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Good Estimation or Good Luck?
Growth Accelerations revisited

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Abstract

This dissertation contributes to the good policy versus good luck debate by updating Hausmann et al. (2005), a seminal article that seeks to identify significant determinants of growth accelerations. Based on the evidence from the replication, the dissertation argues that the results in Hausmann et al. (2005) are fragile to changes in sample and alternative measures: Out of the 83 growth accelerations originally identified, only 45 are found robust using two updated GDP datasets. External shocks are not significantly associated with growth accelerations but tend to lower average growth. Changes in standard policies such as investments or population are not robustly associated with accelerations at all. If any robust evidence is found, it is that economic reforms are correlated with sustained accelerations, while negative regime changes are associated with any type of accelerations. Given the failure to fully replicate the original results, the conclusion cautions that recent results focusing on turning points might be more good luck than good estimation.

Good estimation or good luck?
Growth accelerations revisited

By Candidate 25509

1 Introduction

This dissertation contributes to the good policy versus good luck debate by updating Hausmann et al. (2005), a seminal article that seeks to identify significant determinants of growth accelerations. The original dataset is corrected for significant coding errors and extended from 1992 up to 2000, increasing the sample size by 50%. Based on the evidence from the replication, the dissertation argues that the results in Hausmann et al. (2005) are fragile to changes in sample and alternative measures:

Out of the 83 growth accelerations originally identified, only 45 are found robust using two updated GDP datasets. External shocks are not significantly associated with growth accelerations but tend to lower average growth. Changes in standard policies such as investments or population are not robustly associated with accelerations at all. If any robust evidence is found, it is that economic reforms are correlated with sustained accelerations, while negative regime changes are associated with any type of accelerations. Given the failure to fully replicate the original results, the conclusion cautions that recent results focusing on turning points might be more good luck than good estimation.

The argument is structured as follows: Section I embeds the discussion into the existing literature and stresses some methodological issues of turning point studies. Section II describes the correction and extension of the dataset. Section III replicates the results and allows for minor variations. Section IV summarizes the findings and concludes.

1.1 Low persistence of growth

The motivation for Hausmann et al. (2005) to analyze patterns of growth accelerations bases on the empirical puzzle of volatile growth. As one of the first to note, Easterly et al. (1993) pointed to the cross-decade (linear) correlation of average growth ranging from only 0.21 for 1960-1970 to 0.31 for 1970-1980. This low persistence is contrasted with the high persistence of country characteristics and domestic policies. For example, the cross-decade correlation of the investment ratio, an often used proxy for savings rate in empirical models, is 0.9 for the 1960-1970s and 0.85 for the 1970-1980s: If the best guess for tomorrow's policy is simply today's policy, it is surprising that today's growth performance is not a good predictor of tomorrow's growth performance.

Hausmann et al. (2005) tackle this puzzle "head on" by identifying sudden periods of growth accelerations and examining changes in policies and shocks around the turning points. As such, it is an attempt to either isolate robust relationships between changes in policy and growth trajectory *or* quantify the effect of external shocks on cross-decade volatility. Related studies heavily drawing upon longitudinal data include Pritchett (2000), a descriptive article classifying different patterns of growth and Hausmann et al. (2006), analyzing growth collapses. Notable recent contributions that focus on the effects of external shocks and policies on turning points include Easterly et al. (2000), Ostry et al. (2007), Jones and Olken (2008) and Chauvet and Collier (2008).

1.2 Ideal experiment and real constraints

While finding triggers of growth accelerations is important for policy makers, actually identifying a causal effect of a macro policy is difficult. Despite countless articles, *"there aren't too many policies that we can say with certainty [...] affect growth"*[1]: Levine and Renelt (1992), for example, find that almost all variables are fragile upon inclusion of other regressors. While the Bayesian test in Sala-I-Martin et al. (2004) offered some evidence on robust variables[2], recent contributions such as Jarocinski and Ciccone (2009) suggest that even these results are fragile once alternative GDP data is used.

Ideally, these questions could be addressed by a randomized controlled trial (Banerjee and Duflo, 2008): To disentangle the effect of policies from shocks, one would randomly assign countries to treatment and control groups, and then only manipulate a policy variable in the treatment group. Given the exogenous assignment *ex-ante*, shocks and other unobserved confounds would be balanced across both groups. Any differential in growth performance across groups can then be causally attributed to the treatment.

1 Harbinger in 2003, as cited in Easterly (2009).
2 Such as income per capita, relative price of investment or primary enrollment.

While such macroeconomic experiments are impossible, the growth accelerations article can be interpreted as a pragmatic version of the RCT approach. Similar to an RCT, the strategy in Hausmann et al. (2005) is to isolate effects of policies and shocks by comparing a treatment to a comparison group. The comparison, however, is constrained in several ways: First, there are no exogenously created treatment and control groups. Instead, Hausmann et al. (2005) flag countries with accelerations as "successful" treatments only *after* the acceleration is observed. By doing so, the authors compare countries and periods with growth accelerations to those without. Second, the treatment itself (if any) is unknown and in fact the interest of study. Finally, while the validity in RCTs can be improved by repeating the experiment, the macro analysis is restricted to the number of countries and time periods for which past realizations are available.

