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ASSESSING THE VALIDITY OF STRUCTURAL REFORM AGGREGATIONS IN ECONOMIC GROWTH MODELS OF 24 TRANSITION ECONOMIES

MA Thesis

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Tartu 2021

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Martin Daniel Hayford
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ABSTRACT

During the early 1990s a trend of aggregating policy variables emerged out of econometric examinations of growth in transition countries, where more conventional growth models proved inadequate or incomplete. This thesis examines the use of one aggregation in particular, the cumulative liberalization index (CLI) representing structural reforms, and how different constructions of the index based on its conceptual structure can lead to different results. Further, the implications of different conceptual structures can lead to far different conclusions in applications of policy. This analysis shows, that although the different aggregations have similar capacities to predict growth, there are important differences in how the results are interpreted and applied. Namely, different conclusions about the effect of liberalization policy can be formed or obscured based on the outcomes of economic models that use different aggregations of policy indicators. A direct line can be drawn from the concept structuring through to the results and interpretation. Additionally, a conceptually simplified model based on the substantive results of the analysis is proposed that eliminates the need for a structural reforms construct entirely.

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1. INTRODUCTION

The use of data and models to support policy decisions is considered standard practice in the empirically minded and data-driven culture of current times. Citizens of democracies trust that the researchers and analysts processing and modeling this data as well as the policymakers that use these results to craft legislation are well-informed and relatively unbiased in the translation of empirical evidence to policy. There are, of course, plenty of examples of misrepresented data and improper application of evidence that the data literate among experts are quick to point out. But in general, if presented with the findings from a trustworthy source, people will accept them with little question.

The data-laden field of economics is perhaps the pinnacle of data influencing policy. Models upon models are produced out of the uncountable dimensions of data from the level of household consumers to multi-national corporations to governments themselves. Central banks, think tanks, and international economic organizations all use this data to predict, model, and explain the functioning of economies. And when the time came to analyze the economies of countries in transition from command to market economies in the early 1990s, groups like the International Monetary Fund (IMF) jumped at the chance to explain economic phenomena like gross domestic product (GDP) growth using the models and data they were so familiar with. However, when presented with the peculiar nature of these economies, it was concluded that alternative measures should be investigated. Such measures included variables that measured policy changes in the form of liberalizing reforms. This was not just data that represented reforms, but newly generated data that sought to measure the act of policy adoption itself. This was, in many ways, a departure for measuring economic growth, and it relied on the formation of new indices to capture this phenomenon.

Would anyone question the validity of such indices coming from the likes of the IMF? It is unlikely. There is a general understanding in economics—that was perhaps even stronger in the 1990s—that these explorations were at the very least valid. This is not to insinuate that the IMF was always accurate or in some way immune to criticism, just that it was generally trusted. The results of the studies using these indices was that

organizations like the IMF could use them in their work with transitioning countries to determine metrics for conditionalities and influence policy (Dreher 2006, p. 769)—and economic growth was a specific policy objective (Dreher 2006, p. 772).

What if these new indices were improperly constructed? What if the econometric minds of the IMF were not fully aware of the considerations and ramifications of constructing indices that were more social science than data science? This could result in serious implications when the time came for interpreting the results of studies conducted using these indices. The widespread use of the IMF developed cumulative liberalization index (CLI) to represent structural reforms in transition countries is a perfect example of this problem in action. Without proper consideration given to the structuring of the concept of interest, the construction of the index could be considered less valid from the very start. This validity then transfers along the index's path of application, from measurement to model to results and policy implications.

In this paper, these implications of using the cumulative liberalization index will be explored by practically examining four different models for economic growth—each utilizing a differently constructed aggregation for structural reforms. First, the CLI will be analyzed in the lens of concept structuring. By using proper concept structuring theory, the CLI will be reconstructed along with three alternatives of aggregation that attempt to solve issues of concept structure-measurement inconsistency and vagueness. Corresponding methods of measurement for these new aggregations will be determined. In addition to the reconstructed CLI, one model will use a statistically derived additive method of measurement, another a multiplicative method, and the last will be left disaggregated. Next, a model of economic growth in which to embed them for testing will be specified and appropriate data will be collected. The models will be compared in a practical examination of construct validity, assessing their capacity to predict economic growth. The analysis and discussion of these results will follow, including a discussion of the implications of the use of these different models and substantively derived alternatives to the use of a structural reform construct.

2. THEORETICAL FRAMEWORK: ECONOMIC GROWTH IN TRANSITION ECONOMIES AND CONCEPT STRUCTURING

2.1. Economic Growth in Transition Economies

With the fall of the Soviet Union and the beginning of transition, economists of the time were interested to examine these new cases through the lens of economic growth. Whereas growth was certainly not a new avenue of exploration, a testing bed of multiple countries simultaneously switching from a planned to market economy provided a novel and interesting opportunity for study. The beginning of transition coincided with two other trends in the field of economics: the rise of endogenous growth theory and the dominance of neoliberal economic schools of thought.

Growth theory in the 1990s was, at its core, based on a refutation of neoclassical exogenous growth explanations, whereby growth was determined by largely external and macroeconomic functions (Romer 1994, p. 3). Endogenous growth theory sought to look more inwards to explain growth, while also borrowing certain tools and methods from microeconomic analysis. As this theory was gaining popularity in the times of Keynesian economic thought, there was an effort to establish Keynesian integration with endogenous growth theory as it previously maintained most of the assumptions of neoclassicalism (Palley 1996). Politically, all these evolutions occurred during the rise of liberal capitalist economic hegemony—informally led by the shadowy "Washington consensus;" perhaps less obscurely, groups such as the International Monetary Fund, the World Bank, and networks of other, smaller liberal policy institutions largely based in the United States (US). It is then not difficult to imagine the convergence of these groups and their ideologies upon the veritable test bed of the newly liberalizing and independent countries of Central and Eastern Europe (CEE) and the Former Soviet Union (FSU).

Almost immediately, the focus for explaining growth was concentrated on policies of liberalization. Many of the earliest studies contained examinations of the impact of policy, as the period of transition was found to be too different or specific in nature to explain growth using conventional metrics of growth theory (Merleverde 2000, p. 2). The first studies to examine the role of policy concerned explanations for the initial recessions

ubiquitous among the transition countries (see Blanchard 1997; Kornai 1994). In these publications, transition from buyers' to sellers' market, speed of reform, and investment and foreign trade are all explored as priorities for addressing the recessions (Kornai 1994). Additionally, factors such as reallocation of resources and restructuring of firms was considered (Blanchard 1997). Generally, the consensus was that "the closer a country is to a market economy, the more it benefits from the market's growth generating (allocational) efficiency" (Merleverde 2000, p. 7). Hence, this led to the use of policy variables in subsequent examinations utilizing econometric models to predict growth.

In addition to policy liberalization, the literature converged upon two more determinants of growth: macroeconomic stabilization and initial conditions (Havrylyshyn et al, 1998, p. 12; Lee and Jeong, 2006, p. 242; Merleverde 2000, p. 2). Achieving macroeconomic stabilization was consistently found to be a necessary precursor to growth (Fischer et al. 1996, p. 64; Lougani and Sheets 1997, p. 397). Initial conditions, on the other hand, were a bit more controversial regarding their impact. While there is plenty of evidence that they have an impact on growth, their relative importance is usually subsumed by their impact on or correlation with the policy variables in a model (de Melo et al. 2001, p. 27; Fischer and Sahay 2000, p. 22). Subsequent studies have gone on to lessen the role of initial conditions by showing that their effects greatly decrease over time (Berg et al. 1999, p. 53). For this reason and for reasons relating to data structure, initial conditions will not be included in the models examined in this paper. Rather, a fixed effects estimate will be used instead.

As the various determinants of growth were established, the econometric analysis of growth began to accelerate. With this came a need for establishing various indicators and aggregations to represent these determinants. The simplest aggregator was the use of a proxy for macroeconomic stabilization in the form of the inflation rate (see Havrylyshyn et al. 1998; Lee and Jeong 2006). Other studies utilized further indicators for stabilization, such as exchange rate regime dummy variables (Fischer et al. 1996). This proxy seems to be widely accepted, with Lee and Jeong explaining that it "represent[s] the willingness of governments to utilize stabilization policy" (2006, p. 246). The most commonly used aggregates for the other two determinants were both established by similar groups of researchers from the IMF. The initial conditions were aggregated using a principal

components analysis (PCA) method in 1997 (republished in 2001) in their exploration "Circumstances and Choice: The Role of Initial Conditions and Policies in Transition Economies" (de Melo et al. 2001). They reduced the dimensionality of 11 different initial conditions to two components, accounting for roughly 67% of the variation in the original 11 variables (de Melo et al. 2001, p. 9). These clusters were used in some subsequent studies as aggregations representing initial conditions (see Havrylyshyn et al. 1998; Merleverde 2003), while others developed their own methods for including initial conditions (see Fischer et al. 2000; Fischer and Sahay 2004; Lee and Jeong 2006).

Perhaps the aggregation with the greatest impact, however, was the cumulative liberalization index. The CLI was determined by IMF economists and incorporated elements of liberalization policy across internal markets, external markets, and privatization based on expert opinions from IMF and national experts (de Melo et al. 1996, p. 403). Since policy changes were considered such an important part of explanations of growth, it is no surprise that this index was so popular—especially considering the difficulties posed by measurement of such indicators. It was considered the default indicator for policy liberalization and was used by almost every researcher at the time as such (see de Melo et al. 2001; Fischer and Sahay 2000; Fischer and Sahay 2004; Lougani and Sheets 1997). Since the period of the CLI was limited, later studies went on to utilize the extremely similar European Bank of Reconstruction and Development (EBRD) transition report liberalization indicators. These appear to have borrowed extensively from the CLI format, although this is unconfirmed. What is evident, however, is that they were similar enough to be two data sources for a single indicator of policy liberalization (see Havrylyshyn et al. 1998, p. 14; Merleverde 2003, p. 655).

A synthesis of all these aggregations was the stylized regression model introduced in 1998 by IMF economists Oleh Havrylyshyn, Ivailo Ivorski, and Ron van Rooden, wherein they made use of the inflation rate proxy, the initial condition clusters, and the CLI to explain economic growth in the transition countries. The model (hereafter Havrylyshyn et al.'s model or the original model) was fairly successful in explaining growth in a relatively simplified manner (Havrylyshyn et al. 1998, p. 16). For these reasons, it will serve as a representative example of economic models of the time. One issue with this paper was

the short period of study—only 1990 through 1997. The researchers were constrained by the beginning of transition and only had this small period to work with, but that is a rather short period to be evaluating panel regressions within. The other issue that persists is the use of aggregations. At a surface level, they all seem to make sense and are widely utilized in similar studies. But closer examination raises some questions, especially with the CLI. The inflation rate proxy is widely used and seems to make sense as an indicator. The initial conditions were dimensionally reduced using a PCA. But the CLI seems too simple. How can all the information about liberalization be contained in one numerical indicator? And why does the CLI assign seemingly arbitrary values to each subcomponent? The question this all raises is: which aggregation of the structural reform policy variables (if any) are most valid in the context of explaining economic growth?

2.2. Concept Structuring and Construct Validation

The cumulative liberalization index suffers from issues of concept structuring. If we view it though a lens of Goertzian concept structuring, we can visualize the three-level conceptualization starting with "structural reform" (or "structurally reformed" in reference to a specific case) at the basic level. The result of the CLI is used to determine how reformed a country is. De Melo et al. create a continuous classification system, with some general outlines for low medium and high level of reform according to the end CLI score (1996, p. 404). This follows with Goertz's prescription of continuous classification, which minimizes measurement error by not forcing a dichotomous classification and allowing for gray zones (Goertz 2006, p. 34). In the case of the CLI it makes even more sense as it is a ranking system and is intended to be used in econometric evaluations—a dichotomous classification would be too simplified. We can also see that they have adequately theorized the negative pole, which in the case of the CLI represents no change from a planned economy.

The next levels are formed based on this basic concept. The is where the conceptual structuring of the CLI begins to break down. The second level is where a determination must be made whether a concept follows the necessary and sufficient condition (AND) or family resemblance (OR) one (Goertz 2006, p. 36). However, based on the quantitative

construction of the index, we must assume that the family resemblance conditions are used. The CLI's basic construction is as follows:

> "The liberalization index is the weighted average of the rankings of liberalization in the following three areas:
>
> - Internal markets (I)—liberalization of domestic prices and the abolition of state trading monopolies (weight: 0.3)
> - External markets (E)—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 high import tariffs; current-account convertibility (weight: 0.3)
> - Private sector entry (P)—privatization of small-scale and large-scale enterprises and banking reform (weight: 0.4)" (De Melo et al. 1996, p. 403).

There is nothing inherently wrong with this, but conceptually this determination must be made as it has methodological implications. In the use case of structural reform, it seems that family resemblance conditionality is the simpler determination, as there is no need to then ponder whether a country could be considered reformed without one of three second level concepts: internal market reform, external market reform, and privatization. Defining the theoretical relationship between this second level and the basic level concept can also help to make this determination by informing the interactions of the formal, mathematical principles of concept structuring.

If, in fact, the conditionality is more accurately described as the necessary and sufficient condition, then there is an inconsistency between concept and measurement. The creators of the CLI make use of an additive (averaging) measurement (de Melo et al. 1996, p. 403), which is inconsistent with the necessary and sufficient condition as it implies substitutability (Goertz 2006, p. 98). The literature is unclear about which conditionality is more appropriate. As established, the measurement implies family resemblance. For the most part, this conceptualization holds. It would be possible to classify a country as an advanced reformer even if one of the three second level concepts lags behind the others. However, it might be hard to argue that a country is an advanced reformer if no reform takes place in one category while extensive reforms take place in the other two—an outcome which is possible using the established measurement. For this reason, a conceptualization of necessary and sufficient conditionality and multiplicative measurement must be examined.

Even if one acquiesces that structural reform can be conceptualized with family resemblance conditionality, the method of additive measurement must be justified accordingly. It is not explained, for example, why privatization receives a higher weighting than internal or external market reform. Without the benefit of peering into the mind of the researchers in this case, one is left wondering about a more transparent way to aggregate these variables. Perhaps the simplest way is to let the data "speak for itself." Following the lead of the IMF economists in another case of aggregation, one method would be to use a PCA to determine the weighting for each indicator within the concept as was done with the initial conditions (de Melo et al. 2001). In the course of this and through the lens of full substitutability, it would also make sense to simplify the concept structure to that of a two-level structure where the indicator level is directly theoretically related to basic level of the concept. For this reason, a simplified reconceptualization of de Melo et al.'s additive conceptualization will be examined utilizing more transparent aggregation methods.

Finally, if we take the idea of substitutability to the extreme, it might also be possible to conceptualize each different policy indicator as its own concept. The conceptual implication of the CLI is that the latent variable of structural reform can be and has been adequately captured by this measurement and aggregation. Perhaps though, in the context of explaining growth this conceit places too much emphasis on the concept of structural reform writ large and not enough on the various impacts of specific reforms. This non-aggregative conceptualization will also be examined. It is worth noting, however, that there are perhaps methodological reasons for aggregations such as the CLI beyond conceptualization. In this case, as noted by the CLI authors, there is high correlation between components of the CLI (de Melo et al. 1996, p. 404). This indicates that there could be mathematical issues of multicollinearity among the various indicators which would decrease the validity of a model which does not in some way aggregate them.

In addition to each of these conceptualizations, a reconstructed version of the original CLI will be examined. The examination will be a practical process that explores and assesses the relative strengths and weaknesses of each of the above conceptualized cases

in the context of economic growth models. As previously determined, the model used will be the representative Havrylyshyn et al. model, with a few modifications to account for data and the much-extended period of examination. Through the different conceptualizations and thusly implied methods of aggregation, we will identify the differences in overall output (strength of explanation of economic growth) and also the changing nature of the structural reforms component in relation to the other components of the model. Each of these presents an opportunity to tell a different story about the impact of structural reforms on growth in transition economies and will offer different implications in terms of the importance of policy.

This concept structuring is embedded in a larger framework of construct validity. Construct validity is either one form of validity (Carmines and Zeller 1979, p. 3), or perhaps—as the more recent definitions propose—the underlying form of all validity (Strauss and Smith 2009, p. 7). It is a nonjustificationist theory that defines constructs based on their place in a network of relationships (Strauss and Smith 2009, p. 9). The test portion of this study will be formed around the idea of informative construct validity testing. There is no all-encompassing, single test that could be done to determine the validity of a construct like the CLI. But the aim for such an investigation as this one should merely be to improve the understanding of the construct validity and inform other researchers such that they may do their own further testing (Strauss and Smith 2009, p. 10).

Construct validation is a testing process involving three steps:

1. The theoretical relationship between concepts must be specified,
2. The empirical relationship between measures of the concepts must be examined, and,
3. The empirical evidence must be examined. (Carmines and Zeller 1979, p. 9).

This simple formula allows for the use of concept structuring to accomplish the first step and a practical examination of each of those concepts in an otherwise identical model for the second step. To address the third step, both the results of these models and the

implications associated with the use of each different conceptualization will be examined and discussed. With this, there is an aim to provide an informative study on construct validation while perhaps also gaining some substantive insight on the use of structural reforms indicators and aggregations in models of economic growth.

2.3. Conceptualizing Structural Reform and Aggregation Implications

For illustrative purposes, each of the conceptualizations to be used in this study will be detailed and diagrammed. There will also be a brief discussion of the measurement technique that would apply to each of them. The diagrams will include symbols that represent different theoretical relationships between the different levels of the concept. Table 2.1 shows how different theoretical relationships imply formal relationships.

Table 2.1 Diagramming theoretical relationships and their implied conditionalities

Theoretical Relationship	*AND*	*OR*	*Symbol*
Ontological	Yes	Yes	═══
Causal	Yes	Yes	⟶
Conjunction of necessary causes	Yes	No	⟶⟵
Noncausal conjunction of necessary conditions	Yes	No	⋯⟶⟵⋯
Substitutability	No	Yes	⋯⟶

Source: based on Table 2.5 in Goertz 2006, p. 54

The indicator level concepts are taken directly from EBRD transition reports (EBRD 1994). Their application in this study will be explained in greater detail in Section 3.2., but they are, in effect, a known substitute for the expert opinion data gathered by de Melo et al. (1996). The EBRD indicators span roughly the same spectrum of measurements as the original indicators.

The conceptualization of the CLI as specified by de Melo et al. (1996) will be assumed based on the measurement implied and what little conceptual context is given to it in the original construction. As previously mentioned, the additive nature of the measurement necessitates that it is a family resemblance (OR) conditionality. There is also little

evidence to suggest that the relationship between structural reform and the component parts of internal market liberalization, external market liberalization, and privatization represent a causal relationship in either direction. With these subcomponents, there is instead an implication that they are simply representations of different aspects of reform, which places them into an ontological relationship. There certainly seems to be an amount of substitutability at the indicator level, but this lessens between the three component parts and the main concept. While substitutability is not precluded in the ontological relationship, it is perhaps a less precise way to classify the relationship. Therefore, the concept structure can be visualized as it is in Figure 2.1.

