ideal "well-functioning market economy." Our reference point is the maximum reform effort observed across our sample of countries in the respective time window. The major cost of this choice is that enlarging the time window and/or the sample of countries can potentially affect the values of our indexes. Needless to say, we believe that this drawback is preferable to those of benchmarking against something that cannot be defined with sufficient precision, neither across countries nor over time.

In aggregating the underlying variables, we must address the issue of weights. In our view, it is impossible to determine the 'true' set of weights. Further, these weights probably change from country to country and over time. As a result, we decided it is more transparent to use equal weights for all underlying variables. This can clearly be improved upon, although this would be difficult to accomplish in a non-arbitrary manner.

How do we separate out reform inputs from reform outcomes? The distinction between input and output variables is not always clear cut. When measuring reform, it is advisable to focus on the indicators that are directly under the control of the government (Rodrik, 1996; Loayza and Soto 2003). Including outcome indicators in the construction of aggregate reform indexes may introduce bias in estimating the degree of liberalization. This is so for the simple reason that outcome indicators can be the result of many things other than reform inputs.[19] In addition, it may well be the case that there is a time lag between reform

[19] For instance, the share of trade with non-transition countries may be strongly affected by the geographical proximity to non-transition countries. Therefore this variable should not be included in an input-only index of external liberalization efforts.

inputs and reform outputs. Therefore, we construct input-only measures for the three reform policies yearly for 25 former communist economies, but also compute indicators combining inputs and outcomes as a robustness check. Our prior is that the correlations between the existing reform indexes will be lower with respect to our input-only indexes (which are the ones we prefer) than with respect to our indexes that combine reform inputs and outcomes. As noted below, our results support this statement which can be taken as evidence that the existing subjective indicators do indeed mix inputs and outcomes (although we can not be sure of that as the precise lists of underlying variables are seldom provided).

Notice also that in selecting reform outcomes variables we want to minimize the probability that a "true reform input" is mistakenly classified as a "reform outcome." By doing that, the resulting list of reform outcomes may contain variables that are clearly reform outcomes and, conversely, the list of reform inputs will contain variables for which a certain level of reform outcomes is present. The reason for doing this is to try to "stack the cards" against our indexes as this will surely minimize the differences (over time and across countries) between ours and the existing reforms measures. We consider as outcome indicators the following variables (the first column of Table 1 list reform inputs, while the second lists reform outcomes). For internal liberalization, we single out as an outcome indicator the share of administered prices in CPI. This is because this is a function of the share of non-administered prices in CPI. Thus if the introduction of new goods is beyond the control of government's internal liberalization policies so is the total number of goods (prices) in the economy. For external liberalization, we consider as outcome variables the share of trade with non-transition countries, openness, import duties as percent of tax revenues, tariff revenues as percent of imports and the taxes collected on international trade. For privatization, we consider as outcome variables the private sector share in GDP, credit to private sector and private sector investment.

How do our input-only objectives indexes of reform change across countries and over time? There are two general observations we need to make at the outset. The first is that the correlation coefficients among our measures of reforms are all positive, but rather low. The correlation between internal and external liberalization is 0.48, between internal and privatization is 0.39 and between external liberalization and privatization is 0.66.

The second general observation refers to reform reversals. While reform measured by the World Bank and EBRD indexes is better portrayed as a smooth, uninterrupted process of continuous improvement (note that this is even more so when considering the cumulative version of those indexes), it is a much more turbulent process according to our measures. Ours show a fair amount of trial and error and experimentation which translates in the occurrence of numerous reform policy reversals. This matters because reform reversals are at the heart of the theoretical literature (e.g., Dewatripont and Roland, 1995). Merlevede (2003) calculates reform reversals using the EBRD indexes and reports 21 cases in which the subsequent value of a reform indicator is lower than the current value (we also adopt this definition of reform reversal here). Because this is from a total of 237 changes, it implies that reversals observed in 8.9 percent of the cases. Also note that Merlevede considers reversals across all nine EBRD reform indicators. Considering just our three indexes, we obtain a much larger number of reversals, we find: 42 reversals out of 295 (14.2%) in the internal liberalization reform indicator; 61 reversals out of 300 (20.3%) in the external liberalization reform indicator; and 44 reversals out of 243 (18.1%) in the reform indicator for privatization efforts. Further, using the EBRD reform measures Merlevede (2003) finds that only half of the countries experienced reform reversals. According to our indexes basically all countries have experience at least one reversal in one of the three reform dimensions we consider.[20]

[20] Given the small numbers of reversals previous studies found, it is common practice to comment on each one of them. There are too many reform reversals in our data to comment on each one of them individually (we do comment on a selected few below), therefore in the following section we provide an econometric analysis of the determinants of reform reversals.

3.1 Internal Liberalization Index

Figure 1 shows our input-only internal liberalisation index on a yearly basis for all the 25 countries in our sample.[21] Overall, there is clear progress across countries over time in terms of the liberalization of prices and wages. Notice, however, that reform reversals abound. As for example, in Albania and Ukraine in 1997, Lithuania in 1995 and Uzbekistan in 1994. Most of these seem related to wage regulation, which plays a significant role in the index. For example, Lithuania according to our data stopped regulating wages in 1995 and consequently the value of the index increases accordingly. Similarly, a large drop in the value of index for Uzbekistan is a result of introducing wage controls in July 1994 (Anderson and Pomfret, 2002). Albania deregulated wages in 1997 resulting in a large increase in the value of the index. Slovakia and Ukraine reintroduced wage regulation in the years 1997-1998 and the value of index declines accordingly. This is as good a moment as any to highlight the fact that these jumps could easily have been put aside if we, for example, chose to weight wage regulation in a different manner. For instance, if we arbitrarily halve its weight then the overall index would appear smoother than it actually is. Because this particular index is composed by very few variables which are equally weighted, any changes in the underlying variables will have large consequences in terms of the aggregate index. [22]

In terms of the overall ranking of countries, we find Kazakhstan highly placed. Although this is somewhat surprising, it can easily be explained by tracing the index through its underlying variables: Kazakhstan has the lowest number of goods subject to price regulation and the lowest possible score for wage regulation for all 25 countries during 7 of the 13 years (1989 to 2001). The second and third places are (less surprisingly) occupied by Estonia and Hungary, respectively, while at the bottom of the ranking we find Romania,

[21] See Appendix 1 for country-specific data.
[22] The source of most pre-1991 data for the former Soviet Union countries is national authorities.

Belarus, Russia and Moldova. Countries such as Albania or Georgia made great progress in terms of price and wage liberalization only more recently. On the other hand, Turkmenistan and Uzbekistan seem to have moved towards greater regulation over time. Overall, the internal liberalization index seems unaffected by the exclusion of outcome variables and to splitting the time window. The Baltic countries and countries from the Caucasus seem to be slightly more liberalized in terms of price and wage regulation. Using the input-only index, we still find the ranking topped by Estonia, Kazakhstan and Hungary. At the bottom, we find Moldavia and Belarus which few observers would find surprising.

How do these measures compare to the existing subjective indicators? Note that the EBRD liberalization indexes cover the years 1991-2001 and the World Bank index developed by de Melo et al. (1996) is available only for 1989-1997. Thus, correlation coefficients are based only on the years for which all the corresponding data are available. The correlation between our input-only internal liberalization index with the EBRD's is 0.52 and with the World Bank's is 0.38, while the correlation between our index combining inputs and outcomes is 0.42 with the World Bank's and 0.56 with the same EBRD's index.[23] As noted, while our measure is at first sight very similar to the EBRD's, the correlation between them is lower than in the cases of external liberalization and privatization (see below).

Figures 2 and 3 show the behaviour of our internal liberalization index over time, when we divide the sample into two groups, namely Central and Eastern European Countries (CEECs) and the Commonwealth of Independent States (CIS). CIS include all former Soviet Union republics except the Baltic States. For the sake of comparison, we normalize the EBRD indexes in that smaller values refer to less liberalization effort. We present the two

[23] If we use only data in the period of 1997-2001, the coefficient falls slightly to 0.49 for both our indexes (with and without outcome indicators). The correlation of our index with WB index in the years 1989-1994 is 0.42 and 0.38 after excluding the outcome indicators. If we extend the WB index to include the years up to 1997, the corresponding correlation coefficient rises to 0.48, respectively 0.53. Additionally, the correlation between our internal liberalization index with and without the outcome indicators stands at 0.97.

indexes in Figures 1-3 below. Visual comparison suggests that the EBRD may have been somewhat more generous in rating internal liberalization in the CEEC vis-à-vis the CIS, and this may have been particularly so for the period 1989-1995.

3.2 External Liberalization Index

Our external liberalisation index is shown in Figure 4. It is worth noting that, as it is the case with the internal liberalisation index, the EBRD external index clearly indicates more liberalisation on average than our index for all years but 2001 (although the gap between the measures early on is not as great as in the case of internal liberalisation). Except for 2001 (marginally), our indexes suggest that external liberalisation efforts were less intensive, on average, than internal liberalisation efforts.

In terms of the overall ranking of countries, we find Latvia on top closely followed by Estonia (see Appendix 1). The third post is (maybe) surprisingly occupied by Kyrgyzstan and this is because of extremely high values for external reform effort from 1993 onwards across the range of 29 variables underlying our index. At the bottom of the ranking we have Uzbekistan, Turkmenistan and Albania. The greatest jump we observe in the value of index is for Turkmenistan in 1999. Examining the underlying variables, we find that Turkmenistan, *inter alia*, liberalized interest rates and abolished multiple exchange rates in 1999. Other examples worth mentioning are: Uzbekistan reintroduced severe exchange rate controls as a reaction to a balance of payments crisis in the autumn of 1996 (Pomfret, 2000). This is reflected in the large decline in the value of its external liberalization index from 0.32 in 1996 to 0.24 in 1997.

One issue that is important to keep in mind is that there is considerably more variation in terms of external liberalisation both across countries and over time than it is the case for internal liberalisation so a closer analysis of the trajectories of each country is worthwhile: we

learn that there are more cases of policy reversal but none of them as severe as the ones seen for price liberalization. Admittedly, this can be caused by the smaller number of underlying variables for the latter measure.

Examining the relative rankings, the results are in line with expectation both for the top and for the bottom countries. Indeed for the top three it is maybe mildly surprising that the Baltics are still such intensive reformers even in the very late transition years. The external liberalization indexes show that the highest average values were found for the Czech Republic, Latvia and Estonia while the lowest values were for Turkmenistan, Uzbekistan, and Belarus. As noted, the correlation coefficients between our external liberalization and the EBRD's and World Bank's indexes are higher than for internal liberalization. The correlation with the World Bank's is 0.73 and with the EBRD's is 0.81, while the same coefficients for our input-only external liberalization indexes are lower, at 0.65 and 0.79, respectively.[24]

Figures 4 to 6 show that our external liberalization indexes never "crosses" the EBRD index after 1991, in other words, the EBRD index indicates more liberalisation on average than our index for almost all years. Again, the gap tends to be larger for the CEEC than for the former Soviet Union countries.

3.3 Privatization Index

Figure 7 presents our privatization index. Concerning country-specific results, it is worth noting that only Hungary and Poland privatized their economies, to a certain extent, before 1991. Generally, Hungary shows the greatest extent of privatization, followed by Slovakia, Macedonia and Latvia. On the other hand, Belarus and Turkmenistan have made the least

[24] If we restrict sample to 1997-2001, the correlation is somewhat higher at 0.81 and 0.79 excluding the outcome indicators. The correlation coefficient with 1989-1994 World Bank's index is 0.73 and 0.65 excluding the outcome indicators. The correlations are almost unchanged, if we use 1989-1997 World Bank's index. They are 0.71 and 0.69 without outcome indicators for our index. Finally, the correlation coefficient is 0.93 between our external liberalization index with and without outcome indicators.

progress in privatization. It is interesting to note that for the most intensive reformers in this respect the process is much less bumpier than in those countries which privatized their economy only partially (see Appendix 1). Further examining major changes in the values of the privatization measure, Hungary's privatization revenues (normalized by GDP) increased sharply in 1995 resulting in a substantial improvement of the index (see Canning and Hare, 1996). The index declines sharply for Macedonia in 1994 and 1995 for a number of reasons but principally because of a large reduction of credit to private sector as per cent of GDP which falls from 45 to 23 per cent from 1994 to 1995.