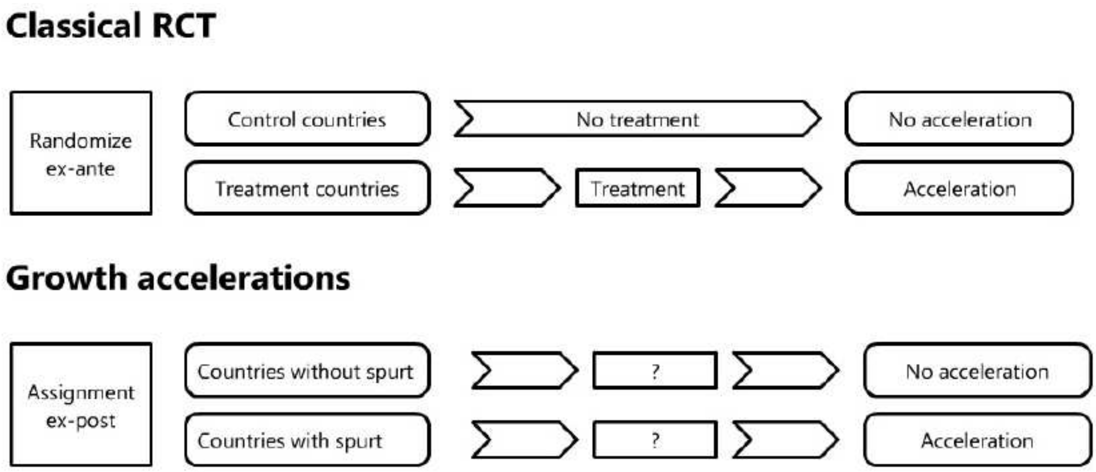

Figure 1: A conventional RCT and the "pragmatic" growth accelerations approach

The limitations bear econometric concerns. When comparing episodes with accelerations to episodes without, a crucial assumption is that both groups are comparable. If the probability of a growth acceleration is related to any other (uncontrolled) differences apart from the (unknown) policy treatment, the estimates will be biased. There are also too many possible factors that could have driven the acceleration, posing a degrees-of-freedom problem when trying to find it (Johnson et al., 2004). Even worse, there are also many ways in which a history confound could interfere in one group following the policy treatment, thus temporarily depressing the acceleration so it is not identified as such *ex-post*. And even *if* a robust relationship was found, policies are endogenous. In other words, turning point studies such as Hausmann et al. (2005) suffer the same methodological issues as typical cross-country regressions, complicating identification.

Deriving strong policy implications based upon (possibly even spurious) correlations, however, is not only irresponsible but simply not rigorous social science: In order to ensure intersubjective testability of empirical results, the need for replication is particularly important. As part of such scientific scrutiny, this dissertation revisits the evidence of Hausmann et al. (2005) by first replicating and then allowing for minor extensions to address a few of the econometric concerns outlined. As most of the subsequent literature bases on variations of the turning point approach, revisiting the seminal evidence is likely to yield useful insights for the other papers, too.

2 Growth accelerations revisited

The main datasets used to construct the dependent variable are the revised PWT 6.3 and Maddison dataset. Before identifying the turning points, the time series are split into 8 year (least squares) average growth episodes. This preparatory exercise itself allows for a quick replication of Easterly's low persistence finding for the most recent episodes: Quickly correlating the results, the linear correlation of 8 year periods is about 0.17 in PWT 6.3 for 1957-2000 and 0.28 in Maddison for 1957-2001. Table 1 reports the correlations once divided into different base decades.

		60	70	80	90	Total
PWT6.3	Correlation	0.15	0.27	0.06	0.03	0.17
	R^2	0.02	0.07	0.004	0.001	0.03
Maddison	Correlation	0.14	0.35	0.23	0.39	0.28
	R^2	0.02	0.12	0.05	0.15	0.08

Table 1: Cross-decade correlation for 8 year periods in PWT 6.3. and Maddison

The stark differences in the 1980s in both datasets are mainly driven by outliers. Rodrik (1999), for example, finds an of 0.12 when predicting average growth between 1975-1989 with growth in 1960-1975: Once the East Asian cases are removed, the drops to 0.04. When Botswana is removed, past period growth loses statistical significance altogether. Plotting the relationship between past 8 period growth and current 8 period growth for 1970-1977 to 1977-84, the same ambiguous pattern emerges in the scatter.

Easterly et al. (1993) interpreted the low persistence of growth despite high persistence of policies as caused by external shocks. Corroborating evidence is found in cross-country regressions, where including shocks improves the explanatory power of the model: The partial of policies is 0.26 against 0.14 for shocks in the 70s and 0.1 against 0.15 in the 80s, with terms-of-trade shocks exerting a large and significant effect on growth: If taken seriously, it might not only take good policy but good luck to grow.

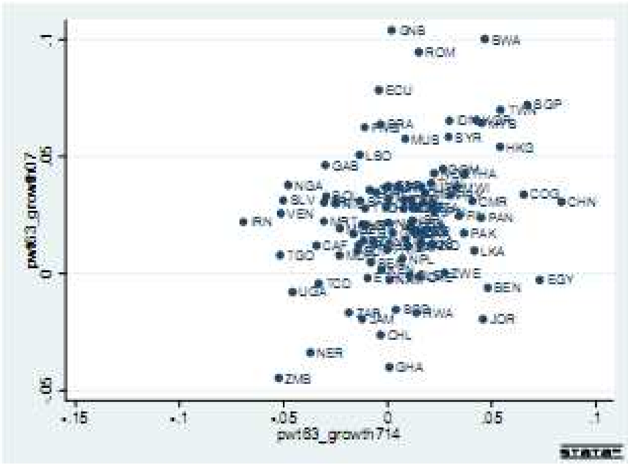

Figure 2: Average PWT 6.3 growth in 1977-1984 (x-axis) against 1970-1977 (y-axis).

The Hausmann et al. (2005) article offers a particularly suitable framework for addressing the policy versus shock debate. Since evidence such as in Easterly et al. (1993) or Rodrik (1999) mostly rely on cross-sectional averages, they do not exploit the country-specific longitudinal variation. In contrast, the growth accelerations approach allows an explicit test of whether turning points are driven by policy changes or external shocks: Concluding their study, Hausmann et al. (2005) find that *"political regime changes are statistically significant predictors of growth accelerations. External shocks tend to produce growth accelerations that eventually fizzle out, while economic reform is a statistically significant predictor of growth accelerations that are sustained."*

Departing from this original conclusion, the robustness of their results will be verified using replication: If quoted relationships are robust, extending the sample and allowing for small modifications should only increase the statistical power of the estimates. Section II proceeds to describe the identification of growth accelerations and the construction of the extended dataset required for the replication exercise in Section III.