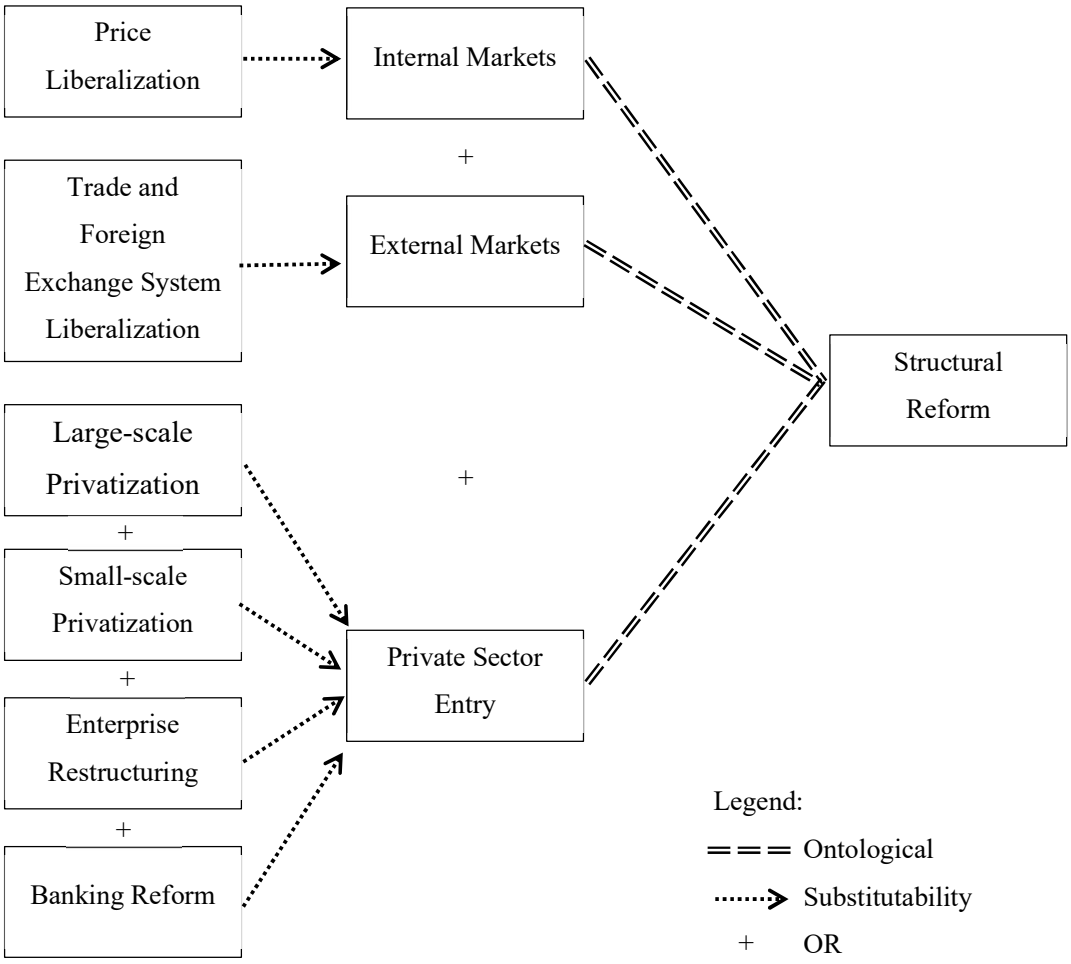


Figure 2.1 Three Level Concept Structure of the Original CLI
Source: de Melo et al. 1996; EBRD 1994; based on Goertz 2006

The relationship between the third level indicators and second level categories implies full substitutability, i.e., the absence of one such indicator can still result in a measurement across the full spectrum of categorization. This point is functionally meaningless for internal and external markets since they only have one indicator apiece. Theoretically it is still useful for further applications in which researchers might want to add more indicators to these categories. With the relationship fully defined as it is here, there would be no further questions in aggregation. For private sector entry, this is useful for this study. The four indicators that comprise the measurement of this category will be additively aggregated. The three second level indicators are likewise additively aggregated, as is done in the original construction, where a weighted average of the three component scores is applied to arrive at the final index represented structural reform.

To keep with the spirit of the original construction while simplifying the concept structure, the confusion among theoretical relationships can be done away with by eliminating the second level of conceptualization. The second level components make it difficult to determine the overall structure. Since there is substitutability implied by the additive measurement, it may make more sense to consider the ontological relationship between the second and third level redundant. Instead, the concept structure would be that of a two-level concept, as shown in Figure 2.2. By doing this we can eliminate any questions of whether a family resemblance conditionality is appropriate in this case. If there is a relationship of substitutability, then let that be the defining conceptualization that the family resemblance (OR) conditionality can follow from—and the additive measurement can follow from that.

This leads to a question of weighting in the additive aggregation, as the only given indication of weighting from the de Melo et al. structure was at the second level. For this study, weighting for the two-level additive model will be addressed statistically with a PCA, as whatever method was used in the original CLI is undefined.

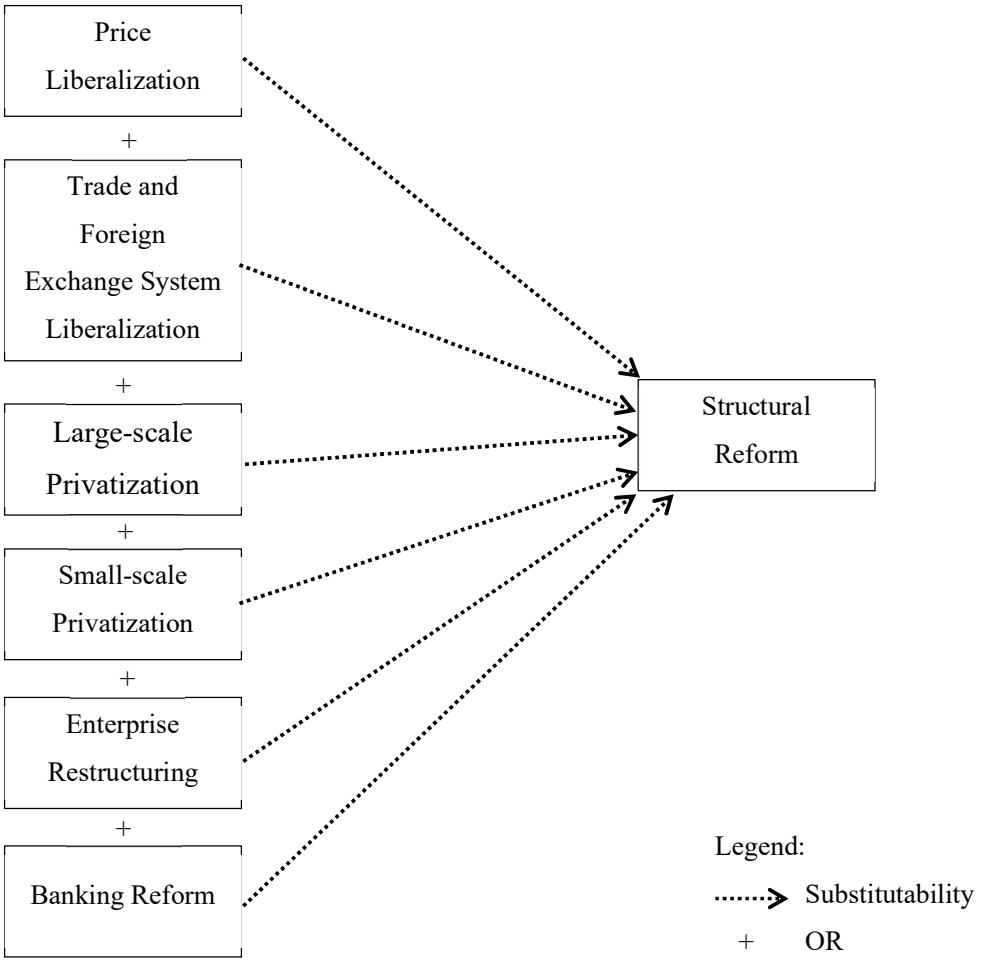


Figure 2.2 Two Level Concept Structure of a Family Resemblance CLI Alternative
Source: EBRD 1994; based on Goertz 2006

Conversely to this simplified additive concept structure, another approach could be to lean into the ambiguity that might suggest a family resemblance conditionality is not accurate in the case of structural reform. Instead, what if the three subcomponents of internal and external market liberalization and privatization are all necessary and sufficient (AND) to indicate structural reform? The relationship could remain as ontological, which is technically agnostic to the different conditionalities. It has been established that there is no causal mechanism, which leaves a noncausal conjunction of necessary conditions as the only remaining relationship option between the top and second level concepts. In this case, the ontological relationship will remain, as it is far

more sensible to conceptualize the three components as an intrinsic part of structural reforms than to view them as a conjunction of three independent phenomena. This would result in a relationship shown in Figure 2.3.

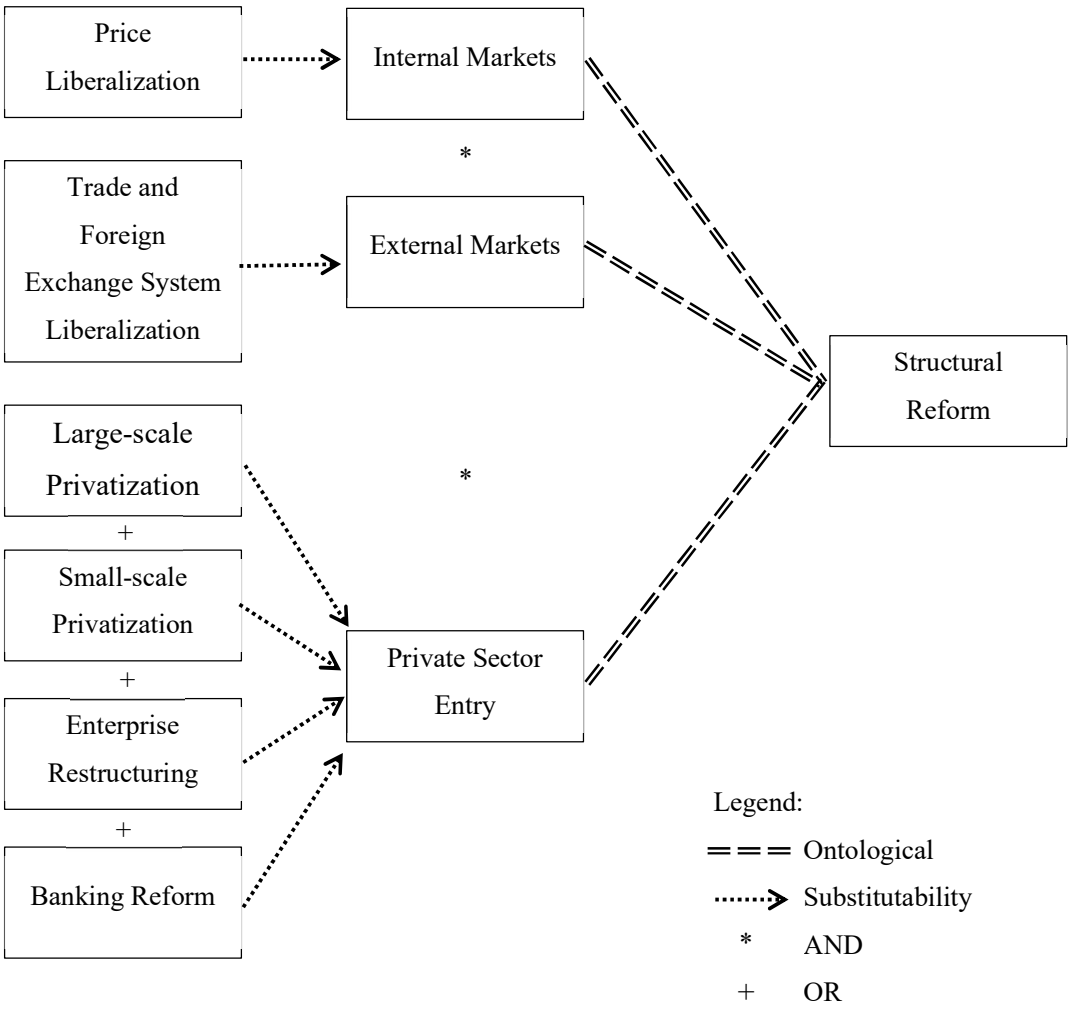


Figure 2.3 Three Level Concept Structure of a Necessary and Sufficient CLI Alternative
Source: de Melo et al. 1996; EBRD 1994; based on Goertz 2006

In this case, the only real change from the assumed structure of the original CLI is the implication of a multiplicative measurement instead of an additive one. The substitutability of the other structures carries through at the indicator level. This is partially for the sake of conceptual simplicity, and partially due to the even greater lack

of theory to inform the conditionality between the second and third level concepts. Whereas it would be possible to conceptualize an aggregation of structural reforms that requires the presence of all three subcomponents, there is less certainty in requiring the presence of each indicator. This is a theoretical debate that would exist well outside the scope of this study.

The final concept structure is the simplest, and that is one without aggregation. Each indicator exists as its own, separate concept and measurement. There is no diagram required for this structure, as it is just six concepts without relationships to higher level concepts and thus no conditionalities or implied measurement techniques.

In the next section, these concept structures will be formally integrated into a comparative framework to test the construct validity.

3. METHODOLOGY: ASSESSING STRUCTURAL REFORM AGGREGATIONS THROUGH COMPARISON OF REGRESSION MODELS

3.1. Case Selection and Period of Study

When selecting cases for examination, there were three considerations:

1. The cases should be as similar as possible to the cases selected in Havrylyshyn et al.'s (1998) original study for the sake of parity.
2. There should be as many cases as possible selected to capture the most data, and
3. They must have a nearly complete tranche of data for the indicators selected—with a few small exceptions.

Thus, this study started with the 25 transition countries from the original model: Albania, Armenia, Azerbaijan, Belarus, Bulgaria, Croatia, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyz Republic, Latvia, Lithuania, Moldova, North Macedonia, Poland, Romania, Russia, Slovak Republic, Slovenia, Tajikistan, Turkmenistan, Ukraine, and Uzbekistan. From there, any attempt to expand the study was immediately rebuffed by the lack of consistent data. This mainly comes down to the former Republics of Yugoslavia that did not have consistent borders during the 1990s. This is likely the same reason they were not included in the original study—some were not even considered sovereign nations in 1998. This list includes the countries now known as Bosnia and Herzegovina, Montenegro, Serbia, and Kosovo. Croatia, North Macedonia, and Slovenia all appeared to have adequate data, ostensibly because of their earlier declarations of independence. There are also several other "transitioning" countries found in later editions of the EBRD annual transition reports, but these lack earlier data and are not part of the "transition countries" of CEE and the FSU. The final consideration of complete data sets eliminated just one country: the Czech Republic. This is due to a request in 2007 for the Czech Republic to no longer receive funding from the EBRD, which is referred to as a "graduation" of sorts (Reiserer 2021). Whatever the impetus or reasoning, there are seven years of vital missing data that cannot be supplemented from elsewhere, meaning that the study drops from 25 countries to 24. This should have little to no impact on the

final outputs, although the Czech Republic is often symbolically presented as a success in transition (Ekiert 1998).

The period of evaluation was similarly decided by the availability of EBRD data. The specific indicators used were available only between 1994 and 2014 (EBRD 1994-2014). A further discussion of this EBRD data as it relates to the period analyzed can be found in Section 3.2., in which the decision not to include earlier data from a separate source as a supplement is justified.

3.2. Data Selection

The largest initial problem to overcome in this study is that of data availability. The original model's database was purpose built by Havrylyshyn et al. (1998) using some metrics that are not widely available—most notably, the cumulative liberalization index. Additionally, data must be collected for the extended period of study. Certain data was relatively easy to recreate, while other indicators required reconstruction or representative indicators. In all instances, much like case selection, there was an effort to maintain parity with the data and indicators of the original model. Aside from the variously conceptualized and aggregated structural reforms components, the rest of the model and underlying data will remain the same throughout the testing.

GDP percent growth, inflation rate data, government expenditures, and foreign direct investment data were available from the World Bank Open Data database. The CLI was recreated in a similar manner to its supplementation in Havrylyshyn et al.'s original data: EBRD report statistics. For the reasons explained in Section 3.4., initial conditions will not be part of these models.

There are some indicators that should remain largely unchanged. The dependent variable in this model is GDP growth (annual percent) and was taken from the World Bank Open Data database, which is a subset of World Bank's Data Bank (World Bank, 2021). The original model used the same GDP growth metric from "the authorities," as it is likely reliable estimates from international institutions were difficult to come by at the time

(Havrylyshyn et al. 1998, p. 14). With the reliability and data capturing abilities of our current international financial institutions, this is now less of an issue. Havrylyshyn et al. (1998) are also very specific in their classification of the dependent variable as the growth rate of *real* GDP (Havrylyshyn et al. 1998, p. 13). This is as opposed to nominal GDP. The World Bank GDP metrics all measure real GDP unless it is specifically demarcated as "nominal." This indicator in the updated models should be functionally identical to the dependent variable in the original models.

Using GDP percent growth to represent economic growth is a widely accepted practice. Certainly, at the time the original studies on transition economy growth were taking place, it was the default option with few challengers. In recent times, GDP percent growth as an indicator has come into question as it may not fully capture the depth of economic growth—especially when considering ideas such as sustainable growth (European Environment Agency 2021). While there are movements towards developing more comprehensive growth metrics, GDP percent growth continues as the default indicator (van den Bergh and Antal 2014). For the purposes of this study, it is also the most appropriate metric to consider. While the reasoning behind abandoning or supplementing GDP percent growth is compelling, this study is not a critique of growth indicators. There is no intention to comment on the ethics or responsibility in the field of economics. Further, since this study is a critique of the earlier models and indices used, there should be as much continuity as possible in the newly constructed models for comparative reasons. With certain indicators already being replaced out of necessity in data availability, as much of the original model should be preserved as possible. However, when considering the results of this econometric analysis, further studies could be pursued using alternate indicators of economic growth.

The proxy of macroeconomic stabilization policies will remain as an inflation metric. The literature is quite in agreement that stabilization is an important indicator and a likely candidate for predicting growth (Havrylyshyn et al. 1998, p. 11). In effect, the macroeconomic policies aimed at stabilization are usually concerned primarily or secondarily with controlling the rate of inflation (OECD 2014, p. 4). In light of this, and the difficulty of measuring macroeconomic policies in a more comprehensive manner,

the rate of inflation will continue to be used to measure macroeconomic policies and should not be attempted to be disaggregated. Whether or not to use the natural logarithm linearized version of this indicator is a matter of subjective debate.

The original model makes use of this linearization method and the justification is not explicitly stated, but clear enough. With inflation rate values into the thousands in the early transition period in some countries, these could be seen as outliers that might skew the data wildly. Intuitively it also makes some sense to interpret inflation on a log-scale—if the inflation rate is 1% that is good, if it then jumps to 10% immediately after, that is bad. But if it jumps to 20 or 30%, it is roughly the same level of bad. If it jumps 100 or 200% in one year, it does not really make a difference—there are major issues occurring either way. On the other hand, linearizing the inflation rates with a logarithm presents some problems of its own.

The number one issue is the presence of negative inflation rates in the data—one cannot take a logarithm of a negative value. While they are not common, these are interesting points of data that one might imagine could have valid explanatory value. If the inflation rate is linearized, there are two options—neither all that appealing. The data can be left blank, which not only means the explanation of negative inflation will be missing but that the entire entry for that country for the year in which the inflation was negative will be excluded from the regression calculations. This has a compounding effect in the overall model validity. The other option is to add a constant to every point of inflation rate data such that they are all positive. This is, in essence, shifting the origin to a more favorable position. Before a transformation occurs, this comparison should not affect the explanatory value of the indicator—for every percent increase in inflation, it will result in the same change to the growth rate. It is after the logarithmic transformation that this could result in problems. If the constant is relatively small, then the effect would be minimal. Therefore, if the highest absolute value of negative inflation is only one or two percent, the linearization would be like that of a situation with no negative inflation. However, in our case, the largest negative inflation is roughly -18.8%. This means a constant of roughly 18.9% would need to be added, which throws off the earlier explored intuitive explanation for inflation orders of magnitude. For that reason, the inflation rate

data in this study will remain un-linearized. The raw inflation rates might introduce some outlier data in the early years of transition with exceptionally high inflation rates, but that is the more acceptable alternative in this case to preserve the explanatory power of the indicator.