Generally, the greatest privatization efforts seem to have been undertaken in Hungary, Slovakia and the Czech Republic, while little privatization seem to have been attempted in Belarus and Turkmenistan. We should also note that typically the values of our indexes are lower for privatization than for internal and external liberalization which hints at a specific reform sequence adopted by these countries that entailed leaving privatization for later. The correlation between our privatization index and those from the EBRD is 0.8 and 0.66 if we exclude the outcome indicators. If we restrict the sample to 1997-2001, the simple correlation coefficients are 0.82 and 0.64 (after the exclusion of outcome indicators). The correlation with 1989-1994 World Bank's index is 0.52 and 0.43 after exclusion of outcome indicators. The correlation coefficient between our privatization index with and without the outcome indicators is 0.94.

Figures 7 to 9 show that the verdict from our index of privatization efforts is also less optimistic than the assessment from the EBRD and World Bank. Interestingly, the difference between ours and the EBRD index for privatization is larger for CEEC, where we find relatively smaller progress in privatization. Again, CEEC countries seem to have been more aggressive privatization reformers than those in the CIS.

4. What difference do objective measures make? Determinants and reversals

The objective of this section is to assess the usefulness and reliability of these new objective measures of reform. We try to do so in three ways: by studying the factors that explain reform dynamics; by analyzing the determinants of reform reversals; and by addressing the potential endogeneity of reform vis-à-vis growth by re-estimating various econometric models from the literature.

4.1 Reform Determinants

What are the main determinants of economic reform? Here we consider some of the key insights from the theoretical literature on the political economy of reform to throw light on the determinants of each of our three indicators. The literature discussed above has generated a number of hypotheses that have rarely been tested empirically so this is an important exercise in itself (Roland, 2000). One set of potential reasons is related to economic conditions: favourable changes in economic conditions (e.g., less unemployment) allow greater margins to the reforming government to compensate losers and thus implement "more reform" (Aghion and Blanchard, 1994). A closely related notion is that of the consequences of a sharp deterioration in economic conditions: an economic crisis will, almost by definition, increase the number of supporters (that is, potential winners) of reform (Fernandez and Rodrik, 1991). Another well understood set of reasons relate to politics, e.g., countries becoming more democratic would be able to implement more reform (because, for instance, repeated democratic elections allows the monitoring of compensation promises to -and by- potential losers). Further, in this particular context, less concentrated political power (an example would be communists loosing seats in Parliament) would be associated with the implementation of "more reform" (Hellman, 1998).

Let us now turn to the econometric methodology. There are two main questions of interest: (a) what are the factors that determine the dynamics of reform across countries and over time? And (b) are the set of determinants the same for each of the three reform areas (namely, privatization, and external and internal liberalization)? While the rationale for the first question has already been presented, that for the latter is still needed. Based on the large case study evidence on reform dynamics cited above, on what we learned about this process in collecting the underlying data for our objective indexes and the attendant discussion with public officials, our prior is that these three reforms are driven by different factors. This is partly because we observe a distinct sequence of reforms. Internal liberalization happened immediately after the fall of communism (often with the exception of wage regulation). This was followed by external liberalization and then by privatization efforts. This sequence makes it unlikely that the same set of determinants would hold for each of our three different reforms measures. Moreover, if there were an identical set of determinants there will be little reason to focus on individual reforms, as an aggregate index would be a proper way to proceed. In our view, such strategy would not do justice to the complex dynamics we observe with respect to reform efforts across these countries over time (our results below corroborate this insight).

In what follows, we will examine the determinants of each of the three reform areas separately.[25] We first estimate the following equation:

$$R_{itc} = \beta_0 + \beta_1 GDPgrowth_{tc} + \beta_2 Unempl_{tc} + \beta_3 Democracy_{tc} + \beta_4 IC_{tc} + \beta_5 V_{tc} + \varepsilon_{itc} \qquad (2)$$

where R_{itc} stands for our objective indexes of reform, with i denoting reform area (privatization, external or internal liberalization), t denoting year and c denoting country. $GDPgrowth_{tc}$ is the rate of real per capita GDP growth (in country c and year t), $Unempl_{tc}$ is the unemployment rate, $Democracy_{tc}$ is the Freedom House index of democracy (the

[25] A standard Hausman test is used to determine whether fixed- or random-effects estimator is appropriate.

continuous version from their *Nations in Transit* project), IC_{tc} is a principal components index of initial conditions[26] and V_{tc} is a vector of auxiliary control variables. We expect the coefficient on GDP growth, unemployment and initial conditions to be positive (the hypotheses are that faster growth, higher unemployment and more favourable initial conditions are more conducive to the implementation of reform) and expect the coefficient on democracy to be negative (the hypothesis being that democracy is more conducive to the implementation of reform).

Table 2 has our econometric results on the determinants of each of our three indexes of reform. The first three columns show these results for our internal liberalization index. In this case, the Hausman test indicates that the random-effects estimator is appropriate so we report accordingly.[27] The results in column 1 broadly confirm our hypotheses: GDP growth, unemployment and democracy all exert a positive and statistically significant impact on internal liberalization efforts. The addition of initial conditions (reported in the next column) has little impact on those coefficients. Yet, the results suggest that countries with more favourable initial conditions are more likely to implement internal liberalization reforms. Also notice the substantial increase in the R-squared once initial conditions are accounted for. In the third column we add a Herfindahl index of concentration of political power (referring to the number of seats in the lower house). Although this addition proves to be of interest (we find that in parliaments in which political power is less concentrated, reforms move farther), the coefficients on growth and unemployment are now less precisely estimated.

The next set of columns has the results for external liberalization. In this case we have a more robust set of determinants in that now GDP growth, unemployment and democracy all have the expected effects. It is interesting to note that while the effect of democracy is

[26] Initial conditions are a measure of the macroeconomic distortions as of 1989 and are constructed in line with, among others, EBRD (2002) and Merlevede (2003).
[27] There is very little difference with respect to the fixed-effects estimates (with the obvious exception of the initial conditions measure, which is excluded by the latter).

somewhat smaller for internal liberalization than for external liberalization and privatization, this is inverted with regard to initial conditions. We believe this is in part because of the timing of these reforms: internal liberalization takes root much faster than external and privatization, thus leaving little time to the process of democratization to play a fuller role (notice however that low concentration of political power is an important determinant in this respect) with a similar reasoning applying for the role of initial conditions on privatization (the latter generally takes place too late for the effect of those initial conditions to be fully felt). Also notice that we report that the growth of OECD countries, as a measure of global economic conditions, is an important reform determinant in this case: external liberalization is more likely to be implemented under favourable global economic conditions.

As it also can be seen from Table 2, the results for privatization are in line with those for external liberalization. GDP growth, unemployment rates and initial conditions show a positive and significant impact, while the coefficient on democracy also accords to our priors. It should be noted that there are potentially important endogeneity issues to which we return in section 4.3. For example, the consistent result we obtain showing that higher unemployment rates are associated with more reform efforts can be understood in conflicting manners: it can well be that rising unemployment enlarges the ranks of potential winners and would thus increase the support for reform leading to the intensification of reform efforts (which is, as noted above, consistent with the Fernandez and Rodrik model) or it can be that reform directly causes a (temporary) increase in the rate of unemployment (consider the case of privatization).

We subjected the results above to various sensitivity checks (see Appendix 2). We find that greater inflation rates decrease external liberalization and privatization efforts (but not internal liberalization.) We also investigate the role of financial (measured as the weighted average of exchange rate pressure and interest rate differentials) and do not find that

it affects our reform indicators with the somewhat surprising exception of privatization (similarly, fiscal deficit is never statistically significant). We have also examined a number of political issues. EU negotiations (a dummy variable taking the value of 1 from the year when EU accession negotiations started) is found to affect positively external liberalization as well as privatization efforts, but not internal liberalization. This is maybe a consequence of price and wage liberalization occurring well before negotiations started. The occurrence of violent conflict was found to have a surprisingly limited impact: controlling for initial conditions in our baseline specifications, the coefficient on wars is statistically significant at conventional levels only for the case of privatization. Following Frye and Mansfield (2004), we create a variable capturing the electoral calendar (the number of years until elections). Surprisingly, we find little evidence for the timing of elections in driving any of our reform indicators except for external liberalization. Further, we employ various measures of changes in government's ideological orientation to address the potential effects of political alternation on reform. For this purpose we use the number of ideological alternations (e.g., from center-left to center-right) and the number of leadership changes.[28] We find that cumulative leadership and political changes are positively associated with our three indexes of reform. Not surprisingly, for two of them (internal and privatization) we find that these measures of political alternation substitute for democracy as the latter becomes insignificant after the inclusion of any of the two alternation variables. Finally, we have undertaken a spatial econometrics exercise to examine the role of distance among countries. The idea here is to assess the possibility of reform contagion: whether countries are more likely to implement reform say because of learning from the experience of close neighbors or because reform in close neighbors directly induces domestic reform (consider the case of competition among

[28] We are grateful to Branko Milanovic for these data. Ideological alternation is defined as any change of the government's ideological orientation on a 16-cell ideological grid. Leadership change captures personal changes in the locus of power whether it is a change of prime minister (due for example to a change in the governing majority) or president. For details, see Hoff, Horowitz and Milanovic (2005).

countries for FDI inflows). However using a variety of such measures (distance from Brussels, distance between capital cities, whether or not previously part of the Soviet Union), we fail to find that reforms are driven by how much other "close" countries reform. In a nutshell, with the exception of the political and ideological alternation variables, we find no robust additional determinant of reform efforts, while our principal results remained unchanged.

4.2 Reform Reversals

A second way to assess the usefulness and reliability of our objective reform indicators is to try to explain reform reversals. There are at least two motivations for this: one is that although reform reversals occupy a central place in the theoretical literature, there are to the best of our knowledge no systematic efforts to try to explain these reversals empirically. The potential reversibility of reforms plays a fundamental role in the theoretical normative political economy of reform literature. Most of the theoretical contributions in this line of research explicitly attempt to devise ways of designing reform packages that have a low probability of reversal (see, e.g., Dewatripont and Roland, 1995.)[29] The second motivation is that this gives us a way to assess the reliability of our new objective measures. It may well be the case that the larger number of reversals is not an indication of our indexes' quality but, a critic may charge, they reflect the fact that these indexes are finely measured and small change in the underlying variables as well as country and time coverage may have an undesirably large impact on our measures. One way to try to put aside this concern is to explain reversals: if we can not provide a reasonable explanation then the critics may be right. We try to model reversals in two ways. In the first a reversal is simply defined as a decrease

[29] Some of the mechanisms devised to deal with this issue are compensating packages, gradual implementation of reform and adherence to institutions that commit to future transfers (or directly to the implementation of reforms associated with WTO or EU accession).

in the value of any of our three indexes in two consecutive years (a dependent dummy variable that takes the value of 1 if a reversal occurred, and zero otherwise), while in the second we consider a particular measure of the severity or persistence of reversals, namely how many times reversals have occurred consecutively (the dependent variable is 1 when reversal occurred, 2 when reversals occurred in two consecutive years, and so on.)

One feature of the available subjective indexes of reform is that, according to them, reform reversals seldom occur. Although this can of course be explained by well-informed policy-makers, other non-excludable explanations include that there may be political pressure on the international organizations constructing these indexes not to lower previous scores, or that national authorities mistakenly understand the survey questions as referring to their cumulative efforts. As such, the empirical analysis of reform reversals has been scarce especially vis-à-vis the central role it occupies in the theoretical literature. To the best of our knowledge, the only paper dealing with reform reversals is Merlevede (2003) who reports 21 instances of reversals out of a total of 237 changes (in other words, only 8.9% of annual changes in the EBRD indexes are negative). Here we only use our three indicators. As mentioned above, in terms of our index of internal liberalization, we observe 42 reversals out of 295 annual changes (14.2%), in terms of our index of external liberalization we observe 61 out of 300 (20.3%) and in terms of our index of privatization we observe 44 out of 243 (18.1%). These ratios are much higher, indicating twice as many cases of reversals than before. As discussed, this can well be caused by the fact that our objective indicators are measuring reform too finely or it could also be caused by the non-availability of a few variables for that particular year (as the panel data set for the underlying variables is in some cases unbalanced).

What explains reform reversals in these economies?[30] It is important to note that in the empirical literature this question has not been raised, as Merlevede (2003) is concerned mainly about the impact of reversals on growth. One potential reason for reform reversals is unfavourable changes in economic conditions. An economic crisis or, more mildly, a sudden increase in unemployment or a sudden slowdown in growth rates may change the support for reform and cause a reversal. Another potential reason is a change in political conditions. For instance, if we observe major turnovers of the leading party in government, then reversals are likely (a left-wing that succeeds a right-wing government may favour re-distribution or compensation).