2.1 Extending the GDP estimates

Hausmann et al. (2005) identify growth spurts using three criteria. Let denote the least squares average growth rate from *t* to *t+n* and the change in average growth rate at *t* over horizon *n*. By definition, a growth acceleration is given if:

$$g_{t,t+7} \geq 3.5ppa \text{ Growth is rapid} \quad (1)$$

$$\Delta g_{t,t+7} \geq 2ppa \text{ Growth accelerates} \quad (2)$$

$$y_{t+7} \geq \max(y_i), i \leq t \text{ Post-growth output exceeds pre-episode break} \quad (3)$$

A growth acceleration is sustained if the (least squares) average growth in and unsustained otherwise. If several subsequent periods qualify as a growth acceleration, Hausmann et al. (2005) use a structural break test to date the growth acceleration on the year where the test statistic is highest. As a result, their exercise yielded 83 growth accelerations for 110 countries from the PWT 6.1, a "surprisingly large number".

These conditions are applied to the PWT 6.3 and Maddison data. The filter was rewritten and tested on the PWT 6.1 to ensure reliability. While all episodes are found, there are minor discrepancies in dating the onset for subsequent qualifying periods. This is due to the ambiguous definition in the original article, which is interpreted as a Chow test (Chow, 1960). The difference between the onsets, measured by the average standard deviation, is only 0.32 years and there is no reason why the original rule should be more "true" (Jong-A-Pin and Haan, 2008). If the original results are not artefacts of the filter, such small differences should not cause any significant differences in results[3].

Between 1957-2001, 128 growth accelerations were found based on PWT 6.3. Restricted to a comparable time period and set of countries that overlap with PWT 6.1, the number of accelerations is cut to only 49. Re-running the filter with the Maddison dataset, 161 growth accelerations are found between 1957-2001. Limited to a comparable sample, however, the number of acceleration decreases to 40. If the PWT 6.3 is directly compared to the original PWT 6.1, only 40 of the accelerations are exactly matched in both datasets (Appendix 1). If taken seriously, this would suggest that more than half of the original 83 growth accelerations

3 Considerable effort has been put in to reverse engineer the original rule. The authors, however, did not respond to requests regarding the timing rule.

could be artefacts of measurement error.

It is discouraging that such errors even show up after heavy averaging (Johnson et al., 2009)4. For example, the PWT 6.1 identifies Haiti 1990 as a growth acceleration, with an average growth of 12.7% in 1990-1997. Both recent datasets, however, show an actual *negative* average growth of -1.2% (PWT 6.3) and -4.5% (Maddison) throughout the same period. Similarly, the 1973 Chad acceleration was 7.3% in PWT 6.1 but is now revised down to -4.8% (PWT 6.3) and -4.5% (Maddison). While these selective examples comprise the large discrepancies, these measurement errors pose no exception:

To account for these errors, a synthesis of all datasets is used to obtain robust cases: By definition, a growth acceleration is robust if it is identified in more than one dataset. When checking the original PWT 6.1 growth accelerations against those found in the two recent datasets, only 16 accelerations are exactly matched. Because the rewritten filter yielded slightly different results for timing onsets, the definition is relaxed by allowing the onsets to differ by two years $[t-2,t+2]$ from the original acceleration at t. By doing so, the number of robust accelerations for three datasets increases to 45. But since the PWT 6.1 is outdated, a growth acceleration is sufficiently robust if the PWT 6.3 can be matched against the Maddison dataset, allowing for two years difference: This leaves 52 robust accelerations for 1957-1992 and 20 for the extended period 1993-2000.

Finally, a sustained acceleration is robust if the average growth of a robust acceleration is for both the PWT 6.3 and Maddison datasets. While 37 growth accelerations were sustained in the original article, the number is reduced to 12 robust cases within the comparable sample. In total, 26 robust sustained accelerations are identified between 1957-2000: Among accelerations previously excluded from the sustained sample (as it was impossible to know if they would turn out to be sustained), four growth accelerations are robustly found as sustained, Chile 1986, Spain 1984, South Korea 1984 and Malaysia 1988. Two accelerations, Mauritius 1984 and Portugal 1984, previously not even accelerations, turned out to be sustained growth accelerations in PWT 6.3 and Maddison. These changes at the end of the original dataset could significantly twist the results Hausmann et al. (2005) found for sustained accelerations.

4 The authors discuss the fragility of findings upon different revisions and also briefly apply the filter to PWT6.2. The changes identified in PWT 6.3. and Maddison are in line with their argument.

2.2 Identifying growth collapses

In order to symmetrically account for growth accelerations and "decelerations", it is also useful not to neglect the bad performing episodes. In particular, it would be interesting to see if growth accelerations were somehow related to extreme decelerations, as the mean reversion argument suggests (Easterly, 2002). As any conditions to filter growth patterns are to some extent *ad-hoc*, the exact opposite conditions of growth accelerations are applied. By definition, a growth collapse is given if[5]:

$g_{t,t+7}$	$\leq$ *-3.5ppa Growth is strongly negative*	(4)
$\Delta g_{t,t+7}$	$\leq$ *-2ppa Growth collapses*	(5)
y_{t+7}	$\leq max(y_i),\ i \leq t$ *Post-growth output below pre-episode break*	(6)

This exercise is only repeated for the Maddison and PWT 6.3 data. Despite the strict conditions for collapses, PWT 6.3 and Maddison identify 61 and 67 growth collapses for 1957-2000/1. Restricted to a comparable sample, there are 25 (PWT 6.3) and 21 (Maddison) growth collapses, making 15 robust growth collapses. For the extended period, there are five (seven) collapses in PWT 6.3 (Maddison). The three robust growth collapses between 1993-2000/1 are Guinea-Bissau 1996, Kuwait 1994 and Zimbabwe 1999.

