Journal of Development Economics 91 (2010) 169–179

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Structural policies and growth: Time series evidence from a natural experiment[☆]

Theo S. Eicher [a,*], Till Schreiber [b]

[a] University of Washington, Ifo Institute of Economic Research, United States
[b] College of William and Mary, United States

ARTICLE INFO

Article history:
Received 13 September 2007
Received in revised form 25 April 2009
Accepted 12 May 2009

JEL classification:
040
P48
011

Keywords:
Structural policies
Growth
Dynamic panel
Transition economies

ABSTRACT

Documenting the long term impact of structural policies on economic performance has generated tremendous interest in the development literature. In contrast, contemporary effects of structural policies are difficult to establish. Structural policies seldom change sufficiently in the short run, and accepted instruments to control for endogeneity in cross sections are inappropriate for time series analysis. In this paper we utilize an eleven year panel of 26 transition countries to identify short term effects of structural policies that are large and significant. A ten percent change in the quality of structural policies (or the Rule of Law) towards OECD standards is shown to raise annual growth by about 2.5%. To control for endogeneity, we develop an instrument using the *hierarchy of institutions* hypothesis and find that it holds a robust explanatory power. We also document that early reformers reap the greatest benefits, but that it is never too late to begin structural policy reforms.

1. Introduction

A growing strand of the empirical growth literature focuses on the explanatory power of structural policies or institutions to account for differences in living standards across countries.[1] In general, structural policies evolve slowly, and empirical studies focus on their long term influences on *income levels* (e.g., Hall and Jones 1999; Acemoglu et al., 2001).[2] Instead of examining long term effects of structural policies that are captured in cross sections, we investigate their contemporary short term effects on *economic growth* in a panel of countries.

Two issues have prevented researchers from identifying the growth effects of structural policies. A panel approach requires a sufficiently large variation not only in structural policies, but also in the relevant instruments that are necessary to control for endogeneity. Generally this variation does not exist in the data. We resolve both issues by utilizing the fall of the Iron Curtain as a natural experiment that allows us to examine how communist-to-capitalist system changes are associated with rapid changes in structural policies that catch up to OECD standards. The fall of the Iron Curtain provides a unique controlled, or natural, experiment in that the initial institutional change is clearly exogenous, which potentially mitigates the endogeneity bias. It also provides a unique opportunity to analyze the impact of subsequent structural policy changes on growth in a sizeable number of countries, with similar initial conditions, over the same period of time.

The overriding feature in our panel is change in three dimensions. First, we observe diverse patterns of output changes over time. Second, structural policies evolved at varying speeds as countries transitioned from centrally-planned towards market-based systems. Third, political institutions moved progressively, and again at varying speeds, from autocracy to democracy after the fall of one-party regimes. The differential performances in transition countries have been closely linked to differences in institutions or structural policies across countries.[3] For most countries in most times there is no inherent reason to expect a contemporary effect of structural policies on economic performance. Institutions or structural policies are usually seen as persistent, but this specific period and set of countries provides an excellent example of what North (1990) coined "discontinuous institutional change." The approach has been formalized by Krasner (1993) and Norris (1997) in a "punctuated institutional equilibrium" that describes institutions in long periods of stasis, interrupted by crises that bring about abrupt change.

The endogeneity of institutions or structural policies raises serious econometric issues that have been amply documented in the previous literature (see Acemoglu, 2005 for a survey). Fixed effect panel analysis cannot utilize the established cross-sectional instruments such as latitude, language, settler mortality, or any other history-based

☆ We thank Daron Acemoglu, Sascha Becker, Christa Heinz, Stephan Klasen, Charles Nelson, Chris Papageorgiou, Richard Startz, Judy Thornton, Farid Toubal, Steve Turnovsky, and especially Jon Temple for helpful comments. Any remaining errors are our own. Theo Eicher thanks the German Science Foundation for financial support.

* Corresponding author.
E-mail address: te@u.washington.edu (T.S. Eicher).

[1] The literature also uses the terms "economic institutions," "reforms," "structural reforms," "structural policies," "growth promoting policies," and "social infrastructure." For a discussion of the terminology, please see Section 2.

[2] For example, the type of colonial history is shown to impact current institutions and thus current output levels.

[3] We discuss the literature in detail below.

0304-3878/$ – see front matter © 2009 Elsevier B.V. All rights reserved.
doi:10.1016/j.jdeveco.2009.05.003

variables. Our task is to identify new instruments that are uncorrelated with the disturbances, but that vary sufficiently over time to isolate the effects of ongoing institutional change on economic performance. Our strategy is two-pronged. On the one hand, we use the system GMM estimator, which Blundell and Bond (1998), Bond et al. (2001), and Bond (2002) argue to be unbiased in the presence of endogenous independent variables when lags go beyond 2 (we use lags 3 to 5). Alternatively, we develop instruments using the *hierarchy of institutions hypothesis* (Williamson, 2000; Acemoglu et al., 2005; Roland, 2004) to address endogeneity. Here the notion is that political institutions are critical determinants of economic institutions or structural policies, which subsequently determine economic outcomes. Hence political institutions could theoretically function as instruments in our analysis.

In the tradition of the long run institutions literature, we first report cross section results that establish the importance of the initial quality level of structural reforms on subsequent income and growth.[4] Then we use panel analysis to examine the impact of short term effects of structural policies across economies. Even after controlling for endogeneity, the contemporaneous effect of structural policies on growth is shown to be large. For example, a ten percent improvement in institutions is shown to raise the annual growth rate by about 2.7% in the panel. This result is remarkably robust to a variety of different specifications, including changes in the time horizon, averaging over time periods, using transition time, the use of alternative instruments, or institution measures.

The specific set of countries in our panel has been the subject of extensive theoretical and empirical studies. Aghion and Blanchard (1994), Kornai (1994), Blanchard (1997), Blanchard and Kremer (1997), Hellman (1998) and Roland (2000) provide theories of growth, U-shaped output responses, market imperfections, and the reallocation of resources within/between public/private sectors in transition economies. A rich empirical literature uses EBRD indices and the similar (and earlier) De Melo et al. (1997) liberalization index to access determinants of transition performance. De Melo et al. (2001), Aslund et al. (1996), Fischer et al. (1996a,b), and Selowsky and Martin (1997) analyze the impact of the cumulative De Melo et al. (1997) index on growth, interpreting it as a measure of the speed of reform. Havrylyshyn et al. (1998), Berg et al. (1999), Havrylyshyn and van Rooden (2003), use ERBD and De Melo et al. (1997) indices together with policy variables (inflation and fiscal deficits) to find that their combination explains most of the variations in transition growth.

Initial studies focus on cross sections or short panels, assuming a one-way causation from structural policies to growth. Wolf (1999) controls for the endogeneity of policy variables and Heybey and Murrell (1999) (in a cross section) and Falcetti et al. (2002) (in a panel) estimate a simultaneous system to allow feedback via structural reforms that are instrumented with one period lags. Berg et al. (1999) and Ghosh (1997) instrument stabilization policies whose significance was highlighted by Kornai (1994). Beck and Laeven (2006) use natural resource endowments and time under communism as instruments for initial conditions in cross sectional analysis. Theirs is the first paper that presents a conceptual framework of institutional development in transition countries based on predetermined factors and tests this framework using data on endowments and outcome measures of institutional development. They also investigate the relationship between the exogenous component of institutional development and economic growth for transition economies and find significant feedbacks and large differences compared to OLS regressions. Their analysis motivates us to examine the growth effects of structural policies in a panel with a theory-specified set of instruments.

Our paper features three distinct departures from the previous literature. First, we present long time series that allows us to apply formal econometric methods to address endogeneity. Second, we use theory-based instruments to control for endogeneity in a panel setting building on the cross-sectional approach by Beck and Laeven (2006). Falcetti et al. (2006) examine the effects of structural policies in transition economies in a panel, using one period lags as instruments. Falcetti et al. (2006) also employ a simultaneous equations approach in which reform is influenced by civil liberties. Thus, we extend their analysis both in methodology by focusing mostly on system-GMM estimation and by using a fully specified and theory-based approach to control for the endogeneity of structural policies. Also, instead of following a Barro (1997) approach that establishes an exhaustive list of growth determinants, we adopt the approach of Hall and Jones (1999) and Acemoglu et al. (2001) and focus only on the impact of structural policies. This is not meant to detract from alternative explanations, variables, or approaches; it is simply an attempt to capture and highlight the aspects of institutional change. Our robustness checks also consider alternative contemporaneous growth determinants. Much like Acemoglu et al. (2003), we find that structural policies dominate.