The original model uses the consumer price index (CPI) model for year-on-year inflation rate change from the same "authorities" as the GDP data (Havrylyshyn et al. 1998, p. 14). In our updated models we will instead use the inflation rates calculated based on the GDP deflator. Though they are different methods of calculation, they should theoretically measure the same thing. The reasoning for the switch is purely a matter of data availability. While maintaining parity to the original model is the first goal of reconstructing the database, there are a few countries with limited or no data for CPI inflation rates—namely the Central Asian countries. In the compromise between eliminating several of the Central Asian cases and using a different method of determining inflation rates, maintaining similar cases to the original model is a far more acceptable solution in terms of comparability. The data for inflation based on the GDP deflator as an annual percent in this study comes from the World Bank Open Data (2021) database, who in turn sourced it from World Bank national accounts and OECD National Accounts data files.

An indicator to measure the effects of government size was included in the original model, and though it was not aggregated, it was loosely associated with structural reforms as a sort of control variable to represent "factors such as crowding out, distortions through high taxation, and large bureaucracies" (Havrylyshyn et al. 1998, p. 13). The original model uses the indicator of general government expenditures as a percent of GDP, which came from their own IMF data and staff estimates. We will use the same indicator under a different name. The "general government expenditures as a percent of GDP" metric is now referred to as "government final consumption as a percent of GDP." The data for this indicator was also sourced from the World Bank Open Data (2021) database and was originally found in World Bank national accounts and OECD National Accounts data files.

There is one indicator that will be included in this study that was not found in Havrylyshyn et al.'s original model, although it was tested for inclusion. In the context of the original model, it was determined that foreign direct investment (FDI) "only gives significant results when structural reforms are not accounted for in the model specification" (Havrylyshyn et al, 1998, p. 24). Judging from preliminary analysis, it is likely that this has changed. With an extended period of analysis, FDI may account for as much if not more effect on growth than structural reforms. For this reason, it will be included in the updated models. Once again, the World Bank Open Data (2021) database is the source of FDI net inflows as a percent of GDP data, but the sources the database pulls from for this indicator are the International Monetary Fund, International Financial Statistics and Balance of Payments databases, World Bank, International Debt Statistics, and World Bank and OECD GDP estimates.

The cumulative liberalization index was the most difficult part of recreating the data set. Because of the "expert opinion" nature and limited time frame, it is unlikely that a completely accurate recreation can be devised. There is however, a very similar and useful alternative that Havrylyshyn et al. (1998) used in their original model to supplement the CLI data for years that the CLI did not cover. This can be found in the European Bank of Reconstruction and Development annual transition reports (1994-2014), where they assign yearly scores across roughly the same spectrum of indicators as De Melo et al. (1996) in their original CLI. Ostensibly, while supplementing this data in the data set for the Havrylyshyn et al. model there was some necessary yet undocumented data transformations that took place. The important takeaway from this, however, is that the EBRD indicators were considered close enough to the original CLI indicators to be considered a candidate for integration, not just replacement. Although the methodology in the first EBRD transition report does not cite the CLI as the basis of their scoring system, it seems to be a spiritual successor (EBRD 1994, p. 9). Aside from differences in scale—each area in the CLI is assigned a score from 0 to 1, while each indicator in the EBRD reports ranges from 1 to 4—the subcomponents seem to match quite well. The six indicators in the first EBRD annual report are: Large-scale privatization, small-scale privatization, enterprise restructuring, price liberalization and competition, trade and foreign exchange system, and banking reform. Based on the earlier concept structuring

assumed for the CLI, a recreation of the CLI using the EBRD report statistics could be mapped as shown in Figure 3.1.

With the reconstructed CLI mapped, an indicator using this process can be included in the data set. Again, it is not the suggestion of the author that this reconstructed CLI is an appropriate construction of an index. Rather, it is as close to the original CLI as possible.

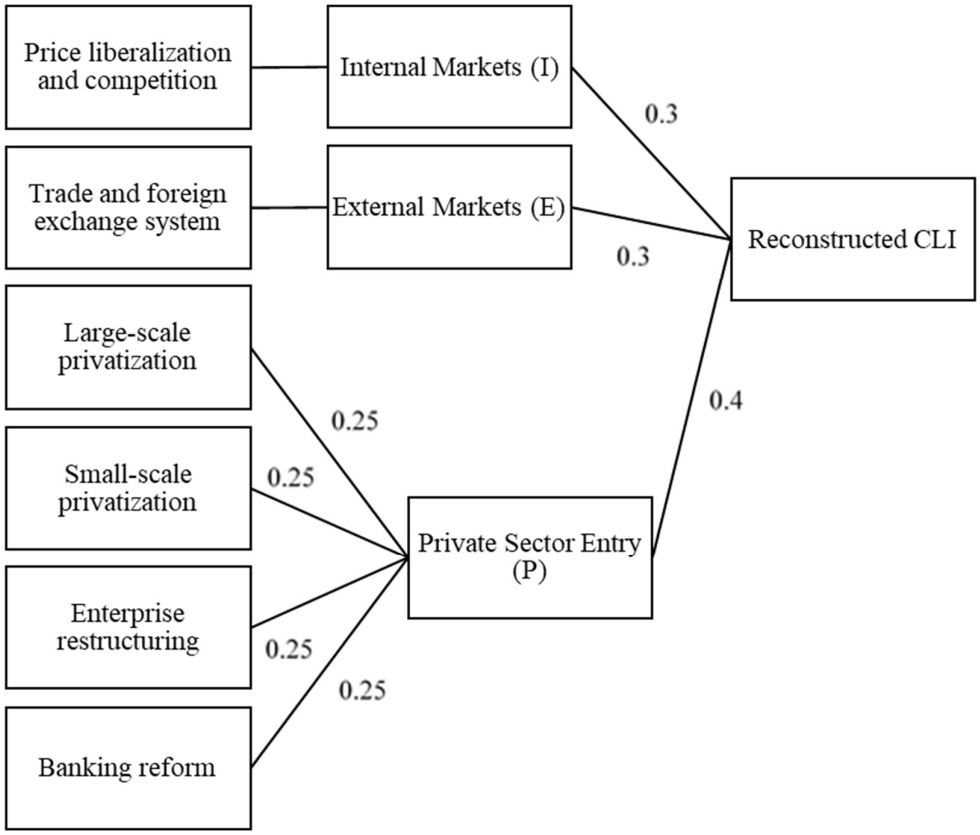


Figure 3.1 Reconstruction and weighting of the CLI from EBRD indicators
Sources: De Melo et al, 2001; and EBRD Transition Report, 1994

The availability of EBRD transition indicators was nearly the sole determinant of the time period of this study. The last year in which these indicators were published was 2014 (ERBD 2014); continuing the analysis beyond this point would be nearly impossible. The starting point, however, is a more interesting question. With the integration of the original CLI data, Havrylyshyn et al. (1998) were able to conduct their analysis between 1990 and

1997. The CLI was determined for the transition countries between 1989 and 1994, while the EBRD began publishing their statistics in 1994. What this indicates is an unenviable position of compromise for Havrylyshyn et al. To conduct the most effective analysis the longest possible time period should be used, but they were limited to sets of indicators from two separate and rather short periods. Although they were, as mentioned, highly comparable in their construction, it is still a compromise of validity to use data from two separate sources—especially if it is expert opinion data from two different panels of experts. They also started perhaps a bit early, as by 1990 transition was not in full effect in every country—the USSR was still united. Though it may be missing some of the earliest transition data, we now have the luxury of using one set of indicators to form the reconstructed CLI with 21 years' worth of data. The original model was, by necessity, evaluating only short-run growth. Now, it will be possible to evaluate this method based on medium-to-long run growth.

3.3. Four Aggregations of Structural Reform in Models and Measurement of Impact on Economic Growth

In addition to the reconstructed CLI, there are three other aggregations to explore that have been implied by our conceptualization in Section 2.3.:

1. The reconstructed CLI is an intuitively weighted additive aggregation, meant to recreate the original CLI of de Melo et al. (1996).
2. The PCA CLI is a statistically weighted additive aggregation utilizing a principal component analysis.
3. The multiplicative CLI (mCLI) is a multiplicative aggregation implied by the low-substitutability, necessary and sufficient conditionality conceptualization.
4. The disaggregated policy indicators are not aggregated in any way.

For the PCA CLI, a simple PCA will be performed on the EBRD indicators and the resultant statistics will inform the choice of how many components should be used to capture the variance across these indicators. This will assign new weightings for each indicator and will thusly form a new, statistically justified additive aggregation to satisfy

our concept structure. The use of PCA to form descriptive aggregations in time series and panel data is widely used, and the descriptive nature enables us to largely ignore some of the more technical complications present with PCA (Jolliffe 2010, p. 299).

Recall the two-level concept structure presented in Figure 2.2 on page 17. A quick assessment can be made whether a PCA will make sense to run by examining the correlation between these variables. As seen in Figure 3.2, all of these variables are quite highly, positively correlated to each other. This lends extra validity to this measure for statistical reasons along with our descriptive motivation for aggregation. The PCA is run and the results are visualized in Figure 3.3 and Table 3.1 With the first component of the PCA explaining over 70% of the variance among these indicators, it adequately captures enough of the variation to be the sole component necessary to create the new aggregation.

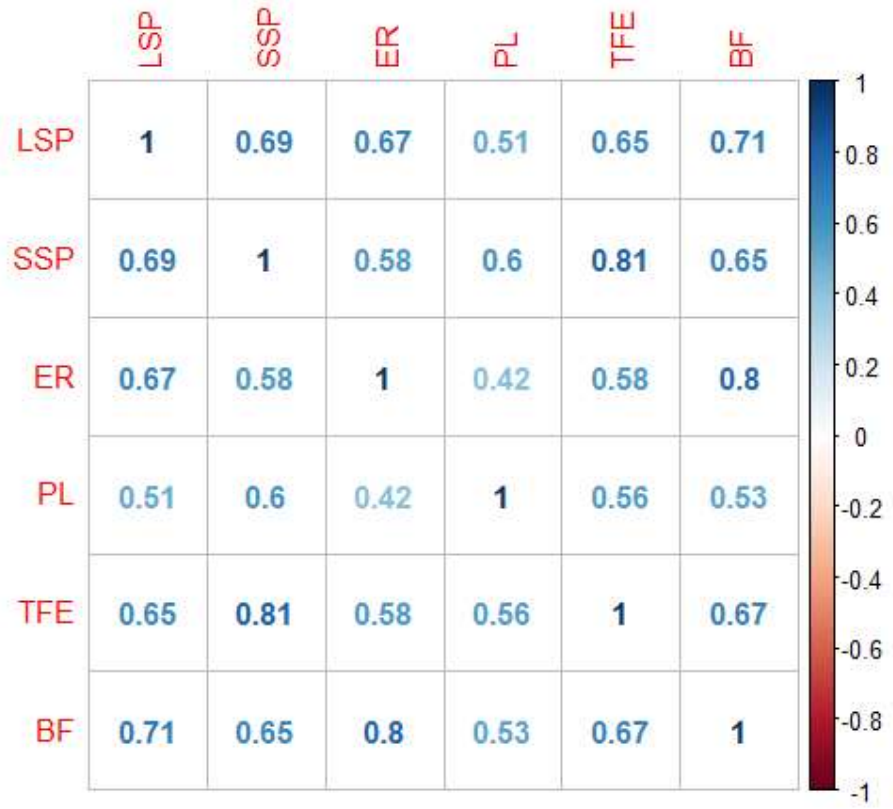


Figure 3.2 Correlation matrix of EBRD indicators

Source: EBRD 1994; Author's data

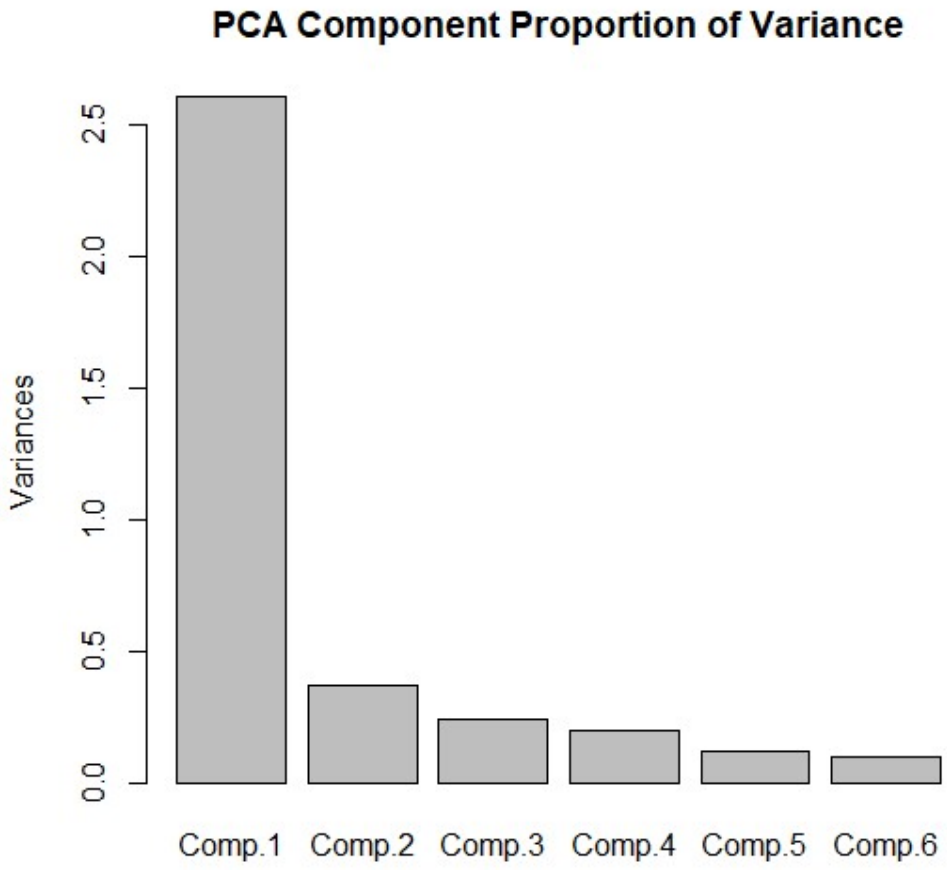


Figure 3.3 Visualized proportion of variance attributed to each PCA component

Source: EBRD 1994; Author's data

Table 3.1 Numerical proportion of variance attributed to each PCA component

	Comp. 1	Comp 2.	Comp. 3	Comp. 4	Comp. 5	Comp. 6
Proportion of Variance	0.716	0.103	0.066	0.054	0.0338	0.027
Cumulative Proportion	0.716	0.819	0.885	0.939	0.973	1.000

Source: EBRD 1994; Author's data

Table 3.2 Component 1 loadings for each EBRD indicator

Indicator	*Component 1 Loading*
Large-scale Privatization	0.466
Small-scale Privatization	0.376
Enterprise Restructuring	0.377
Price Liberalization	0.241
Trade and Foreign Exchange System Liberalization	0.467
Banking Reform	0.472

Source: EBRD 1994; Author's data

The weighting or "loadings" associated with each indicator in the first component are given in Table 3.2 The aggregation of EBRD indicators using the PCA Component 1 loadings will be considered the PCA CLI. A comparison of the given index values between the reconstructed and PCA CLIs can be seen in Figure 3.4. While the two additive measures tend to trend in the same directions, there is an observable tendency of the PCA CLI to exaggerate the differences from year to year compared to the "flatter" reconstructed CLI values.

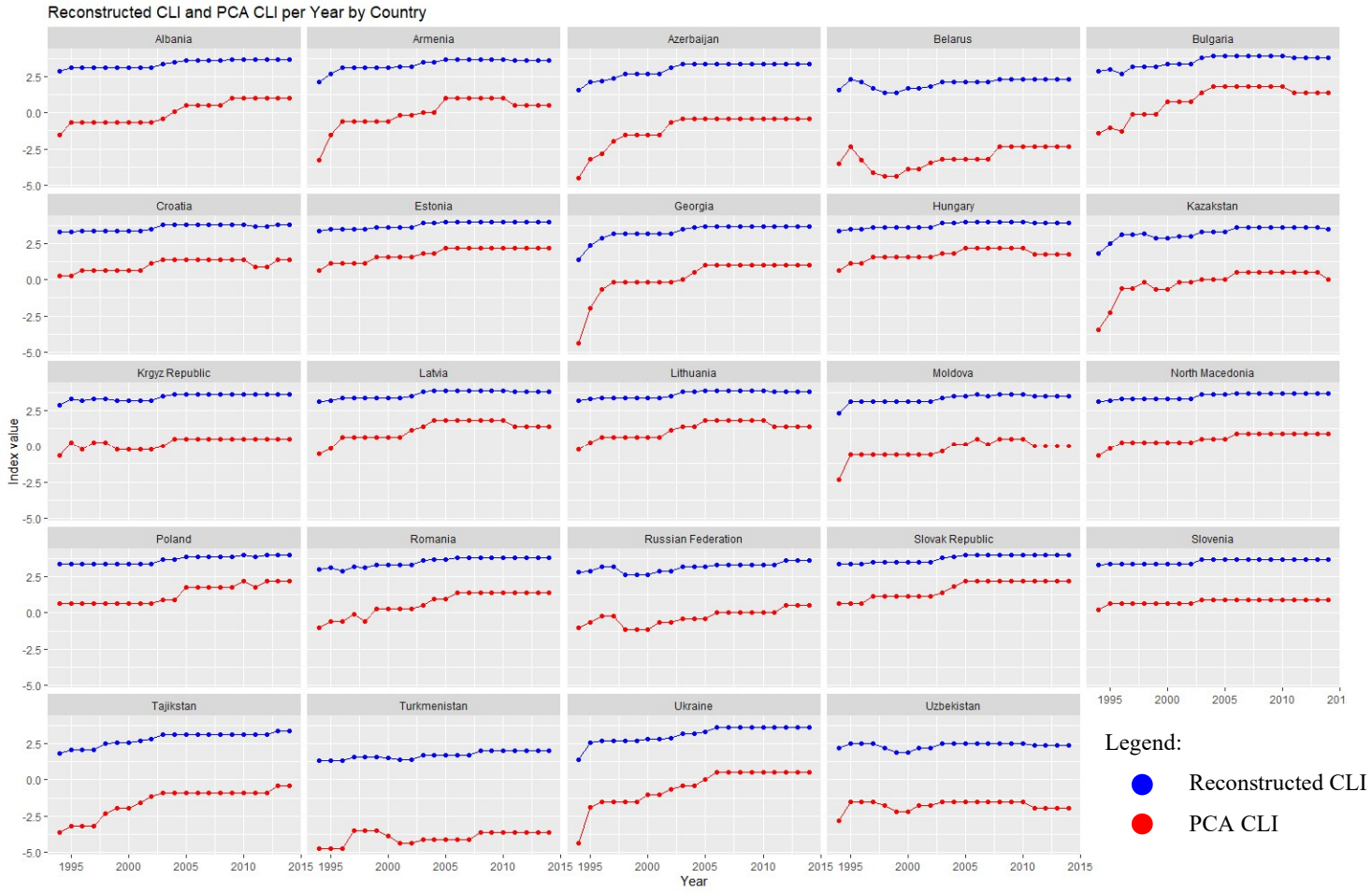


Figure 3.4 Comparison of additive aggregations over time across countries: Reconstructed CLI and PCA CLI

Source: de Melo et al. 1996; EBRD 1994; Author's data

The mCLI will be constructed using a multiplicative measurement. The first step is to transform all the EBRD data into an appropriate scale, since the basis of multiplicative measurements is to indicate an absence of the concept if one indicator (or second level concept, in our case) has a value of zero. Since the EBRD scale starts at one in the case of absence of change, one will be subtracted across all the indicators such that it will instead span from zero to three. This scale transformation will have no other impact than allowing the correct use of a multiplicative measurement. The four indicators for the privatization component will be averaged according to the conceptual structure in Section 2.3. and Figure 2.3 (p. 18). Then, the three second level concepts will be multiplied together. This is a different scale from the additive measurements, but they are still comparable if the impact on the coefficient estimate in the regression is considered. When examining the plots of the mCLI (see Figure 3.5), the same trends as the two additive models are still evident—albeit in a far more exaggerated manner. Not only are the lower scores heavily penalized (see Turkmenistan, Belarus), but smaller increases in score can lead to large jumps in the scale (see Latvia, Slovak Republic).