There are even sustained growth collapses, where average growth for ten years after the end of the growth collapse still contracts by more than 2%[6]. In PWT 6.3, 9 out of 61 (about 15%) collapses are sustained, whereas it is 7 out of 67 (roughly 10%) for Maddison. Within a comparable sample, four collapses are sustained in the PWT 6.3 dataset and one collapse is sustained in the Maddison. The only three robust sustained collapses are Nicaragua 1975, Iraq 1979 and Niger 1979 (Appendix 2).

There is also a wide range of countries that have experienced both growth accelerations and collapses: In PWT 6.3, there are 23 countries that have had at least one growth acceleration and collapse between 1957-2000. Nigeria, the most extreme case, had three growth

5 Hausmann et al. (2006) develop a different filter to identify collapses. By treating spurts and collapses asymmetrically, however, the results are not directly comparable.

6 This is the analogous definition based on Hausmann et al. (2005)

accelerations and three collapses in only 43 years: A sustained growth acceleration in 1957, followed by a growth collapse in 1962, an acceleration in 1967, collapses in 1975 and 1980 and finally a growth acceleration in 2000. Jordan, with two accelerations and two collapses is another illustrious example. While these pose the extreme cases of low persistent growth, they are usually rare: On average, countries with growth accelerations tend to have less growth collapses, with a linear correlation coefficient between the numbers of accelerations against collapses of -0.41.

		Growth accelerations			Growth collapses		
Decade	PWT6.1	PWT6.3	Mad	Robust	PWT6.3	Mad	Robust
1950	13/12	13/12	24/13	7/6	0/0	1/0	0/0
1960	23/11	29/16	45/20	18/7	3/0	5/0	2/0
1970	23/7	27/8	33/7	11/4	24/6	23/5	12/3
1980	16/7	21/10	16/10	11/9	25/3	27/2	11/0
1990	8/0	29/0	20/1	13/0	8/0	10/0	6/0
2000	na	9/0	23/0	8/0	1/0	1/0	0/0
Total	83/37	128/46	161/51	70/26	61/9	67/7	31
Countries	110	125	137	121	125	137	121

Table 2: Distribution of growth accelerations and growth collapses across decades. Sustained episodes behind "/", i.e. episodes/sustained episodes.

2.3 Extending the regressors

The regressors are extended to prepare the subsequent probit replication. The variables of interest are *tot_thresh90*, *econlib*, *poschange* and *negchange*. *tot_thresh90* is a dummy capturing strong terms of trade changes (defined as being in the highest decile in the sample); *econlib* is a dummy capturing economic reforms, *poschange* and *negchange* capture the direction of regime changes. These variables form the baseline for the original regressions and are meant to proxy the effect of external shock and policy changes. In addition, , *tenure*, *civilwar* and *warend* are included in alternative models to control for additional shocks. All variables are extended up to 2000.

2.3.1 Polity IV

regchange, poschange and *negchange* come from the Polity IV dataset by Marshall and Jaggers (2009). By definition, regime changes are changes in the Polity IV index by at least three unit points. Hausmann et al. (2005), however, have coded *any* change in Polity IV as a regime change, thus interpreting small scale transitions as fundamental changes[7]: For example, Ghandi's interupted rule in 1977, a one unit point change towards democracy, is coded as a positive regime change. Similarly, the takeover of the more liberal leaning Deng after 1976 is a one unit point change towards democracy but coded as a regime change. In addition to these systematic mistakes, there are some (apparently) random miscodings, particularly when regime reversals occured: Given these errors, the Polity IV index has been recoded from scratch to ensure consistency.

A direct comparison of the original and extended index reveals that about 10% of the observations are miscoded. For *poschange*, 263 observations were false positives - a regime change even though there was none - and 52 false negatives - no regime change despite actually being one. Similarly 146 cases were false positives and 47 false negatives for *negchange*. Extending the dataset, there are in overall 55 new regime changes in the extended sample between 1993-2001, 17 negative and 38 positive.

2.3.2 Economic reforms and reversals

econlib is derived from the Sachs and Warner (1995) index for trade liberalization. Albeit used to capture economic reforms, it was originally designed for capturing strong policy changes regarding openess. *econlib* can be easily extended by drawing upon the updated Wacziarg and Welch (2003) which extends the dataset throughout the 1990s.

Comparing the adjusted index with the original index, a few minor discrepancies emerged: For 1957-1992, about 3% of the observations in the original data were coded differently. These differentials are based on a few adjustments done in Wacziarg and Welch (2003), where some changes in openess were timed slightly differently. The good fit, however, should be sufficient to ensure that the extension is consistent with the old data: Overall, there were 92 economic reforms between 1957 and 2000, with 16 economic reforms occuring in the extended period 1993-2000. This increases the large number of economic reforms in the 1990s to 38 (largely driven by the demise of USSR), suggesting that including the 1990s could include some additional leverage.

7 This has also been confirmed and noted by Jong-A-Pin and Haan (2008). However, their corrected index itself had some miscoded observations. Both authors have confirmed this in correspondence.

Because reversals are often a concern in evaluating the success of reforms (Rodrik, 1996), reversals in openess are captured in *adjeconlib_neg*. This variable is 0 if there is no reversal and 1 if the economy shifted from an open to a closed economy. In line with Hausmann et al. (2005), the variable is lagged and 1 for the subsequent four periods after a reversal. This results in 21 economic reversals between 1957-2000. Almost half of these happened during the 1960s. During the extended period, the only country facing a reversal was Venezuela 1993, arguably capturing the 1994 banking crisis.