2. Data

We examine the impact of structural policies and the rule of law on real GDP per capita growth between 1991 and 2001 for 26 transition economies (Fig. 1). Since eight of these countries concluded their EU accession negotiations in 2002, we end our analysis in 2001 to avoid potential structural breaks. Below we simply refer to "structural policies," although Persson (2005) highlights that the literature's focus on "institutions" often associates identical measures with different labels. Hall and Jones (1999) use "social infrastructure," Acemoglu et al. (2005) refer to "economic institutions," Rodrik et al. (2004) simply say "institutions," while Persson (2005) uses "growth promoting policies" or "structural policies." All of these terms are used to refer to identical or very similar fundamental data (specifically ICRG measures of property rights protection and/or openness).

Time series for these traditional proxies of structural policies do not exist for transition economies. Instead the components of the EBRD liberalization index were designed to capture "the task of building market-supporting institutions" (EBRD, 1994, Chapter 1). We utilize these EBRD (2000–2002, 2005) measures to construct a *structural policy index* (see Fig. 1), consisting of price liberalization, foreign exchange/trade liberalization, small/large scale privatization, enterprise reform, competition policy reform, banking sector reform, and reform of non-banking financial institutions. Since the individual components are highly correlated, we follow Hall and Jones (1999) and sum all into one composite index that is normalized to a range from zero to unity (1 = OECD quality).

Fischer and Sahay (2004) use the identical index as "measures of the extent and success of the institution building that took place in the last decade" in transition countries. More recently, Roland (2005) uses the exact same subindices as "institutional indicators" to graphically assess progress in "institutional transition" in new EU member countries from 1991 to 1999. Clearly the structural policy index only proxies economic institutions and does not represent them. The index is highly correlated with the ICRG government anti-diversion index (for the overlapping years, in 19 countries, the correlation is equal to 0.7). As an alternative institutions index, we also use an ICRG-based Rule of Law measure. A complete ICRG time series exist for only seven transition countries. Partial time series exist for 18 other countries. We extend the ICRG index by using countries with overlapping years to estimate a fit of Campos' (2000) Rule of Law index into the ICRG index. Campos' (2000) criteria for establishing Rule of Law are identical to ICRG's.

In our search for alternative instruments we utilize common proxies for political institutions, specifically "executive constraints"

[4] See also Knack and Keefer (1995) and Barro (1997) among numerous studies for OLS growth regressions that include institution indices. For a review of the role of initial conditions in transition economies, see Murrell (1996).

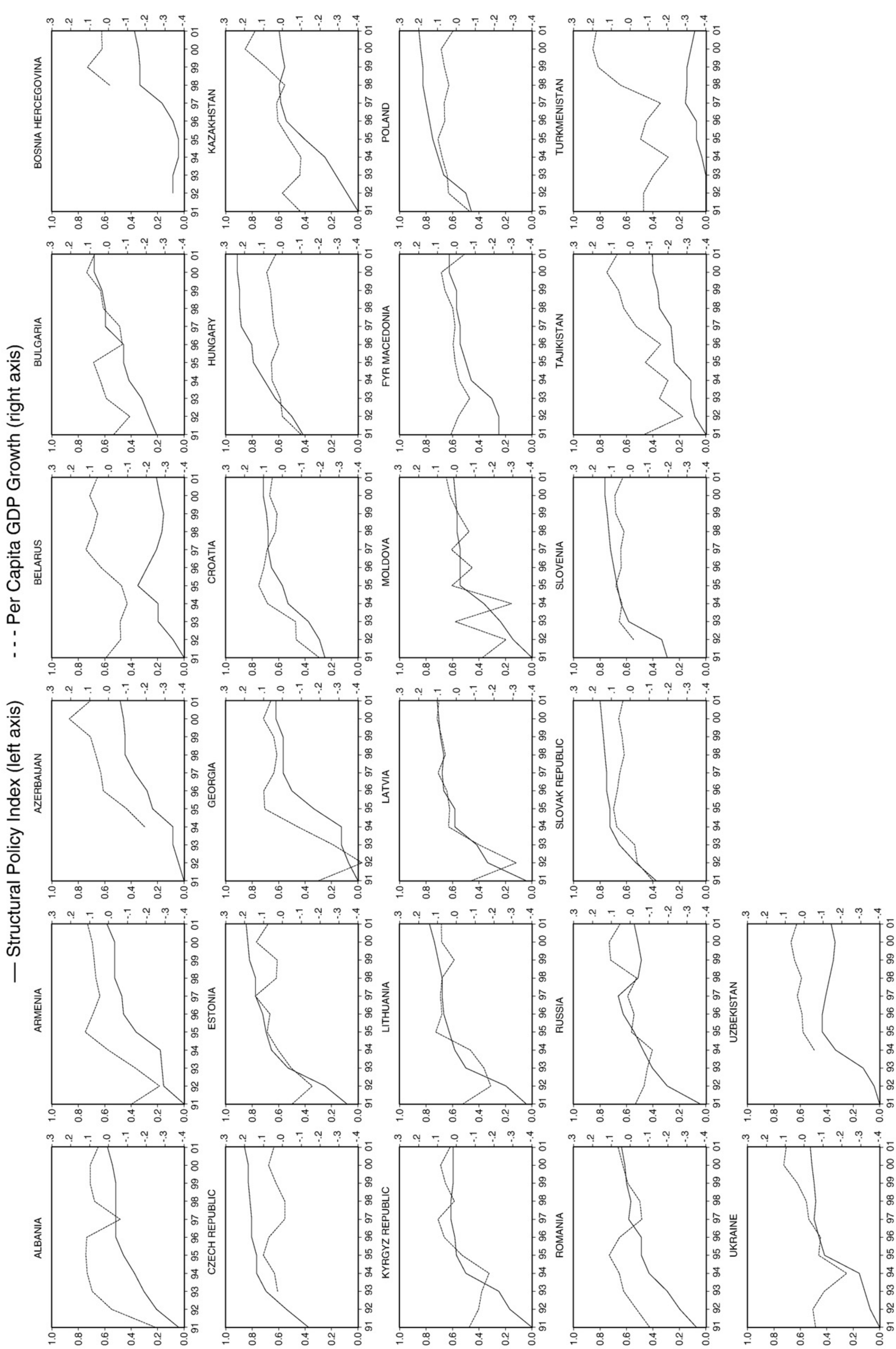

Fig. 1. Per capita GDP growth and structural policy index.

and "democracy" variables from Polity IV that reflect de facto independence of the executive branch and the degree of democratic institutions, respectively. We also use the related "check and balances" and "executive indices of electoral competitiveness" from the World Bank *Database of Political Institutions* (Beck et al., 2001) for robustness checks. Finally, macro policy variables (fiscal deficit, government consumption, and inflation) are added in further robustness exercises and are obtained from EBRD (2005).

3. Exploratory cross-country regressions

While we are ultimately interested in the time series implications of structural policies, we commence by examining the effects of initial conditions on long run economic performance. This exploration acknowledges a large literature that concentrates on the significance of initial conditions in the transition process. Reviewing the transition literature, Murrell (1996) suggests that political change and institutional change are both related to initial conditions.[5]

Fig. 2a and b shows the positive correlation between structural policies and standards of living across countries. In 1991 there already existed considerable variation in structural policies across countries in our sample, indicating diverse initial conditions. The figures also highlight that the change in structural policies was not constant across countries. By 2001 some initial laggards (Estonia, Lithuania, and Latvia in particular) had made substantial progress towards OECD quality structural policies. But six of the 26 countries did not even achieve a 0.5 rating by 2001. A possible mean reversion argument can be ruled out from Fig. 2. Clearly, many of the early frontrunners are still the most advanced countries 11 years later. Many countries in the Former Soviet Union which started at 0 in 1991 are still below average in 2001.