Finally, and once again, the disaggregated model need not be "constructed," as each indicator will stand on its own when evaluated.

The four aggregations above will then be plugged into otherwise identical models of economic growth. These four models serve as the independent variable of the thesis and are categorized according to their conceptually implied aggregation of structural reforms:

1. The intuitive-additive model, which uses the reconstructed CLI to aggregate structural reforms.
2. The statistical-additive model, which uses the PCA CLI to aggregate structural reforms.
3. The multiplicative model, which uses the mCLI to aggregate structural reforms.
4. The disaggregated model, which does not aggregate structural reforms.

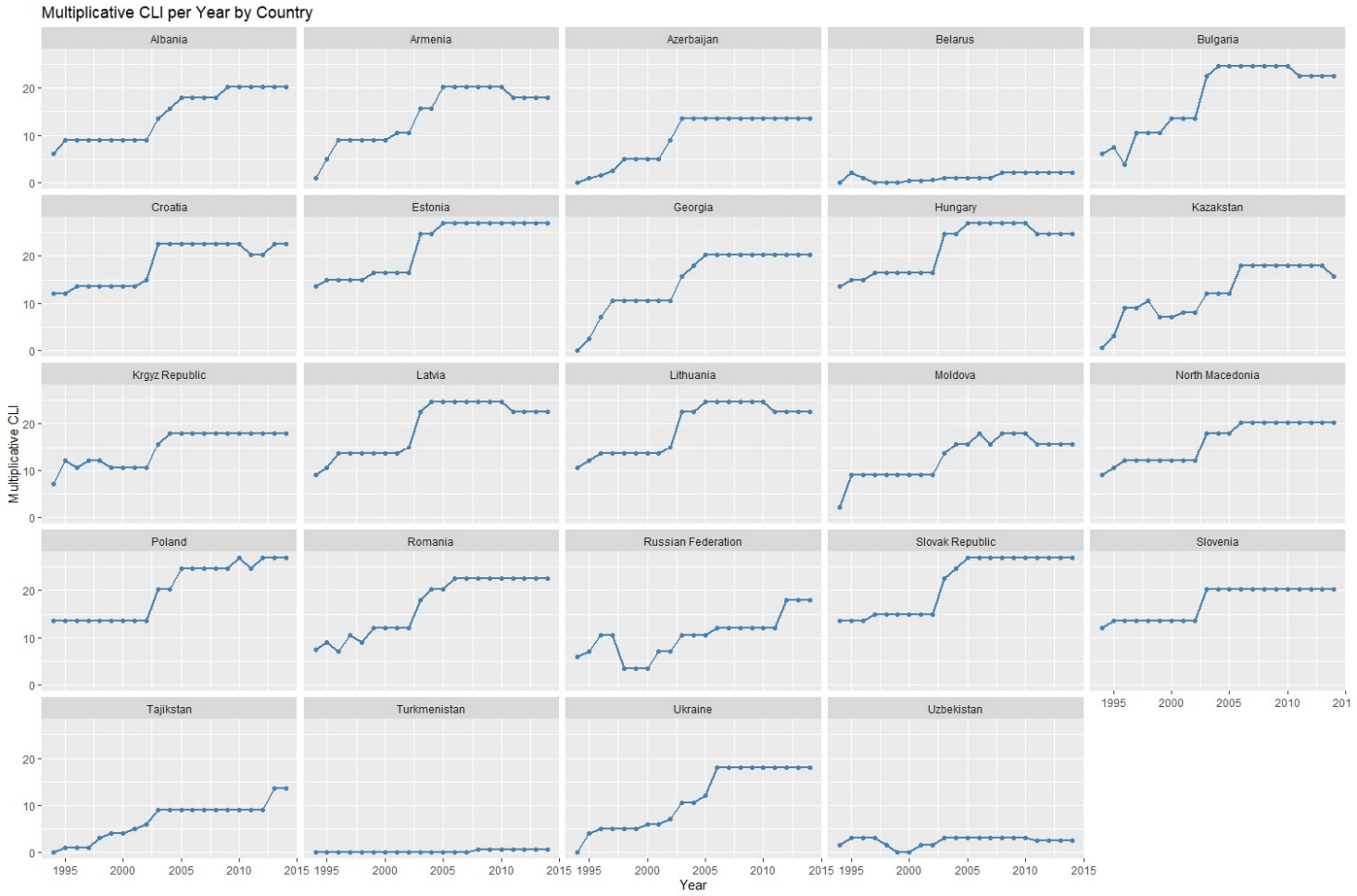


Figure 3.5 Multiplicative CLI over time across countries

Source: EBRD 1994; Author's data

We can observe a direct line running all the way through from conceptualization to aggregation to modelling, where a change at the concept level can have a direct impact on how each model will function.

Comparing the models will be an assessment that involves comparing their explanation of economic growth. The adjusted coefficient of determination (R^2_{adj}) will be used to preliminarily identify the overall model goodness of fit. The R^2_{adj} is more resilient to the bias introduced with more independent variables than the non-adjusted R^2 statistic, but it is not immune. Therefore, we will likely see a higher R^2_{adj} in the case of the disaggregated model. For this reason, we will also directly compare the prediction errors of the models by plotting them against the actual GDP growth data. This will give more illustrative examples of how the models differ in their predictive power.

The implications of these findings will be discussed in the context of how these models can be used to promote or obscure different findings when it comes to the aggregation of structural reforms—and the possible policy implications thereof. For this we will return to the regression statistics and evaluate how each aggregation performs within the model in relation to the others. Again, we may experience some issues with the disaggregated model in the form of multicollinearity between the EBRD indicators. This could result in inflated significance of certain variables.

Finally, if possible, the substantive results of the analysis can be used to propose modifications to the use of structural reforms aggregations or indicators that would increase the validity of the construct.

The general structure of the testing method is as follows:

1. Establish the "base model" in which only the lagged dependent variable is used to explain economic growth. All other models will be evaluated in the context of this finding.
2. Run a regression and report the resultant statistics of the intuitive-additive, statistical-additive, and multiplicative models.
3. Test the disaggregated model for multicollinearity.
4. Run a regression and report the resultant statistics of the disaggregated model.
5. Compare the results of the regression statistics.
6. Compare the prediction errors of the models.
7. Discuss these findings, limitations, and implications of the different aggregations.
8. Propose modifications to the use of structural reforms indicators or aggregations.

3.4. Specification of Models

To obtain the necessary outputs and statistics to compare our models, we will perform a fixed-effects panel regression on each set of data inputs. These regressions will be performed in the statistical computing software environment R using the "plm" package of linear models for panel data. The plm package enables us to run an ordinary least squares (OLS) model across panel data while accounting for heterogeneity between countries without including the country specific constants in the goodness of fit calculations. This is an alternative to using a least-squares dummy variable (LSDV) model which would assign the country specific constants to dummy variables and include them in the goodness of fit. Since we have decided that we are primarily interested in the effects of the variable data on economic growth, the plm package enables a simple solution to include fixed-effects while ignoring their explanatory impact.

As opposed to a normal regression model, a panel regression allows for comparison of multiple variables across time periods from multiple cases or locations (in our case, the selected transition countries). Whereas a simple regression would only be able to address this kind of data across a single case, the panel regression takes the data from every case into consideration. This type of regression is perfectly suited to compare similar data

across time for many different cases. There is, however, a danger of bias implicit to this type of comparison when the fundamental or constant differences between cases are considered. How can all these countries be compared without accounting for all the differences between them like size, geography, differences in income and cost of living, and countless other small factors that could contribute to a model of growth? To account for these factors, we introduce the idea of fixed-effects.

Surely, initial conditions are important when investigating growth in a transition economy. As determined in the literature review, this is an area that received plenty of attention at the time. But the stylized regression model proposed by Havrylyshyn et al. should be a fundamentally different exploration; it sought to determine how policy indicators which vary over time explained economic growth, with what should have been no regard for constant factors. The inclusion of these effects cannot simply be discarded, but that is precisely why fixed-effects methods exist. By accounting for the constant differences between cases, a distinct constant value is included in each individual case's regression. This has the desired effect of neatly packaging all the extraneous constant information that varies between cases to allow for direct comparison of the desired factors and how they vary within cases.

In Havrylyshyn et al.'s conclusion, it was therefore no surprise that they found their various initial conditions had very little explanatory value in the model (Havrylyshyn et al. 1998, p. 32). Additionally, Havrylyshyn et al. (1998, p. 32) find that earlier assumptions about initial conditions might be incorrect, as the usefulness of initial conditions in predicting growth diminishes over time. In our case this will be even more pronounced, as we start from a later point and have expanded the period of study. Therefore, a fixed-effects estimate remains the most appropriate way to account for differences between cases without specifying the model to a degree of granularity that would defeat the very purpose of a "stylized" model. Mathematically, the use of fixed versus random effects in regression modelling for panel data tends to make no difference when the number of cases (countries in our case) is fixed and the period of study is large (Hsiao 2014, p. 47). A period of 20 years is considered large when evaluating panel data

as will be determined regarding the lagged dependent variable. Therefore, we will stick with our theoretical understanding of fixed effects and use them in our models.

The other idiosyncrasy of the model implied by the data structure is the lagged dependent variable (LDV). Utilizing a Breusch-Godfrey Test for serial correlation in panel models, it was determined that all proposed models were highly serially correlated, which is not surprising in a case like growth prediction. The growth rate last year will likely influence the growth rate this year. The original model would have almost certainly included the LDV as a predictor were it not for the high degree of biased parameter estimates it introduces to a fixed effects model over short periods of evaluation (Beck and Katz 2011, p. 342). This is one benefit of expanding the period of study. When the period is 20 years or greater (as in our case), this bias becomes very small to the point where it can be disregarded (Beck and Katz 2011, p. 342). The LDV will be used in this study to give a baseline predictive model and increase the overall predictive powers of each model. It is the same GDP percent growth data as the dependent variable, lagged by one year.

With the considerations and peculiarities of these models dealt with, the models can now be specified.

1. Intuitive-additive model:

$$GR_{i,t} = \alpha_i + \beta_1 GR_{i,t-1} + \beta_2 CLI_{i,t} + \beta_3 IR_{i,t} + \beta_4 GE_{i,t} + \beta_5 FDI_{i,t} + \in_{i,t}$$

where:
i represents the case or country,
t represents the year,
GR is the indicator for percent GDP growth,
α is the country specific fixed-effect,
β_n are the coefficient estimates for each independent variable,
CLI represents the reconstructed CLI,
IR is the indicator for inflation rate based on the GDP deflator,
GE is the indicator for general government final consumption as percent of GDP,

FDI is the indicator for FDI as a percent of GDP, and
$\in$ is the error term.

2. Statistical-additive model:

$$GR_{i,t} = \alpha_i + \beta_1 GR_{i,t-1} + \beta_2 PCA_{i,t} + \beta_3 IR_{i,t} + \beta_4 GE_{i,t} + \beta_5 FDI_{i,t} + \in_{i,t}$$

where:
PCA represents the PCA CLI, and the other terms remain the same as in the intuitive-additive model.

3. Multiplicative model:

$$GR_{i,t} = \alpha_i + \beta_1 GR_{i,t-1} + \beta_2 mCLI_{i,t} + \beta_3 IR_{i,t} + \beta_4 GE_{i,t} + \beta_5 FDI_{i,t} + \in_{i,t}$$

where:
$mCLI$ represents the multiplicative CLI, and the other terms remain the same as in the other models.

4. Disaggregated model:

$$GR_{i,t} = \alpha_i + \beta_1 GR_{i,t-1} + \beta_2 LSP_{i,t} + \beta_3 SSP_{i,t} + \beta_4 ER_{i,t} + \beta_5 PL_{i,t} + \beta_6 TFE_{i,t} + \beta_7 BF_{i,t} + \beta_8 IR_{i,t} + \beta_9 GE_{i,t} + \beta_{10} FDI_{i,t} + \in_{i,t}$$

where:
LSP is the policy indicator for large-scale privatization,
SSP is the policy indicator for small-scale privatization,
ER is the policy indicator for enterprise restructuring,
PL is the policy indicator for price liberalization,
TFE is the policy indicator for trade and foreign exchange system liberalization,
BF is the policy indicator for banking reform, and the other terms remain the same as in the other models.

3.5. Predictions

The expectations for the results of this study, having seen the data and based on previous results with similar indicators, are that the intuitive-additive model will have the lowest explanatory value and the disaggregated model will have the highest. The statistical-additive and multiplicative models will lie somewhere in the middle. The main concern in terms of explanatory power comes from the EBRD transition policy variables.

While the reliability of the data is not under question, its validity as a predictor of economic growth from 1994 to 2014 is not promising. As shown in Figure 3.4 on page 31, there is a quite distinct tendency of the reconstructed CLI data to attain a certain level of progress according to the EBRD and then simply stagnate. When looking at Estonia, Hungary, Poland, Lithuania, or the Slovak Republic, this is not surprising as they all near the top of the scale after a time. But even for countries which failed to achieve the highest scores like Belarus, Russia, and the Central Asian nations, they still seem to stagnate at a certain point. What this indicates is that countries adopted virtually all the liberal policies that they were going to adopt relatively early in transition, then simply stopped. By the mid-2000s, even those who seemed to be lagging were not making any more progress on the EBRD transition scale. With this stagnation in the later years of the period of examination, the CLI and even the individual EBRD indicators almost begin to resemble constants—meaning whatever explanatory power they might have had in early transition will likely be muted by the lack of variation in later transition. With that in mind, the aggregation of the EBRD indicators into the reconstructed CLI seems an even worse idea. What little variation there might have been left will be completely obscured through the averaging process. The PCA and multiplicative CLI reaggregation processes, while more effective than the reconstructed CLI, will still yield lesser results than the fully disaggregated model in this case. It would be considered a qualified success if the statistical-additive or multiplicative model is nearly as useful for predicting growth as the disaggregated model, as they are supposed to be “stylized” versions. Realistically, the complexity difference between any of these models is not enough to justify the loss of explanatory power in the name of dimensionality or complexity reduction.

Another dimension of increasing the time period of the study that must be taken into account is the effect of exogenous shocks, most notably the global financial crisis of 2008. When viewing the GDP growth trends in Figure 3.6, it is plainly evident that the years directly following the crisis were deeply impacted by it. While it is possible that some indicators might capture this variation incidentally (increasing inflation, decreasing FDI or government expenditures), we know from examining the raw EBRD data (see Appendix A for raw input data) and reconstructed CLI that these policy indicators show no signs of change to account for this. Outside of the early years of transition, the impact of the crisis is the most significant change in GDP growth data. This should result in another mark against the explanatory power of the reconstructed CLI as well as the individual EBRD indicators.

The choice in this study to eliminate the initial condition constants in favor of a fixed-effects model should result in some change to explanatory power. Because the method of analysis specifically does not include the estimated constant country differences in the evaluation of goodness of fit, this should result in lower goodness of fit across all three models we will test. However, this is justified by the fact that we are deciding to exclude the country specific differences in initial conditions from our analysis in favor of a more mathematical approach; we are interested in explanatory power of what can be compared directly across countries. The fixed-effects regression gives us the opportunity to do this by mathematically accounting for these differences instead of relying on manually determined constant inputs from the researchers. These inputs would be impossibly exhaustive in the best case, and detrimentally incomplete in the worst case.

The goodness of fit effects introduced by the fixed effects will likely be entirely offset by the positive effects of including the LDV. Since we know these models are serially correlated, we can assume that a high amount of explanatory power resides in the LDV. Therefore, it is expected that across models the LDV will have the highest significance.

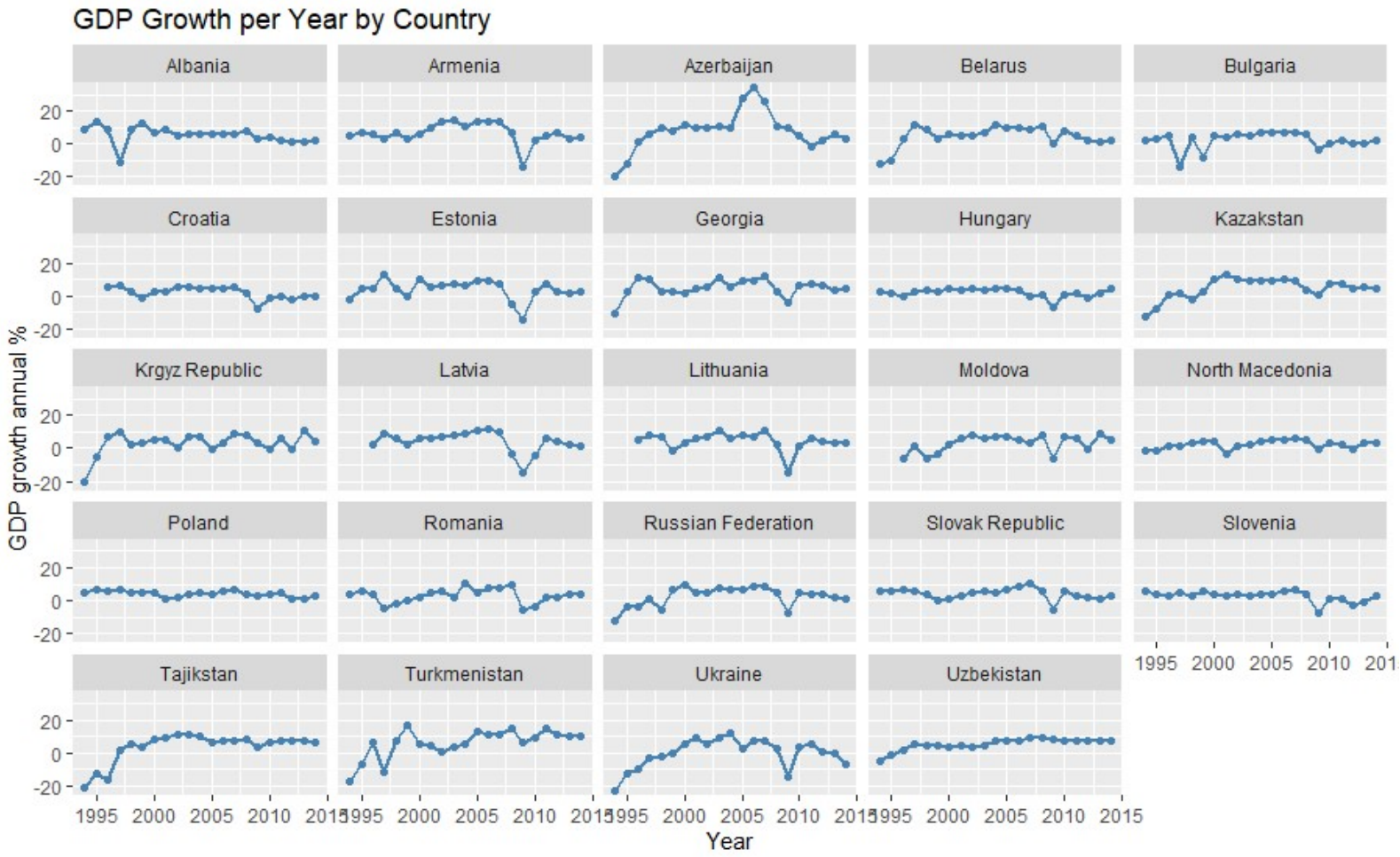


Figure 3.6 GDP Growth per year by county

Source: World Bank Open Data (2021)

The last expectation that should be mentioned is the role of the FDI variable. Although excluded for lack of explanatory power in the original model, we expect that to change as the period of study lengthened and progress in transition increased greatly. We know that in CEE and the FSU where technology gaps are not very wide that the countries with the most significant growth and success are also the countries with some of the highest rates of FDI (Lim, 2001). Based upon these indications as well as some preliminary analysis, it seems likely that FDI has taken on much more importance in explaining economic growth since 1997. Upon the decision to exclude FDI from the original model, Havrylyshyn et al. (1998. p. 24) posited that while there is a broad correlation between economic growth and FDI, this correlation is more likely related to both being explained by the policy indicators; the same factors that promote growth may also attract FDI. While this may be true, we expect to find that FDI has more of an impact on growth than originally estimated. Further, even if growth and FDI are influenced by the same factors, those factors are unlikely to be something like the CLI or EBRD indicators—at least not directly.