2.3.3 Terms-of-trade shocks

Among the regressors, *tot_thresh90* was the most difficult to extend due to the poor documentation of its construction. The variable appears to be derived based upon Easterly's terms-of-trade data8, but the article does not explicitly mention the source. As a best guess, the terms-of-trade data from Easterly's GDN Dataset is used, even though the data only begins in 1980. In line with the sparse documentation, every change in terms-of-trade is coded as a shock if it is in the highest decile and lagged by four periods.

When comparing the datasets, however, the reconstruction appears poor: 18% of the observations are coded differently across the variables, with a tendency of the new index to report more shocks than the old variable shows (15% of the miscodings). However, there is also evidence that the old variable had some coding errors: Even though the article reports the inclusion of lags, this does not seems to be the case when examining the data.

Nonetheless, this is a serious problem as an inconsistent extension will complicate commensurability and possibly downward bias the estimated effect of shocks. Despite time-consuming efforts to reverse-engineer the variable, it was impossible to reconstruct a more precise variant. For pragmatic reasons, this variable will be used to extend the time series and the direction of bias will be given attention when interpreting estimates.

Again, to treat the direction of the effects evenly, negative terms-of-trade shocks are computed similarly. When tabulating the effects of the changes, the direction of shocks seem to be rather balanced, with 49% positive shocks and 51% negative shocks.

8 The naming of the file (*etot_thresh90*) bears similarity with variables in Easterly's regressions. The authors did not respond to enquiries regarding the source of the data.

2.3.4 Further variables

Lastly, *lead_death*, *tenure*, *civilwar* and *warend* are extended up to 2000. *lead_death* captures (exogenous) leader deaths and has been constructed by Jones and Olken (2005), who also provide the recent data. This adds 14 leader deaths to the data. *tenure* records the years of tenure before death and is extended based on the same source. *civilwar* and *warend* denote the beginning and ending of a civil war. The data is from the Correlates of War by Singer and Small (2010) and is extended using the updated fourth version. Some descriptive statistics for the new dataset are below in Table 3.

	PWT6.1		PWT6.3		Maddison	
(a) Growth accelerations	57-92	93-00	57-92	93-00	57-92	93-00
Economic liberalization	12.04%	na	8.79%	35%	7.1%	32%
Economic reversal	na	na	2.1%	0%	1.2%	0%
Positive regime change	10.38%	na	6.67%	23.07%	5.5%	25%
Negative regime change	12.98%	na	12.22%	3.84%	15.38%	2.7%
Positive shock	19.5%	na	19%	21.74%	13%	13.3%
Negative shock	9.7%	na	10.71%	30.43%	10.1%	20%
(b) Sustained accelerations	57-92	93-00	57-92	93-00	57-92	93-00
Economic liberalization	16%	na	14%	35%	14%	28%
Economic reversal	2%	na	2%	0%	0%	0%
Positive regime change	10%	na	8%	26%	9%	25%
Negative regime change	8%	na	8%	4%	15%	0
Positive shock	11%	na	17%	21%	14%	13.3%
Negative shock	8%	na	6%	30%	8%	20%
(c) Growth collapses	57-92	93-00	57-92	93-00	57-92	93-00
Economic liberalization	na	na	1%	0%	1.2%	4.4%
Economic reversal	na	na	1%	0%	1.2%	0%
Positive regime change	na	na	4.4%	9%	2.2%	8.8%
Negative regime change	na	na	3.3%	0%	2.2%	0%
Positive shock	na	na	8.3%	5.3%	11%	23%
Negative shock	na	na	13%	0%	10%	0%
(d) Sustained collapses	57-92	93-00	57-92	93-00	57-92	93-00
Economic liberalization	na	na	0%	0%	0%	0%
Economic reversal	na	na	0%	0%	0%	0%
Positive regime change	na	na	0%	0%	0%	0%
Negative regime change	na	na	0%	0%	10%	0%
Positive shock	na	na	0%	50%	33%	0%
Negative shock	na	na	0%	50%	33%	0%

Table 3: Proportion of episodes preceded or accompanied by adjusted regressors. Notes: Preceded includes 4 lags. Denoinator is the subsample of all accelerations where there is no missing value in the regressor.

3 Replication and extension

Overall, the data gathering exercise extends the baseline by almost up to thousand observations, depending on the underlying GDP dataset used. This increases the sample size by up to 50%, thus improving the statistical power of the inference. The empirical strategy is as follows: First, the estimation is confined to the old sample period and the original baseline is extended by plugging in the adjusted and extended regressors. The equations are re-estimated using the full sample size, but still with the original specification. Finally, extensions are estimated that account for symmetry in treatment of regressors and include some additional, exogenous variables that capture shocks.

3.1 Basic replication

In line with Hausmann et al. (2005), the general specification for all models is:

$$episodeit = \beta 0+\beta 1tot\_thresh90it+\beta 2econlibit+\beta 3poschangeit+ \beta 4negchangeit+X1T+X2Z \quad (7)$$

where is 1 if there is a growth acceleration or collapse (depending on the pattern examined) within *[t−1, t+1]* in country *i* and 0 otherwise. *tot_thresh90*, *econlib*, *poschange* and *negchange* are dummies that take the value 1 in [*t*, *t*+4] following an event at *t*. *T* are time dummies to capture shocks common to all countries. Finally, *Z* are control variables included in the extended regressions. All specifications are estimated using a probit model, but the results do not change substantially when employing a linear probability model. Heteroskedasticity robust standard errors are computed.