We augment Fig. 2a–b with two-stage least squares estimations that address endogeneity. In the spirit of instruments that identify the impact of "colonial history" on long term economic performance as in Acemoglu et al. (2001), we utilize "communist history" as an instrument for transition countries. Our "independence" dummy identifies whether a country was independent in 1988 or not. One hypothesis is that previously independent countries did not have to reform structural policies quite as profoundly as previously dependent countries (including all of former Yugoslavia) that were forced to start from scratch after independence. Here the fall of the Iron Curtain provides our first natural experiment. We utilize the initial variation across countries with respect to their need to establish entirely new structural policies in order to identify the impact on income levels, Y_i and growth, $\hat{Y}_i$. Table 1 shows that independence is indeed a strong instrument, as indicated by the first stage R^2. As expected, the instrument is weaker when used to identify contemporaneous (2001) structural policies, but it remains significant. Note that the positive and significant first stage coefficient indicates that previously independent countries started off with better initial conditions with regard to the quality of structural policies.

Table 1 also shows that the instrumented initial quality of structural policies is highly significant in explaining 2001 income levels as well as 1991–2002 income growth. Initial conditions thus seem surprisingly important in explaining long term output levels and subsequent growth rates, which contrasts with findings of De Melo et al. (1997, 2001) and Berg et al. (1999), who argue that the quantitative impact of initial conditions was declining over time in a shorter sample.[6] However, the convergence term in the growth regressions is not

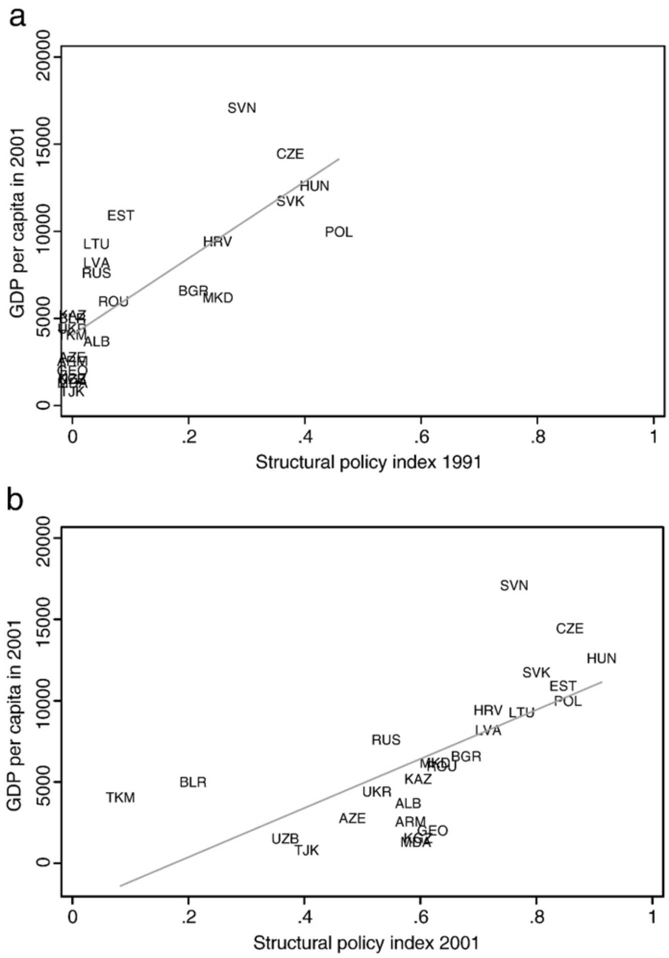

Fig. 2. Effects of initial conditions and structural policies on economic performance.

significant, and structural policies are significant at the 10% level only. This leads us to suspect that initial conditions do not capture all determinants of ongoing growth in transition economies.

4. The importance of sustained improvements in structural policies

Cross-sectional analysis in the context of transition and growth suffers drawbacks that limit the insights that can be derived. Given the short time horizon, the initial output decline, and subsequent recovery, the regression analysis should include time trends. In addition, the very nature of those structural policies that changed the economy from plan to market implies that all nations experienced an output drop and a subsequent recovery. This makes it difficult for structural policies to yield an unambiguously positive or negative impact on growth. We thus turn to time series analysis, which derives its power to correlate changes in structural policies with economic growth from the variations in a) the size of the contraction, b) the length of the contraction, c) the speed of the recovery, d) the dramatically different growth experiences, and finally e) whether the recovery could be sustained.

The core focus of the paper is thus to identify the impact of *ongoing and sustained* structural policy improvements on economic growth. A broad feature of Fig. 2a is how closely the speed of the recovery and the subsequent plateauing of growth coincide with the initial speed of structural policy change and how long it was sustained. This implies that the transition experience was not one of uniform improvement in structural policies and growth, but that the varied country experiences are actually determined by the ongoing structural policy change. Our

[5] Balcerowicz and Gelb (1995), De Melo et al. (1997, 2001), Fischer et al. (1996a,b), Denizer (1997), and Beck and Laeven (2006) include initial conditions in their analyses.

[6] Note that these studies refer to the initial conditions as characteristics of the country prior to the beginning of transition. The panel results in Section 4 will make this distinction clearer than the cross-sectional approach can.

Table 1
Effects of initial conditions and structural policies on income and growth.

	Structural policies$_{1991}$	Y_{2001}	Structural policies$_{2001}$	Y_{2001}	Structural policies$_{1991}$	$\hat{Y}_{1991–2001}$
Independence	0.192*** (0.066)		0.184** (0.068)		0.143** (0.065)	
Structural policy index$_{1991}$		3.813*** (1.102)				2.631* (1.352)
Structural policy index $_{2001}$				3.891*** (1.234)		
Y_{1991}					0.122*** (0.037)	−0.16 (0.315)
R^2	0.34		0.19		0.45	
N	25	25	26	26	22	22

Superscripts */**/*** denote 10, 5, and 1% significance levels. White-standard errors in parentheses. A constant (not reported) was included in all regressions. Estimation: 2SLS; instrument: country-independence prior to 1989.

hope is that these dynamics assist us in identifying the effect of structural policies on growth in our panel.

The unique advantage of our panel dataset is that we can trace the annual impact of structural change on growth over the eleven-year period. Note that the approach is very different from event studies that examine the effects of structural policies in "transition time," which identifies how long it takes to have sufficiently strong structural policies in place to generate growth after the fall of communism in a country. Instead we utilize the variation from different transition experiences to understand how structural policies affect growth over time. In that sense it is even helpful for us that reforms started in two waves, with Former Soviet Union countries entering transition later. This provides additional variation while controlling for time fixed effects. However we compare our results to transition time results below for completeness.

4.1. Time series methodology

To control for variables that do not change over time such as history, geography, and independence, we estimate the fixed effects OLS (LSDV) and system-GMM regressions for:

$$\hat{Y}_{i,t} = \alpha + \beta I_{i,t} + \gamma lnY_{i,t-1} + \eta_i + \nu_t + \varepsilon_{i,t} \qquad (1)$$

where $\hat{Y}_{i,t}$ is per capital income growth in country i at time t, I is the structural policy index described above, α is a constant, η_i captures country-specific fixed effects, and ν_t time fixed effects. The inclusion of time fixed effects ensures that our results are not contaminated by a possible common trend in the variables of interest. Nickell (1981) shows that the LSDV estimator in Eq. (1) is biased in a dynamic panel; simulation studies demonstrate that the lagged dependent variable LSDV coefficient is biased downwards, whereas other coefficients are less affected (see Judson and Owen, 1999; Gaduh, 2002; Hauk and Wacziarg, 2004). Kiviet (1995) derived a correction for this LSDV bias, which we implement using Bruno's (2005a,b) procedure.

To explore the time-series properties of the data, we conduct a Levin et al. (2002) panel unit root test, which rejects the null hypothesis of a common unit root for both the growth rate and for the structural reform index. In addition, an Im et al. (2003) test rejects the null hypothesis of individual unit roots for growth rates and the structural reform index. Following Bond et al. (2001), whenever lagged GDP is included as an explanatory variable, we express the lagged GDP in deviations from time means. This eliminates a common time trend in $\ln Y_{i,t}$ over the eleven-year horizon and renders this variable stationary. Thus all variables entering Eq. (1) are stationary when controlling for time fixed effects.