In this light, we estimate a random-effects logit model in which the dependent variables is coded 1 if there was a reform reversal in that indicator in that year for that country, and zero otherwise. In the following model we investigate political and economic factors as potential explanations for reform reversals:

$$P(reversal_{itc} = 1) = \Phi(\beta_0 + \beta_1 GDPgrowth_{tc} + \beta_2 Unempl_{tc} + \beta_3 Democracy_{tc} + \beta_4 V_{tc}) \qquad (3)$$

where $reversal_{itc}$ is a binary variable indicating whether reform i in country c at year t has experienced a reversal (defined as a decline in the absolute value of the index); $GDPgrowth_{tc}$ is the rate of real per capita GDP growth (in country c and year t), $Unempl_{tc}$ is the unemployment rate, $Democracy_{tc}$ is the Freedom House index of democracy (the continuous version from their Nations in Transit); V_{tc} is a vector of auxiliary control variables; and Φ is the cumulative logistic distribution function. Note that this means we do not expect the reform determinants we found above to play an important role in the reversals case. By

[30] Notice that this exercise differs from the one on reform dynamics above. The latter explains both positive and negatives variations from the mean. Its validity is an empirical question: if the set of determinants turns out to be different between reform efforts and reform reversals this should reinforce the confidence we can attach to our measures. The latter also captures a sense of the magnitude of these variations (including reversals of course). However, in this exercise we treat all reversals in the same way, giving them equal weight to small and large reversals in trying to explain their occurrence. In our view, this stacks the odds against our indicators and as such is a conservative test of their usefulness and reliability.

performing a separate analysis of the bottom-half of the distribution of our reform indicators we expect they are driven by different factors than the mean itself. If that turns out to be the case, we gather additional support for the reliability of our measures. Yet failing to explain reversals can work the other way as it would suggest that our measures are maybe too fine and are not reflecting changes in reform efforts but are picking up mostly noise. Accordingly, we should expect that the majority of the explanations will come from the vector of auxiliary control variables, ideally with individual variables playing a role for the reversals of each of our three individual reform measures.

The first columns of Table 3 show our baseline results for internal liberalization reversals. These seem driven primarily by political factors, specifically by a very direct form of protest (namely labour strikes). In some specifications, high rates of unemployment are associated with the likelihood of reversals, but this result is not robust. The significance of labour strikes points to the importance of a better grasp of political initial conditions in understanding reform dynamics. It also highlights the timing of reforms: because internal liberalization (according to our indexes) was implemented before external liberalization and privatization, the most effective way to revert internal liberalization may have been by direct influence.

The results with respect to our two other indexes are also intuitive. We find that in terms of privatisation reversals the main explanatory factor are FDI inflows, while those factors explaining external liberalisation are average growth rates of OECD economies (an increase in those rates imply a higher probability of reversal), while terms of trade shocks show a negative impact in the probability of reversal.

Table 4 repeats these specifications but instead of explaining whether or not a reversal occurred, we try to understand the persistence of reform reversals. We report random-effects negative binomial estimates in which the dependent variable is the number of consecutive

years for which we observe a reform reversal. Interestingly, the results for persistence are in line with those for reversals: labour strikes explain the persistence of internal liberalization reversals, OECD growth explains the persistence of external liberalization reversals and FDI inflows explains the persistence of privatization reversals.

We subjected these results to a similar battery of sensitivity tests we subjected our reform determinants above. Although we find that our main results are indeed robust to the inclusion of a number of important variables, none of them proved to be systematically related to reform reversals. We find no systematic effects from inflation, financial crises, fiscal deficit, EU negotiations, war, the timing of elections, ideological alternations, the number of leadership changes, and distance from Brussels, distance between capital cities, whether or not previously part of the Soviet Union as measures of distance in out spatial econometrics exercise (see Appendix 2). The results are similar with respect to the persistence of reform reversals, with three exceptions. Two of them regarding the external liberalization index: we find that inflation and proximity as measured by being a former Soviet Union Republic increase the persistence (or severity) of external liberalization reversals. We also find that ideological alternation tends to increase the persistence of internal liberalization reversals. In sum, we find no robust additional determinants of reform reversals (or of their persistence), while our principal results remained unchanged.

4.3 Endogeneity issues: Do the new objective indexes change existing results?

We believe that the two sets of results above (on the determinants of reform dynamics and on reform reversals) suggest that our indexes are useful and reliable in helping further our understanding of reform. Yet, the literature on the economic effects of reform has long recognized that growth and reform may be jointly determined. The reform process is carried out in the expectation that its outcomes will translate into faster growth rates, while at the

same time a growing economy enables a reformist government to compensate losers from reform and thus continue, or even intensify, the reform process itself. There is also the notion that the impact of reform on growth occurs with a time lag: the contemporaneous effect of reforms on growth may be negative, while at the same time the lagged effect may be positive. It is thus important to investigate what are the ultimate consequences, in terms of existing econometric results, of using our indexes. Moreover, our results so far do not reflect the concerns expressed in the more recent literature regarding endogeneity and robustness. One way to address such issues is to directly re-estimate some of the "reform equations" from the literature using our reform indicators instead and compare the ensuing coefficients. Also, there is now a somewhat sizable literature on the effects of reform on growth which has, generally speaking, found a positive impact of reform on growth when reform is proxied by the subjective indicators we discussed above. It is also important to investigate whether our objective indexes change these results.

We select four well-known papers which report a "first-stage" reform equation. Notice that all of these are part of a system of equations (that is, they are all estimated jointly with a growth equation), which if reported in the original paper is also replicated in the present study. To this end, Table 5 contains the "reform equations," while Table 6 has the corresponding "growth equations." The reform equations we re-estimate are originally from Heybey-Murrell (1998), Merlevede (2003), Falcetti, Reiser and Sanfey (2002) and Kim and Pirttila (2003). It is worth noting that we use exactly the same variables in the replication with one exception, which is that we change the reform measure used by the authors with our aggregate index of reform.[31]

[31] In order to replicate these results we need to generate an aggregate index of overall reform effort (which is done using the Lora algorithm described above and averaging the three reform indexes we computed).

Heybey and Murrel (1998) specify that reform (as measured by the World Bank indexes discussed above) is a function of economic growth, democracy as measure by the Freedom House index, the extent of initial economic liberalization and the share of manufacturing on GDP. Their results are reproduced in the second column of Table 5: although economic growth facilitates the implementation of economic reforms, an extensive history of reforms attempts seems to be a hindrance (maybe reflecting Kornai's "reform windmill"). Using our reform indexes, we are able to reproduce their results with respect to the initial level of liberalization and indeed strengthen them in the sense that the coefficient is larger and estimated more precisely. Yet, we could not reproduce the result for economic growth.[32]

Merlevede (2003) analyses the impact of reform reversals. The paper reports the coefficients from a reform equation and we are therefore able to replicate them using our indexes. Merlevede's data show that reform (measure by the EBRD indexes) is driven by contemporaneous economic growth and democracy as measured by the inverted Freedom House (in the inverted index, higher figures indicate more democracy). He also reports that initial conditions matter in explaining reform dynamics. Using our reform indexes, we are able to replicate the finding that democracy is an important factor in driving reform. However, we do find that although contemporaneous growth is positively associated with reform progress, one-year lagged economic growth turns out to be inversely related to reform. The latter result is usually interpreted as supporting the notion that economic crises are important determinants of reform. Finally, we find little support for the role of initial conditions.

As it can also be seen from Table 5, one main difference between the reform equation by Falcetti, Reiser and Sanfey (2002) and that by Merlevede (2003) regards the

[32] The replication results for Heybey and Murrel's growth equation is available upon request.

comprehensiveness of initial conditions.[33] Otherwise, the two models are identical for practical purposes, especially because in Merlevede this one differing coefficient is statistically insignificant. Our reform indexes not only reproduce the growth effects reported by Falcetti et al., but they are also in this case estimated more precisely and the size of the coefficients is once again larger. The situation with respect to democracy is similar, though the size of the coefficient is now smaller. One main difference we obtain when treating reform and growth as jointly determined is that we find little evidence supporting the notion that initial conditions play a significant role in explaining reform dynamics (recall that in section 4.1 above we find that initial conditions matter for reform dynamics, but that was without taking endogeneity into account).

The last reform equation in Table 5 is from Kim and Pirttila (2003). It explains reform dynamics using various macroeconomic variables. More specifically, growth and budget surpluses are expected to foster reform efforts (because, for instance, both increase the government credibility in terms of compensating potential losers) and unemployment and inflation are expected to hinder reform efforts (because, for instance, both increase individual and aggregate uncertainty, as in Dewatripont and Roland, 1995). Our reform indexes not only reproduces the growth effect reported by Kim and Pirttila (2003), but have it estimated more precisely and, once again, the value of the coefficient is larger than in the original. Interestingly, however, we find opposite results with respect to unemployment: in ours, changes in the rate of unemployment (note that the Arellano-Bond estimates refers to variables in first-differences) are associated with an increase in reform efforts. Interestingly,

[33] Following this literature, we used the principal components method to cluster initial conditions in two groups: the first denoted by IC1 is interpreted as capturing the macroeconomic distortions inherited from socialism as the largest loadings are for the exchange rate black market premium before 1989, repressed inflation during 1987-90 and share of CMEA trade over GDP in 1990. The second cluster of initial conditions, IC2, can be interpreted as the level of socialist development as the largest loadings are for real GDP per capita in 1989 and share of population in urban areas in 1990.

the coefficients on inflation and unemployment both have the same sizes as in Kim and Pirttila, but also opposite signs.

We now turn to the replication of the attendant growth equations to investigate whether using our objective reform indexes change existing results on the economic impact of reform. The growth equations we re-estimate are originally from Fidrmuc (2003), Merlevede (2003), Falcetti, Reiser and Sanfey (2002) and Kim and Pirttila (2003). In our view, these are representative of the best efforts available in trying to understand the growth implications of reform as they incorporate most of the insights from theory. For instance, these authors examine the dual effect of reform on growth (a positive lagged effect is normally found jointly with a negative contemporaneous effect), the separate importance of cumulative reform, and the role of initial conditions.

The specification from Fidrmuc (2003) has as main arguments contemporaneous reform, fiscal balance, school enrolment and investment rates, and involvement in armed conflict. The first column in Table 6 shows Fidrmuc's original coefficients while the second column shows ours. As it can be seen, our reform indicators are able to replicate all the original results, but with our measure these coefficients turn out to be larger and more precisely estimated (notice that originally the coefficient on investment was not statistically different from zero.)

The specifications from Falcetti et al. (2002) and from Merlevede (2003) are similar. The main difference is the latter having an additional dimension for initial conditions as well as a dummy variable capturing the occurrence of reform reversals. From the Falcetti et al. equation, we are able to replicate and improve upon the reform results (again more precisely estimated and larger). Note however that using our indicators, we find little support for the role of initial conditions as well as for the role of fiscal balances.[34] The outcome is similar

[34] It is important to keep in mind that in replicating these results, we have measured the variables used,

with respect to Merlevede's specification: the coefficients on our objective reform indicator are more precisely estimated and larger in size than in the original paper.

Finally, the results from Kim and Pirttila (2003) are shown in the last two columns. These authors argue that growth is mainly driven by reform, cumulative reform, fiscal balance, investment, and inflation. Using our objective reform indexes, we are again able to replicate these results although this was the only case in which our coefficients are slightly smaller and not in every case more precisely estimated.

In sum, our new objective measures of reform seem able to replicate key results from the literature. These new measures show a much stronger effect on growth (contemporaneous and lagged), but a smaller (yet still significant) effect of cumulative reform. Crucially, re-estimating some of the main growth equations from the literature using these new measures provide less support for economic initial conditions (raising, among other issues, the possibility that political initial conditions maybe a more important aspect albeit largely neglected so far).

5. Conclusions

This paper tries to contribute to the growing literature on the political economy of reform. There have been very few efforts trying to take the many theoretical insights to the data. This is in part because reform is a political economy topic *par excellence*: reform is multidimensional and it is driven by the complex interplay of political and economic forces. The transition from communism to capitalism in Central Europe and the former Soviet Union is arguably the largest natural experiment on economic reform in recent history and it is paradoxical to say the least that efforts to systematically measure such reforms efforts have

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mimic the specifications and chosen exactly the same estimators. In other words, to be the best of our abilities, everything else is exactly the same as in the original papers but the reform measures.

not emerged. In particular, the fact that objective indicators of reform are still unavailable is, in our view, a reason for serious concern. This paper tries to address this gap by constructing objective measures for three main reform areas in all transition economies for all years from 1989 to 2001. Compared to the existing (subjective) indexes, our measures generate not only a less optimistic assessment of the reform process, but also depict this process as much less smooth than previously thought (more specifically, according to our measures reform reversals abound while that is not the case with the subjective measures). We also believe this is one of the first papers to try to implement the distinction between reform efforts inputs and outcomes. This distinction is important because the inclusion of outcome indicators can severely bias the resulting measures. Among the main determinants of reform, we find domestic growth for external liberalization and privatisation, concentration of political power for internal liberalisation, and democracy for all three of them. We also find that FDI inflows reduce the probability of privatization reversals, labour strikes increase that of internal liberalization reversals, and OECD growth increase the likelihood of an external liberalization reversal. Finally, we replicate the results from the main econometric studies of the effects of reform on growth and find that those effects, using our objective measures of reform, are larger, more precisely estimated and more robust.

