Policy Reform Vol. 3 pp. 121-137
Reprints Available directly from the publisher
Photocopying permitted by license only

The Relationship between Economic Growth and the Speed of Liberalization During Transition

Berta Heybey and Peter Murrell*

Department of Economics and IRIS Center, University of Maryland, College Park, MD 20742, USA

(Received 1 December 1997; Revised March 1998; In final form May 1998)

We examine existing results on the effect of speed of liberalization on growth during transition. We highlight methodological problems in existing studies, noting the existence of simultaneity and the use of variables that are not valid measures of the phenomena they supposedly represent. We implement solutions, examining the simultaneous relationship between growth and speed of liberalization. Initial conditions are much more important than policy changes in determining growth performance in the first four years of transition. Growth performance during the early years of transition has a strong effect on liberalization speed.

Keywords: Transition strategies; Economic growth; Economic reform; Liberalization speed; Eastern Europe; Former Soviet Union

Classification Categories: P21, P27, P51, O57

*Corresponding author: e-mail: murrell@econ.umd.edu

1. Introduction

Given the novelty of the transition from socialism and its importance for world economic development, it was inevitable that there would be flourishing debates concerning the advisability of different approaches to economic reform (e.g. Lipton and Sachs 1990 and Murrell 1992.) Just as surely, there would come a time when economists would be able to use data derived from the experience of reform to assess the positions taken in these debates. The past few years have witnessed a burgeoning set of papers assessing the effectiveness of diverse reform programs.

An important sub-set of these papers focuses on the general character of reforms: if one could characterize the tenor of reforms with a one-dimensional variable, what value of that variable would be an optimal choice? Obviously, addressing this question entails the adoption of a specific measure of the tenor of reforms. In the empirical literature, the speed of reforms has garnered the most attention, despite many misgivings concerning the validity of this variable in capturing underlying theories. The influential *World Development Report* has cemented this focus on speed, by placing this variable at the center of its discussion of transition (World Bank 1996: 9-10). In giving qualified empirical support to the hypothesis that increased speed produces better results, the *Report* is representative of a number of recent studies.

The purpose of the present paper is to take stock of the evidence on the relationship between reform speed and economic growth during transition. We examine existing studies of that relationship, pointing out common methodological problems. We then present our own econometric results, which place a question mark over the conclusions of previous studies.

Because this paper has the limited goal of assessing existing conclusions, we accept without question the pertinent literature's focus on the speed of reforms. We also follow existing studies in their focus on liberalization and we employ the data that existing studies use. Our purpose is not to quibble with these aspects of existing studies, but rather to question the conclusions of the studies using the general framework in which these conclusions were developed.

The analysis begins in Section 2 with a critical examination of existing studies. We suggest that there are five important methodological problems in these studies, problems that might cause estimates to be biased or coefficients to be misinterpreted. In Section 3, we examine how these problems might be solved and implement solutions, presenting estimates of the simultaneous relationship between growth and reform speed. These esti-

mates are sufficiently different from those in previous studies that we conclude that the methodological problems in the existing studies are significant. We find no evidence that speed of reforms affects growth during the first four years of transition, while we do find evidence of the inverse relationship: growth affects the speed of reforms. Initial conditions are important determinants of both reform speed and growth.

2. A Review of Existing Empirical Studies

2.1 Measuring liberalization

Liberalization is at the heart of post-socialist reforms and it can be implemented very early in the transition process. Therefore, many existing studies choose to proxy speed of reforms by examining measures of liberalization. Construction of these measures necessitated a special data collection effort, which was undertaken by de Melo, Denizer and Gelb (1996a; henceforth DDG) in connection with the writing of the *World Development Report.* In consultation with country specialists, DDG assembled data on the level of liberalization in three areas: internal markets, external markets, and private sector entry. Their data set provides annual measures on these three components for 28 transition economies from 1989 to 1995.