Highly persistent time series may introduce weak instrument bias, in which case the Arellano and Bond (1991) difference-GMM estimator may not be appropriate in the growth context.[7] The Blundell and Bond (1998) system-GMM estimator alleviates the issue:

$$\hat{Y}_{i,t} - \hat{Y}_{i,t-1} = \beta\Delta I_{i,t} + \gamma\Delta lnY_{i,t-1} + \Delta\nu_t + \Delta\varepsilon_{i,t} \qquad (2)$$

As in Arellano and Bond (1991) this estimator uses a difference equation to eliminate constant country-specific fixed effects and Δ represents a variable's time difference. To identify the coefficients in Eq. (2), Blundell and Bond (1998) suggest that lags two and higher of $I_{i,t}$ and $\ln Y_{i,t-1}$ are used as instruments. When measurement error and endogeneity pose additional problems, Bond (2002) suggests longer lags (we thus use lags three to five). To avoid overfitting bias, once the number of instruments increases relative to the number of observations, we restrict the instruments to one for each variable and time lag.

In the presence of highly persistent series, the difference-GMM estimator may be subject to weak instrument bias. Blundell and Bond (1998) show that Eq. (1) provides additional moment conditions, given the explanatory variables' mean stationarity that can be utilized to estimate the coefficients consistently. These moment conditions use lagged differences of the explanatory variables in Eq. (1) as instruments. Since endogeneity and measurement error may pose a potential problem, we use lag two (instead of lag one) as instruments for the moment conditions derived from Eq. (1).

Since we control for lagged income, the current growth rate should be influenced only through current structural policies. The three to five year lags then influence growth only through their effects on current institutions. This is the crucial assumption for the validity of using the system-GMM approach to eliminate endogeneity bias in our case. We will also use a theory-based, alternative IV strategy in Section 5 below. The lag structure then also hypothesizes that lagged structural policies may affect current growth other than through current institutions. The presence of multiple lags allows a test of this exclusion restriction with the help of an over-identification test. We report the results from a Sargan (Hansen J) overidentification test, as well as the Arellano–Bond test for AR(2) serial correlation in the residuals. Both tests allow an assessment of whether the chosen identification strategy of the system-GMM estimator is valid.

In our discussion we concentrate on the system-GMM coefficients as the most reliable estimates since this method best addresses potential endogeneity bias as outlined in Bond (2002) and Bond et al. (2001). Nevertheless, the Nickell-bias corrected LSDV estimates serve as useful benchmarks and provide country fixed effects. The usual caveat applies that all instrumental variables estimations rely on an "essentially non-testable hypothesis" (Acemoglu, 2005). In our approach, we must assume that three- to five-year lagged values are unaffected by current growth and have no direct effect beyond their impact on contemporaneous institutions. Instead of relying only on lag structures, we will also provide an additional theory-based alternative below.

4.2. Time series evidence

In Table 2 we first report the coefficient produced by what, in our view, is the most appropriate econometric methodology (system GMM). Across all specifications, the magnitude of the structural policy estimates is surprisingly large. In our baseline specification (column 2), a 10% increase in the structural policy index is associated with a 2.68% increase in growth. These are sizable *growth possibilities* that can be reaped from structural policy changes. Note, however, that this sizable impact does not imply a constant change in the growth rate. The lagged income variable allows us to calculate level effects that we discuss below.

[7] See Hauk and Wacziarg (2004), Bond et al. (2001), and Levine et al. (2000).

Large impacts of institutions on economic performance are not new to the literature. Prominent studies by Hall and Jones (1999, p. 105) and Acemoglu et al. (2001, p. 1387) find that ten percent increases in institutional quality raise *long run output levels* by roughly 67% and 160%, respectively. Using the above estimates on the structural policy index and the estimate on lagged output per capita also allows us to calculate an implied level effect and compare it to the estimates from the long run institutions literature. We find that a ten percent improvement in the quality of structural policies implies an increase of GDP per capita in the long run by about 55%. Here we define the long run as half a century. These level effects are therefore in line with Hall and Jones (1999) and bit lower than the very long run findings in Acemoglu et al. (2001) where the long run is probably better understood as a 200–400 year period.

We must keep in mind that the usefulness of the natural experiment is almost by definition confined to the short and medium run. When countries finish their transition and enter the EU, for example, a structural break occurs. What we find is an example of a significant growth acceleration along the lines of Hausmann et al. (2005). It is tempting to associate the impact of structural policies with great strides in early phases of the transition. However, column 5 confirms that if we start the analysis in 1994, the magnitude of the coefficient is just about identical — and its standard error is even slightly smaller. The robustness test that excludes early reform years thus provides evidence that ongoing reforms are more crucial for growth than initial periods of opportunity.

While the coefficients in Table 2 may appear large at first, the magnitude is remarkably robust across different specifications that include interaction terms (columns 3–4, 10, 12, 14, 16), a shorter panel (column 4), the inclusion of macro policy variables that determine growth (column 5), annual growth rates (columns 2–5, 9–10, 13–16), event studies based on "transition time"[8] (columns 9–10), or three-year averaged growth[9] (columns 11–12). In all specifications the Arellano and Bond AR(2) and the Sargan (Hansen J) tests support our assumption of instrument exogeneity and report no evidence of serial correlation. The exceptions are the three-year averaged results where we judge the dynamic panel to be too short to gain confidence in the instruments. The other exception is the introduction of macro policy variables where the instrumentation with lagged dependent values does not seem to provide good instruments. Acemoglu et al. (2003) point out that this biases results in favor of macro policies.

Our results extend the existing literature by using both panel data and allowing for endogeneity of institutions. Falcetti et al. (2006) also employ system-GMM estimation and find a significantly positive effect of structural policies on growth. They instrument with a one period lag and do not explore theory based instruments in a dynamic panel setting. Beck and Laeven (2006) use cross-sectional analysis to estimate the causal effect of institutions and structural policies on growth. They motivate natural resource endowments and socialist entrenchment as instruments for institution building. These instruments are, however, not available to us in panel analysis, since they lack a time dimension.

We can, however, build on the Beck and Laeven (2006) results by establishing a stronger case for causality of structural policies in a panel setting while using a variety of techniques to control for endogeneity. In addition to the analysis in this section we also employ a hierarchy of institutions approach below which uses political institutions as instruments for structural policies. Havrylyshyn and van Rooden (2003) observe the importance of both institutions and

[8] At times, the transition literature accounts for different starting dates with event studies. We follow Merlevede's (2003) definition for each country's $t=1$ in which communism and central planning were abandoned.

[9] Averaging is a common robustness test; see, e.g., Acemoglu et al. (2003) or Burnside and Dollar (2000).

Table 2
Dynamic effects of structural policies on growth.

	Dependent variable														
	Annual growth$^{A)}$				Gov't Bal./Y	G/Y	Inflation	Annual growth transition time$^{AA)}$		3 year averaged growth$^{C)}$		Annual growth$^{A)}$			
	2 System-GMM	3 System-GMM	4 System-GMM$^{B)}$	5 System-GMM	6 System-GMM	7 System-GMM	8 System-GMM	9 System-GMM	10 System-GMM	11 System-GMM	12 System-GMM	13 Bias-corrected LSDV	14 Bias-corrected LSDV	15 Diff.-GMM	16 Diff.-GMM
Structural policy index	0.268**	0.260**	0.291***	0.332***	−0.028	−0.133	−6.815***	0.309**	0.249**	0.205**	0.166***	0.193***	0.292***	0.551***	0.613***
	(0.123)	(0.127)	(0.076)	(0.106)	(0.074)	(0.154)	(1.669)	(0.099)	(0.121	(0.088)	(0.095)	(0.080)	(0.089)	(0.047)	(0.182)
Structural policy index$_{1991}$ · structural policy index$_t$		0.016	0.013						0.025		0.020		−0.091*		−0.015
		(0.019)	(0.020)						(0.0123)		(0.015)		(0.049)		(0.056)
Gov't expend. (G/Y)				0.223											
				(0.202)											
Gov't budget balance (balance/Y)				1.333***											
				(0.492)											
Y_{t-1}	−0.059**	−0.076*	−0.077***	−0.080**	0.003	0.126	0.687*	−0.060**	−0.086**	−0.024	−0.053*	−0.128***	−0.087**	−0.412***	−0.309***
	(0.028)	(0.043)	(0.039)	(0.034)	(0.017)	(0.035)	(0.397)	(0.025)	(0.037)	(0.023)	(0.032)	(0.038)	(0.037)	(0.076)	(0.124)
Sargan overid p-value	0.189	0.278	0.169	0.062	0.155	0.147	0.005	0.256	0.511	0.003	0.028			0.352	0.038
AR(2) test of residuals p-value	0.806	0.956	0.752	0.636	0.895	0.577	0.020	.0780	0.835	–	–			0.423	0.879
N	249	249	204	237	241	238	249	240	240	73	73	249	249	244	244