In terms of future work, the main suggestions we offer are as follows. First, it is important to provide objective indicators for more reforms, while simultaneously offering a more disaggregate look at the three main reforms we focused on here. For instance, future work should separate out wage from prices liberalization in order to throw light on what seems to be their very different dynamics. Our results give us reasons to suspect, for instance, that the factors explaining reversals in the two sub-components of our internal liberalization indexes will be rather different. In terms of additional reforms that we believe should be examined, those of a more institutional nature that fall under the heading "2nd generation

reforms" should be given top priority. These encompass important areas such as competition policy, anti-corruption initiatives and judiciary reform. The second main suggestion we offer for future research builds upon the first: to study interdependencies between the reform areas (that is, to focus on the issues of sequencing and speed of reforms). Because we have focused on few reform areas, the number of potential sequences of reform is limited. Further, because these are defined broadly there is maybe too blurred a consensus from the data on the actual choice of sequences. Future research would do well in examining the relationship between objective indicators of reform in more areas and attempt to identify differences in their sequencing and speed so as to allow a well-informed discussion (based on data and facts) of the possible relationship between the different reform speeds and sequences, on the one hand, and political development, reform outcomes and aggregate economic performance, on the other.

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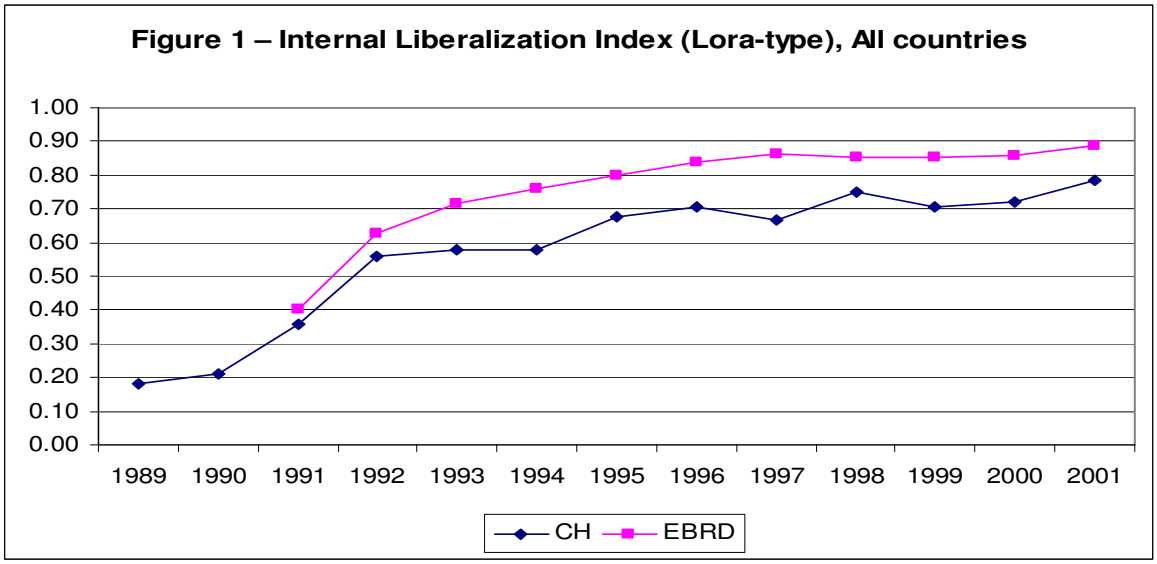
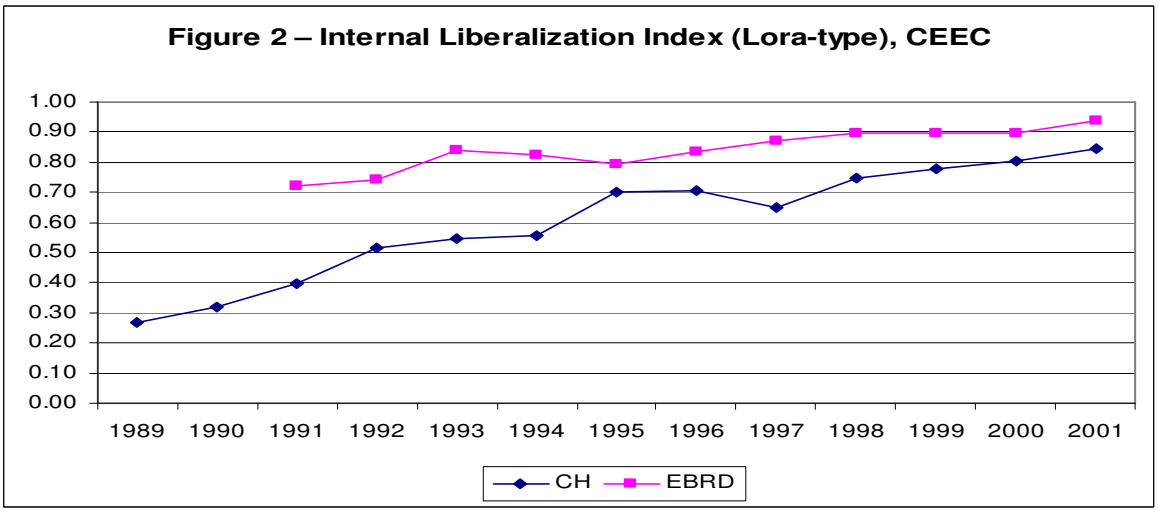
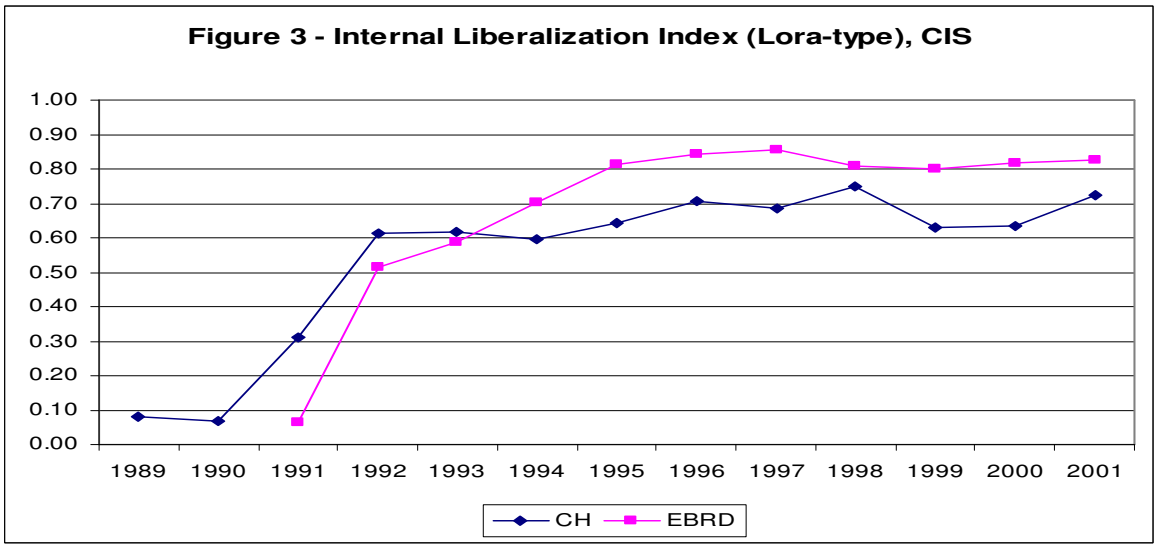
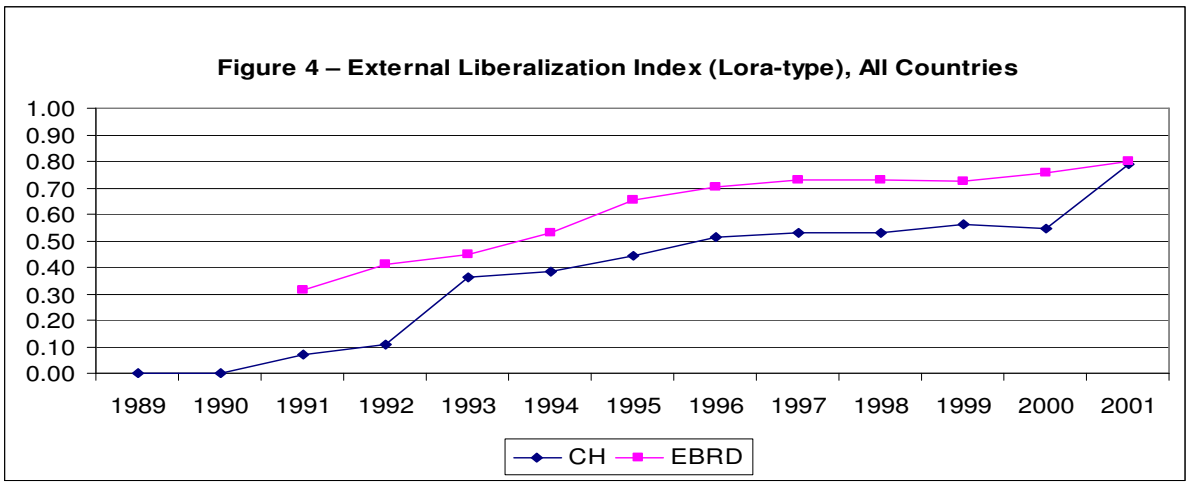
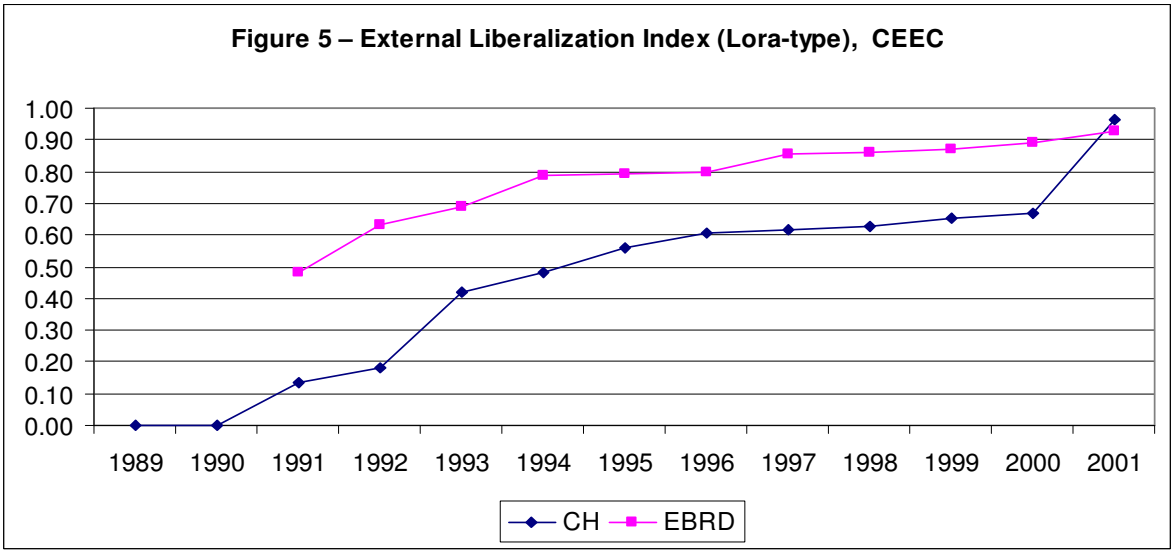


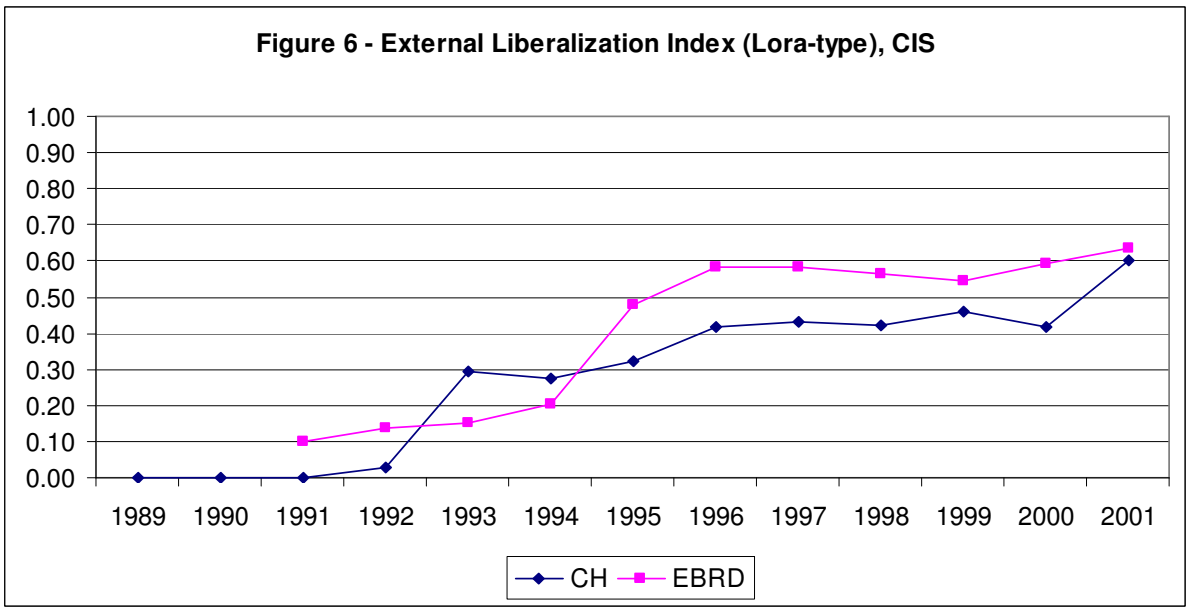




Note: CH stands for the reform indicator developed in this paper, CEEC refers to the Central and Eastern European and Baltic countries, while CIS refers to the former Soviet Union countries.