In the original baseline, terms-of-trade shocks and regime changes show up significant, with at least $\alpha<0.1$. The effect of economic reforms is insignificant and the estimated effect of all variables is positive (Table 4, Column I). When replacing *poschange* and *negchange* with the corrected variant (Column II), however, the sign of positive regime changes swings, turning significantly negative. While surprising, this change is due to dropping the small scale transitions towards democracy that were previously falsely coded as regime changes (in fact, these small transitions usually capture elections). The significance levels of *tot_thresh90*, *negchange* and *econlib* remain the same.

Including the adjusted version of economic reforms, the significance of reforms increases somewhat more (Column III). But the overall estimates remain about the same, suggesting that the extended reform index is comparable to the old *econlib*. Finally, replacing the terms-of-trade dummy reduces the positive effect of shocks, while keeping all the other estimates roughly unchanged (Column IV). The reduced effect of shocks is not surprising, given that it proves to be more sensitive towards shocks, thus also capturing relatively smaller changes as terms-of-trade shock.

	Dependent variable: episode			
	Original (I)	Polity (II)	Reforms (III)	Shocks (IV)
poschange	0.029* (1.97)			
negchange	0.108* (5.80)			
econlib	0.022 (1.10)	0.034 (1.57)		
tot_thresh90	0.045*** (2.62)	0.047*** (2.66)	0.047*** (2.64)	
adjposchange		-0.028* (-1.72)	-0.028* (-1.72)	-0.027 (-1.64)
adjnegchange		0.072*** (3.47)	0.071*** (3.46)	0.071*** (3.45)
adjeconlib_pos			0.038* (1.65)	0.04* (1.71)
adjtot_thresh90_pos				0.029** (2.29)
Observations	2140	2060	2060	2060
Accelerations	51	50	50	77
Pseudo-R^2	0.059	0.044	0.045	0.044

Table 4: Baseline with corrected and extended regressors. Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Number in parenthesis are robust t-statistics. * p <0.01, ** p < 0.5, *** p < 0.01. All regressions include year dummy variables.

Overall, however, it appears that the extended right-hand-side variables are comparable to the old indices: Apart from the sign swing driven by the previous miscoding for *poschange*, the results remained about the same. Given this encouraging evidence, the next subsection proceeds to an extended specification.

3.2 Full sample

Table 5 reports the estimates based on different versions of the dependent variable. The estimate in Column I is based upon the PWT 6.1 data and limited to the original sample size: As before, negative regime changes, economic reforms and terms-of-trade shocks are significantly associated with growth accelerations. When extended to the full sample, however, the only robust correlate of accelerations are negative regime changes:

Using the PWT 6.3 data, the sample size is increased by 50%, with 14 new accelerations added. Now, the positive effect of economic reforms and external shocks turns insignificant, leaving only negative regime changes highly significant (Column II). This effect persists when exchanging the PWT 6.3 data with the Maddison data, but now economic reforms swing back to significant again (Column III).

	Dependent variable: episode based on different datasets			
	PWT 6.1 (I)	PWT 6.3 (II)	Mad (III)	Robust (IV)
adjposchange	-0.027 (-1.64)	-0.022 (-1.54)	-0.011 (-0.75)	-0.010 (-0.92)
adjnegchange	0.071*** (3.45)	0.052*** (2.78)	0.035** (1.96)	0.066*** (4.10)
adjeconlib_pos	0.04* (1.71)	0.024 (1.46)	0.033* (2.00)	0.012 (0.97)
adjtot_thresh90_pos	0.03** (2.29)	0.014 (1.17)	0.005 (0.39)	-0.003 (-0.36)
Observations	2060	3105	2819	2994
Accelerations	77	91	77	55
Pseudo-R^2	0.044	0.048	0.063	0.054

Table 5: Full sample size with different GDP datasets. Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Number in parenthesis are robust t-statistics. * p <0.01, ** p < 0.5, *** p < 0.01. All regressions include year dummy variables.

In order to account for measurement errors, Column IV reports a synthesis of the PWT 6.3 and Maddison data. Instead of using either dataset, captures only those accelerations that are commonly identified in both datasets: Like before, an acceleration at t in PWT 6.3 is defined robust if the respective Maddison acceleration lies within $[t-2,t+2]$. Once more, the robust results suggest that the only reliable correlates of accelerations are negative regime changes, with economic reforms now insignificant.

Given the imperfect extension of some regressors, however, it is possible that the changes in results are driven by replacing the original regressors. For example, it is possible that the insignificant effect of terms-of-trade shocks is caused by the extended *adjtot_thresh90*, which was more sensitive in capturing shocks. To ensure that this is not the case, regressions were run using the original regressors and sample period, only with varying dependent variables based on PWT 6.3, Maddison and the robust synthesis. The estimates suggest that the original results were dependent upon the PWT 6.1 data. Even with regressors and sample period unchanged, replacing PWT 6.1 with the new datasets causes terms-of-trade shocks to turn insignificant (Appendix 3).

3.3 Additional controls

The robust effect of negative regime changes is striking. While current research has not yet come to a conclusive result for the democratization and growth link (Doucouliagos and Ulubasoglu, 2006), it appears puzzling that only moves towards autocracy would robustly produce growth accelerations. In order to strengthen the evidence, a few control variables are introduced. In line with standard growth literature (Mankiw et al., 1992), *dn* and *di* are dummies controlling for extreme changes in population growth and investment ratio (changes in the highest decile)9. In line with the prediction that cross-decade growth should be less persistent for countries close to their steady state (Easterly et al., 1993), the (log real) GDP per capita level *(log_rgdp)* is also included to capture convergence and control for influences that correlate with the GDP level.

It is also possible that including only positive terms-of-trade shocks and economic liberalization leads to omitted variable biases. Therefore, negative terms-of-trade shocks *(adjtot_thresh90_neg)* and reform reversals *(adjeconlib_neg)* are calculated in a symmetric way and used as control variables. The extended versions of some additional shock controls proxying for exogenous leader death *(adj_lead_death)*, the beginning and the end of a civil war (*adj_civilwar*), are also included. In order to account for measurement errors, the robust version will be used for estimation. However, re-running the regressions with PWT 6.3 or Maddison does not change the results.