Notes: Superscripts */**/*** denote 10, 5, and 1% significance levels. Country and time fixed effects included, standard errors in parentheses. LSDV bias corrected (see Kiviet, 1995) with bootstrapped standard errors. $^{A)}$ Time period: 1992-2001, $^{AA)}$ time period: "transition timing" event study with $t=0$ set at date of independence, (see Merlevede, 2003). Transition time loses one observation for Former Soviet Union countries whose $t=0$ is 1992, and gains observations for early starters prior to 1991. The net loss is nine observations. $^{B)}$ Time period: 1994-2001, $^{C)}$Time periods: 1992–1994, 1995–1997, and 1998–2000.

policies in transition. We follow this direction in a formal two-stage empirical model in this section and in Section 5 below.

Many studies apply a similar form of analysis to individual countries that are included in our sample, or to specific reforms that are captured in our structural policy index. Chaptea (2007) analyzes the impact of trade liberalization and trade relations with EU countries. Bennett et al. (2007) and focus on the effects on growth of different forms of privatization, finding a strong positive correlation between voucher privatization and growth. Wacziarg and Welch (2008) include transition economies in their analysis of trade liberalization and growth and find that average annual growth rates increase by about 1.5%. De Macedo and Martins (2008) find evidence for a channel from structural reforms to growth and also concentrate on the interactions of the different components of the index. Guariglia and Poncet (2008) show the positive effect of market driven reforms in financial markets for growth in China, Havrylyshyn (2008) focuses on the differences between countries in the CIS and Eastern Europe. Popov (2007) divides the transition experience into two parts: an early adverse supply shock and a later recovery. He also finds a positive correlation between liberalization, which is building on strong institutions, and growth.

The value-added of our paper lies in its structured approach to estimation in a dynamic panel setting with a broad range of structural policies. We use a broad index of structural policies for all available transition economies and our results show a strongly positive causal effect that is present when employing a variety of different econometric specifications. In addition we can confirm evidence for the relative importance of initial conditions over the long panel.

In the cross section, Table 1, initial conditions matter. The dynamic estimations in Table 2 attribute great explanatory power to ongoing structural change. It is impossible to separate out the individual effects in the latter since initial conditions are indistinguishable from country fixed effects in the dynamic panel. We judge this to be an advantage of the panel estimation, since it helps us avoid the lively discussion regarding the correct initial conditions that are to be examined (see De Melo et al., 2001; Campos and Coricelli, 2002, for excellent surveys). In an effort to relate initial conditions to ongoing structural policy reform and growth, we include a term which interacts the initial 1991 structural policy level with subsequent values. Campos and Coricelli (2002) suggest that such an interaction term highlights the relationship between initial conditions and subsequent structural policy development. There seems to be broad agreement in the literature that the level of initial conditions may affect the intensity of the structural policy changes.[10] The interaction term is never significant in any GMM specifications, whereas the structural policy index's magnitude and significance remain unchanged throughout. This has three important implications. First, excellent growth can be attained even with unfavorable initial conditions. Second, there exists no "growth bonus" for countries that reformed early. Third, a "growth penalty" does not exist even if reforms start late, as long as they eventually reach sufficient quality. These findings support the robustness results in Table 2, column 4, that ongoing structural policy change matters even when initial years of transition are excluded.

While our general approach follows Hall and Jones (1999) and Acemoglu et al. (2001) in focusing on the impact of institutions only, alternative variables have been suggested in the literature to also determine economic performance in transition economies. Consensus variables include the fiscal deficit, government consumption, and inflation.[11] As an additional robustness check we add these variables to the regressions. Structural policy and the fiscal deficit are significant; however, government consumption is not. Structural policies are highly correlated with inflation (column 8), which rules out a strategy to include both in one regression.

As a further robustness exercise, we offer the Kiviet (1995) bias-corrected LSDV results (columns 13–14) and as well those based on the original Arellano and Bond (1991) difference-GMM estimator that allows for shorter lags (one to three) as instruments (columns 15–16). In terms of significance, the results are just about identical to the System-GMM results; however, the estimates are slightly higher, reflecting the biases discussed in Section 4a.

As in previous empirical analyses of transition economies, country fixed effects are prominent in the dynamic panel. Fixed effects range from 0.18 (Slovenia) to −0.2 (Tajikistan). The difference between the highest and lowest country's fixed effect is almost 0.4, and therefore considerably larger than the LSDV coefficient on structural policies. Hence not only structural change, but also the influence of fixed effects on growth is considerable. We can only conjecture about the sources of the fixed effects. The correlation between fixed effects and the Fidrmuc (2001) "Distance to Brussels" measure is surprisingly large (−0.73), indicating that close proximity to the EU generated a significant growth bonus.[12] Such a bonus could be explained by a multitude of causes ranging from the EU-accession-induced institutional changes to simple gravity or multinational entry/production diversification decisions.

The correlation between fixed effects and the quality of initial structural policies is of the same magnitude (0.62), indicating that initially better structural policies potentially exert a level effect on subsequent growth. These two variables exhaust by no means all possible interpretations of the fixed effects estimates. The goal of our dynamic panel analysis is solely to show that even short-term changes in structural policies can substantially impact growth after fixed factors have been taken into account. Among these factors are naturally all variables related to geography and history such as, for example, natural resource endowment and socialist entrenchment which were used, for example, by Beck and Laeven (2006) or Falcetti et al. (2006). It is also common in the transition literature to include other, time varying factors such as the size of the government debt, inflation etc. Here we follow the strict Hall and Jones (1999) approach that focuses squarely in the structural policies of interest to avoid that other regressors, that may share a covariance structure with structural policies, may conflate the effects of structural policies.

A key additional exercise is to examine whether these strong short-term growth results hold up to alternative measures of structural policies/institutions. Table 3 reports the results for our ICRG-based Rule of Law measure which is perhaps closer and more narrowly focused on institutional change. Though we normally think of the Rule of Law as relatively constant over time, the transition countries provide a unique laboratory where this assumption is less likely to hold. For example Albania saw its Rule of Law measure increase from 0.33 in 1992 to 0.833 in 1994/95 and then fall back to 0.5 in 1997/98. There is every reason to believe that firms should respond quickly to improvements in this area, or at least no more slowly than to changes in structural policies analyzed above. Such responses have not yet been convincingly examined. Since the structural change and Rule of Law measures are normalized in identical fashion, their coefficients can be readily compared. The results are just about identical in terms of significance and magnitude. A ten % increase in the Rule of Law index toward OECD levels increases growth by 2.4 % (column 2). Again the estimates are remarkably robust in terms of economic and statistical significance across all different specifications that include shorter panels (column 4), the inclusion of macro policy variables that

[10] See, e.g., Di Tommaso et al. (2007), De Melo et al. (2001, 1997), Balcerowicz and Gelb (1995), and Aslund et al. (1996).

[11] It is common to treat macro policy variables as exogenous in the literature. The GMM methodology also instruments for macro variables using lags.

[12] Interestingly the −0.44 correlation with "Distance to Moscow" is neither positive, nor as large as one might expect.

Table 3
Dynamic effects of Rule of Law on growth.