4. ANALYSIS AND DISCUSSION OF RESULTS: EVALUATING STRUCTURAL REFORM AGGREGATIONS THROUGH COMPETING MODELS FOR GROWTH IN 24 TRANSITION ECONOMIES FROM 1994 TO 2014

4.1. Regression Results

The analysis begins by established the baseline model of LDV explanation of economic growth. Table 4.1 provides a comparison table of the regression statistics across all of the tested models. The R^2_{adj} value is not particularly high in the case of the baseline model, but enough to give a good idea of what to expect from the other models in terms of better explanation. From here, the other models can be analyzed.

Next is the analysis of the intuitive-additive model, using the aggregation of structural reforms based on the original CLI (see Table 4.1, column "Intuitive-Additive"). The initial impressions are much as expected. The inclusion of extra variables indicates a better goodness of fit, but that is probably as much related to the number of variables as it is their explanatory power. We also see the relative explanatory power of the reconstructed CLI is the lowest of any variable. Perhaps the most surprising result is the negative relationship between the reconstructed CLI and the dependent variable of economic growth. This marks a departure from the results of other studies using the CLI. Assessing changes in the relation of structural reform measures to economic growth is not the primary goal of testing these models, but it is interesting to note nonetheless.

Initial observations based on the R^2_{adj} and significance of the PCA CLI indicate that this aggregation can explain economic growth slightly better than the reconstructed CLI (See Table 4.1, column "Statistical-Additive"). Really though, the difference is miniscule. Although the PCA loadings result in different values for the PCA CLI, they are not so dissimilar from the weightings given in the reconstructed CLI that a drastically different result is achieved when it is plugged into the model.

Table 4.1 Fixed effects panel regression results for four models of economic growth and baseline model

	Dependent Variable: Economic Growth				
	Baseline Model	*Intuitive-Additive*	*Statistical-Additive*	*Multiplicative*	*Disaggregated*
Reconstructed CLI		-1.603*			
		(0.050223)			
PCA CLI			-0.769**		
			(0.039727)		
Multiplicative CLI				-0.110**	
				(0.018521)	
Large-scale Privatization					-1.132*
					(0.05763)
Small-scale Privatization					2.564***
					(0.001705)
Enterprise Restructuring					-0.98
					(0.184486)
Price Liberalization					-0.112
					(0.846611)
Trade and Foreign Exchange System Liberalization					-1.426*
					(0.060718)
Banking Reform					-0.487
					(0.392833)
Lagged Dependent Variable	0.441***	0.344***	0.345***	0.339***	0.294***
	(<0.00000001)	(<0.00000001)	(<0.00000001)	(<0.00000001)	(<0.00000001)
Inflation Rate		-0.013***	-0.013***	-0.013***	-0.011***
		(0.0000011)	(0.00000094)	(0.0000013)	(0.000065)
Government Expenditures		-0.367***	-0.355***	-0.363***	-0.384***
		(0.0000637)	(0.0000874)	(0.0000642)	(0.000039)
Foreign Direct Investment		0.107***	0.110***	0.110***	0.117***
		(0.006277)	(0.005411)	(0.004897)	(0.002908)
Countries	24	24	24	24	24
Years	18-20	18-20	18-20	18-20	18-20
Observations	472	470	470	470	470
R2	0.26	0.32915	0.32975	0.33175	0.35002
Adjusted R2	0.221	0.28655	0.28719	0.28932	0.30082
F statistic	157.36 (df = 1; 447)	43.274 (df = 5; 441)	43.392 (df = 5; 441)	43.786 (df = 5; 441)	23.479 (df = 10; 436)

Note: *p<0.1; **p<0.05; ***p<0.01; p-values are listed in parentheses below coefficient estimates

Source: See section 3.2. for the sources of each variable; Author's data

The multiplicative model continues this trend of very slight improvement. Once again, there is a slight improvement in terms of the R^2_{adj}—this time it is an improvement over both the additive models (See Table 4.1, column "Multiplicative"). The significance of the structural reform aggregation has also increased slightly. It is interesting to note that even though the scale and construction of the aggregation has changed, the results are still rather similar.

Lastly, we will assess the performance of the disaggregated model. Since it will make use of six variables representing different kinds of structural reform instead of aggregating them into one, the usefulness of comparing the R^2_{adj} and significance of the variables to the other models will be lessened. R^2_{adj} will be inflated through the introduction of extra variables and the significance of the individual EBRD indicators could be inflated through multicollinearity. To predict how multicollinearity might affect the results we will use a Farrar-Glauber test on a pooled OLS version of the disaggregated model to provide diagnostics that inform our assessment. Remembering the correlation matrix (see Figure 3.2, p. 28), we can proceed in assuming we are likely to find multicollinearity among the EBRD indicators since they are all relatively highly correlated with each other. Indeed, the output for overall diagnostic testing indicates that collinearity was detected. For diagnostics relating to the individuals, the variance inflation factor (VIF) is used as an indicator. A VIF in excess of 10 is considered high, and a VIF above 5 is considered medium. Table 4.2 shows the associated VIF scores for each EBRD variable. Small-scale privatization has the highest VIF at 4.12. The rest of the indicators have VIF scores below four. This test reveals that while there is a concern of multicollinearity influencing the results of a regression, it is not so high that variables should be eliminated of aggregated for mathematical reasons. Therefore, we will proceed with the disaggregated regression, keeping in mind how the multicollinearity may impact the results.

Table 4.2 VIF scores per EBRD indicator

Indicator	*VIF*
Large-scale Privatization	2.63
Small-scale Privatization	4.12
Enterprise Restructuring	2.97
Price Liberalization	1.86
Trade and Foreign Exchange System Liberalization	3.43
Banking Reform	3.82

Source: EBRD 1994; Author's data

For the reasons listed above of adding extra variables and introducing heightened multicollinearity, the R^2_{adj} and significance of the variables cannot be reliably compared to the other models examined in this section. The R^2_{adj} is higher than the other models which indicates a better goodness of fit, but it is not clear whether that is entirely due to the additional variables or if the predictive power is truly better. In the discussion section this will be addressed by comparing the accuracy of prediction using the prediction errors generated by the regression. Likewise, the significance of small-scale privatization seems to be much higher than any of the other EBRD indicators, but it is difficult to say whether this is simply a result of its outsized VIF. Since the VIF is below five, it would be safe to assume that at least some amount of its significance is truly due to the predictive power inherent in the indicator. Assigning some specific measurement to this, however, remains elusive.

In the discussion section which follows, the four models will be compared using the statistical results of the regressions and an analysis of the fitted versus actual values each model generates. Limitations encountered in this examination will also be addressed. Finally, the implications of the use of these models will be discussed.

4.2. Discussion

In comparing the four different models, there is an overwhelming pattern observed of similar goodness of fit and predictive capabilities. Looking at Table 4.3, we can see a summary of the most relevant regression statistics across models.

Table 4.3 Selected regression statistics for comparison

Model	R^2_{adj}	*Significance of Structural Reforms Aggregation (p-value)*
Baseline	0.221	-
Intuitive-Additive	0.28655	0.050223
Statistical-Additive	0.28719	0.039727
Multiplicative	0.28932	0.018521
Disaggregated	0.30082	0.001705 - 0.846611*

*The range of all significance statistics across six indicators is represented.

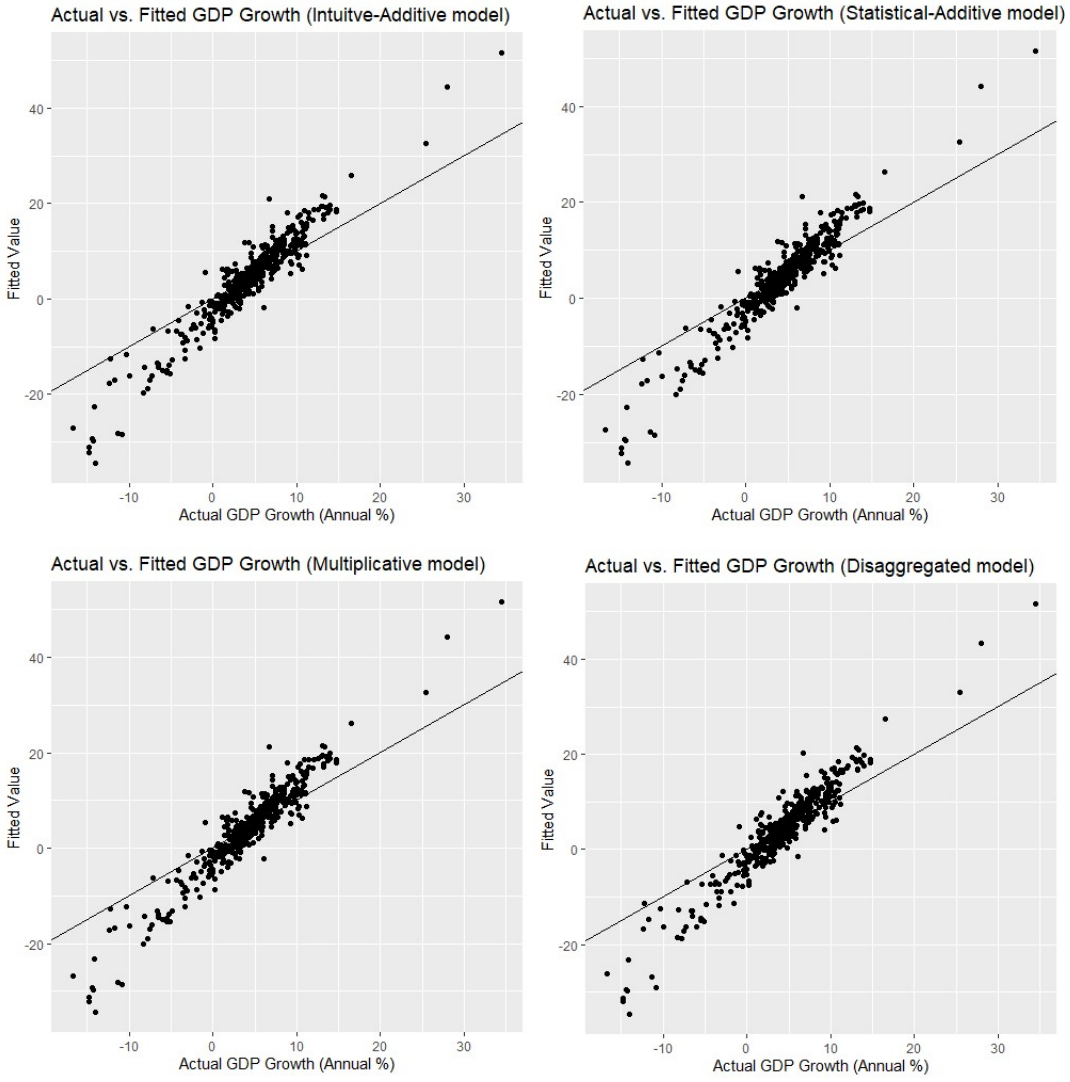
Source: Table 4.1, p. 44

The first three models can be compared more directly using these statistics, as they have the same number of variables. In comparing them, we see that the goodness of fit is incredibly similar (though increasing in descending order). The p-values of the structural reform aggregation components are a bit more differentiated, as it transitions from a significance of over 0.05 in the intuitive-additive model (above the significance threshold in many cases), to 0.03 in the statistical-additive model and 0.01 in the multiplicative model. The latter two p-values are below the standard 0.05 threshold for significance in regression modeling. The disparity between these two statistics indicates that while the type of structural reform might not have a large impact on the overall goodness of fit of the model it is contained within, the type of aggregation can lend validity to the model with a lower p-value and thus higher significance. For reasons enumerated in the previous section, neither of these measures are very useful in comparing the disaggregated model with the other three. What they do reveal is that—multicollinearity notwithstanding and

largely addressed—the indicators themselves can vary quite widely in significance within the model.

To compare the disaggregated model more directly with the others, and to observe general trends in prediction among all the models, a plot of actual vs. fitted values for GDP growth percent will be examined. As is shown in Figure 4.1, once again all the models show a general trend towards similar predictive power.

Figure 4.1 Actual vs. fitted GDP Growth across four models



Source: World Bank Open Data 2021; Author's Data

By assessing the level to which the fitted values on the y-axis match with the actual values along the x-axis (perfect prediction would align along the line of equality), we can see that there a very few discernable differences between the models. Among all four there is a general tendency to under-predict growth when it is low or negative and over-predict as it gets higher. The disaggregated model is basically identical with the other models. This result aligns with the results from the goodness of fit comparison, indicating that the R^2_{adj} value of the disaggregated model is likely higher in part due to the additional variables. But not so much higher that it is noticeably worse when comparing the actual and fitted values.

Considering the combined understanding provided by these two analyses, the initial prediction is confirmed. The disaggregated model has the best explanatory value of economic growth, followed by the multiplicative model, then the statistical-additive model, and the intuitive-additive model has the lowest. More practically though, the differences between them are negligible.

There are a fair few caveats and limitations to consider when viewing these results. The first is a question of reliability in the reconstructed CLI of the intuitive-additive model. It was an imperfect recreation and involved some significant guesswork. There was no hope of recreating the multi-stage expert opinion aggregation of de Melo et al.'s (1996) original CLI, and without any prescription from Havrylyshyn et al.'s (1998) CLI-EBRD indicator hybrid it is almost guaranteed that this is an imperfect reconstruction. The overall critique regarding consistency between concept structure and measurement still stands, but it is possible that there is a more accurate way to recreate the CLI in the original manner that is simply not available to the author.

Time has clearly wreaked havoc upon the usefulness and predictive power of structural reform indicators and aggregations alike. The results of these panel regressions were nowhere near the goodness of fit statistics achieved in earlier examinations. In the original formulation of the CLI, it was proposed that there may be a theoretical basis for building a decay function into the weighting of the CLI—though at the time there was not an observable basis to specify this (de Melo et al. 1996, p. 404). This study, as well as an

observation of the stagnant state of the structural reform indicators, could serve as this very basis.

Along with the decline in the power of the CLI and structural reform aggregations, there is a rise in the significance of other variables. This is observable through the inclusion of FDI in this study, although there are likely others in this context that have not been tested for. FDI outperformed the structural reform indicator in all four models, excepting only the disaggregated indicator for small-scale privatization. For a variable that was struck from earlier models due to lack of significance, this represents a great departure. Again, this is likely due in almost equal parts to the declining usefulness of the structural reform indicators alongside the increased role of FDI.

The lack of data availability for certain indicators in certain countries was a limitation that had a potential impact on the panel regression models. To minimize bias, the goal was to have 20 years of data for each country. This was achieved in most cases, but for certain countries inflation rates, general government final consumption, or FDI data was not available for the earliest one or two years. This resulted in unbalanced panels, where these countries were only modeled over 18 or 19 years, while the others were evaluated across all 20. For our purposes this was acceptable, as even a period of 18 years is still considered a large enough span to minimize bias and missing data entries at the beginning of the time span have a minimal effect on the validity of the results (Beck and Katz 2011, p. 333).

In the end, these limitations are largely dismissible as it is not the goal or aim of this paper to create the best overall model for economic growth. Rather, the effects of different aggregations are being tested through embedding them within models of growth that are otherwise identical. Technically, we have found that the disaggregated model predicts economic growth the best in this situation. But we also found that on a more practical level, they all performed very similarly. The implications to be drawn from this are not at all related to the practical use of aggregations, but rather to following the thread back through the models, past the measurement, and back to the concept structuring. By tweaking the language and conceptualization of structural reforms, the outcome of the

models has been influenced. None of the data was changed, and none of the other model variables or methods were changed. All that changed was the concept structuring and the associated measurements.

Of the three aggregated models, the multiplicative performed the best by a slim margin. An economist who believes that each component of structural reforms is necessary and sufficient for a country to be considered highly reformed might feel vindicated by this result, as it proves that their conceptualization was correct. They would, of course, be wrong. Higher predictive power or significance does not equate to theoretical correctness. Concept structuring cannot be influenced by the eventual outcome—the opposite must be true. Causality in this situation flows out of the concept structuring. Because there is theoretical justification based on certain observable or conjecturable characteristics about the very nature of the concept or relationships within it, that then could imply a specific type of measurement that must be used—this measurement having its own, unique impact on any results. This is a situation vulnerable to abuse, whether it is intentional or unintentional.

A researcher might try some different methods of aggregation of structural reforms and find that the multiplicative performs best and thus, should be used in their model. This *a posteriori* justification is ignorant to the importance of clearly structured concepts. The researcher might not care or understand that a multiplicative measure necessitates that the aggregation takes on a necessary and sufficient conditionality. If this model is then used to assess determinants of growth in transition countries and the structural reforms component is found to be highly significant, then there could be actual policy implications to that unintentionally incorrect, *a posteriori* justification. Policymakers might use the results of this model to determine which policy objectives to focus on. If the model makes the conceptual assertion that each component of the structural reforms aggregation is necessary and sufficient, then policymakers may take that at face value. This same scenario could play out in a more sinister fashion, where an actor with a specific agenda could intentionally use a multiplicative model even if they are conceptually aware it is not the most fitting. Thus, they can misrepresent the concept structure to purposefully obtain a result that supports their agenda.

The analysis of the disaggregated model reveals some even more opportunities for abuse of aggregations to color perceptions of the results. The first and most obvious in this case is to use an aggregation like the cumulative liberalization index to obscure the variations within the more specific indicators. A liberalization proponent might see the results of the disaggregated model in this study as unfortunate and possibly a bit threatening. Only one of the liberalization indicators has a positive effect on growth, and only three are at all significant. An easy way to obscure this is to aggregate the indicators such that the negative effects are offset by the positive ones. If given enough weighting, small scale privatization turns a result of one positive and 5 negative relationships into a single positive relationship. This would promote liberalization as a whole, rather than indicating to policymakers that there are certain liberalizing policies that may work better than others. This represents the intentional side of abuse in the case of aggregation, but again, there is also room for unintentional abuse. To aggregate simply for the sake of "stylization," or descriptive purposes can result in the same obscuring of the data without even realizing it. This should not be interpreted as: one must always have a statistical justification for aggregation. Descriptive aggregations can be perfectly fine—if they are conceptualized and justified appropriately. In fact, de Melo et al.'s (1996) original CLI was a good example of this, at least until the issues of concept structuring arise. But the justification for descriptive aggregation of the CLI was theoretically sound. Without this justification, then there is danger in losing variation and information about specific indicators simply through carelessness.