9 In line with Hausmann et al. (2005), short-run fluctuations are smoothed out by using the change in four period means, e.g.

Accounting for both directions of external shocks and economic reforms, the estimates remain unchanged (Column II). Including GDP per capita, the estimates suggest that robust growth accelerations tend to be associated with high initial income (Column III). Sharp increases in investments do not significantly correlate with accelerations, but decreases in investment ratio have a positive sign (Column IV). Similarly, a sudden increase in population growth is significantly negatively correlated with an acceleration, while a sudden decrease has no effect at all (Column V).

Changes in the standard growth determinants *di* and *dn* appear asymmetric. The result that strong increases in investments are not associated with accelerations, whereas decreases exert a highly significant effect is particularly puzzling. If taken at face value, it is in stark contrast to standard models of accumulation (Solow, 1956) or poverty traps (Murphy et al., 1988), where increases in the investment ratio accelerate capital accumulation and economic growth. One possible explanation is found in Jones and Olken (2008), where the authors argue that most of the accelerations are not driven by factor accumulation but productivity gains arising from sectoral re-allocation of resources.

This "efficiency story" can be corroborated by including an interaction term between the initial level of investment *i* and a sudden decrease in investment ratio : The interaction term suggests that reductions in the investment ratio significantly increases the probability of a subsequent acceleration the higher the initial investment ratio was, with the effect of alone now turning negative (Column VI): Countries that saw investment reductions associated with accelerations did not have an initially lower investment ratio than countries that increased their investment ratio, with a similar average initial level of about 0.2410. If an investment ratio of 24% was too high and inefficient, reductions could have indeed led to a more efficient reallocation of resources than an increase.

Overall, however, including additional controls does not substantially affect the results of the estimates. Including the additional controls for exogenous leader death, the beginning and end of a civil war leaves the results almost unaffected (Column VII-IX). The highly significant and positive effect of negative regime change still persists and does not appear to be strongly driven by omitted variables.

10 The test that mean initial *i* in countries with and are statistically equal, is not rejected.

	Dependent variable: *robust_episode*								
	Base (I)	Sym (II)	GDP (III)	Invest (IV)	Pop (V)	Int (VI)	Lead (VII)	Civil (VIII)	War(IX)
adjposchange	-0.010	-0.011	-0.008	-0.009	-0.008	-0.010	-0.001	-0.002	-0.001
	(-0.92)	(-1.01)	(-0.72)	(-0.82)	(-0.77)	(-0.96)	(-0.08)	(-0.16)	(-0.06)
adjnegchange	0.066***	0.064***	0.072***	0.0765***	0.076***	0.072***	0.075***	0.068***	0.064***
	(4.10)	(4.00)	(4.23)	(4.41)	(4.40)	(4.28)	(4.37)	(4.21)	(4.00)
adjeconlib_pos	0.012	0.011	0.012	0.012	0.011	0.012	0.009	0.01	0.009
	(0.97)	(0.89)	(0.94)	(0.99)	(0.91)	(0.97)	(0.66)	(0.63)	(0.61)
adjtot_thresh90_pos	-0.003	-0.001	0.004	0.003	0.004	0.007	0.010	0.007	0.008
	(-0.36)	(-0.10)	(0.38)	(0.31)	(0.37)	(0.70)	(0.88)	(0.62)	(0.65)
adjeconlib_neg		-0.002	-0.000	-0.003	-0.005	-0.005	-0.001	-0.001	-0.001
		(-0.11)	(-0.02)	(-0.13)	(-0.20)	(-0.23)	(-0.06)	(-0.04)	(-0.05)
adjtot_thresh90_neg		0.008	0.012	0.010	0.010	0.013	0.021	0.004	0.004
		(0.80)	(1.15)	(0.93)	(0.96)	(1.27)	(1.70)	(0.33)	(0.36))
log_rgdp			0.007**	0.007**	0.006*	0.007	0.005	0.002	0.003
			(2.30)	(2.03)	(1.95)	(1.45)	(1.42)	(0.78)	(0.80)
di_pos				-0.006	-0.005	-0.002			
				(-0.77)	(-0.59)	(-0.25)			
di_neg				0.026***	0.026***	-0.018			
				(2.79)	(2.85)	(-1.24)			
dn_pos					-0.023**	-0.024***			
					(-2.35)	(-2.60)			
dn_neg					-0.000	0.000			
					(-0.03)	(0.01)			
i						-0.001			
						(-0.89)			
di_neg*i						0.002***			
						(2.83)			
adj_lead_death							0.004	0.007	0.006
							(0.26)	(0.40)	(0.35)
adj_civilwar								0.004	-0.007
								(0.25)	(-0.54)
adj_warend									0.028**
									(2.13)
Observations	2994	2994	2994	2994	2994	2994	2634	2428	2418
Accelerations	55	55	55	55	55	55	48	43	43
Pseudo-R^2	0.053	0.054	0.058	0.06	0.071	0.077	0.065	0.004	0.068

Table 6: Robustness checks using original sample and full sample

Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Numbers in parenthesis are robust t-statistics. * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$. All regressions include year dummy variables.

3.4 Sustained and unsustained accelerations

Predicting accelerations lumps different types of accelerations together. As Section II showed, accelerations can be classified into unsustained accelerations (those driving low persistence) and sustained accelerations, where the growth trend is relatively permanent. If both volatile and more persistent changes in episode growth are driven by different determinants, it might not be so surprising that not distinguishing between unsustained and sustained accelerations does not yield many conclusive insights.