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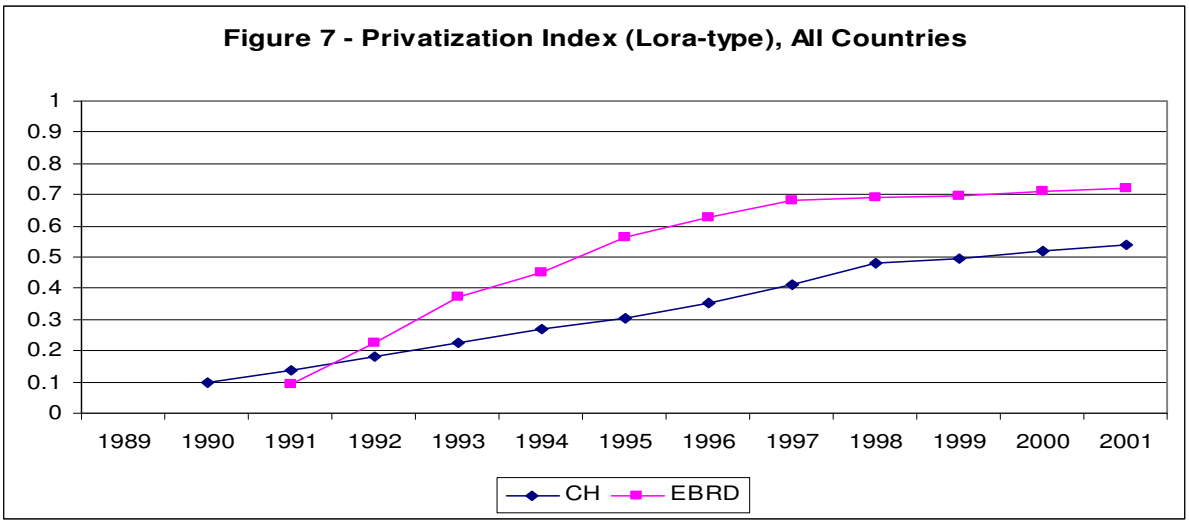

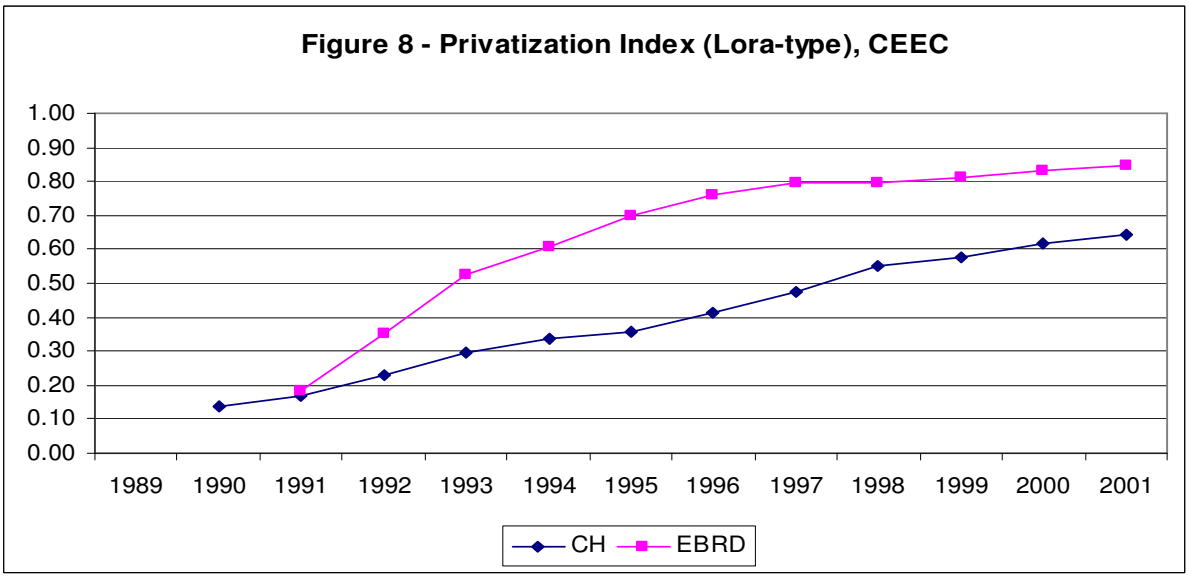

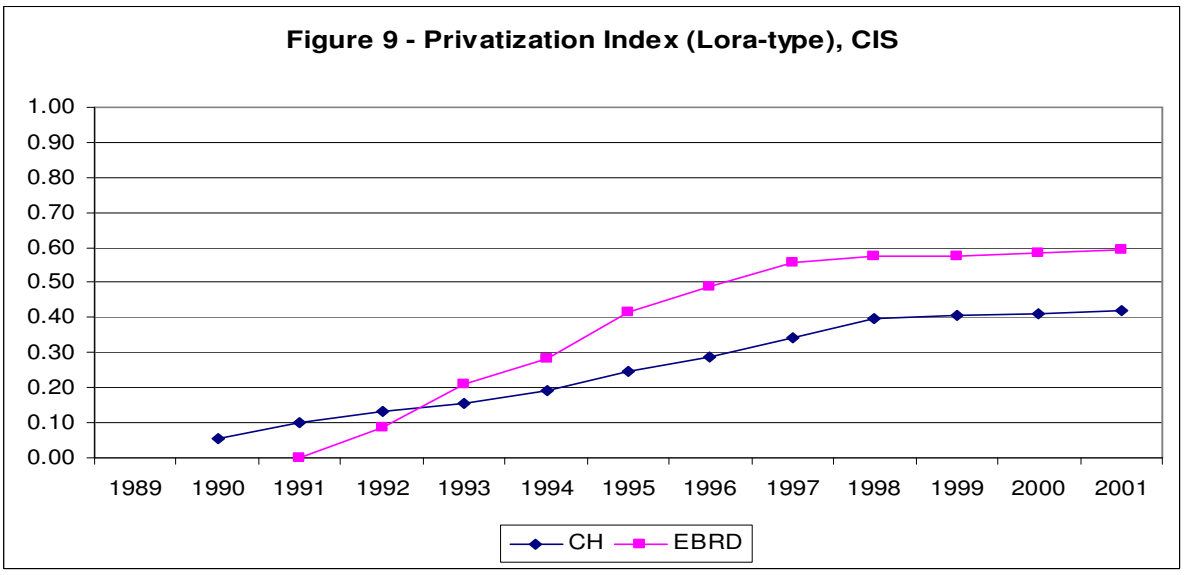

Note: CH stands for the reform indicator developed in this paper, CEEC refers to the Central and Eastern European and Baltic countries, while CIS refers to the former Soviet Union countries.

Table 1
Variables Underlying Each of the Reform Indexes

Reform Inputs	Reform Outcomes
Internal Liberalization Index	
1. Number of goods subject to price regulation (basket 15 goods) 2. Wage regulation	3. Share of administered prices in CPI
External Liberalization Index	
1. Compatibility with Article VIII 2. Controls on commercial credit 3. Controls on foreign direct investment 4. Controls on liquidation of FDI 5. Documentation requirements for release of foreign exchange for imports 6. Exchange rate taxes 7. Export duties as percent of tax revenue 8. Export licences 9. Export taxes 10. Import licences and quotas 11. Import tariff rate 12. Interest rate liberalization 13. Investment transactions 14. Multiple exchange rates 15. OECD membership 16. Permission for foreign exchange accounts held abroad by residents 17. Permission for foreign exchange accounts held domestically by residents 18. Permission of foreign exchange accounts for non-residents 19. Repatriation requirements 20. Repatriation requirements for invisible transactions 21. Surrender requirements 22. Surrender requirements for invisible transactions 23. Tariff code lines 24. WTO membership	25. Share of trade with non-transition countries 26. Openness 27. Import duties as % of tax revenue 28. Tariff revenues as % of imports and taxes on international trade 29. Taxes revenues from international trade
Privatization Index	
1. Privatisation revenues 2. Share of small firms privatised 3. Asset share of private-owned banks (in per cent) 4. Total number of enterprises privatised 5. Total number of small and medium enterprises privatised 6. Total number of large enterprises privatised 7. Share of foreign-owned banks in total number of banks 8. Total number of private enterprises	9. Private sector share in GDP 10. Credit to private sector 11. Private sector investment as a share of GDP

Table 2
What explains reform?
Panel estimates

	CH Index Internal Liberalization			CH Index External Liberalization			CH Index Privatization		
GDP growth	0.003**	0.003*	0.003	0.001***	0.01***	0.007***	0.007***	0.007***	0.004***
	[0.002]	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Unemployment	0.011***	0.014***	0.006	0.019***	0.016***	0.017***	0.015***	0.011***	0.009***
	[0.004]	[0.004]	[0.004]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Democracy	-0.04***	-0.04***	-0.04***	-0.06***	-0.07***	-0.05***	-0.05***	-0.04***	-0.04***
	[0.011]	[0.011]	[0.012]	[0.010]	[0.010]	[0.010]	[0.010]	[0.009]	[0.001]
Initial conditions		0.126***	0.112**		0.096***			0.062**	0.043*
		[0.042]	[0.046]		[0.034]			[0.026]	[0.026]
Herfindahl index			-0.123*						
			[0.064]						
OECD growth						0.055***			0.088***
						[0.013]			[0.015]
Observations	262	262	189	264	264	244	247	247	227
No. of countries	25	25	24	25	25	25	25	25	25
R-squared (overall)	0.07	0.16	0.16	0.38	0.47	0.42	0.27	0.31	0.36
Method	RE	RE	RE	FE	RE	FE	FE	RE	RE
Hausman test (p-value)	7.68 (0.05)	1.22 (0.75)	3.86 (0.43)	---	5.72 (0.13)	---	---	5.78 (0.12)	3.38 (0.50)

Robust standard errors in brackets. * significant at 10% level, ** Idem., 5% level, *** Idem., 1% level . Fixed effects estimates are presented, if the consistency of random effects is rejected by Hausman test at 5% level.