	Dependent variable														
	Annual growth[A)]					Gov't bal./Y	G/Y	Annual growth transition time[AA)]		Averaged growth[C)]		Annual growth[A)]			
	2 System-GMM	3 System-GMM	4 System-GMM[B)]	5 System-GMM	6 System-GMM	7 System-GMM	8 System-GMM	9 System-GMM	10 System GMM	11 System-GMM	12 System-GMM	13 Bias-Corrected LSDV	14 Bias-Corrected LSDV	15 Diff.-GMM	16 Diff.-GMM
Rule of Law	0.240***	0.044	0.222**	0.205**	0.192**	0.109**	−0.053	0.211***	0.169	0.154***	0.108	0.144***	0.181	0.461***	0.981
ICRG	(0.088)	(0.161)	(0.103)	(0.086)	(0.082)	(0.050)	(0.097)	(0.088)	(0.129)	(0.053)	(0.103)	(0.036)	(0.129)	(0.175)	(0.645)
Rule of Law $_{1991}$ ·		0.032							0.039		0.072		−0.062		−0.136
Rule of Law$_t$		(0.021)							(0.159)		(0.137)		(0.192)		(0.097)
Gov't expend.				−0.298	−0.228										
(G/Y)				(0.262)	(0.224)										
Gov't budget				0.398	0.403										
balance				(0.659)	(0.499)										
(balance/Y)															
Structural					0.211*										
policy index$_t$					(0.112)										
Y_{t-1}	−0.026	−0.083*	−0.022	0.011	−0.029	−0.009	0.100***	−0.019	−0.024	0.000	−0.021	−0.100***	−0.097***	0.187	−0.180
	(0.019)	(0.047)	(0.023)	(0.028)	(0.031)	(0.011)	(0.022)	(0.016)	(0.032)	(0.018)	(0.039)	(0.034)	(0.031)	(0.154)	(0.142)
Sargan overid p-value	0.483	0.893	0.329	0.408	0.532	0.573	0.590	0.478	0.582	0.951	0.970			0.000	0.000
AR(2) test of residuals p-value	0.939	0.725	0.859	0.899	0.990	0.874	0.638	0.383	0.207	–	–			0.691	0.927
N	221	221	176	209	209	213	210	223	223	66	66	221	221	221	221

Notes: Superscripts */**/*** denote 10, 5, and 1% significance levels. Country and time fixed effects included, standard errors in parentheses. LSDV bias corrected (see Kiviet, 1995) with bootstrapped standard errors. [A)] Time period: 1992-2001, [AA)] Time period: "transition timing" event study with $t=0$ set at date of independence, (see Merlevede, 2003). Transition time loses one observation for Former Soviet Union countries whose $t=0$ is 1992, and gains observations for early starters prior to 1991. [B)] Time period: 1994-2001, [C)]Time periods: 1992–1994, 1995–1997, and 1998–2000.

determine growth (column 5–6), annual growth rates (columns 2–6, 9–10, 13–16), event studies based on "transition time" (columns 9–10), or three-year averaged growth (columns 11–12). Even the same bias pattern can be observed in the LSDV and difference GMM regressions (columns 13–16) as in Table 2. We also include a specification that includes macro policy variables, Rule of Law, and structural policies (column 6) to show the independent effects of either institutional variable even in the presence of macro policy determinants. One important difference exists between Tables 2 and 3. For all regressions with interactions between initial levels and subsequent changes in the Rule of Law, both estimates are not significant. This is most likely due to a high degree of multicollinearity. Initial Rule of Law varied from 0.1 to 0.9 in 1991 while it varied only from 0.1 to 0.4 for structural policies. Hence in the Rule of Law regressions we cannot disentangle the separate effects of initial conditions and ongoing change. However, the regressions do show that even after controlling for initial Rule of Law with fixed effects, ongoing changes in Rule of Law are highly significant.

5. Alternative instruments: the hierarchy of institutions hypothesis

The previous section has shown that structural policies and institutions have a large causal effect on growth when lagged values of institutions are used as instruments. This section explores an alternative identification strategy that relies not on lagged values, but on a clear structurally formulated hypothesis and a formal two stage estimation process to identify the effect of structural policies on growth. Acemoglu et al. (2005) clearly outlined those political institutions that allocate excessive power to an individual (or a small group) to render it unlikely that structural policies are sustained to protect property rights for all. The authors describe a chain of events where political institutions determine the distribution of *de jure* political power, which in turn affects the choice of structural policies ("economic institutions" in their terminology). This framework is introduced as a *hierarchy of institutions*, where political institutions influence equilibrium structural policies, which then determine economic outcomes.[13]

Acemoglu et al. (2005) summarize examples of political institutions as "democracy vs. dictatorship or autocracy, and the extent of constraints on politicians and political elites." Acemoglu and Johnson (2005) motivate the Polity IV variable "executive constraints" as "conceptually attractive since it measures institutional and other constraints that are placed on presidents and dictators. Theoretically, we expect a society where elites and politicians are effectively constrained to experience less infighting between various groups to take control of the state, and to pursue more sustainable policies." This certainly applies directly to transition countries were successful structural policies can be seen as the outcome of effective political participation. Acemoglu and Johnson (2005) also address specifically how executive constraints are uniquely applicable to transition economies and our natural experiment. In societies with weak constraints on rulers, "[f]ollowing a change in the balance of political power, groups that gain politically may then attempt to use their new power to redistribute assets and income to themselves, in the process creating economic turbulence. In contrast, this source of turbulence would be largely absent in societies where institutions prevent this type of redistribution." And "politicians may be forced to pursue unsustainable policies in order to satisfy various groups and remain in power, and volatility may result when these policies are abandoned." Acemoglu et al. (2005) note also, however, the common caveat that

[13] A large literature has analyzed the importance of political institutions for economic development. See, e.g., Acemoglu (2003), Acemoglu and Johnson (2005), Acemoglu et al. (2005), Persson (2004), Rigobon and Rodrik (2005), Keefer (2007), Kaufmann and Kraay (2002), and Beck and Laeven (2006).

Table 4
Dynamic effects of institutions on growth LSDV (Stage 1) and system-GMM (Stage 2).

	1 Structural policies $_t$	2 $\hat{Y}_{1992-2001}$	3 Structural policies $_t$	4 $\hat{Y}_{1992-2001}$	5 Structural policies $_t$	6 $\hat{Y}_{1992-2001}$	7 $\hat{Y}_{1992-2001}$	8 $\hat{Y}_{1992-2001}$	9 $\hat{Y}_{1992-2001}$
Structural policy index		0.177*		0.346**		0.262*			
		(0.103)		(0.136)		(0.141)			
Executive constraints$_{t-1}$	0.254***						−0.053		
	(0.059)						(0.176)		
Executive election competition $_{i-1}$			0.039*					0.011	
			(0.021)					(0.086)	
Checks and balances$_{t-1}$			−0.006					0.001	
			(0.005)					(0.021)	
Democracy $_{t-1}$					0.007***				0.014
					(0.002)				(0.015)
Y_{t-1}	−0.024	−0.045	−0.068*	−0.081**	−0.033	−0.061	0.009	−0.015	−0.061
	(0.027)	(0.031)	(0.039)	(0.035)	(0.027)	(0.038)	(0.033)	(0.039)	(0.059)
Sargan overid *p*-value		0.474		0.670		0.574	0.624	0.994	0.685
AR(2) test *p*-value		0.913		0.808		0.908	0.239	0.421	0.896
R^2	0.83		0.74		0.82				
N	241	241	200	200	245	245	241	200	245

Notes: Superscripts */**/*** denote 10, 5, and 1% significance levels. Standard errors in parentheses. All regressions include country- and time fixed effects.

empirics cannot distinguish between the exact channels that link political institutions to structural policies.

Alternative measures are proposed by Keefer (2007) who argues that the "check and balances" index and the "executive indices of electoral competitiveness" from the World Bank *Database of Political Institutions* (Beck et al., 2001) are better variables to gauge the effects of political institutions on growth. Keefer (2007) argues that checks and balances capture the essential ingredients necessary for secure property rights: elections and checks on the executive branch. However, unlike the Polity IV executive constraints measure, Keefer (2007) argues, checks and balances captures only the formal constraints that theory predicts should protect property rights, not whether those formal constraints are binding in practice. Electoral Competitiveness is employed by Keefer (2007) to reflect that political systems in which government turnover is competitive make it harder for special interests to "capture" the government and/or the state as a whole (see also Hoff et al., 2005). Persson et al. (2000) refine the concept further by specifying whether the executive is subject to a confidence requirement in the legislature to distinguish between the type of democracy and the resulting structural policies. Their type of democracy variable is not available for transition economies; we proxy it with the Polity IV democracy variable.