The measure of internal market liberalization reflects domestic price liberalization and progress in abolishing state trading monopolies. The external market measure assesses the liberalization of the foreign trade regime, including currency convertibility and the elimination of export controls and taxes and substitution of low to moderate import duties for import quotas and high import tariffs. Finally, the score on private sector entry reflects the openness of the economy to private sector development.[1]

For the years 1989-95, DDG rated each country in each of the three areas on a scale from zero to one, with zero representing an unreformed Soviet-style economy and one representing a "basically-reformed" economy. To construct a composite annual liberalization index,[2] DDG calculated a weighted average of the three components, with the weights based on "notional estimates of the relative impact of [internal markets] and [external markets], which represent liberalization through introduction of competitive, flexible-price markets, and [private sector entry], which represents liberalization through changing ownership of fixed assets." (DDG 1996a: 6) The weights given to internal and external markets were 0.3 each, while private sector entry received a weight of 0.4.

DDG also created a cumulative liberalization index (*CLI*), which in any year is the sum of all previous annual liberalization indices from 1989 on. The *CLI* is intended to measure the cumulative effects of the liberalization process, the rationale for its use being that "...economic performance at any given moment is determined not only by the degree of liberalization currently prevailing but also by the structural, institutional, and behavioral changes stimulated by prior policy reforms." (DDG 1996b: 404) This logic justifies the use of the *CLI* as a determinant of growth. But the question that then arises is whether the *CLI* can be interpreted as a valid measure of the speed of reforms.[3]

The transparent answer to this question is no, despite the fact that a majority of the studies examined below interpret the *CLI* as a measure of the speed of reforms. To see this point vividly, suppose that we added the USA to our sample of countries. In 1989, the USA was already a "basically reformed economy," and presumably would have scored at the highest levels on the three measures of liberalization. Moreover, the USA remained at these high levels from 1989 to 1995. Therefore, the USA would have scored far higher on the *CLI* than would any transition country. Does this mean that the *speed of reforms* in the USA has been greater than in any of the transition countries? Obviously not. Even tardy Tajikistan and unhurried Uzbekistan have seen far greater changes in their economic policies in the 1990's than has the USA.

There are two major problems in using the *CLI* when examining hypotheses on the relationship between speed of reform and growth. First, use of the *CLI* glosses over the distinction between policy and policy changes. The three disaggregate liberalization measures discussed above do not reflect changes in the policy stance, but rather the state of policy. Thus, a country such as Slovenia, which started reforms in the 1950's, had high levels of the three liberalization indices in 1989, and therefore the *CLI*, even though post-socialist reforms in that country have not been particularly speedy.

Secondly, the *CLI* cumulates the liberalization measures over time, from 1989 onwards, introducing a further problem in trying to discern the effects of speed of reform. While communism fell in some countries in 1989, it lasted until 1991 in others. Since communist regimes severely limited the scope of economic reforms, one group of countries (those formerly in the USSR) would have a lower score on the *CLI* in 1995 simply because they were captives of communism until 1991. This makes the *CLI* a proxy for many other factors, such as being a former republic of the USSR or having had a less hard-line communist regime in the 1980's. Since these factors

are plausibly related to growth performance, but none of these factors reflects speed of reforms, the *CLI* will reflect much more than the influence of speed.

2.2 Previous empirical studies

DDG examine twenty-six transition countries, including those in Central and Eastern Europe (CEE) and the former Soviet Union (FSU.)[4] They regress average real GDP growth over 1989-94 (*AVGR*) on the *CLI* in 1994 (*CLI*), per capita income in 1989 (*PCY*), and a dummy variable for economies that have been affected by severe regional tensions during the reform period (*RT*). They obtain the following result:

	$AVGR = -9.1 + 2.6CLI - 0.54PCY - 6.5RT$				Adjusted R^2=0.65
(t-statistics)	(-5.4)	(4.7)	(-1.9)	(-4.8)	Number of observations = 26

DDG (1996a: 9) state that these results suggest that "cumulative liberalization has positive association with output changes in CEE/FSU, within the overall context of a 'transitional recession'." This study was the primary input into the *World Development Report*, which concluded that speedy liberalization usually allows countries to grow faster, blurring the distinctions between policies and policy changes and between cumulative and non-cumulative measures.

Apart from the already identified problems in interpreting the effect of the *CLI*, there are three further problems in the formulation of the above regression model. First, while policy changes affect growth, it is also possible that growth might affect policy changes. A country that had a relatively small output decline in the early years of transition might well reform faster in later years if the good fortune is popularly attributed to the reforms. In a regression summarizing the interaction between growth and policy over five years, it is surely possible that there can be two-way causation. The regression results reported above might suffer from simultaneous-equations bias.