Table 3
What explains reform reversals?
Panel logit estimates

	CH Index Internal Reversal			CH Index External Reversal			CH Index Privatization Reversal		
Unemployment	0.045	0.050	0.038	0.062**	0.007	0.038	0.024	0.028	-0.029
	[0.030]	[0.040]	[0.041]	[0.026]	[0.027]	[0.048]	[0.027]	[0.031]	[0.031]
GDP growth	0.042	0.052*	0.058*	0.11**	0.031	0.002	-0.016	-0.012	-0.001
	[0.028]	[0.031]	[0.031]	[0.054]	[0.025]	[0.034]	[0.021]	[0.029]	[0.029]
Democracy	-0.042	-0.092	-0.076	-0.265	0.011	0.058	0.062	-0.029	-0.028
	[0.109]	[0.110]	[0.041]	[0.213]	[0.086]	[0.151]	[0.097]	[0.120]	[0.125]
Labor strikes		0.825**							
		[0.335]							
Labor strikes*Unempl.			0.112**						
			[0.040]						
Growth OECD					1.28***	1.572***			
					[0.325]	[0.576]			
Terms of trade						-0.022*			
						[0.012]			
FDI								-0.229**	-0.230**
								[0.117]	[0.119]
EU negotiations									-0.026
									[0.868]
Observations	260	232	233	250	243	147	228	175	175
No. of countries	25	24	24	23	25	25	25	25	25
McFadden R-squared	0.13	0.24	0.25	0.41	0.25	0.69	0.10	0.33	0.33
Method	RE	RE	RE	FE	RE	RE	RE	RE	RE
Hausman test (p-value)	0.52 (0.92)	2.15 (0.71)	1.38 (0.85)	---	3.89 (0.42)	3.77 (0.58)	3.91 (0.27)	9.63 (0.05)	4.90 (0.43)

Robust standard errors in brackets. * significant at 10% level, ** Idem., 5% level, *** Idem., 1% level . Fixed effects estimates are presented, if the consistency of random effects is rejected by Hausman test at 5% level.

Table 4
What explains the persistence of reform reversals?
Negative binomial estimates

	CH Index Internal			CH Index External			CH Index Privatization		
Unemployment	0.038	0.027	0.027	0.093*	0.001	0.019	0.023	0.023	0.023
	0.030	0.066	0.036	0.048	0.021	0.041	0.026	0.026	0.026
GDP growth	0.032	0.038	0.048*	0.079***	0.052**	0.013	-0.009	-0.003	-0.033
	0.026	0.032	0.028	0.024	0.022	0.031	0.019	0.025	0.025
Democracy	-0.053	-0.319*	-0.088	-0.150	0.055	0.098	0.600	-0.109	-0.021
	0.016	0.193	0.013	0.174	0.075	0.132	0.093	0.103	0.107
Labor strikes		0.647**							
		0.261							
Labor strikes*Unempl.			0.08***						
			0.014						
Growth OECD					0.816***	1.21**			
					0.253	0.497			
Terms of trade						-0.014			
						0.01			
FDI								-0.239**	-0.238**
								0.109	0.111
EU negotiations									-0.044
									0.784
Observations	260	172	233	250	243	147	228	175	175
No. of countries	25	17	24	23	25	25	25	25	23
McFadden R-squared	0.09	0.25	0.24	0.41	0.22	0.72	0.11	0.35	0.34
Method	RE	FE	RE	FE	RE	RE	RE	RE	RE
Hausman test (p-value)	0.22 (0.97)	---	7.01 (0.14)	---	7.11 (0.13)	3.77 (0.58)	5.14 (0.16)	4.85 (0.30)	4.99 (0.42)

Robust standard errors in brackets. * significant at 10% level, ** Idem., 5% level, *** Idem., 1% level . Fixed effects estimates are presented, if the consistency of random effects is rejected by Hausman test at 5% level.

Table 5
Replication of 'Reform Equations'

	Heybey Murrell		Merlevede		Falcetti et al.		Kim Pirttila	
	Original	Replica	Original	Replica	Original	Replica	Original	Replica
Growth	0.006***	0.006	0.056***	0.035***	0.08***	0.36***	0.002	0.003**
	[0.002]	[0.03]	[0.004]	[0.001]	[0.01]	[0.05]	[0.002]	[0.001]
Growth-lagged			-0.003	-0.011***	-0.01***	-0.12***		
			[0.002]	[0.004]	[0.004]	[0.04]		
Time*IC1			-0.34***	0.004	-0.02***	0.004		
			[0.091]	[0.005]	[0.007]	[0.04]		
Time*IC2			-0.12	0.0001				
			[0.12]	[0.005]				
Freedom House	0.008	-0.014	0.81***	-0.049***	-0.15***	-0.05***		
	[0.005]	[0.1]	[0.32]	[0.014]	[0.06]	[0.01]		
Industry	0.13*	0.35						
	[0.07]	[0.62]						
Lib. Index 1989	-0.2***	-0.63***						
	[0.05]	[0.23]						
Inflation							0.007	-0.007
							[0.01]	[0.02]
Unemployment							-0.03***	0.02***
							[0.01]	[0.007]
Gov. balance							0.002	0.002
							[0.002]	[0.003]
R-2/Chi-sqr.	0.48	0.35	770***	123***	434***	123***	0.59	0.23
Estimation	3SLS	3SLS	3SLS	3SLS	3SLS	3SLS	2SLS	2SLS

Robust standard errors in brackets. * significant at 10% level, ** Idem., 5% level, *** Idem., 1% level. Original results of the authors are in the left column labelled as *original*, while replicated results are in the right column, labelled as *replica*.

Table 6
Replication of 'Growth Equations'

	Fidrmuc		Falcetti et al.		Merlevede		Kim Pirttila	
	Original	Replica	Original	Replica	Original	Replica	Original	Replica
Liberalization	23.3***	26.3***	-13.3	-48.6***	-8.35	-73.2***	-0.86	2.16
	[6.65]	[4.1]	[8.26]	[13.5]	[10.84]	[19.1]	[0.81]	[1.74]
Lib.Index (lag)			10.84***	52.7***	10.79**	75.1***		
			[3.99]	[11.6]	[4.82]	[16.7]		
Time*IC1			0.27***	0.06	0.78***	0.12		
			[0.09]	[0.09]	[0.16]	[0.1]		
Time*IC2					0.11	-0.09		
					[0.23]	[0.12]		
Fiscal	0.073	0.16	0.34***	0.09	0.22**	0.08	0.38***	-0.09
	[0.11]	[0.11]	[0.12]	[0.08]	[0.1]	[0.1]	[0.13]	[0.1]
School	0.019	-0.02						
	[0.137]	[0.04]						
Investment	0.104	0.001**					0.24***	-0.0001
	[0.244]	[0.0003]					[0.05]	[0.0002]
War	-5.97***	-12.7***						
	[1.729]	[2.99]						
Reform reversal					25.47*	111.7***		
					[13.55]	[28.1]		
Growth – lagged							0.37**	0.29***
							[0.11]	[0.1]
Cum Lib. Index							9.04***	3.07*
							[3.4]	[1.46]
Fiscal - lagged							-0.92	0.55***
							[0.89]	[0.21]
Inflation - lagged							0.52	0.01
							[1.13]	[0.16]
R2/Chi-squared	0.76	0.28	241***	196***	411***	170***	3263***	1238***
Estimation	FE	FE	3SLS	3SLS	3SLS	3SLS	AB	AB

Standard errors in brackets. * significant at 10% level, ** Idem., 5% level, *** Idem., 1% level. Original results are in the left column labelled as *original*, while replication results are in the right column, labelled *replica*. AB stands for Arellano-Bond estimator.

Appendix 1

Reform Indicators – Raw Data

Table A1
Input-only Yearly Index of Internal Liberalization for 25 Transition Economies (Lora transformation)

	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	Average
Albania	0	0	0	0	0.17	0.17	0.37	0.4	0.9	0.9	0.8	1	1	0.44
Armenia	0	0	0	0.8	0.86	0.87	0.9	0.93	0.93	0.95	0.91	0.96	0.96	0.7
Azerbaijan						0.22	0.56	0.84	0.91	0.91	0.58	0.58	0.91	0.69
Belarus	0	0	0.35	0.4	0.45	0.5	0.58	0.65	0.44	0.44	0.44	0.44	0.45	0.4
Bulgaria	0	0.25	0.81	0.85	0.77	0.69	0.67	0.64	0.57	0.59	0.59	0.58	0.6	0.59
Croatia	0.73	0.73	0.73	0.73	0.73	0.73	0.83	0.83	0.61	0.61	0.95	0.94	0.98	0.78
Czech Rep.	0	0	0.56	0.74	0.74	0.78	0.78	0.78	0.91	0.91	0.91	0.91	0.91	0.69
Estonia	0	0	1	1	1	0.86	0.87	0.85	0.85	0.85	0.85	0.85	0.84	0.91
Georgia	0	0	0	0.67	0.67	0.77	0.8	0.84	0.91	0.99	0.99	0.99	0.99	0.66
Hungary	0.91	0.92	0.63	0.63	0.63	0.63	0.96	0.96	0.95	0.94	0.94	0.94	0.94	0.84
Kazakhstan	0	0	0.97	0.99	0.99	0.99	1	1	1	1	0.67	0.67	1	0.94
Kyrgyzstan	0	0.05	0.53	0.67	0.73	0.93	0.93	0.93	0.97	0.97	0.97	0.97	0.97	0.74
Latvia	0	0	0	0.9	0.9	0.85	0.85	0.84	0.89	0.89	0.88	0.88	0.88	0.67
Lithuania	0	0	0	0.2	0.3	0.3	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.56
Macedonia	0.6	0.7	0.74	0.78	0.83	0.86	0.86	0.84	0.56	0.89	0.89	0.94	0.95	0.8
Moldova	0.27	0.27	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.17	0.17	0.17	0.23	0.17
Poland	0.57	0.81	0.56	0.55	0.55	0.54	0.58	0.58	0.59	0.61	0.64	0.66	1	0.63
Romania	0	0.11	0.18	0.28	0.44	0.5	0.5	0.5	0.6	0.98	0.64	0.63	0.62	0.46
Russia	0	0	0	0.27	0.27	0.3	0.33	0.33	0.83	0.93	0.93	0.93	0.93	0.47
Slovakia	0	0	0.2	0.33	0.48	0.48	0.82	0.82	0.49	0.52	0.85	0.87	0.87	0.52
Slovenia	0.67	0.67	0.33	0.48	0.47	0.49	0.48	0.48	0.51	0.57	0.57	0.6	0.6	0.53
Tajikistan	0	0	0.5	0.87	0.93	0.93	0.97	1	1	1	0.67	0.67	0.67	0.71
Turkmenistan	0.63	0.4	0.4	0.66	0.71	0.74	0.74	0.74	0.5	0.56	0.56	0.58	0.58	0.6
Ukraine	0	0	0	0.27	0.27	0.6	0.87	0.87	0.43	0.93	1	1	1	0.56
Uzbekistan	0	0	1	1	0.83	0.33	0.47	0.47	0.47	0.47	0.47	0.47	0.47	0.65

Table A2
Index of Internal Liberalization, Averaged
Input-only versus Indexes Combining Inputs and Outcomes
(Lora transformation)
Whole Period versus Late Transition

Country	*Combining Inputs and Outcomes*		*Input-only*	
	1989-2001	*1997-2001*	*1989-2001*	*1997-2001*
Albania	0.44	0.92	0.44	0.92
Armenia	0.70	0.94	0.69	0.95
Azerbaijan	0.69	0.78	0.58	0.70
Belarus	0.40	0.44	0.39	0.30
Bulgaria	0.59	0.59	0.57	0.47
Croatia	0.78	0.82	0.74	0.74
Czech Rep.	0.69	0.91	0.66	0.93
Estonia	0.91	0.85	0.94	0.90
Georgia	0.66	0.97	0.65	0.98
Hungary	0.84	0.94	0.85	1.00
Kazakhstan	0.94	0.87	0.92	0.80
Kyrgyzstan	0.74	0.97	0.75	0.97
Latvia	0.67	0.88	0.69	0.93
Lithuania	0.56	0.93	0.56	0.93
Macedonia	0.80	0.85	0.80	0.85
Moldavia	0.17	0.17	0.17	0.17
Poland	0.63	0.70	0.49	0.58
Romania	0.46	0.69	0.33	0.59
Russia	0.47	0.91	0.47	0.91
Slovakia	0.52	0.72	0.48	0.67
Slovenia	0.53	0.57	0.42	0.43
Tajikistan	0.71	0.80	0.61	0.70
Turkmenistan	0.60	0.55	0.42	0.37
Ukraine	0.56	0.87	0.56	0.87
Uzbekistan	0.65	0.47	0.65	0.47

Table A3
Input-only Index of External Liberalization for 25 Transition Economies (Lora transformation)