The results of the analysis seem rather unremarkable at first glance—all the tested models predict growth in very similar ways. However, this serves to set the scene for then analyzing implications thereof. If all the models are mathematically relevant in the same way, why use one over the other? The first answer is, of course, because a particular aggregation or non-aggregation is what is demanded by the established concept structuring. But they also involve different implications in the result phase. If the cohesiveness of the concept structure, measurement, and analysis is well-planned, then these implications are much more valid than if none of this is established.

Substantively, what we can learn from these four models—and especially the disaggregated model—is that perhaps there is a deeper problem with the construct validity of structural reforms. The significance of the structural reform aggregation in each model using an aggregation is consistently the lowest among the independent variables. The aforementioned problems with time certainly contribute to this, but we can also see from the disaggregated model that there is still some explanatory power left within the indicators; it is concentrated mainly in small scale privatization, with large scale privatization and trade and foreign exchange system liberalization being the only other indicators with any significance. All of this is to suggest: What if there is no special meaning or significance to liberalizing structural reforms? What if, as a construct, it is largely irrelevant to economic growth?

If we concede that most of the explanatory power coming from a structural reforms index (or set of indicators) is coming from indicators of privatization, then that is likely not measuring and representative of liberalizing structural reforms. Privatization, being the transfer of ownership from public to private, is almost a pure measurement of capitalism—either level of or possibly commitment to. In this function, large- and small-scale privatization seem to have captured all the necessary effects of structural changes that come along with a transition to a market economy. Taken alongside the increased explanatory power of FDI, a new, simplified model can be determined. An increasingly committed capitalist economy that has a higher level of support from external market actors will lead to more growth. Coupling this with the more obvious indicators of stabilization and size of government, a model can be specified that has higher explanatory power than any of the aggregate models, nearly the same explanatory power of the disaggregated model, and a simpler, more direct message on the determinants of growth. The resultant statistic from this model, which will be referred to as the proposed model, can be seen in Table 4.4.

Table 4.4 Statistical results of the proposed model fixed-effects panel regression

	Dependent Variable Economic Growth
LDV	0..299*** (<0.00000001)
Large-scale Privatization	-1.512*** (0.0049931)
Small-scale Privatization	1.757** (0.0187858)
Inflation Rate	-0.011*** (0.0001080)
Government Expenditures	-0.333*** (0.0001932)
FDI	0.105*** (0.0068127)
Countries	24
Years	18-20 (unbalanced panel)
Observations	470
R2	0.33878
Adjusted R2	0.2952
F statistic	37.573 (df = 6; 440)

Note: *p<0.1; **p<0.05; ***p<0.0; p-values are listed in parentheses below coefficient estimates

Source: See Section 3.2 for the source of each variable; Author's data

Where this explanation breaks down might be in the relationship between large-scale privatization and growth, which is negative. So, not all moves towards a market economy are equally advantageous to growth. Perhaps a more accurate description of this simplified model would be: A considered commitment to a capitalist economy starting at the lowest levels of enterprise, along with higher levels of support from external market actors, macroeconomic stabilization, and controlling the size of the government will lead to more growth.

With this, the special importance of liberalizing reforms is excluded from models of growth. Reforms are only important insofar as they indicate a move towards a market economy, indicating openness to investors (measured through FDI) and done within a context of a stable economy and efficient central government (measured through inflation

rate and government expenditures, respectively). This is a direct blow to the construct validity of structural reforms in growth models of transition economies. While there was certainly reason to examine this aggregation initially, continued study of it indicates that it may only be partially conceptually useful—and the parts that are useful really should be conceptually structured in a way that does not indicate structural reforms as such. Another way to think of this may be as follows: it is not the liberalization that impacts growth, but rather the message sent through the act of liberalizing. For this simple message, privatization indicators are fully capable of capturing the impact.

The cumulative liberalization index and the concept of structural reforms are best left as a product of their time when liberalization was thought to be of the utmost importance and synonymous with movement towards a market economy. In critically examining the validity of such constructs, there is plenty of evidence to indicate that variation and explanation can be lost in aggregation. This suggests that at the very least no aggregative measures can improve on the explanation of the disaggregated measures. Further, by evaluating the results of this study more substantively, the very nature of a structural reforms construct is found to be flawed. If the historical relevance and use of such a construct is disregarded, there are better, simpler ways to explain and characterize growth in transition economies.

5. CONCLUSIONS

By testing four different models of economic growth in transition countries differentiated by the method of aggregation used to represent structural reforms, we can conclude that the precise method does not affect the predictive power to any great degree of significance. However, what this study has enabled us to observe is the connection between concept structuring and the interpretation of results. This is something that may often be overlooked in the use of aggregations and indices—especially those issuing forth from large and trustworthy institutions.

The lack of differentiation of predictive power is a potential mathematical reason to write off the use of other conceptual structures in favor of the one chosen by a researcher. If it does not affect the output of a model, then it may seem as if there is no reason to consider each as better or worse. But we have shown that different conceptual structures can influence the interpretation of results. In some cases, this could be used malevolently to push certain agendas or hide results that might negatively affect a message. In other cases, it can be a simple error on the part of researchers who fail to accurately detail the concept they are examining. In either case, though, the validity of the results is diminished. The development and continued use of the CLI falls firmly into the latter category. Very much a product of its time, those creating the CLI were most probably too wrapped up in the fervor of liberalization to even consider these important—though admittedly, quite densely theoretical—questions of concept structuring and construct validity.

In the case of this study, perhaps the most revealing portion was the examination and results of the disaggregated model. The very inclusion of the disaggregated model for examination points to the author's own intuitive curiosity of the aggregation of structural reforms; that being, there is a very distinct possibility that besides a general, categorical connection, these indicators are all representative of distinct concepts that could have very different effects and significance in the context of an economic growth model. The results of the study confirm that this understanding was accurate as it was not liberalizing structural reforms as an aggregate impacting growth, but rather a smaller subset of certain liberalizing reforms—most notably, small-scale privatization. With no intention of falling into the *a posteriori* justification trap of previous researchers, this is not evidence that

disaggregating the indicators is necessarily a superior conceptualization. It is simply a result that shows different interpretations and vastly different policy applications of the results based on the chosen conceptual structuring and subsequently implied method of measurement. If there are clear conceptual or even statistical reasons to keep these variables aggregated, then the disaggregated results should not be considered. In absence of these reasons, there is evidence to contend that a disaggregated conceptualization and measurement is, at present, the best way to include these policy indicators in models of economic growth in transition countries. An examination more based in economic theory could provide a more definitive answer regarding the most appropriate conceptual structure of structural reforms in transition economies than this practical examination. But this study could serve as the impetus for such future examination.

The passage of time has also not proven kind to the application of measures of liberalizing structural reforms—a measure which is, by its nature, sensitive to time—in any configuration or aggregation. The idea of policy variables in growth models is still quite intriguing though, especially in the context of transition countries. Thus, another avenue for further study would be a broader search for more relevant policy variables that could better explain economic growth over a longer period of time or better explain the dynamic nature of these variables.

Finally, by using the substantive results of this examination, it is possible to propose a new model that does away with the concept of structural reforms entirely. By replacing the concept of liberalizing structural reforms with a simpler idea of commitment to a market economy and using only indicators of privatization, a model with nearly equivalent explanatory power can be determined. This is a result that strikes at the very heart of the construct validity of structural reforms. Going forward, it would make more sense to use different indicators or aggregations to capture the explanatory power of concepts that aggregations such as the CLI were only capturing partially.

In many ways, this study could be characterized as a cautionary tale to researchers and their audiences. It is an esoteric contribution to the idea of data literacy, past the more basic levels of interpreting graphs or communicating results. There are not many who

would question the foundational validity of a widely used, IMF developed index. Even in the preliminary stages of this paper, a comprehensive evaluation of the CLI was not the target or method of examination. It is simply something that is overwhelmingly accepted as valid. By examining it critically, it was possible to determine that some unassuming changes to the concept structure can lead to major differences in the interpretation of results even without changing the underlying data or method of analysis. These interpretations have a further impact on the application of these indicators, aggregations, and models to inform policy.

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APPENDIX A: RAW INPUT DATA

Country	Year	% of real GDP Growth	EBRD Policy Indicators						Inflation rate - GDP deflator	General government final consumption as % GDP	FDI as % GDP	Reconstructed CLI
			Large scale Privatization	Small scale privatization	Enterprise restructuring	Price liberalization	Trade and foreign exchange system	Banking reform				
Albania	1994	8.30	1	3	2	3	4	2	35.84	14.62	2.82	2.9
Albania	1995	13.32	2	4	2	3	4	2	9.97	13.83	2.93	3.1
Albania	1996	9.10	2	4	2	3	4	2	38.17	9.45	2.82	3.1
Albania	1997	-10.92	2	4	2	3	4	2	11.24	10.45	2.10	3.1
Albania	1998	8.83	2	4	2	3	4	2	6.73	10.84	1.77	3.1
Albania	1999	12.89	2	4	2	3	4	2	2.10	11.06	1.28	3.1
Albania	2000	6.95	2	4	2	3	4	2	5.64	9.69	4.11	3.1
Albania	2001	8.29	2	4	2	3	4	2	3.81	10.63	5.29	3.1
Albania	2002	4.54	2	4	2	3	4	2	3.64	11.34	3.10	3.1
Albania	2003	5.53	2	4	2	4	4	2	5.20	11.10	3.17	3.4
Albania	2004	5.51	2	4	2	4	4	3	3.16	11.21	4.75	3.5
Albania	2005	5.53	3	4	2	4	4	3	3.30	11.03	3.26	3.6
Albania	2006	5.90	3	4	2	4	4	3	2.48	10.53	3.65	3.6
Albania	2007	5.98	3	4	2	4	4	3	4.39	10.48	6.11	3.6
Albania	2008	7.50	3	4	2	4	4	3	4.12	10.38	9.68	3.6
Albania	2009	3.35	4	4	2	4	4	3	2.42	11.11	11.17	3.7
Albania	2010	3.71	4	4	2	4	4	3	4.49	11.16	9.14	3.7
Albania	2011	2.55	4	4	2	4	4	3	2.31	10.97	8.14	3.7
Albania	2012	1.42	4	4	2	4	4	3	1.04	10.84	7.45	3.7
Albania	2013	1.00	4	4	2	4	4	3	0.29	11.03	9.82	3.7
Albania	2014	1.77	4	4	2	4	4	3	1.55	11.45	8.69	3.7
Armenia	1994	5.40	1	3	1	3	2	1	4107.30	11.54	0.61	2.1
Armenia	1995	6.90	2	3	2	3	3	2	161.16	11.32	1.72	2.7
Armenia	1996	5.87	3	3	2	3	4	2	19.59	11.34	1.10	3.1
Armenia	1997	3.32	3	3	2	3	4	2	17.74	11.48	3.17	3.1
Armenia	1998	7.30	3	3	2	3	4	2	10.70	11.29	12.27	3.1
Armenia	1999	3.30	3	3	2	3	4	2	0.05	12.23	6.61	3.1
Armenia	2000	5.90	3	3	2	3	4	2	-1.37	10.80	5.45	3.1
Armenia	2001	9.60	3	4	2	3	4	2	4.03	10.34	3.30	3.2
Armenia	2002	13.20	3	4	2	3	4	2	2.36	9.17	4.66	3.2

Armenia	2003	14.00	3	4	2	4	4	2	4.60	9.38	4.38	3.5
Armenia	2004	10.50	3	4	2	4	4	2	6.28	9.36	6.91	3.5
Armenia	2005	13.90	4	4	2	4	4	3	3.21	9.71	5.96	3.7
Armenia	2006	13.20	4	4	2	4	4	3	4.62	9.27	7.31	3.7
Armenia	2007	13.70	4	4	2	4	4	3	4.28	9.40	7.25	3.7
Armenia	2008	6.90	4	4	2	4	4	3	5.99	9.40	8.09	3.7
Armenia	2009	-14.10	4	4	2	4	4	3	2.50	12.28	8.79	3.7
Armenia	2010	2.20	4	4	2	4	4	3	7.77	12.04	5.72	3.7
Armenia	2011	4.70	4	4	2	4	4	2	4.28	11.91	6.44	3.6
Armenia	2012	7.20	4	4	2	4	4	2	5.35	10.91	4.68	3.6
Armenia	2013	3.30	4	4	2	4	4	2	3.37	11.94	3.11	3.6
Armenia	2014	3.60	4	4	2	4	4	2	2.31	12.09	3.50	3.6
Azerbaijan	1994	-19.70	1	1	1	3	1	1	1386.07	23.51	0.66	1.6
Azerbaijan	1995	-11.80	1	1	2	3	2	2	545.66	12.79	10.81	2.1
Azerbaijan	1996	1.30	1	2	2	3	2	2	26.42	12.02	19.75	2.2
Azerbaijan	1997	5.80	2	3	2	3	2	2	9.24	12.61	28.13	2.4
Azerbaijan	1998	10.00	2	3	2	3	3	2	-0.97	15.16	23.01	2.7
Azerbaijan	1999	7.40	2	3	2	3	3	2	2.16	15.61	11.14	2.7
Azerbaijan	2000	11.10	2	3	2	3	3	2	12.49	15.15	2.46	2.7
Azerbaijan	2001	9.90	2	3	2	3	3	2	2.52	13.65	14.36	2.7
Azerbaijan	2002	9.44	2	4	2	3	4	2	4.21	12.40	32.47	3.1
Azerbaijan	2003	10.21	2	4	2	4	4	2	6.96	12.39	55.08	3.4
Azerbaijan	2004	9.25	2	4	2	4	4	2	9.25	12.90	54.37	3.4
Azerbaijan	2005	27.96	2	4	2	4	4	2	14.72	10.42	33.80	3.4
Azerbaijan	2006	34.47	2	4	2	4	4	2	11.33	8.54	21.38	3.4
Azerbaijan	2007	25.46	2	4	2	4	4	2	20.58	9.66	13.90	3.4
Azerbaijan	2008	10.59	2	4	2	4	4	2	27.97	8.50	8.16	3.4
Azerbaijan	2009	9.37	2	4	2	4	4	2	-18.90	11.12	6.55	3.4
Azerbaijan	2010	4.79	2	4	2	4	4	2	13.83	10.88	6.34	3.4
Azerbaijan	2011	-1.57	2	4	2	4	4	2	24.61	10.13	6.80	3.4
Azerbaijan	2012	2.20	2	4	2	4	4	2	2.84	10.53	7.60	3.4
Azerbaijan	2013	5.84	2	4	2	4	4	2	0.41	10.28	3.53	3.4
Azerbaijan	2014	2.80	2	4	2	4	4	2	-1.33	10.89	5.89	3.4
Belarus	1994	-11.70	2	2	2	2	1	1	1945.75	20.51	0.07	1.6
Belarus	1995	-10.40	2	2	2	3	2	2	661.50	20.55	0.11	2.3
Belarus	1996	2.80	1	2	2	3	2	1	53.71	20.55	0.71	2.1
Belarus	1997	11.40	1	2	1	3	1	1	71.65	20.32	2.49	1.7
Belarus	1998	8.40	1	2	1	2	1	1	76.58	19.86	1.33	1.4
Belarus	1999	3.40	1	2	1	2	1	1	316.79	19.50	3.66	1.4
Belarus	2000	5.80	1	2	1	2	2	1	185.29	19.48	0.93	1.7
Belarus	2001	4.73	1	2	1	2	2	1	79.53	21.55	0.78	1.7
Belarus	2002	5.05	1	2	1	2	2	2	44.89	21.03	1.69	1.8
Belarus	2003	7.04	1	2	1	3	2	2	30.69	21.38	0.96	2.1
Belarus	2004	11.45	1	2	1	3	2	2	22.68	20.60	0.71	2.1

Belarus	2005	9.40	1	2	1	3	2	2	18.97	20.79	1.01	2.1
Belarus	2006	10.00	1	2	1	3	2	2	10.75	19.21	0.97	2.1
Belarus	2007	8.60	1	2	1	3	2	2	12.87	18.52	3.99	2.1
Belarus	2008	10.20	2	2	2	3	2	2	21.21	16.52	3.60	2.3
Belarus	2009	0.20	2	2	2	3	2	2	9.26	16.10	3.69	2.3
Belarus	2010	7.80	2	2	2	3	2	2	11.29	16.02	2.44	2.3
Belarus	2011	5.38	2	2	2	3	2	2	71.04	13.45	6.48	2.3
Belarus	2012	1.69	2	2	2	3	2	2	75.28	13.57	2.23	2.3
Belarus	2013	1.00	2	2	2	3	2	2	21.26	13.43	2.97	2.3
Belarus	2014	1.73	2	2	2	3	2	2	18.11	13.75	2.36	2.3
Bulgaria	1994	1.82	2	2	2	3	4	2	72.70	17.19	1.09	2.9
Bulgaria	1995	2.86	2	3	2	3	4	2	135.97	15.35	0.48	3
Bulgaria	1996	5.14	2	3	2	2	4	2	63.07	13.85	0.89	2.7
Bulgaria	1997	-14.19	3	3	2	3	4	3	914.13	12.14	4.46	3.2
Bulgaria	1998	4.29	3	3	2	3	4	3	33.31	18.35	3.57	3.2
Bulgaria	1999	-8.28	3	3	2	3	4	3	3.19	19.61	6.01	3.2
Bulgaria	2000	4.78	4	4	2	3	4	3	7.18	19.79	7.56	3.4
Bulgaria	2001	3.82	4	4	2	3	4	3	6.12	19.37	5.73	3.4
Bulgaria	2002	5.95	4	4	2	3	4	3	3.77	19.18	5.52	3.4
Bulgaria	2003	5.15	4	4	3	4	4	3	2.27	20.11	9.92	3.8
Bulgaria	2004	6.44	4	4	3	4	4	4	5.65	19.36	11.75	3.9
Bulgaria	2005	7.15	4	4	3	4	4	4	6.50	18.15	13.72	3.9
Bulgaria	2006	6.80	4	4	3	4	4	4	6.76	18.15	22.90	3.9
Bulgaria	2007	6.56	4	4	3	4	4	4	11.08	16.70	31.25	3.9
Bulgaria	2008	6.09	4	4	3	4	4	4	8.13	17.16	18.91	3.9
Bulgaria	2009	-3.37	4	4	3	4	4	4	4.05	16.74	7.49	3.9
Bulgaria	2010	0.56	4	4	3	4	4	4	1.15	16.60	3.66	3.9
Bulgaria	2011	2.35	4	4	3	4	4	3	5.94	15.93	3.67	3.8
Bulgaria	2012	0.36	4	4	3	4	4	3	1.53	15.87	3.31	3.8
Bulgaria	2013	0.33	4	4	3	4	4	3	-0.67	17.14	3.58	3.8
Bulgaria	2014	1.89	4	4	3	4	4	3	0.46	16.89	1.92	3.8
Croatia	1994		3	4	2	3	4	3				3.3
Croatia	1995		3	4	2	3	4	3		24.91	0.48	3.3
Croatia	1996	5.91	3	4	3	3	4	3	3.81	22.44	2.07	3.4
Croatia	1997	6.20	3	4	3	3	4	3	6.85	21.72	2.50	3.4
Croatia	1998	2.29	3	4	3	3	4	3	8.14	22.68	3.93	3.4
Croatia	1999	-0.95	3	4	3	3	4	3	3.54	23.55	6.20	3.4
Croatia	2000	2.86	3	4	3	3	4	3	4.36	21.55	4.69	3.4
Croatia	2001	2.96	3	4	3	3	4	3	4.31	19.43	4.50	3.4
Croatia	2002	5.68	3	4	3	3	4	4	3.86	19.12	3.66	3.5
Croatia	2003	5.64	3	4	3	4	4	4	4.30	18.55	5.32	3.8
Croatia	2004	4.15	3	4	3	4	4	4	3.61	18.60	3.16	3.8
Croatia	2005	4.31	3	4	3	4	4	4	3.13	18.49	4.00	3.8
Croatia	2006	4.99	3	4	3	4	4	4	3.85	18.44	6.64	3.8