Second, such a bias could be compounded by the omission of important independent variables, those reflecting the initial conditions of the countries. One such initial condition is the proportion of trade within the CMEA, which is directly related to costs of adjustment and therefore inversely related to growth. For obvious reasons, countries closer to Western Europe conducted a lower proportion of trade within the CMEA. These countries also entered the post-socialist era earlier and hence the *CLI* is higher for

them, because the *CLI* is cumulated from 1989 onwards. Hence, in the above formulation, the coefficient on *CLI* will likely be upwardly biased because the *CLI* is correlated with an omitted initial condition.

Third, DDG use the same calendar years for all countries in the sample, implying that different proportions of the post-communist economic cycle are included in observations for different countries. When such data are used, the *CLI* is correlated with the calculated growth rate, since both the *CLI* and growth are correlated with the number of years of post-communism experienced by the country. (The *CLI* is correlated with the number of years because of cumulation; growth is correlated with the number of years because later-reforming countries will still be in the trough of the recession at the end of the sample period.) A positive coefficient on *CLI* might simply be an artefact of the timing of data collection rather than any significant economic phenomenon.

Åslund, Boone, and Johnson (1996; henceforth ABJ) also use the *CLI* in their analysis of the relationship between liberalization and economic growth. Regressing percentage change in output from 1989 to 1995 (ΔOUT) on the cumulative liberalization index (*CLI*), a dummy for the ruble zone (*RUBLE*), and a dummy for a war-torn country (*WAR*), they obtain:

$$\Delta OUT = 0.84 + 0.0\,CLI - 0.32\,RUBLE - 0.19\,WAR \qquad \text{Adjusted } R^2 = 0.74$$

(t-statistics) (6.46) (0.0) (-4.0) (-3.17) Number of observations = 24

ABJ (1996: 233) find that "there is no robust significant correlation between output change and any measure of reform." Of course, this conclusion cannot be derived from the above regression in any simple way.[5] The regression does not contain a variable that could be called a "measure of reform." ABJ's interpretation of the *CLI* as an indicator of reform is vitiated by the presence of the problems in the use of this variable that we have identified in Section 2.1 above. Moreover, even if the *CLI* were a valid measure of reform, the estimate of its coefficient would still suffer from the biases that we have identified above for DDG.[6]

A third formulation of the liberalization-growth relationship can be found in Sachs and Woo (1997). They regress average annual growth rates between 1989 and 1995 (*AVGGR*) on log of 1989 income (*LINC*) and an index of reform constructed by the EBRD (*RINDX*). The EBRD index covers more activities than those included in the *CLI*, assessing the progress of reform in nine areas including changes in the enterprise sector, markets, foreign trade, financial institutions, and law (EBRD 1995: 11). The index varies from a minimum of 10 in Turkmenistan to a maximum of 33 in the Czech Republic and Hungary. Sachs and Woo obtain the following results:[7]

	AVGGR= -0.024 - 0.029*LINC* + 0.007*RINDX*				Adjusted R^2=0.35
(t-statistics)		(-0.13)	(-1.07)	(3.43)	Number of Observations = 25

Since the EBRD index "assesses the status rather than the pace of change" (EBRD 1995: 11), there is no warrant for Sachs' and Woo's (1997: 8) conclusion that "faster reform was associated with higher, not lower, growth." No variable in the regression validly measures the speed of reform. This regression also has the aforementioned potential problems of simultaneity bias and the omission of variables characterizing initial conditions, especially those that capture differences between CEE and the FSU, which will be correlated with *RINDX*.

Fischer, Sahay and Végh (1996a; henceforth FSV) estimate cross-section time-series regressions for 25 countries of CEE and the FSU, using fixed effects models. They examine the period 1992-94, excluding prior years because "macroeconomic policy as commonly understood in market economies simply did not exist in more than half the countries before 1992." (FSV 1996a: 60). But this procedure itself could bias results, since the early years of income decline in the CEE are not used, and these are the years when the *CLI* is smaller, by definition. They regress annual growth rates of real GDP (*AVGGR*) on the *CLI*, a dummy variable representing whether the exchange rate was fixed or flexible (*FIXED*), the fiscal balance of the government (*FISCAL*), and dummy variables for each country, obtaining the following result:[8]