Political institutions are not entirely exogenous either, however. They change slowly and Acemoglu et al. (2005) point out that societies change their constitutions during transitions from dictatorship to democracy to modify executive constraints. It is hard to argue, however, that lagged political institutions affects subsequent year-to-year output growth. For one, the hierarchy of institution hypothesis clearly outlines that political changes first affect structural policies and then output. No exact test can reveal how long the time lag is, however; the ultimate test is that neither variable possesses a direct effect on output as seen in columns 8–10 in Table 4. Clearly, the crucial assumption here is that these political institutions affect growth only through structural policies. In the case of our sample one additional explanation for the lack of a direct effect may be that through economic reform via structural policies democratic institutions gain additional traction. Thus any growth effect has to come through changes in structural policies in these unique circumstances of transition from plan to market. This argument is similar to the recent discussion of the need to build democratic capital in order to develop using a much longer time horizon (Persson and Tabellini 2007). We also perform a Sargan test, which supports our assumption of exogeneity. Theoretical and empirical support for our choice of instruments is also provided by a prominent strand of the transition literature which hypothesizes about extensive links between political transition and the intensity of economic institution reform (although not necessarily output changes).[14]

The hierarchy of institutions approach is implemented in Table 4 using both LSDV and GMM. The LSDV regressions show the strength of the instrument in a mimic first stage, and the system-GMM then delivers unbiased second stage coefficients. To investigate the strength of the instruments, we regress structural policies on the various political institutions that were motivated above, together with time fixed effects (columns 2, 4, and 6). The goodness of fit in the first stages is substantial; the political institutions are all significant with the exception of Checks and Balances in the Keefer (2007) motivated specification (column 3). Together with fixed effects and lagged income, political institutions account for between 74 to 83% of the variation in economic institutions. The *F*-tests reject the null-hypothesis of no explanatory power of all regressors at the one percent level. These results show the interdependence of political institutions and structural policies and are thus important confirmations of the hierarchy of institutions hypothesis. Note that these political institutions do not seem to influence growth significantly when introduced directly in system-GMM regressions as in Section 4 (columns 7, 8, and 9). This is further support for the hierarchy of institutions approach. Also the Sargan over-id tests do not suggest misspecification for any of the regressions. Yet, as with any instrument, we cannot ultimately test its validity but believe the case for this new approach to be strong, both theoretically and empirically. To our knowledge this is the first paper to provide a theory-based approach to control for endogeneity in this context in a panel setting. It thus builds on Beck and Laeven (2006), who provide a theory-based IV strategy in a cross-sectional set-up.

Having established sufficiently strong instruments, we present the instrumented system-GMM structural policy estimates of Eq. (1) in Table 4. Here we use the instrument in the level equations only. The coefficients on structural policies using the hierarchy of institutions as identification are exactly in line with the ones observed in Tables 2 and 3. While structural policies are slightly less significant (as compared to the System-GMM coefficients using lags in Tables 2 and 3), we take this as important, independent support of the significant effects of ongoing structural change on growth. Both the Sargan overidentification test and the AR(2) test of the residuals provide support for this identification strategy.

[14] See e.g., Balcerowicz and Gelb (1995), De Melo et al. (1997), Aslund et al. (1996).

6. Conclusion

Transition economies offer a unique natural experiment to assess the impact of structural policy changes on economic growth. Two alternative approaches that control for the endogeneity of structural policy and time fixed effects find similar strong economically and statistically significant impact. A ten percent improvement of structural policies toward OECD standards increases subsequent growth by about 2.7%. The magnitude of the result is robust to variations in the length of the panel, to event studies in transition time, to averaged growth rates as dependent variables, and to the inclusion of alternative public policy variables. Examining the interaction between initial and subsequent structural policies allows us to show that early (late) movers do not incur a growth bonus (penalty). To test whether the results hold for other measures of institutions as well, we construct the first complete time series for an ICRG based Rule of Law indicator in transition countries (using Campos, 2000, and ICRG data). Here results are just about identical in terms of magnitude and significance.

Our results show more than the importance of institutions for a specific set of countries. We replicate the results that institutions matter in the long run, but more importantly we are able to document the contemporaneous effect of institutional change on economic growth. This should be comforting for policy makers. Economic institutional reform and well designed structural policies can lead to rapid growth benefits; it does not have to take decades for the effects to show. Our use of multiple estimation techniques, all of which generate comparable results in terms of economic and statistical significance, highlights the power of this natural experiment to track institutional change across a sizeable number of countries.

References

Acemoglu, D., 2003. Why not a political coase theorem? Social conflict, commitment and politics. Journal of Comparative Economics 31 (4), 620–652.

Acemoglu, D., 2005. Constitutions, politics and economics: a review essay on Persson and Tabellini's “The economic effects of constitutions.” Journal of Economic Literature 43 (4), 1025–1048.

Acemoglu, D., Johnson, S., 2005. Unbundling institutions. Journal of Political Economy 113 (5), 949–995.

Acemoglu, D., Johnson, S., Robinson, J., 2001. The colonial origins of comparative development: an empirical investigation. American Economic Review 91 (5), 1369–1401.

Acemoglu, D., Johnson, S., Robinson, J., Thaicharoen, Y., 2003. Institutional causes, macroeconomic symptoms: volatility, crises, and growth. Journal of Monetary Economics 50 (1), 49–123.

Acemoglu, D., Johnson, S., Robinson, J., 2005. Institutions as the fundamental cause of long-run growth. In: Aghion, P., Durlauf, S. (Eds.), Handbook of Economic Growth. InNorth Holland, Amsterdam, pp. 385–472.

Aghion, P., Blanchard, O., 1994. On the speed of transition in Central Europe. In: Fischer Rotemberg (Ed.), NBER Macroeconomic Annual 1994, pp. 283–320.

Arellano, M., Bond, S., 1991. Some tests of specification for panel data: Monte Carlo evidence and an application to employment questions. Review of Economic Studies 58 (2), 277–297.

Aslund, A., Boone, P., Johnson, S., 1996. How to stabilize: lessons from post-communist countries. Brookings Paper on Economic Activity 1, 217–291.

Balcerowicz, L., Gelb, A., 1995. Macropolicies in transition to a market economy: a three-year perspective. Proceedings of the World Bank Annual Conference on Development Economics, 1994, pp. 21–44.

Barro, R., 1997. Determinants of Economic Growth: a Cross-country Empirical Study. MIT Press, Cambridge and London.

Beck, T., Laeven, L., 2006. Institutions building and growth in transition economies. Journal of Economic Growth 11 (2), 157–186.

Beck, T., Clarke, G., Groff, A., Keefer, P., Walsh, P., 2001. New tools in comparative political economy: the database of political institutions. World Bank Economic Review 15, 165–176.

Bennett, J., Estrin, S., Urga, G., 2007. Methods of privatization and economic growth in transition economies. Economics of Transition 15 (4), 661–683.

Berg, A., Borensztein, E., Sahay, R., Zettelmeyer, J., 1999. The evolution of output in transition economies: explaining the differences. IMF Working Paper WP/99/73.

Blanchard, O., 1997. The Economics of Post-Communist Transition. Oxford University Press, Oxford.

Blanchard, O., Kremer, M., 1997. Disorganization. Quarterly Journal of Economics 112 (4), 1091–1126.

Blundell, R., Bond, S., 1998. Initial conditions and moment restrictions in dynamic panel data models. Journal of Econometrics 87 (1), 115–143.

Bond, S., 2002. Dynamic panel data models: a guide to micro data methods and practice. Portuguese Economic Journal 1 (2), 141–162.

Bond, S., Hoeffler, A., Temple, J., 2001. GMM estimation of empirical growth models. CEPR Discussion Paper No. 3048.

Bruno, G., 2005a. Approximating the bias of the LSDV estimator for dynamic unbalanced panel data models. Economics Letters 87, 361–366.

Bruno, G., 2005b. Estimation and inference in dynamic unbalanced panel data models with a small number of individuals. CESPRI Working Paper no. 165. InCESPRI, Bocconi.