Croatia	2007	5.07	3	4	3	4	4	4	4.20	19.24	7.73	3.8
Croatia	2008	1.89	3	4	3	4	4	4	5.56	18.81	7.47	3.8
Croatia	2009	-7.32	3	4	3	4	4	4	2.97	20.65	4.90	3.8
Croatia	2010	-1.32	3	4	3	4	4	4	0.92	20.61	2.58	3.8
Croatia	2011	-0.20	3	4	3	4	4	3	1.65	20.74	2.00	3.7
Croatia	2012	-2.39	3	4	3	4	4	3	1.48	20.74	2.59	3.7
Croatia	2013	-0.45	4	4	3	4	4	3	0.75	20.52	1.61	3.8
Croatia	2014	-0.34	4	4	3	4	4	3	0.14	20.92	5.52	3.8
Estonia	1994	-1.64	3	4	3	3	4	3	39.67	23.30	5.31	3.4
Estonia	1995	4.53	4	4	3	3	4	3	31.46	24.60	4.60	3.5
Estonia	1996	4.94	4	4	3	3	4	3	22.54	22.64	3.47	3.5
Estonia	1997	13.05	4	4	3	3	4	3	9.92	20.43	5.32	3.5
Estonia	1998	4.33	4	4	3	3	4	3	6.89	20.73	10.51	3.5
Estonia	1999	-0.40	4	4	3	3	4	4	6.69	22.12	5.66	3.6
Estonia	2000	10.11	4	4	3	3	4	4	3.71	19.75	7.31	3.6
Estonia	2001	5.98	4	4	3	3	4	4	6.76	18.80	9.47	3.6
Estonia	2002	6.80	4	4	3	3	4	4	4.81	18.30	4.59	3.6
Estonia	2003	7.57	4	4	3	4	4	4	3.90	17.98	10.51	3.9
Estonia	2004	6.77	4	4	3	4	4	4	4.67	17.50	8.95	3.9
Estonia	2005	9.49	4	4	4	4	4	4	5.91	16.96	21.69	4
Estonia	2006	9.72	4	4	4	4	4	4	9.02	15.99	10.33	4
Estonia	2007	7.57	4	4	4	4	4	4	12.42	16.09	13.51	4
Estonia	2008	-5.09	4	4	4	4	4	4	6.90	18.69	8.11	4
Estonia	2009	-14.43	4	4	4	4	4	4	-0.18	21.11	9.45	4
Estonia	2010	2.69	4	4	4	4	4	4	1.83	20.07	13.16	4
Estonia	2011	7.44	4	4	4	4	4	4	5.39	18.74	4.78	4
Estonia	2012	3.12	4	4	4	4	4	4	4.02	18.52	7.71	4
Estonia	2013	1.35	4	4	4	4	4	4	4.04	18.99	4.34	4
Estonia	2014	2.99	4	4	4	4	4	4	2.95	19.09	6.65	4
Georgia	1994	-10.40	1	2	1	2	1	1	6041.59	9.83	0.32	1.4
Georgia	1995	2.60	2	3	2	3	2	2	162.73	10.91		2.4
Georgia	1996	11.20	3	4	2	3	3	2	43.03	7.70		2.9
Georgia	1997	10.52	3	4	2	3	4	2	6.54	10.17	6.91	3.2
Georgia	1998	3.10	3	4	2	3	4	2	6.94	11.08	7.34	3.2
Georgia	1999	2.87	3	4	2	3	4	2	9.73	10.63	2.94	3.2
Georgia	2000	1.84	3	4	2	3	4	2	4.68	8.54	4.30	3.2
Georgia	2001	4.81	3	4	2	3	4	2	5.38	9.64	3.41	3.2
Georgia	2002	5.47	3	4	2	3	4	2	5.92	9.79	4.72	3.2
Georgia	2003	11.06	3	4	2	4	4	2	3.42	8.12	8.39	3.5
Georgia	2004	5.79	3	4	2	4	4	3	8.43	11.43	9.61	3.6
Georgia	2005	9.59	4	4	2	4	4	3	7.94	14.38	7.07	3.7
Georgia	2006	9.42	4	4	2	4	4	3	8.45	12.01	15.12	3.7
Georgia	2007	12.58	4	4	2	4	4	3	9.46	16.50	18.60	3.7
Georgia	2008	2.42	4	4	2	4	4	3	9.60	19.40	12.52	3.7

Georgia	2009	-3.65	4	4	2	4	4	3	-2.14	18.52	6.14	3.7
Georgia	2010	6.25	4	4	2	4	4	3	14.19	15.27	7.52	3.7
Georgia	2011	7.40	4	4	2	4	4	3	8.71	13.73	7.75	3.7
Georgia	2012	6.37	4	4	2	4	4	3	0.46	13.62	5.87	3.7
Georgia	2013	3.62	4	4	2	4	4	3	1.35	14.02	6.09	3.7
Georgia	2014	4.43	4	4	2	4	4	3	4.23	14.27	10.42	3.7
Hungary	1994	2.95	3	4	3	3	4	3	19.49	25.47	2.65	3.4
Hungary	1995	1.49	4	4	3	3	4	3	26.73	22.87	10.35	3.5
Hungary	1996	0.08	4	4	3	3	4	3	21.93	21.63	7.05	3.5
Hungary	1997	3.14	4	4	3	3	4	4	20.26	21.26	8.80	3.6
Hungary	1998	3.90	4	4	3	3	4	4	13.77	20.92	6.70	3.6
Hungary	1999	3.07	4	4	3	3	4	4	8.12	21.26	6.90	3.6
Hungary	2000	4.48	4	4	3	3	4	4	9.58	21.20	5.82	3.6
Hungary	2001	4.07	4	4	3	3	4	4	11.05	21.08	7.55	3.6
Hungary	2002	4.74	4	4	3	3	4	4	8.09	21.86	5.39	3.6
Hungary	2003	4.08	4	4	3	4	4	4	5.44	23.01	4.87	3.9
Hungary	2004	4.82	4	4	3	4	4	4	5.09	22.06	4.37	3.9
Hungary	2005	4.24	4	4	4	4	4	4	2.63	22.17	24.33	4
Hungary	2006	4.03	4	4	4	4	4	4	3.66	22.02	16.16	4
Hungary	2007	0.24	4	4	4	4	4	4	5.44	20.81	50.46	4
Hungary	2008	1.06	4	4	4	4	4	4	4.79	21.40	47.50	4
Hungary	2009	-6.70	4	4	4	4	4	4	4.19	22.07	-2.14	4
Hungary	2010	1.12	4	4	4	4	4	4	2.53	21.44	-15.75	4
Hungary	2011	1.94	4	4	4	4	4	3	1.93	20.62	7.58	3.9
Hungary	2012	-1.38	4	4	4	4	4	3	2.89	20.01	8.42	3.9
Hungary	2013	1.86	4	4	4	4	4	3	2.83	19.76	-2.65	3.9
Hungary	2014	4.23	4	4	4	4	4	3	3.71	20.03	9.28	3.9
Kazakstan	1994	-12.60	2	2	1	2	2	1	1546.73	10.68	3.10	1.8
Kazakstan	1995	-8.20	2	2	1	3	3	2	160.89	13.58	4.73	2.5
Kazakstan	1996	0.50	3	3	2	3	4	2	38.90	12.91	5.41	3.1
Kazakstan	1997	1.70	3	3	2	3	4	2	16.14	12.38	5.96	3.1
Kazakstan	1998	-1.90	3	4	2	3	4	2	5.66	10.78	5.20	3.2
Kazakstan	1999	2.70	3	4	2	3	3	2	13.28	11.54	9.41	2.9
Kazakstan	2000	9.80	3	4	2	3	3	2	17.43	12.08	7.49	2.9
Kazakstan	2001	13.50	3	4	2	3	3	3	10.16	13.41	12.72	3
Kazakstan	2002	9.80	3	4	2	3	3	3	5.80	11.61	10.51	3
Kazakstan	2003	9.30	3	4	2	4	3	3	11.74	11.26	8.05	3.3
Kazakstan	2004	9.60	3	4	2	4	3	3	16.13	11.61	13.01	3.3
Kazakstan	2005	9.70	3	4	2	4	3	3	17.87	11.25	4.46	3.3
Kazakstan	2006	10.70	3	4	2	4	4	3	21.55	10.18	9.40	3.6
Kazakstan	2007	8.90	3	4	2	4	4	3	15.53	11.05	11.42	3.6
Kazakstan	2008	3.30	3	4	2	4	4	3	20.94	10.19	12.60	3.6
Kazakstan	2009	1.20	3	4	2	4	4	3	4.69	11.66	12.38	3.6
Kazakstan	2010	7.30	3	4	2	4	4	3	19.54	10.81	5.04	3.6

Kazakstan	2011	7.40	3	4	2	4	4	3	20.54	10.48	7.14	3.6
Kazakstan	2012	4.80	3	4	2	4	4	3	4.79	11.52	6.56	3.6
Kazakstan	2013	6.00	3	4	2	4	4	3	9.50	10.17	4.23	3.6
Kazakstan	2014	4.20	3	4	2	4	4	2	5.77	10.69	3.30	3.5
Krgyz Republic	1994	-20.09	3	4	2	3	3	2	180.87	18.90	2.27	2.9
Krgyz Republic	1995	-5.42	4	4	2	3	4	2	42.03	19.54	5.79	3.3
Krgyz Republic	1996	7.08	3	4	2	3	4	2	35.34	18.52	2.58	3.2
Krgyz Republic	1997	9.92	3	4	2	3	4	3	19.31	17.30	4.74	3.3
Krgyz Republic	1998	2.12	3	4	2	3	4	3	9.08	17.85	6.64	3.3
Krgyz Republic	1999	3.66	3	4	2	3	4	2	37.57	19.12	3.56	3.2
Krgyz Republic	2000	5.44	3	4	2	3	4	2	27.16	20.04	-0.17	3.2
Krgyz Republic	2001	5.32	3	4	2	3	4	2	7.33	17.48	0.33	3.2
Krgyz Republic	2002	-0.02	3	4	2	3	4	2	2.03	18.62	0.29	3.2
Krgyz Republic	2003	7.03	3	4	2	4	4	2	3.97	16.83	2.37	3.5
Krgyz Republic	2004	7.03	4	4	2	4	4	2	5.11	18.17	7.93	3.6
Krgyz Republic	2005	-0.18	4	4	2	4	4	2	7.13	17.51	1.73	3.6
Krgyz Republic	2006	3.10	4	4	2	4	4	2	9.39	17.99	6.42	3.6
Krgyz Republic	2007	8.54	4	4	2	4	4	2	14.88	17.10	5.47	3.6
Krgyz Republic	2008	8.40	4	4	2	4	4	2	22.22	17.52	7.33	3.6
Krgyz Republic	2009	2.89	4	4	2	4	4	2	4.04	18.43	4.04	3.6
Krgyz Republic	2010	-0.47	4	4	2	4	4	2	10.03	18.13	9.86	3.6
Krgyz Republic	2011	5.96	4	4	2	4	4	2	22.48	18.23	11.06	3.6
Krgyz Republic	2012	-0.09	4	4	2	4	4	2	8.66	20.11	3.95	3.6
Krgyz Republic	2013	10.92	4	4	2	4	4	2	3.18	18.45	8.34	3.6
Krgyz Republic	2014	4.02	4	4	2	4	4	2	8.42	17.47	4.59	3.6
Latvia	1994		2	3	2	3	4	3				3.1
Latvia	1995		2	4	2	3	4	3		23.56	4.22	3.2
Latvia	1996	2.57	3	4	3	3	4	3	12.40	21.97	5.55	3.4
Latvia	1997	8.86	3	4	3	3	4	3	5.87	21.04	7.27	3.4
Latvia	1998	6.34	3	4	3	3	4	3	4.82	22.65	5.19	3.4
Latvia	1999	2.74	3	4	3	3	4	3	1.49	22.57	4.63	3.4
Latvia	2000	5.67	3	4	3	3	4	3	3.61	20.91	4.08	3.4
Latvia	2001	6.32	3	4	3	3	4	3	2.29	20.57	2.08	3.4
Latvia	2002	7.08	3	4	3	3	4	4	5.09	20.65	1.68	3.5
Latvia	2003	8.43	3	4	3	4	4	4	4.94	21.14	2.70	3.8
Latvia	2004	8.50	4	4	3	4	4	4	6.80	19.86	4.11	3.9
Latvia	2005	10.73	4	4	3	4	4	4	11.17	17.73	4.78	3.9
Latvia	2006	11.99	4	4	3	4	4	4	12.44	17.11	7.93	3.9
Latvia	2007	10.03	4	4	3	4	4	4	20.07	17.49	8.75	3.9
Latvia	2008	-3.33	4	4	3	4	4	4	11.62	19.60	4.01	3.9
Latvia	2009	-14.26	4	4	3	4	4	4	-9.73	18.90	-0.57	3.9
Latvia	2010	-4.41	4	4	3	4	4	4	-0.42	18.27	1.99	3.9
Latvia	2011	6.47	4	4	3	4	4	3	6.37	18.07	5.31	3.8
Latvia	2012	4.25	4	4	3	4	4	3	3.60	17.27	3.81	3.8

Latvia	2013	2.31	4	4	3	4	4	3	1.64	17.53	3.25	3.8
Latvia	2014	1.07	4	4	3	4	4	3	1.92	17.78	3.34	3.8
Lithuania	1994		3	4	2	3	4	2				3.2
Lithuania	1995		3	4	2	3	4	3		24.00	0.92	3.3
Lithuania	1996	5.16	3	4	3	3	4	3	18.99	24.08	1.82	3.4
Lithuania	1997	8.31	3	4	3	3	4	3	11.45	24.16	3.58	3.4
Lithuania	1998	7.48	3	4	3	3	4	3	3.35	25.88	8.17	3.4
Lithuania	1999	-1.14	3	4	3	3	4	3	-1.26	23.67	5.15	3.4
Lithuania	2000	3.70	3	4	3	3	4	3	1.30	22.43	3.30	3.4
Lithuania	2001	6.53	3	4	3	3	4	3	-0.32	21.17	3.62	3.4
Lithuania	2002	6.75	4	4	3	3	4	3	0.32	20.59	4.63	3.5
Lithuania	2003	10.57	4	4	3	4	4	3	-0.81	19.57	1.16	3.8
Lithuania	2004	6.57	4	4	3	4	4	3	2.68	19.19	3.89	3.8
Lithuania	2005	7.73	4	4	3	4	4	4	6.89	18.52	4.95	3.9
Lithuania	2006	7.41	4	4	3	4	4	4	6.74	19.15	7.49	3.9
Lithuania	2007	11.11	4	4	3	4	4	4	8.55	17.47	6.55	3.9
Lithuania	2008	2.61	4	4	3	4	4	4	9.71	18.68	3.61	3.9
Lithuania	2009	-14.84	4	4	3	4	4	4	-3.30	21.27	-0.96	3.9
Lithuania	2010	1.65	4	4	3	4	4	4	2.53	19.84	2.97	3.9
Lithuania	2011	6.04	4	4	3	4	4	3	5.35	18.32	4.32	3.8
Lithuania	2012	3.84	4	4	3	4	4	3	2.73	17.45	1.58	3.8
Lithuania	2013	3.55	4	4	3	4	4	3	1.28	16.66	1.65	3.8
Lithuania	2014	3.54	4	4	3	4	4	3	0.83	16.61	0.74	3.8
Moldova	1994		2	2	2	3	2	2				2.3
Moldova	1995		3	3	2	3	4	2		25.90	3.82	3.1
Moldova	1996	-5.88	3	3	2	3	4	2	27.85	25.97	1.40	3.1
Moldova	1997	1.65	3	3	2	3	4	2	12.50	28.81	4.08	3.1
Moldova	1998	-6.54	3	3	2	3	4	2	9.46	24.71	4.45	3.1
Moldova	1999	-3.37	3	3	2	3	4	2	39.78	15.32	3.23	3.1
Moldova	2000	2.11	3	3	2	3	4	2	27.33	14.66	9.90	3.1
Moldova	2001	6.10	3	3	2	3	4	2	12.09	14.36	6.99	3.1
Moldova	2002	7.80	3	3	2	3	4	2	9.83	20.23	5.06	3.1
Moldova	2003	6.60	3	3	2	4	4	2	14.87	19.68	3.72	3.4
Moldova	2004	7.40	3	3	2	4	4	3	7.99	14.90	5.81	3.5
Moldova	2005	7.50	3	3	2	4	4	3	9.34	16.44	6.38	3.5
Moldova	2006	4.80	3	4	2	4	4	3	13.42	19.99	7.59	3.6
Moldova	2007	3.00	3	3	2	4	4	3	15.91	19.95	12.18	3.5
Moldova	2008	7.80	3	4	2	4	4	3	9.24	20.41	12.00	3.6
Moldova	2009	-6.00	3	4	2	4	4	3	2.17	23.76	4.78	3.6
Moldova	2010	7.10	3	4	2	4	4	3	33.30	18.21	4.16	3.6
Moldova	2011	5.82	3	4	2	4	4	2	8.19	16.56	4.27	3.5
Moldova	2012	-0.59	3	4	2	4	4	2	7.42	16.69	2.88	3.5
Moldova	2013	9.04	3	4	2	4	4	2	3.92	15.31	2.55	3.5
Moldova	2014	5.00	3	4	2	4	4	2	6.35	14.37	3.63	3.5