	AVGGR= 7.42*CLI* + 11.35*FIXED* + 0.3*FISCAL*				Adjusted R^2=0.55
(t-statistics)		(3.54)	(2.0)	(1.42)	Number of observations =75

The authors' conclusion is that the "state of market-oriented reforms, as reflected in the liberalization index *CLI*, appears to have been critical in spurring growth." (FSV 1996a: 61.) As with the other cited studies, this verbal interpretation of the results does not seem warranted given that the *CLI* reflects much more than the "state of market-oriented reforms." By using country dummies, the FSV regression probably controls for initial conditions.[9] But the problem of simultaneity is very much present, as Fischer, Sahay and Végh (1996b) acknowledge in a further paper, which reports similar results. They suggest that the "results should be viewed as a way of describing the data, rather than reflecting deep structural relations." (FSV 1996b: 231)

Krueger and Ciolko (1996) explicitly focus on the effect of including initial conditions in the growth-reform relationship. They examine 18 countries in CEE and the FSU, regressing an index of output in 1995 relative to

output in 1989 (*ΔOUT*) on the *CLI*, a war-torn dummy (*WAR*), the initial share of total exports in national income (*EXP*), the level of GNP per capita in 1987 (*GNP87*), and the share of agriculture in total GNP (*AG*). The export share "is designed to capture 'distortions due to central planning,' while the latter two variables are added to capture relative backwardness and 'reformability'." (Krueger and Ciolko 1996: 4) The results of a regression comparable to those in DDG are:

$$\Delta OUT = 51.97 + 4.01\,CLI - 10.80\,WAR + 91.49\,EXP - 24.61\,AG - 89.27\,GNP87 \quad \text{Adjusted } R^2=.71$$
(t-statistics) (6.52) (1.18) (-1.37) (2.22) (.45) (-2.02) Number of Obs.= 18

Krueger and Ciolko (1996: 6) note that the "*CLI* loses its explanatory power...[when] plausible proxies for initial conditions are added", making a strong case that the inclusion of initial conditions significantly alters the nature of the results emanating from such cross-section regressions.

2.3 Recapitulation

We have identified five basic problems in either the formulation of the set of regressions or the interpretation of the regression results:

1. Some regressions do not include initial conditions, thereby suffering from standard omitted-variables bias, which, we have argued, will be particularly important for the estimate of the coefficient on the *CLI*.
2. Using the *CLI* (or even its first difference, the DDG liberalization index) to represent reform obscures the effect of speed of reforms, since it is not possible to distinguish between the effect of policy and the effect of policy change.
3. The cumulation intrinsic in the *CLI* further obscures the effect of speed, especially because this cumulation ensures that the *CLI* is inversely correlated with such initial conditions as the importance of the CMEA in a country's trade relations.
4. Speed of reform could well be an endogenous variable.
5. Using the same calendar years for all countries in the sample implies that different phases of the post-communist economic cycle will be included in observations for different countries. Then, the *CLI* (or its first difference, the DDG liberalization index) will be correlated with the average yearly growth rate simply because both the *CLI* and growth are correlated with the number of years of post-communism. This will cause an upward bias in the estimate of the coefficient on the *CLI*.

3. Estimating a Model Relating Speed of Reform and Growth

In this section, we formulate and estimate a model of the relationship between speed of reform and growth, pursuing a methodology that avoids the pitfalls described above. Insofar as is possible given our different methodology, we use the same data and the same set of countries as employed by the papers cited above, in order to preserve the comparability of results.

3.1 The Equations

In estimating our model, we take into account each of the problems identified above. First, we use variables capturing initial conditions. Second, we distinguish between policy and policy changes by employing separate variables to measure initial liberalization levels and the rate of change of liberalization. Including the beginning level of liberalization as an initial condition allows us to distinguish between the effects of two different sets of reforms, post-communist and those undertaken in the socialist era. This procedure also enables us to avoid the third problem referred to above, the problem of interpretation caused by cumulation of the liberalization measure.

We address the endogeneity of economic growth and liberalization speed by estimating a system of two equations. By using three-stage least squares for estimation, we avoid biases that might arise from two possible routes of endogeneity. There is the obvious channel, the possible two way causation between growth and speed. Additionally, there is the problem of simultaneity that might result from the omission of unmeasured variables that independently affect both speed and growth. When such variables are omitted, both growth and speed must be treated as endogenous for the purposes of estimation, even if causation between these variables flows in only one direction.