	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	Average
Albania	0	0	0	0	0.4	0.38	0.5	0.588	0.526	0.526	0.5	0.526	0.75	0.36
Armenia	0	0	0	0	0.5	0.35	0.412	0.611	0.7	0.81	0.8	0.8	0.75	0.44
Azerbaijan	0	0	0	0	0.31	0.33	0.316	0.421	0.5	0.5	0.5	0.412	0.5	0.29
Belarus	0	0	0	0	0.24	0.19	0.313	0.25	0.143	0.095	0.095	0.095	0	0.11
Bulgaria	0	0	0	0.33	0.5	0.47	0.471	0.5	0.476	0.571	0.571	0.7	1	0.43
Croatia	0	0	0	0	0.39	0.44	0.5	0.556	0.667	0.619	0.667	0.667	1	0.42
Czech Rep.	0	0	0.25	0.25	0.5	0.5	0.688	0.778	0.762	0.81	0.75	0.75	1	0.54
Estonia	0	0	0	0.33	0.19	0.56	0.688	0.833	0.857	0.857	0.952	0.895	1	0.55
Georgia	0	0	0	0.33	0.47	0.47	0.471	0.706	0.762	0.714	0.762	0.632	1	0.49
Hungary	0	0	0.25	0.25	0.56	0.53	0.588	0.765	0.619	0.571	0.667	0.6	1	0.49
Kazakhstan	0	0	0	0	0.33	0.32	0.368	0.579	0.571	0.571	0.476	0.45	0.75	0.34
Kyrgyzstan	0	0	0	0	0.8	0.81	0.938	0.875	0.85	0.857	0.81	0.842	1	0.60
Latvia	0	0	0.33	0.33	0.77	0.84	0.842	0.85	0.857	0.857	0.95	0.947	1	0.66
Lithuania	0	0	0	0	0.56	0.63	0.611	0.632	0.684	0.667	0.667	0.7	1	0.47
Macedonia	0	0	0	0	0	0.28	0.278	0.278	0.476	0.524	0.571	0.55	0.75	0.28
Moldova	0	0	0	0	0.38	0.28	0.389	0.389	0.381	0.381	0.429	0.421	1	0.31
Poland	0	0	0.33	0.25	0.33	0.33	0.474	0.526	0.571	0.571	0.55	0.55	1	0.42
Romania	0	0	0	0	0.44	0.38	0.438	0.438	0.5	0.55	0.524	0.526	1	0.37
Russia	0	0	0	0	0.25	0.25	0.235	0.368	0.4	0.35	0.3	0.263	0.75	0.24
Slovakia	0	0	0.25	0.25	0.38	0.47	0.588	0.556	0.524	0.571	0.619	0.579	1	0.44
Slovenia	0	0	0.33	0.33	0.47	0.47	0.588	0.6	0.476	0.476	0.524	0.667	1	0.46
Tajikistan	0	0	0	0	0	0	0	0.25	0.25	0.25	0.25	0.5	0.5	0.15
Turkmenistan	0	0	0	0	0	0	0	0	0	0	0.5	0.25	0.25	0.08
Ukraine	0	0	0	0	0.06	0.11	0.222	0.222	0.4	0.3	0.35	0.25	0.75	0.21
Uzbekistan	0	0	0	0	0.2	0.19	0.176	0.316	0.238	0.238	0.238	0.1	0	0.13

Table A4
Index of External Liberalization, Averaged
Input-only versus Indexes Combining Inputs and Outcomes
(Lora transformation)
Whole Period versus Late Transition

Country	*Combining Inputs and Outcomes*		*Input-only*	
	1989-2001	*1997-2001*	*1989-2001*	*1997-2001*
Albania	0.34	0.49	0.36	0.57
Armenia	0.41	0.65	0.44	0.77
Azerbaijan	0.31	0.45	0.29	0.48
Belarus	0.21	0.18	0.11	0.09
Bulgaria	0.50	0.57	0.43	0.66
Croatia	0.47	0.63	0.42	0.72
Czech Rep.	0.50	0.70	0.54	0.81
Estonia	0.54	0.79	0.55	0.91
Georgia	0.46	0.66	0.49	0.77
Hungary	0.54	0.60	0.49	0.69
Kazakhstan	0.35	0.53	0.34	0.56
Kyrgyzstan	0.54	0.74	0.60	0.87
Latvia	0.59	0.76	0.66	0.92
Lithuania	0.44	0.64	0.47	0.74
Macedonia	0.32	0.51	0.28	0.57
Moldavia	0.34	0.51	0.31	0.52
Poland	0.39	0.55	0.42	0.65
Romania	0.42	0.53	0.37	0.62
Russia	0.28	0.41	0.24	0.41
Slovakia	0.43	0.59	0.44	0.66
Slovenia	0.46	0.57	0.46	0.63
Tajikistan	0.22	0.45	0.15	0.35
Turkmenistan	0.15	0.26	0.08	0.20
Ukraine	0.22	0.38	0.21	0.41
Uzbekistan	0.15	0.20	0.13	0.16

Table A5
Input-only Index of Privatization for 25 Transition Economies (Lora transformation)

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	Average
Albania		0.21	0.62	0.66	0.47	0.40	0.40	0.39	0.40	0.43	0.52	0.55	0.46
Armenia			0.09	0.16	0.25	0.32	0.38	0.43	0.53	0.66	0.70	0.73	0.43
Azerbaijan	0.29		0.16	0.14	0.16	0.14	0.19	0.24	0.34	0.35	0.31	0.31	0.24
Belarus		0.00	0.02	0.03	0.12	0.08	0.11	0.15	0.16	0.17	0.19	0.28	0.12
Bulgaria	0.00	0.00	0.05	0.17	0.18	0.24	0.25	0.37	0.47	0.55	0.68	0.72	0.31
Croatia		0.01	0.15	0.26	0.24	0.21	0.29	0.27	0.45	0.45	0.61	0.66	0.33
Czech Rep.		0.02	0.13	0.38	0.39	0.41	0.41	0.50	0.52	0.56	0.63	0.74	0.43
Estonia		0.16	0.26	0.30	0.27	0.33	0.34	0.37	0.49	0.51	0.60	0.62	0.39
Georgia	0.02	0.08	0.22	0.17	0.23	0.33	0.46	0.54	0.62	0.68	0.69	0.71	0.40
Hungary	0.19	0.09	0.15	0.23	0.36	0.64	0.78	0.89	0.87	0.90	0.94	0.90	0.58
Kazakhstan		0.41	0.36	0.13	0.18	0.33	0.38	0.43	0.59	0.64	0.69	0.68	0.44
Kyrgyzstan	0.00	0.01	0.15	0.22	0.21	0.26	0.41	0.49	0.53	0.49	0.53	0.53	0.32
Latvia					0.27	0.29	0.33	0.50	0.60	0.62	0.64	0.67	0.49
Lithuania			0.06	0.16	0.19	0.17	0.26	0.31	0.50	0.49	0.55	0.58	0.33
Macedonia			0.35	0.52	0.37	0.22	0.35	0.35	0.63	0.61	0.72	0.76	0.49
Moldova			0.14	0.06	0.13	0.17	0.22	0.41	0.49	0.55	0.62	0.62	0.34
Poland	0.63	0.22	0.32	0.16	0.21	0.26	0.30	0.36	0.48	0.59	0.67	0.69	0.41
Romania		0.01	0.15	0.08	0.14	0.18	0.22	0.29	0.37	0.49	0.54	0.57	0.28
Russia			0.02	0.03	0.24	0.23	0.19	0.41	0.40	0.35	0.07	0.08	0.20
Slovakia		0.62	0.34	0.31	0.34	0.43	0.50	0.62	0.62	0.63	0.64	0.80	0.53
Slovenia		0.01	0.08	0.17	0.20	0.27	0.20	0.26	0.26	0.28	0.30	0.27	0.21
Tajikistan		0.00	0.08	0.06	0.08	0.28	0.40	0.40	0.46	0.53	0.51	0.52	0.30
Turkmenistan				0.00	0.06	0.18	0.15	0.18	0.22	0.18	0.13	0.13	0.14
Ukraine			0.02	0.01	0.02	0.03	0.05	0.26	0.34	0.36	0.37	0.39	0.18
Uzbekistan		0.00	0.20	0.23	0.22	0.21	0.18	0.24	0.26	0.29	0.17	0.09	0.19

Table A6
Index of Privatization, Averaged
Input-only versus Indexes Combining Inputs and Outcomes
(Lora transformation)
Whole Period versus Late Transition

Country	*Combining Inputs and Outcomes*		*Input-only*	
	1989-2001	*1997-2001*	*1989-2001*	*1997-2001*
Armenia	0.33	0.47	0.43	0.61
Azerbaijan	0.22	0.33	0.24	0.31
Belarus	0.12	0.19	0.12	0.19
Bulgaria	0.33	0.54	0.31	0.56
Croatia	0.35	0.52	0.33	0.49
Czech Rep.	0.46	0.66	0.43	0.59
Estonia	0.41	0.57	0.39	0.52
Georgia	0.36	0.59	0.40	0.65
Hungary	0.58	0.84	0.58	0.90
Kazakhstan	0.37	0.55	0.44	0.61
Kyrgyzstan	0.31	0.47	0.32	0.52
Latvia	0.38	0.58	0.49	0.61
Lithuania	0.32	0.50	0.33	0.48
Macedonia	0.41	0.54	0.49	0.61
Moldova	0.29	0.48	0.34	0.54
Poland	0.42	0.56	0.41	0.56
Romania	0.32	0.46	0.28	0.45
Russia	0.26	0.39	0.20	0.26
Slovakia	0.52	0.73	0.53	0.66
Slovenia	0.33	0.46	0.21	0.27
Tajikistan	0.26	0.43	0.30	0.49
Turkmenistan	0.12	0.16	0.14	0.17
Ukraine	0.20	0.36	0.18	0.34
Uzbekistan	0.22	0.30	0.19	0.21

Appendix 2
Sensitivity Analysis

Table B1
The determinants of internal liberalization
Random-effects panel estimates

GDP growth	0.003	0.005	0.003	0.003	0.004	0.002	0.003	0.003	0.003
	(-0.002)	(-0.004)	(0.002)+	(0.002)+	(0.002)**	(-0.002)	(0.002)**	(0.002)+	(0.002)+
Unemployment	0.009	0.002	0.01	0.011	0.012	0.009	0.008	0.011	0.011
	(0.005)+	(-0.006)	(0.005)**	(0.005)**	(0.005)**	(0.005)+	(-0.005)	(0.005)**	(0.005)**
Freedom H. Index	-0.04	0.0001	-0.041	-0.037	-0.038	-0.016	-0.024	-0.04	-0.039
	(0.013)*	(-0.017)	(0.013)*	(0.013)*	(0.013)*	(-0.014)	(-0.015)	(0.013)*	(0.012)*
Log of inflation	-0.006								
	(-0.011)								
Financial crisis indicator		0.047							
		(-0.403)							
Fiscal deficit			0.001						
			(-0.003)						
EU negotiations				0.044					
				(-0.03)					
Electoral calendar					0.012				
					(-0.011)				
Leadership changes						0.107			
						(0.026)*			
Political alternation							0.079		
							(0.030)*		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									0.0001
									(0.001)
Constant	0.789	0.707	0.771	0.732	0.713	0.511	0.613	0.734	0.731
	(0.098)*	(0.099)*	(0.087)*	(0.079)*	(0.078)*	(0.098)*	(0.093)*	(0.092)*	(0.077)*
Observations	256	169	255	262	257	262	262	262	262
No. of countries	25	21	25	25	25	25	25	25	25

Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%

Table B2
The determinants of external liberalization
Random-effects panel estimates

GDP growth	0.007	0.011	0.011	0.01	0.01	0.009	0.01	0.01	0.01
	(0.002)*	(0.002)*	(0.001)*	(0.001)*	(0.001)*	(0.001)*	(0.001)*	(0.002)*	(0.002)*
Unemployment	0.01	0.003	0.013	0.012	0.015	0.011	0.01	0.014	0.013
	(0.003)*	-0.003	(0.003)*	(0.003)*	(0.004)*	(0.004)*	(0.004)**	(0.004)*	(0.004)*
Freedom H. Index	-0.061	-0.067	-0.055	-0.05	-0.057	-0.028	-0.034	-0.061	-0.06
	(0.009)*	(0.013)*	(0.010)*	(0.009)*	(0.009)*	(0.010)*	(0.010)*	(0.009)*	(0.009)*
Log of inflation	-0.031								
	(0.007)*								
Financial crisis indicator		-0.076							
		-0.558							
Fiscal deficit			-0.001						
			-0.002						
EU negotiations				0.153					
				(0.035)*					
Electoral calendar					0.012				
					(0.007)+				
Leadership changes (cum.)						0.134			
						(0.018)*			
Political alternation (cum.)							0.119		
							(0.024)*		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									0.0001
									(0.001)
Constant	0.722	0.726	0.56	0.532	0.531	0.282	0.374	0.54	0.6
	(0.068)*	(0.071)*	(0.063)*	(0.060)*	(0.068)*	(0.071)*	(0.070)*	(0.073)*	(0.070)*
Observations	258	169	257	264	259	264	264	264	264
No. of countries	25	21	25	25	25	25	25	25	25

Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%

Table B3
The determinants of privatization
Random-effects panel estimates

GDP growth	0.002	0.008	0.008	0.008	0.007	0.006	0.007	0.008	0.008
	(0.001)**	(0.003)*	(0.001)*	(0.001)*	(0.001)*	(0.001)*	(0.001)*	(0.001)*	(0.001)*
Unemployment	0.004	0.006	0.008	0.006	0.008	0.008	0.004	0.008	0.008
	-0.002	(0.003)**	(0.002)*	(0.002)**	(0.002)*	(0.002)*	(0.002)+	(0.002)*	(0.002)*
Freedom H. Index	-0.031	-0.045	-0.031	-0.016	-0.034	-0.004	-0.005	-0.032	-0.032
	(0.008)*	(0.011)*	(0.008)*	(0.008)**	(0.008)*	-0.009	-0.009	(0.008)*	(0.008)*
Log of inflation	-0.044								
	(0.005)*								
Financial crisis indicator		0.859							
		(0.363)**							
Fiscal deficit			0.001						
			-0.002						
EU negotiations				0.228					
				(0.028)*					
Electoral calendar					0.009				
					-0.006				
Leadership changes (cum.)						0.119			
						(0.016)*			
Political alternation (cum.)							0.126		
							(0.020)*		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									0.0001
									(0.001)
Constant	0.604	0.483	0.422	0.34	0.404	0.13	0.195	0.464	0.399
	(0.048)*	(0.057)*	(0.047)*	(0.049)*	(0.048)*	(0.057)**	(0.058)*	(0.058)*	(0.049)*
Observations	243	168	245	247	242	247	247	247	247
No. of countries	25	21	25	25	25	25	25	25	25
Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%									

Table B4
The determinants of internal liberalization reversals
Random-effects panel estimates

GDP growth	0.058	0.006	0.045	0.052	0.057	0.05	0.05	0.052	0.052
	-0.036	-0.049	-0.033	(0.031)+	(0.032)+	-0.032	-0.031	(0.031)+	(0.031)+
Unemployment	0.065	0.055	0.053	0.05	0.052	0.051	0.04	0.049	0.05
	-0.041	-0.048	-0.041	-0.04	-0.04	-0.04	-0.041	-0.04	-0.04
Freedom H. Index	-0.12	0.032	-0.081	-0.091	-0.071	-0.048	-0.004	-0.092	-0.092
	-0.117	-0.147	-0.111	-0.121	-0.113	-0.147	-0.14	-0.11	-0.11
Strikes	0.826	2.042	0.822	0.825	0.823	0.823	0.735	0.825	0.822
	(0.340)**	(0.694)*	(0.337)**	(0.336)**	(0.341)**	(0.336)**	(0.350)**	(0.335)**	(0.338)**
Log of inflation	0.1								
	-0.138								
Financial crisis indicator		51.428							
		-38.022							
Fiscal deficit			0.024						
			-0.046						
EU negotiations				0.004					
				-0.549					
Electoral calendar					0.015				
					-0.143				
Leadership changes (cum.)						0.124			
						-0.28			
Political alternation (cum.)							0.275		
							-0.264		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									0.0001
									-0.002
Constant	-2.462	-2.508	-2	-2.054	-2.143	-2.422	-2.629	-2.039	-2.033
	(0.806)*	(0.796)*	(0.660)*	(0.697)*	(0.692)*	(1.062)**	(0.872)*	(0.965)**	(0.751)*
Observations	228	161	231	232	227	232	232	232	232
No. of countries	24	20	24	24	24	24	24	24	24

Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%

Table B5
The determinants of external liberalization reversals
Random-effects panel estimates

GDP growth	0.052	0.042	0.032	0.03	0.031	0.033	0.029	0.033	0.027
	(0.029)+	-0.038	-0.025	-0.025	-0.025	-0.025	-0.025	-0.025	-0.025
Unemployment	0.02	0.001	0.005	0.007	0.004	0	0.015	0.004	0.009
	-0.029	-0.029	-0.027	-0.027	-0.027	-0.028	-0.028	-0.027	-0.027
Freedom H. Index	0.014	-0.026	0.023	0	0.005	-0.073	-0.067	0.012	0.011
	-0.09	-0.113	-0.087	-0.095	-0.088	-0.125	-0.112	-0.086	-0.086
OECD growth	1.367	0.927	1.314	1.296	1.29	1.347	1.329	1.268	1.321
	(0.331)*	(0.374)**	(0.331)*	(0.331)*	(0.329)*	(0.336)*	(0.331)*	(0.326)*	(0.334)*
Log of inflation	0.155								
	-0.115								
Financial crisis indicator		-8.547							
		-8.027							
Fiscal deficit			-0.024						
			-0.035						
EU negotiations				-0.148					
				-0.551					
Electoral calendar					0.025				
					-0.104				
Leadership changes (cum.)						-0.233			
						-0.254			
Political alternation (cum.)							-0.276		
							-0.258		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									-0.006
									(0.003)**
Constant	-5.84	-3.737	-5.186	-4.951	-4.988	-4.438	-4.557	-4.353	-4.4
	(1.250)*	(1.251)*	(1.151)*	(1.098)*	(1.126)*	(1.228)*	(1.157)*	(1.304)*	(1.139)*
Observations	237	151	236	243	241	243	243	243	243
No. of countries	25	21	25	25	25	25	25	25	25

Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%

Table B6
The determinants of privatization reversals
Random-effects panel estimates

GDP growth	-0.02	-0.045	-0.015	-0.012	-0.012	-0.011	-0.013	-0.003	-0.008
	-0.033	-0.051	-0.03	-0.029	-0.029	-0.03	-0.029	-0.03	-0.029
Unemployment	0.005	0.04	0.028	0.029	0.029	0.021	0.035	0.02	0.024
	-0.036	-0.041	-0.031	-0.031	-0.032	-0.032	-0.033	-0.032	-0.032
Freedom H. Index	-0.025	-0.099	-0.025	-0.028	-0.033	-0.115	-0.09	-0.016	-0.026
	-0.123	-0.208	-0.12	-0.125	-0.126	-0.159	-0.153	-0.122	-0.121
FDI	-0.24	-0.42	-0.228	-0.23	-0.229	-0.198	-0.219	-0.228	-0.228
	(0.127)+	(0.195)**	(0.116)**	(0.119)+	(0.117)**	(0.119)+	(0.117)+	(0.116)**	(0.116)+
Log of inflation	-0.092								
	-0.16								
Financial crisis indicator		40.373							
		-34.376							
Fiscal deficit			0.015						
			-0.038						
EU negotiations				0.026					
				-0.868					
Electoral calendar					0.014				
					-0.133				
Leadership changes (cum.)						-0.288			
						-0.345			
Political alternation (cum.)							-0.223		
							-0.353		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									0.005
									-0.005
Constant	-0.651	-0.693	-1.13	-1.198	-1.208	-0.479	-0.797	0.315	-1.694
	-1.005	-0.96	-0.724	(0.720)+	(0.717)+	-1.1	-0.932	-1.153	(0.852)**
Observations	172	126	175	175	175	175	175	175	175
No. of countries	25	21	25	25	25	25	25	25	25

Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%

Table B7
The determinants of the persistence of internal liberalization reversals
Random-effects panel estimates

GDP growth	0.058	-0.006	0.039	0.043	0.046	0.039	0.039	0.042	0.042
	(0.032)+	-0.043	-0.03	-0.029	-0.029	-0.029	-0.028	-0.029	-0.029
Unemployment	0.056	0.043	0.047	0.046	0.047	0.044	0.027	0.045	0.048
	-0.035	-0.041	-0.038	-0.039	-0.038	-0.038	-0.037	-0.038	-0.039
Freedom H. Index	-0.172	0.004	-0.14	-0.15	-0.131	-0.074	-0.024	-0.147	-0.146
	(0.103)+	-0.133	-0.108	-0.112	-0.109	-0.136	-0.122	-0.107	-0.107
Strikes	0.896	1.39	0.824	0.804	0.813	0.81	0.768	0.811	0.788
	(0.179)*	(0.242)*	(0.259)*	(0.250)*	(0.254)*	(0.242)*	(0.198)*	(0.247)*	(0.252)*
Log of inflation	0.156								
	-0.109								
Financial crisis indicator		16.879							
		-12.974							
Fiscal deficit			0.012						
			-0.045						
EU negotiations				-0.071					
				-0.464					
Electoral calendar					0.027				
					-0.128				
Leadership changes (cum.)						0.203			
						-0.237			
Political alternation (cum.)							0.383		
							(0.213)+		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									-0.001
									-0.002
Constant	-1.401	-0.693	-0.665	-0.624	-0.736	-1.192	-1.464	-0.973	-0.49
	-0.862	-1.141	-0.921	-0.938	-0.955	-1.152	-0.916	-1.09	-0.992
Observations	228	161	231	232	227	232	232	232	232
No. of countries	24	20	24	24	24	24	24	24	24

Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%

Table B8
The determinants of the persistence of external liberalization reversals
Random-effects panel estimates

GDP growth	0.072	0.069	0.049	0.052	0.051	0.053	0.051	0.054	0.05
	(0.024)*	(0.033)**	(0.022)**	(0.022)**	(0.022)**	(0.022)**	(0.022)**	(0.027)**	(0.022)**
Unemployment	0.012	-0.003	0	0.001	-0.004	-0.003	0.006	0.087	0.002
	-0.021	-0.025	-0.021	-0.021	-0.021	-0.022	-0.021	(0.051)+	-0.021
Freedom H. Index	0.051	0.032	0.063	0.055	0.05	0.015	0	-0.094	0.057
	-0.066	-0.092	-0.067	-0.075	-0.067	-0.096	-0.085	-0.204	-0.067
OECD growth	0.888	0.534	0.817	0.816	0.819	0.847	0.848	0.688	0.831
	(0.250)*	(0.283)+	(0.249)*	(0.253)*	(0.251)*	(0.255)*	(0.252)*	(0.268)**	(0.251)*
Log of inflation	0.155								
	(0.084)+								
Financial crisis indicator		-7.438							
		-6.433							
Fiscal deficit			-0.002						
			-0.03						
EU negotiations				0.002					
				-0.422					
Electoral calendar					0.006				
					-0.081				
Leadership changes (cum.)						-0.113			
						-0.2			
Political alternation (cum.)							-0.202		
							-0.201		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									-0.004
									(0.002)+
Constant	4.858	12.532	9.736	10.497	5.239	9.444	2.861	4.843	10.616
	-48.102	-667.583	-510.242	-474.889	-66.19	-561.459	-20.573	-195.138	-442.249
Observations	237	151	236	243	241	243	243	231	243
No. of countries	25	21	25	25	25	25	25	23	25

Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%

Table B9
The determinants of the persistence of privatization reversals
Random-effects panel estimates

GDP growth	-0.011	-0.018	-0.006	-0.003	-0.003	-0.002	-0.004	0.002	0.001
	(-0.028)	(-0.042)	(-0.025)	(-0.025)	(-0.025)	(-0.025)	(-0.025)	(-0.026)	(-0.025)
Unemployment	0.005	0.033	0.022	0.023	0.022	0.017	0.03	0.017	0.019
	(-0.032)	(-0.035)	(-0.026)	(-0.026)	(-0.027)	(-0.026)	(-0.027)	(-0.026)	(-0.027)
Freedom H. Index	-0.012	-0.051	-0.017	-0.02	-0.015	-0.09	-0.089	-0.011	-0.017
	(-0.108)	(-0.175)	(-0.104)	(-0.107)	(-0.109)	(-0.133)	(-0.13)	(-0.104)	(-0.105)
FDI	-0.256	-0.406	-0.238	-0.238	-0.239	-0.209	-0.226	-0.232	-0.238
	(0.118)**	(0.171)**	(0.109)**	(0.111)**	(0.109)**	(0.111)+	(0.108)**	(0.112)**	(0.109)**
Log of inflation	-0.089								
	(-0.136)								
Financial crisis indicator		32.118							
		(-28.317)							
Fiscal deficit			0.013						
			(-0.033)						
EU negotiations				-0.044					
				(-0.784)					
Electoral calendar					-0.014				
					(-0.112)				
Leadership changes (cum.)						-0.249			
						(-0.293)			
Political alternation (cum.)							-0.265		
							(-0.311)		
Distance between capital cities								0.0001	
								(0.001)	
FSU vs. non-FSU									0.004
									(-0.004)
Constant	11.788	14.803	11.931	11.784	11.755	12.693	12.821	3.52	13.55
	-737.865	(1,153.991)	-769.76	-657.948	-762.545	-714.062	(1,024.420)	-6.657	-496.707
Observations	172	126	175	175	175	175	175	175	175
No. of countries	25	21	25	25	25	25	25	25	25
Robust standard errors in parentheses. + significant at 10%; ** significant at 5%; * significant at 1%									