North Macedonia	1994	-1.76	2	4	2	3	4	2	151.89	18.10	0.67	3.1
North Macedonia	1995	-1.11	2	4	2	3	4	3	17.09	17.66	0.20	3.2
North Macedonia	1996	1.19	3	4	2	3	4	3	2.86	17.23	0.24	3.3
North Macedonia	1997	1.44	3	4	2	3	4	3	3.93	18.76	0.40	3.3
North Macedonia	1998	3.38	3	4	2	3	4	3	1.39	19.26	4.02	3.3
North Macedonia	1999	4.34	3	4	2	3	4	3	2.74	19.56	2.36	3.3
North Macedonia	2000	4.55	3	4	2	3	4	3	8.18	16.93	5.77	3.3
North Macedonia	2001	-3.07	3	4	2	3	4	3	4.72	23.83	12.66	3.3
North Macedonia	2002	1.49	3	4	2	3	4	3	0.94	20.75	2.84	3.3
North Macedonia	2003	2.22	3	4	2	4	4	3	1.65	18.08	2.41	3.6
North Macedonia	2004	4.67	3	4	2	4	4	3	-0.17	17.41	5.44	3.6
North Macedonia	2005	4.72	3	4	2	4	4	3	4.90	16.18	2.32	3.6
North Macedonia	2006	5.14	3	4	3	4	4	3	3.25	16.51	6.23	3.7
North Macedonia	2007	6.47	3	4	3	4	4	3	4.59	16.80	8.80	3.7
North Macedonia	2008	5.47	3	4	3	4	4	3	5.49	18.58	6.17	3.7
North Macedonia	2009	-0.36	3	4	3	4	4	3	0.30	19.08	2.76	3.7
North Macedonia	2010	3.36	3	4	3	4	4	3	2.04	18.29	3.20	3.7
North Macedonia	2011	2.34	3	4	3	4	4	3	3.72	18.12	4.84	3.7
North Macedonia	2012	-0.46	3	4	3	4	4	3	1.00	18.65	3.47	3.7
North Macedonia	2013	2.93	3	4	3	4	4	3	4.48	17.50	3.72	3.7
North Macedonia	2014	3.63	3	4	3	4	4	3	1.45	17.11	0.54	3.7
Poland	1994	5.29	3	4	3	3	4	3	37.23		1.69	3.4
Poland	1995	7.10	3	4	3	3	4	3	27.94	18.89	2.57	3.4
Poland	1996	6.12	3	4	3	3	4	3	17.95	18.51	2.81	3.4
Poland	1997	6.45	3	4	3	3	4	3	13.67	18.09	3.08	3.4
Poland	1998	4.64	3	4	3	3	4	3	11.02	17.70	3.65	3.4
Poland	1999	4.65	3	4	3	3	4	3	6.16	17.88	4.36	3.4
Poland	2000	4.56	3	4	3	3	4	3	6.12	18.07	5.42	3.4
Poland	2001	1.26	3	4	3	3	4	3	3.12	18.66	2.97	3.4
Poland	2002	2.04	3	4	3	3	4	3	1.85	18.64	2.06	3.4
Poland	2003	3.50	3	4	3	4	4	3	0.78	18.92	2.47	3.7
Poland	2004	4.98	3	4	3	4	4	3	4.92	18.32	5.44	3.7
Poland	2005	3.51	3	4	4	4	4	4	2.56	18.33	3.61	3.9

Poland	2006	6.13	3	4	4	4	4	4	1.73	18.54	6.23	3.9
Poland	2007	7.06	3	4	4	4	4	4	3.72	18.19	5.83	3.9
Poland	2008	4.20	3	4	4	4	4	4	3.89	18.68	2.73	3.9
Poland	2009	2.83	3	4	4	4	4	4	3.79	18.75	3.19	3.9
Poland	2010	3.74	4	4	4	4	4	4	1.65	19.18	3.83	4
Poland	2011	4.76	4	4	4	4	4	3	3.27	18.14	3.51	3.9
Poland	2012	1.32	4	4	4	4	4	4	2.36	18.08	1.48	4
Poland	2013	1.13	4	4	4	4	4	4	0.30	18.33	0.15	4
Poland	2014	3.38	4	4	4	4	4	4	0.52	18.31	3.65	4
Romania	1994	3.93	2	3	2	3	4	2	139.02	13.77	1.13	3
Romania	1995	6.23	2	3	2	3	4	3	43.93	12.25	1.12	3.1
Romania	1996	3.91	3	3	2	3	3	3	44.00	11.67	0.71	2.9
Romania	1997	-4.85	3	3	2	3	4	3	135.34	12.69	3.42	3.2
Romania	1998	-2.03	3	3	2	3	4	2	48.13	11.89	4.87	3.1
Romania	1999	-0.38	3	4	2	3	4	3	49.52	16.72	2.90	3.3
Romania	2000	2.46	3	4	2	3	4	3	43.18	16.86	2.78	3.3
Romania	2001	5.22	3	4	2	3	4	3	37.96	15.80	2.86	3.3
Romania	2002	5.70	3	4	2	3	4	3	22.71	14.82	2.48	3.3
Romania	2003	2.34	3	4	2	4	4	3	23.15	16.04	3.19	3.6
Romania	2004	10.43	4	4	2	4	4	3	15.46	14.73	8.59	3.7
Romania	2005	4.67	4	4	2	4	4	3	12.01	16.24	6.60	3.7
Romania	2006	8.03	4	4	3	4	4	3	10.61	15.92	9.02	3.8
Romania	2007	7.23	4	4	3	4	4	3	15.82	15.39	5.79	3.8
Romania	2008	9.31	4	4	3	4	4	3	16.02	15.88	6.38	3.8
Romania	2009	-5.52	4	4	3	4	4	3	4.09	16.10	2.66	3.8
Romania	2010	-3.90	4	4	3	4	4	3	3.59	15.47	1.93	3.8
Romania	2011	1.91	4	4	3	4	4	3	3.77	14.27	1.29	3.8
Romania	2012	2.04	4	4	3	4	4	3	3.77	14.57	1.79	3.8
Romania	2013	3.77	4	4	3	4	4	3	3.40	14.29	2.02	3.8
Romania	2014	3.61	4	4	3	4	4	3	1.80	14.50	1.93	3.8
Russian Federation	1994	-12.57	3	3	2	3	3	2	307.30	19.10	0.17	2.8
Russian Federation	1995	-4.14	3	4	2	3	3	2	144.01	19.08	0.52	2.9
Russian Federation	1996	-3.76	3	4	2	3	4	2	46.04	19.49	0.66	3.2
Russian Federation	1997	1.40	3	4	2	3	4	2	15.06	21.07	1.20	3.2
Russian Federation	1998	-5.30	3	4	2	3	2	2	18.54	18.73	1.02	2.6
Russian Federation	1999	6.40	3	4	2	3	2	2	72.39	14.58	1.66	2.6
Russian Federation	2000	10.00	3	4	2	3	2	2	37.70	15.09	1.03	2.6
Russian Federation	2001	5.10	3	4	2	3	3	2	16.48	16.44	0.93	2.9
Russian Federation	2002	4.70	3	4	2	3	3	2	15.66	17.93	1.01	2.9

Russian Federation	2003	7.30	3	4	2	4	3	2	13.66	17.92	1.84	3.2
Russian Federation	2004	7.20	3	4	2	4	3	2	20.26	16.97	2.61	3.2
Russian Federation	2005	6.40	3	4	2	4	3	2	19.28	16.87	2.03	3.2
Russian Federation	2006	8.20	3	4	2	4	3	3	15.12	17.39	3.80	3.3
Russian Federation	2007	8.50	3	4	2	4	3	3	13.84	17.30	4.30	3.3
Russian Federation	2008	5.20	3	4	2	4	3	3	18.01	17.83	4.50	3.3
Russian Federation	2009	-7.80	3	4	2	4	3	3	1.97	20.79	2.99	3.3
Russian Federation	2010	4.50	3	4	2	4	3	3	14.19	18.73	2.83	3.3
Russian Federation	2011	4.30	3	4	2	4	3	3	24.46	17.63	2.69	3.3
Russian Federation	2012	4.02	3	4	2	4	4	3	8.91	17.97	2.29	3.6
Russian Federation	2013	1.76	3	4	2	4	4	3	5.32	18.68	3.02	3.6
Russian Federation	2014	0.74	3	4	2	4	4	3	7.49	18.03	1.07	3.6
Slovak Republic	1994	6.21	3	4	3	3	4	3	13.45	24.03	1.34	3.4
Slovak Republic	1995	5.84	3	4	3	3	4	3	9.89	23.79	0.91	3.4
Slovak Republic	1996	6.62	3	4	3	3	4	3	4.54	25.30	1.26	3.4
Slovak Republic	1997	5.93	4	4	3	3	4	3	4.79	22.99	0.64	3.5
Slovak Republic	1998	4.08	4	4	3	3	4	3	4.85	23.19	2.17	3.5
Slovak Republic	1999	-0.11	4	4	3	3	4	3	7.22	21.08	1.11	3.5
Slovak Republic	2000	1.17	4	4	3	3	4	3	9.49	20.74	7.48	3.5
Slovak Republic	2001	3.25	4	4	3	3	4	3	5.12	20.89	4.98	3.5
Slovak Republic	2002	4.51	4	4	3	3	4	3	3.94	20.38	11.99	3.5
Slovak Republic	2003	5.50	4	4	3	4	4	3	5.33	20.71	2.07	3.8
Slovak Republic	2004	5.28	4	4	3	4	4	4	5.74	18.83	7.09	3.9
Slovak Republic	2005	6.62	4	4	4	4	4	4	2.54	18.48	6.25	4
Slovak Republic	2006	8.49	4	4	4	4	4	4	2.90	18.77	8.06	4
Slovak Republic	2007	10.83	4	4	4	4	4	4	1.12	17.14	5.85	4
Slovak Republic	2008	5.57	4	4	4	4	4	4	2.86	17.53	4.62	4
Slovak Republic	2009	-5.46	4	4	4	4	4	4	-1.16	19.99	1.71	4
Slovak Republic	2010	5.87	4	4	4	4	4	4	0.49	19.36	2.34	4
Slovak Republic	2011	2.85	4	4	4	4	4	4	1.67	18.44	5.48	4
Slovak Republic	2012	1.90	4	4	4	4	4	4	1.26	17.84	1.88	4
Slovak Republic	2013	0.67	4	4	4	4	4	4	0.52	18.09	1.02	4
Slovak Republic	2014	2.64	4	4	4	4	4	4	-0.19	18.38	-0.36	4
Slovenia	1994	5.33	2	4	3	3	4	3	22.59	18.98	0.78	3.3
Slovenia	1995	4.11	3	4	3	3	4	3	25.40	17.82	0.70	3.4
Slovenia	1996	3.20	3	4	3	3	4	3	11.45	17.72	0.81	3.4
Slovenia	1997	5.05	3	4	3	3	4	3	8.43	17.65	1.61	3.4
Slovenia	1998	3.28	3	4	3	3	4	3	7.43	17.64	0.97	3.4

Slovenia	1999	5.33	3	4	3	3	4	3	6.53	17.80	0.47	3.4
Slovenia	2000	3.67	3	4	3	3	4	3	5.57	18.46	0.67	3.4
Slovenia	2001	3.22	3	4	3	3	4	3	8.68	18.83	2.40	3.4
Slovenia	2002	3.51	3	4	3	3	4	3	7.58	18.66	7.87	3.4
Slovenia	2003	2.96	3	4	3	4	4	3	5.64	18.77	1.81	3.7
Slovenia	2004	4.36	3	4	3	4	4	3	3.36	18.83	2.22	3.7
Slovenia	2005	3.80	3	4	3	4	4	3	1.52	18.91	2.68	3.7
Slovenia	2006	5.75	3	4	3	4	4	3	2.22	18.65	1.75	3.7
Slovenia	2007	6.98	3	4	3	4	4	3	4.18	17.45	3.93	3.7
Slovenia	2008	3.51	3	4	3	4	4	3	4.47	18.16	1.95	3.7
Slovenia	2009	-7.55	3	4	3	4	4	3	3.40	20.16	-0.69	3.7
Slovenia	2010	1.34	3	4	3	4	4	3	-1.03	20.42	0.66	3.7
Slovenia	2011	0.86	3	4	3	4	4	3	1.04	20.56	1.70	3.7
Slovenia	2012	-2.64	3	4	3	4	4	3	0.48	20.36	0.07	3.7
Slovenia	2013	-1.03	3	4	3	4	4	3	1.60	19.64	0.21	3.7
Slovenia	2014	2.77	3	4	3	4	4	3	0.46	18.89	2.04	3.7
Tajikstan	1994	-21.30	2	2	1	3	1	1	263.01	17.41	0.79	1.8
Tajikstan	1995	-12.42	2	2	1	3	2	1	294.53	15.81	0.81	2.1
Tajikstan	1996	-16.70	2	2	1	3	2	1	430.41	16.82	1.72	2.1
Tajikstan	1997	1.68	2	2	1	3	2	1	65.25	16.51	1.95	2.1
Tajikstan	1998	5.31	2	2	2	3	3	1	87.88	9.52	2.27	2.5
Tajikstan	1999	3.70	2	3	2	3	3	1	26.50	9.93	0.62	2.6
Tajikstan	2000	8.32	2	3	2	3	3	1	22.63	8.29	2.74	2.6
Tajikstan	2001	9.58	2	4	2	3	3	1	30.95	8.66	0.88	2.7
Tajikstan	2002	10.84	2	4	2	3	3	2	18.78	8.60	2.95	2.8
Tajikstan	2003	10.93	2	4	2	4	3	2	27.17	8.26	2.03	3.1
Tajikstan	2004	10.37	2	4	2	4	3	2	17.36	11.78	13.10	3.1
Tajikstan	2005	6.64	2	4	2	4	3	2	9.57	14.60	2.36	3.1
Tajikstan	2006	7.05	2	4	2	4	3	2	21.01	11.10	11.96	3.1
Tajikstan	2007	7.76	2	4	2	4	3	2	27.29	8.90	9.68	3.1
Tajikstan	2008	7.91	2	4	2	4	3	2	28.15	9.33	9.43	3.1
Tajikstan	2009	3.89	2	4	2	4	3	2	12.13	12.46	2.99	3.1
Tajikstan	2010	6.52	2	4	2	4	3	2	12.44	11.33	1.66	3.1
Tajikstan	2011	7.40	2	4	2	4	3	2	13.32	13.65	2.24	3.1
Tajikstan	2012	7.49	2	4	2	4	3	2	11.88	12.84	3.17	3.1
Tajikstan	2013	7.40	2	4	2	4	4	2	3.63	13.30	3.35	3.4
Tajikstan	2014	6.71	2	4	2	4	4	2	4.76	14.04	3.58	3.4
Turkmenistan	1994	-17.30	1	1	1	2	1	1	952.31	8.49	4.02	1.3
Turkmenistan	1995	-7.20	1	1	1	2	1	1	705.72	11.93	9.39	1.3
Turkmenistan	1996	6.70	1	1	1	2	1	1	1014.01	7.78	4.54	1.3
Turkmenistan	1997	-11.40	2	2	2	2	1	1	61.80	17.87	4.40	1.6
Turkmenistan	1998	7.10	2	2	2	2	1	1	17.62	19.08	2.39	1.6
Turkmenistan	1999	16.50	2	2	2	2	1	1	23.01	15.46	5.10	1.6
Turkmenistan	2000	5.47	2	2	1	2	1	1	23.46	18.51	4.51	1.5

Turkmenistan	2001	4.34	1	2	1	2	1	1	32.31	14.67	4.81	1.4
Turkmenistan	2002	0.26	1	2	1	2	1	1	25.15	12.92	6.19	1.4
Turkmenistan	2003	3.27	1	2	1	3	1	1	27.15	14.01	3.78	1.7
Turkmenistan	2004	5.00	1	2	1	3	1	1	18.35	13.92	5.17	1.7
Turkmenistan	2005	13.03	1	2	1	3	1	1	7.03	14.65	5.16	1.7
Turkmenistan	2006	10.97	1	2	1	3	1	1	12.25	11.71	7.11	1.7
Turkmenistan	2007	11.06	1	2	1	3	1	1	9.27	9.85	6.76	1.7
Turkmenistan	2008	14.70	1	2	1	3	2	1	59.74	7.03	6.63	2
Turkmenistan	2009	6.10	1	2	1	3	2	1	9.76	7.44	22.52	2
Turkmenistan	2010	9.20	1	2	1	3	2	1	2.31	7.13	16.08	2
Turkmenistan	2011	14.70	1	2	1	3	2	1	12.86	6.63	11.60	2
Turkmenistan	2012	11.10	1	2	1	3	2	1	8.27	5.94	8.90	2
Turkmenistan	2013	10.20	1	2	1	3	2	1	1.15	6.06	7.30	2
Turkmenistan	2014	10.30	1	2	1	3	2	1	0.67	6.58	8.80	2
Ukraine	1994	-22.93	1	2	1	2	1	1	953.46	19.36	0.30	1.4
Ukraine	1995	-12.20	2	2	2	3	3	2	415.81	21.27	0.55	2.6
Ukraine	1996	-10.00	2	3	2	3	3	2	66.15	21.76	1.17	2.7
Ukraine	1997	-3.00	2	3	2	3	3	2	18.07	27.40	1.24	2.7
Ukraine	1998	-1.90	2	3	2	3	3	2	12.01	24.61	1.77	2.7
Ukraine	1999	-0.20	2	3	2	3	3	2	27.40	19.83	1.57	2.7
Ukraine	2000	5.90	3	3	2	3	3	2	23.12	20.92	1.90	2.8
Ukraine	2001	9.20	3	3	2	3	3	2	9.84	18.68	2.09	2.8
Ukraine	2002	5.25	3	4	2	3	3	2	5.07	17.51	1.64	2.9
Ukraine	2003	9.52	3	4	2	4	3	2	8.11	18.10	2.84	3.2
Ukraine	2004	12.11	3	4	2	4	3	2	15.15	16.72	2.65	3.2
Ukraine	2005	3.00	3	4	2	4	3	3	24.19	17.36	9.07	3.3
Ukraine	2006	7.44	3	4	2	4	4	3	14.73	17.55	5.21	3.6
Ukraine	2007	7.59	3	4	2	4	4	3	23.10	17.03	7.15	3.6
Ukraine	2008	2.30	3	4	2	4	4	3	28.58	16.98	5.95	3.6
Ukraine	2009	-14.76	3	4	2	4	4	3	13.02	19.18	4.07	3.6
Ukraine	2010	3.83	3	4	2	4	4	3	13.92	19.39	4.74	3.6
Ukraine	2011	5.47	3	4	2	4	4	3	14.20	17.36	4.42	3.6
Ukraine	2012	0.24	3	4	2	4	4	3	7.79	18.65	4.65	3.6
Ukraine	2013	-0.03	3	4	2	4	4	3	4.34	18.58	2.46	3.6
Ukraine	2014	-6.55	3	4	2	4	4	3	15.90	18.67	0.63	3.6
Uzbekistan	1994	-5.20	2	3	1	3	2	1	1238.60	30.10	0.57	2.2
Uzbekistan	1995	-0.90	3	3	2	3	2	2	370.94	32.01	-0.18	2.5
Uzbekistan	1996	1.70	3	3	2	3	2	2	81.56	30.83	0.65	2.5
Uzbekistan	1997	5.20	3	3	2	3	2	2	66.09	20.50	1.13	2.5
Uzbekistan	1998	4.30	3	3	2	2	2	2	39.00	20.50	0.93	2.2
Uzbekistan	1999	4.30	3	3	2	2	1	2	44.12	20.60	0.71	1.9
Uzbekistan	2000	3.83	3	3	2	2	1	2	47.29	18.70	0.54	1.9
Uzbekistan	2001	4.16	3	3	2	2	2	2	45.24	18.50	0.73	2.2
Uzbekistan	2002	3.97	3	3	2	2	2	2	45.48	18.00	0.67	2.2

Uzbekistan	2003	4.23	3	3	2	3	2	2	26.77	17.40	0.82	2.5
Uzbekistan	2004	7.45	3	3	2	3	2	2	15.92	16.20	1.47	2.5
Uzbekistan	2005	6.95	3	3	2	3	2	2	21.43	15.90	1.34	2.5
Uzbekistan	2006	7.45	3	3	2	3	2	2	23.47	15.30	1.00	2.5
Uzbekistan	2007	9.47	3	3	2	3	2	2	21.90	15.61	3.16	2.5
Uzbekistan	2008	9.03	3	3	2	3	2	2	26.79	15.93	2.41	2.5
Uzbekistan	2009	8.05	3	3	2	3	2	2	17.26	15.47	2.50	2.5
Uzbekistan	2010	7.60	3	3	2	3	2	2	39.37	13.33	3.51	2.5
Uzbekistan	2011	7.78	3	3	2	3	2	1	21.48	12.74	2.89	2.4
Uzbekistan	2012	7.38	3	3	2	3	2	1	15.51	13.12	0.88	2.4
Uzbekistan	2013	7.58	3	3	2	3	2	1	11.74	14.13	0.92	2.4
Uzbekistan	2014	7.18	3	3	2	3	2	1	14.35	13.97	1.05	2.4