Burnside, C., Dollar, D., 2000. Aid, policies, and growth. American Economic Review 90 (4), 847–868.

Campos, N., 2000. Context is everything: measuring institutional change in transition economies. World Bank Policy Research Working Paper 2269.

Campos, N., Coricelli, F., 2002. Growth in transition: what we know, what we don't, and what we should. Journal of Economic Literature 40 (3), 793–836.

Chaptea, A., 2007. Trade liberalization and institutional reforms. Economics of Transition 15 (2), 211–255.

De Macedo, J., Martins, J., 2008. Growth, reform indicators, and policy complementarities. Economics of Transition 16 (2), 141–164.

De Melo, M., Denizer, C., Gelb, A., 1997. From plan to market: patterns of transition. In: Blejer, M., Škreb, M. (Eds.), Macroeconomic Stabilization in Transition Economies. InCambridge University Press, Cambridge, pp. 17–72.

De Melo, M., Denizer, C., Gelb, A., Tenev, S., 2001. Circumstance and choice: the role of initial conditions and policies in transition economies. The World Bank Economic Review 15 (1), 1–31.

Denizer, C., 1997. Stabilization, adjustment, and growth prospects in transition economies. World Bank Policy Research Paper No. 1855.

Di Tommaso, M., Raiser, M., Weeks, M., 2007. Home grown or imported? Initial conditions, external anchors, and the determinants of institutional reform in transition economies. Economic Journal 117 (520), 858–881.

EBRD, 1994. Transition Report: Infrastructure. EBRD, London.

EBRD, 2000–2002, 2005. Transition Report. EBRD, London.

Falcetti, E., Raiser, M., Sanfey, P., 2002. Defying the odds: initial conditions, reforms and growth in the first decade of transition. Journal of Comparative Economics 30 (2), 229–250.

Falcetti, E., Lysenko, T., Sanfey, P., 2006. Reform and growth in transition: re-examining the evidence. Journal of Comparative Economics 34 (3), 421–445.

Fidrmuc, J., 2001. “Forecasting growth in transition economies: a reassessment.” mimeo.

Fischer, S., Sahay, R., 2004. Transition economies: the role of institutions and initial conditions. Festschrift in Honor of Guillermo A. Calvo.

Fischer, S., Sahay, R., Vegh, C., 1996a. Economies in transition: the beginnings of growth. American Economic Review 86 (2), 229–233.

Fischer, S., Sahay, R., Vegh, C., 1996b. Stabilization and growth in transition economies: the early experience. Journal of Economic Perspectives 10 (2), 45–66.

Gaduh, A., 2002. Properties of fixed effects dynamic panel data estimators for a typical growth dataset. CSIS Working Paper Series WPE 062.

Ghosh, A.R., 1997. Inflation in transition economies: how much and why? IMF Working Paper WP/97/80.

Guariglia, A., Poncet, S., 2008. Could financial distortions be no impediment to growth after all? Evidence from China. Journal of Comparative Economics 36 (4), 633–657.

Hall, R., Jones, C., 1999. Why do some countries produce so much more output per worker than others? Quarterly Journal of Economics 114 (1), 83–116.

Hauk, W., Wacziarg, R., 2004. A Monte Carlo study of growth regressions. NBER Technical Working Paper 296.

Hausmann, R., Pritchett, L., Rodrik, D., 2005. Growth accelerations. Journal of Economic Growth 10 (4), 303–329.

Havrylyshyn, O., 2008. Growth recovery in CIS countries: the sufficient minimum threshold of reform. Comparative Economic Studies 50 (1), 53–78.

Havrylyshyn, O., van Rooden, R., 2003. Institutions matter in transition, but so do policies. Comparative Economic Studies 45 (1), 2–24.

Havrylyshyn, O., van Rooden, R., Izvorski, I., 1998. Recovery and growth in transition economies: a stylized regression analysis. IMF Working Paper, WP/98/141.

Hellman, J., 1998. Winners take all: the politics of partial reform in postcommunist transitions. World Politics 50 (2), 203–234.

Heybey, B., Murrell, P., 1999. The relationship between economic growth and the speed of liberalization during transition. Journal of Policy Reform 3 (2), 121–137.

Hoff, K., Horowitz, S., Milanovic, B., 2005. Political alternation, regardless of ideology, diminishes influence buying: lessons from transitions in former communist states. Carnegie Endowment for International Peace, Policy Outlook. January 2005.

Im, K., Pesaran, M., Shin, Y., 2003. Testing for unit roots in heterogeneous panels. Journal of Econometrics 115 (1), 53–74.

Judson, R., Owen, A., 1999. Estimating dynamic panel data models: a guide for macroeconomists. Economics Letters 65 (1), 9–15.

Kaufmann, D., Kraay, A., 2002. Growth without governance. Economia 3 (1), 169–229.

Keefer, P., 2007. Beyond legal origin and checks and balances: political credibility, citizen information and financial sector development. World Bank Policy Research WP 4154.

Kiviet, J., 1995. On bias, inconsistency, and efficiency of various estimators in dynamic panel models. Journal of Econometrics 68 (1), 53–78.

Knack, S., Keefer, P., 1995. Institutions and economic performance: cross-country tests using alternative institutional measures. Economics and Politics 7 (3), 207–227.

Kornai, J., 1994. Transformational recession: the main causes. Journal of Comparative Economics 19 (1), 39–63.

Krasner, S.D., 1993. Approaches to the state: alternative conceptions and historical dynamics. Comparative Politics 16 (2), 223–246.

Levin, A., Lin, C., Chu, C., 2002. Unit root tests in panel data: asymptotic and finite-sample properties. Journal of Econometrics 108 (1), 1–24.

Levine, R., Loayza, N., Beck, T., 2000. Financial intermediation and growth: causality and causes. Journal of Monetary Economics 48, 31–77.
Merlevede, B., 2003. Reform reversals and output growth in transition. Economics of Transition 11 (4), 649–667.
Murrell, P., 1996. How far has the transition progressed? Journal of Economic Perspectives 10 (2), 25–44.
Nickell, S., 1981. Biases in dynamic models with fixed effects. Econometrica 49 (6), 1417–1426.
North, D., 1990. Institutions, Institutional Change and Economic Performance. Cambridge University Press, Cambridge.
Norris, P., 1997. Choosing electoral systems: proportional, majoritarian and mixed systems. International Political Science Review 18 (3), 297–312.
Persson, T., 2004. Consequences of constitutions. Journal of the European Economic Association 2 (1), 139–161.
Persson, T., 2005. Forms of democracy, policy and economic development. NBER Working Paper 11171.
Persson, T., Tabellini, G., 2007. Democratic capital: the nexus of political and economic change. Working Paper, version of March 2007.
Persson, T., Roland, G., Tabellini, G., 2000. Comparative politics and public finance. Journal of Political Economy 108 (6), 1121–1161.
Popov, V., 2007. Shock therapy versus gradualism reconsidered: lessons from transition economies after 15 years of reform. Comparative Economic Studies 49 (1), 1–31.
Rigobon, R., Rodrik, D., 2005. Rule of law, democracy, openness, and income: estimating the interrelationships. Economics of Transition 13 (3), 533–564.
Rodrik, D., Subramanian, A., Trebbi, F., 2004. Institutions rule: the primacy of institutions over geography and integration in economic development. Journal of Economic Growth 9 (2), 131–165.
Roland, G., 2000. Transition and Economics: Politics, Markets and Firms. MIT Press.
Roland, G., 2004. Fast-moving and slow-moving institutions. CESifo DICE Report, pp. 16–21.
Roland, G., 2005. After enlargement: institutional achievements and prospects in the new member states. Proceedings of the Third ECB Conference "The New EU Member States: Convergence and Stability".
Selowsky, M., Martin, R., 1997. Policy performance and output growth in the transition economies. American Economic Review 87 (2), 349–353.
Wacziarg, R., Welch, K., 2008. Trade liberalization and growth: new evidence. The World Bank Economic Review 22 (2), 187–231.
Williamson, O.E., 2000. The new institutional economics: taking stock, looking ahead. Journal of Economic Literature 38 (3), 595–613.
Wolf, H.C., 1999. Transition strategies: choices and outcomes. Princeton Studies in International Finance No. 85.