Finally, we employ variables that are defined over the same number of years of post-communism for each country. Therefore, if there is a natural recession-followed-by-recovery cycle after the fall of communism, we avoid any bias that might result from ignoring the effects of such a cycle. All 26 countries in Eastern Europe, the former Soviet Union, and Mongolia had experienced at least four years of post-communist reform by the end of 1995 (the last year for which the DDG liberalization measures are available). Therefore our regressions reflect four years' experience for each country, beginning in the first year in which post-communist reforms were possible in each country.[10]

The first equation of the two in the model focuses on the determinants of growth:

$$GROWTH = \beta_0 + \beta_1 SPEED + \beta_2 L_0 + \beta_3 CMEA + \beta_4 PREGDP + \epsilon$$

GROWTH is average annual real GDP growth over the first four post-communist years. (Table 1 presents definitions of variables, the sources of data, and some basic statistics.) *SPEED* measures the speed of reform during those same four years. It is constructed using the three disaggregate liberalization indices developed by DDG. The (aggregate) liberalization index in any year is calculated as the mean of the internal, external, and private sector indices.[11] *SPEED* equals one-fourth of the difference between the value of the liberalization index in the fourth year of post-communist reform and the value of the liberalization index in the last year of the old regime (L_0), capturing the extent of policy change. L_0 is also included in the equation, because the degree of liberalization at the beginning of post-communist reforms is expected to affect growth performance.

Initial conditions are captured in two variables. Following the studies reported above, we use exports to CMEA member countries as a percentage of GDP at the beginning of reforms (*CMEA*), to capture the effects on trade of the break up of the old institutional mechanisms.[12] Given that there are few degrees of freedom, there is a need to capture a variety of initial conditions with a summary variable that indicates a country's growth potential. We do this with *PREGDP*, average real GDP growth in the last two years of the old regime. Lastly, ϵ is an error term.

In order to allow for the possibility of simultaneity, we formulate an equation describing the determination of speed, which is a subject that has received much less attention in the literature than has the determination of growth. We use the following formulation:

$$SPEED = \gamma_0 + \gamma_1 GROWTH + \gamma_2 L_0 + \gamma_3 IND_0 + \gamma_4 POL_0 + \eta$$

GROWTH is included in the equation on the assumption that early growth performance might affect willingness to reform. L_0 captures the effects of the initial level of policy on further reforms. The initial share of industry in GDP (IND_0) reflects the influence of the level of industrial development on reforms, especially since a bloated industrial share is likely to capture the drain on the economy from an unreformed state enterprise sector. The measure of political freedom immediately after the fall of communism (POL_0) is included because there is probably a popular demand for quick action, which can only be ignored by politicians who do not face re-election pressures. η is an error term, which could be correlated with ϵ.[13]

TABLE 1 The Variables, Sources, and Basic Statistics

Variable	*Definition*	*Source*	*Mean*	*Variance*
GROWTH	Average annual real GDP growth over the first four years of reform.	World Bank (1995)[a]	-10.82	32.9
SPEED	Variable describing the speed of reform over the first four years of transition. Liberalization t years (L_t) after the beginning of transition is the mean of the three separate liberalization indices (internal, external, and private sector) developed by de Melo, Denizer and Gelb. Speed is (L_t - L_0)/4.	de Melo, Denizer and Gelb (1996)	.13	.003
L_0	Level of liberalization in the last year of communism.	de Melo, Denizer and Gelb (1996)	.18	.04
PREGDP	Average real GDP growth in the two years prior to reforms.	World Bank (1995)[a]	-4.01	24.81
CMEA	Share of exports as a percentage of GDP which were traded with CMEA member countries at the beginning of reform.	Fischer, Sahay and Vegh (1996a); United Nations (1992); World Bank (1995)[a]	.20	.01
IND_0	Industrial share of GDP in the year prior to reform.	World Bank (1995)[a]	.43	.01
POL_0	Level of political freedoms, as measured by the Freedom House Index, in the year prior to reform. Measured on a scale of 1-7, with 7 representing full political freedoms.	Freedom House (1995)[a]	4.96	2.68

[a] For the World Bank and Freedom House, data drawn from several years of these annual publications; the cited year refers to the reference list.

3.2 The Estimates

We estimate the model using three-stage least-squares. We use three-stage least squares, rather than single-equation instrumental variable techniques such as two-stage least squares, in order to obtain estimates that allow for the possibility of correlation between η and ϵ.

The first set of results, presented as Model 1 in Table 2, corresponds exactly to the equations presented above. Although the results are self-explanatory, some prominent features deserve comment. The literature summarized above assumed that speed affects growth, but that growth does not affect speed. Our estimates indicate the converse. Growth performance during the early years of transition has a strong effect on liberalization speed. With such a small sample and with variables that are measured with some error, it would be inappropriate to set great store by this result. Rather, the most important implication is that single equation estimates of the effect of speed of reform on growth are likely to be misleading.

We also find that the error terms in the two equations are significantly correlated, with a negative sign. This result also leads one to doubt the accuracy of single equation estimates of the determinants of growth in transition countries. The negative correlation indicates the omission of variables that affect speed and growth in opposite directions. It is not difficult to think of such variables. For example, a country experiencing difficult diplomatic relations with Russia might be persuaded to reform faster to cement ties with other parts of the world, while suffering a steeper decline in GDP due to the precipitate break in trade relations.

The coefficients on the initial conditions all have the expected signs. A high level of liberalization at the beginning of reform leads to higher growth during the first four years of reform, as does a higher pre-reform growth rate. Countries that had high levels of dependence on the CMEA grew more slowly. Hence, initial conditions do have a substantial effect on economic growth during transition, as noted by Krueger and Ciolko (1996). Indeed, the effect of initial conditions seems to be much more important than that of policy changes.

One issue that has arisen in the debates on reform policy is whether there might be a non-linear relationship between speed and growth. Some scholars argue that there is a critical mass of reforms that must be instituted in order to have an effect and others claim that there are diminishing returns to speed. Therefore we test two alternative models: we replace *SPEED* by the logarithm of *SPEED* in Model 2 and by the square of *SPEED* in Model 3.[14] There is virtually no change in the qualitative character of the results as a consequence of using either of these two non-linear variables, hence underscoring our previous conclusions.

TABLE 2 Estimates of Models Relating Speed of Liberalization and Growth

Dependent Variable / *Independent Variables*	*Model 1* GROWTH	*Model 2* GROWTH	*Model 3* GROWTH
Constant	-7.59 (-1.09)	-4.03 (-0.66)	-7.74 (-1.27)
SPEED	12.79 (0.36)	—	—
Ln *SPEED*	—	0.79 (0.23)	—
SPEED-squared	—	—	71.63 (0.44)
L_0	10.38 (1.57)	9.95 (1.52)	10.94 (1.59)
PREGDP	0.70*** (3.67)	0.74*** (4.15)	0.67*** (3.08)
CMEA	-19.91*** (-2.69)	-19.90*** (-2.86)	-18.83** (-2.23)
Adjusted R-squared	0.63	0.64	0.61
Dependent Variable / *Independent Variables*	*SPEED*	*Ln SPEED*	*SPEED-squared*
Constant	0.147*** (3.44)	-4.03** (-2.19)	0.038** (2.20)
GROWTH	0.006*** (2.97)	0.05*** (2.78)	0.002*** (2.84)
IND_0	0.13* (1.79)	1.22* (1.85)	0.32 (1.55)
L_0	-0.20*** (-4.43)	-2.04*** (-5.13)	-0.05*** (-3.61)
POL_0	0.008 (1.43)	0.104 (2.00)	0.001 (0.83)
Adjusted R-squared	0.48	0.56	0.36
Correlation between η and ϵ	-0.64** (-2.23)	-0.50 (-0.67)	-0.73*** (-4.35)

t-statistics in parentheses

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

4. Conclusions

This paper is a methodological note on the interpretation of existing results on the effects of speed of liberalization on growth performance during transition. In addition to highlighting the methodological problems in existing studies, we have made tentative attempts to solve these problems and to ascertain the relationship between speed and growth. Our results suggest that single equation methods will give misleading results. Existing studies that have attributed importance to the speed of liberalization have probably over-estimated that importance.

Our results indicate that initial conditions are much more important than policy changes in determining growth performance in the first four years of transition. The *CMEA* variable and the one measuring previous growth performance (*PREGDP*) are highly significant determinants of growth. Initial conditions are also important as determinants of the speed of liberalization, especially the initial level of liberalization, higher levels of which seem to dampen subsequent reform efforts. But given the small sample and the likely errors in measurement, we believe that these results should be treated as having a substantial degree of error. The main effect should be to cast a cautionary note over the interpretation of the results of the existing studies cited above.

Finally, the reader might ask whether we view these results as indicating that decisions regarding liberalization policy have no effects on growth. We certainly do not draw this conclusion. The insignificant coefficient on *SPEED* probably reflects two opposing effects, the substantial gains from liberalization of entry of new firms and the costs of dislocation in the existing state sector (Murrell 1992). Over time, the costs of dislocation will diminish in importance and the gains will still flow from new entry, meaning that liberalization of the economy will have a positive effect.

Acknowledgment

We gratefully acknowledge the support of the U.S. Agency for International Development under Cooperative Agreement No. DHR-0015-A-00-0031-00 to the Center on Institutional Reform and the Informal Sector (IRIS). We would like to thank Jennifer Cashdan, Alan Gelb, Ratna Sahay, and Stoyan Tenev for sharing their data with us.

References

Åslund, Anders, Peter Boone, and Simon Johnson. (1996) "How to Stabilize: Lessons from Post-communist countries," *Brookings Papers on Economic Activity* 1:1996, 217-313.

de Melo, Martha, Cevdet Denizer, and Alan Gelb. (1996a) *From Plan to Market: Patterns of Transition*, World Bank Policy Research Working Paper No. 1564, January.

de Melo, Martha, Cevdet Denizer, and Alan Gelb. (1996b) "Patterns of Transition from Plan to Market," *World Bank Economic Review* 10:3 397-424.

de Melo, Martha, Cevdet Denizer, and Alan Gelb and Stoyan Tenev. (1997) *Circumstances and Choice: The Role of Initial Conditions and Policies in Transition Economies*, World Bank Policy Research Working Paper No. 1866, December.

European Bank for Reconstruction and Development. (1995) *EBRD Transition Report 1995*, EBRD. London.

Fischer, Stanley, Ratna Sahay, and Carlos A. Vegh. (1996a) "Stabilization and Growth in Transition Economies: The Early Experience," *Journal of Economic Perspectives* Spring 45-66.

Fischer, Stanley, Ratna Sahay, and Carlos A. Vegh. (1996b) "Economies in Transition: The Beginnings of Growth," *American Economic Review* May: 229-33.

Freedom House. (1995) *Freedom in the World: The Annual Survey of Political Rights and Civil Liberties*, Freedom House. New York.

Ickes, Barry. (1996) "Comment," *Brookings Papers on Economic Activity* 1:1996 298-305.

Krueger, Gary and Marek Ciolko. (1996) "A Note on Initial Conditions and Liberalization During Transition," Paper prepared for The National Council for Soviet and East European Research.

Lipton, David and Jeffrey Sachs. (1990) "Creating a Market Economy in Eastern Europe: The Case of Poland," *Brookings Papers on Economic Activity* 1:1990 75-147.

Murrell, Peter. (1992) "Evolution in Economics and in the Economic Reform of the Centrally Planned Economies," In Christopher C. Clague and Gordon Rausser, eds., *Emerging Market Economies in Eastern Europe*, Basil Blackwell. Cambridge, MA.

Sachs, Jeffrey D. (1996) "The Transition at Mid Decade," *American Economic Review* May: 128-133.

Sachs, Jeffrey and Wing Thye Woo. (1997) "Understanding China's Economic Performance," Harvard University, May.

Selowsky, Marcelo and Ricardo Martin. (1997) "Policy Performance and Output Growth in the Transition Economies," *AEA Papers and Proceedings* May: 349-353.

United Nations Economic Commission for Europe. (1992) *Economic Survey of Europe*, United Nations. New York.

Wolf, Holger C. (1997) "Transition Strategies: Choices And Outcomes," Unpublished manuscript, New York University.

World Bank. (1995) *World Tables 1995*, Johns Hopkins University Press. Baltimore.

World Bank. (1996) *From Plan to Market: World Development Report 1996*, Published for the World Bank by Oxford University Press. New York.

Endnotes

1. The authors use privatization and banking reform as proxies for opportunities for private sector entry. DDG (1996b: 403) acknowledge that these proxies "do not capture the overall quality of the legal and regulatory framework or the effectiveness of government in institution-building or in the implementation of reforms - but only because of the difficulty of developing comparative measures."
2. There is a semantic confusion that is worth mentioning here, since this confusion might be related to some of the misinterpretations of results. Strictly speaking, the word liberalization denotes the process of making an economy more liberal; it does not refer to how liberal policies are at any point. This word might better be reserved to describe the rate of change in the degree of economic freedom. Therefore, the DDG data measure the level of economic freedom, not liberalization, the rate of change of freedom. In this paper, we also use this word in the broader, but less felicitous, sense since the paper concerns itself with the results of other papers that use the word liberalization to refer to levels of economic freedom.
3. See World Bank (1996) Chapter 2 for such an interpretation of the *CLI*.
4. The *de facto* politico-economic status of Mongolia before 1990 implies that this country can be included in the FSU for the purposes of analysis of transition experience. DDG include Vietnam and China in some of their analyses, but we report here only their results for analyses without these countries, for purposes of comparability with the other studies that we examine.
5. In comments on the ABJ article, Ickes (1996) makes similar points.
6. The *RUBLE* variable does go some considerable way toward mitigating the problem of omitted initial conditions.
7. Sachs and Woo (1997) do not definitively state how many observations are in their regression. The number we give is based on our supposition. The calculation of the adjusted R-squared depends upon this supposition. See also the earlier paper by Sachs (1996).
8. The coefficients for country-specific effects are not reported, but (FSV 1996a: 61) write that "country-specific effects turned out to be highly significant (using a likelihood ratio test), indicating that there were some differences across countries that are not captured by the explanatory variables."
9. FSV (1996a: 62) write that: "The results also point to the importance of initial conditions — trade dependence and initial per capita income — in influencing the growth rate during the transition.." This claim is based on a regression that is reported in a footnote in their paper.
10. A table showing the years used is available from the authors on request.
11. We weight all three components equally, while DDG (1996a) place a higher weight on opportunities for private sector entry than on the other two components. However, DDG (1996b: 403) state that, "[b]ecause of strong complementarity among the components, varying their relative weights does not result in substantially different correlations with growth...".
    One could also imagine using all three liberalization indices in the regressions. However, the three are so inter-correlated that there is little gain from doing so given the limited number of degrees of freedom. For our sample of 26 countries, all correlations between the three measures are significantly positive at the 0.05% level. The lowest correlation is 0.64.
12. For Bulgaria, the Czech Republic, Hungary, Poland, Romania and Slovakia, the data on *CMEA* are for 1989, the year prior to reform. For other countries,

the data are for 1990, which is either the year prior to reform, or two years prior to reform.

13. In this paper, we undertake only a cross-section analysis based on data aggregated over the first four years of reform. Panel data could also be used to gain a more nuanced picture of the year-by-year changes that occur during the same period. For example, in the case of reversals of early reforms, cross-section data might obscure important elements of a country's experience. Our purpose in using cross-section results in this paper is not to argue their superiority over panel results, but rather to match the majority of existing studies as closely as we can.
    The cited papers of FSV (1996a, 1996b) use panel data as do Selowsky and Martin who estimate a model focusing on the effects of liberalization and initial conditions on growth, for 25 transition economies for the years 1990-95, concluding that " ... policy reforms affect output growth over an extended period..." (1997: 351). de Melo, Denizer, Gelb and Tenev (1997) estimate several models using panel data, employing principal components analysis to focus on the effects of initial conditions, concluding that initial conditions have significant effects on both growth and liberalization. Wolf (1997) concurs, concluding as well that initial conditions and reform policy choices have an endogenous relationship.
14. In the estimates presented in Table 2, we have replaced SPEED by its non-linear transformation in both equations and then estimated the system by using linear three-stage least squares. There is an alternative approach that we could have used. One might leave the un-transformed SPEED as the dependent variable in the second equation, while using the non-linear form of the variable in the first equation, and then estimate the whole system by non-linear three-stage least squares. In fact, we tried this alternative approach and it produces the same qualitative results as are reported in Table 2. Therefore, we do not report the results for the alternative estimation.