Dependent variable: episode based on different data versions				
	Sustained accelerations		Unsustained accelerations	
	PWT6.1 (I)	Robust (II)	PWT6.1 (III)	Robust (IV)
poschange	0.051***		-0.004	
	(3.74)		(-0.34)	
negchange	0.038***		0.076***	
	(2.82)		(4.85)	
tot thresh90	0.01		0.065***	
	(1.20)		(3.63)	
econlib	0.170***		(dropped)	
	(4.14)			
adjposchange		-0.011		0.007
		(-1.10)		(0.71)
adjnegchange		0.017		0.061***
		(1.30)		(4.23)
adjtot_thresh90_ pos		-0.003		-0.006
		(-0.47)		(-0.67)
adjeconlib_pos		0.035**		-0.021
		(2.13)		(-2.30)
Observations	1197	2040	1222	2290
Accelerations	12	23	18	26
Pseudo-R^2	0.11	0.074	0.13	0.057

Table 7: Sustained and unsustained accelerations. Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Number in parenthesis are robust t-statistics. * p <0.01, ** p < 0.5, *** p < 0.01. All regressions include year dummy variables.

Table 7, Column I presents the results from Hausmann et al. (2005). When the sample size is increased and measurement errors accounted for, economic reforms remain significantly associated with sustained accelerations but the effect of regime changes disappears (Column II). The smaller magnitude of reforms is caused by the censored sample in the original article, where a wide range of "unsuccessful" economic reforms in the 80s were excluded. For unsustained accelerations, negative regime changes remain a significant correlate but now sign and significance of terms-of-trade shocks swing (Column IV). As previous checks have shown that the changes are not due to artefacts of inconsistent regressors, the results would suggest differential determinants of accelerations.

While such a result (economic reforms produce sustained accelerations, autocratic transitions produce unsustained accelerations) seems intuitive and convenient for interpretation, introducing additional controls suggests omitted variables: Once the level of GDP per capita is controlled for, the effect of negative regime changes turns significant, once more (Table 8, Column III)11. Since sustained accelerations occur mostly in developed countries (the GDP term is highly significant), whereas negative regime changes never occur in high income countries (Przeworski, 2008), it is possible that the effect of negative regime changes is downward biased as it also captured the effect of income level. In contrast to sustained accelerations, the level of income has no significant explanatory power for unsustained accelerations (Column VI).

	Dependent variable: robust_episode					
	Sustained accelerations			Unsustained accelerations		
	Base (I)	Sym (II)	GDP (III)	Base (IV)	Sym (V)	GDP (VI)
adjposchange	-0.011	-0.010	-0.009	0.007	0.005	0.005
	(-1.10)	(-1.00)	(-0.91)	(0.71)	(0.51)	(0.57)
adjnegchange	0.017	0.023*	0.030**	0.062***	0.052***	0.054***
	(1.30)	(1.82)	(2.15)	(4.23)	(3.83)	(3.76)
adjtot_thresh90_ pos	-0.003	-0.006	-0.000	-0.005	-0.000	-0.000
	(-0.47)	(-0.79)	(-0.03)	(-0.67)	(-0.11)	(-0.03)
adjeconlib_pos	0.035**	0.034**	0.028*	-0.021**	-0.021**	-0.021
	(2.13)	(2.09)	(1.75)	(-2.30)	(-2.48)	(-2.48)
adjeconlib neg		(dropped)	(dropped)		0.024	0.024
					(1.11)	(1.12)
adjtot thresh90 neg		-0.009	-0.004		0.019*	0.020
		(-1.20)	(-0.51)		(1.85)	(1.86)
log rgdp			0.008***			0.001
			(2.78)			(0.51)
Observations	2040	1991	1991	2290	2290	2290
Accelerations	23	23	23	26	26	26
Pseudo-R²	0.074	0.078	0.088	0.057	0.063	0.063

Table 8: Sustained and unsustained accelerations with controls. Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Number in parenthesis are robust t-statistics. * p <0.01, ** p < 0.5, *** p < 0.01. All regressions include year dummy

11 For sake of readability, the extensive replication exercise (Table 6) is not reported. Instead, only the notable discrepancies are presented. A complete tabulation is found in the do file.

variables.

3.5 Predicting growth collapses

If the determinants of growth accelerations exerted a symmetric effect, growth collapses should be driven by the exact opposite effects. In order to test this hypothesis, the robust growth collapses are regressed in the same way as accelerations. Again, for sake of focusing on the main argument, not all robustness checks are reported here12.

The results suggest that growth collapses are driven by almost opposite factors: When omitting controls, the regressors of the original regression point in the opposite direction, with none of them significant (Table 9, Column I). Once including negative shocks and reform reversals, both positive and negative shocks appear to significantly increase the probability of a subsequent growth collapse (Column II). While shocks turn insignificant once the level of income is included (Column III), the effect of economic reversals persists throughout the specifications. Not surprisingly, civil wars are associated with collapses, while the end of wars decrease the probability of a collapse (Column IV).

	Dependent variable: robust_nepisode			
	Base (I)	Sym (II)	GDP (III)	Shocks (IV)
adjposchange	0.006 (0.45)	0.003 (0.29)	0.004 (0.35)	0.004 (0.34)
adjnegchange	-0.011 (-0.74)	-0.014 (-1.20)	-0.015 (-1.29)	-0.017 (-1.50)
adjeconlib_pos	-0.004 (-0.27)	-0.004 (-0.28)	-0.004 (-0.23)	0.001 (0.06)
adjtot_thresh90_ pos	0.013 (1.38)	0.021** (2.08)	0.015 (1.64)	0.012 (1.28)
adjeconlib neg		0.083** (2.05)	0.082** (2.07)	0.099** (2.15)
adjtot thresh90 neg		0.020* (1.77)	0.013 (1.32)	0.013 (1.42)
log rgdp			-0.007** (-2.46)	-0.007*** (-2.69)
adj_civilwar				0.071*** (3.10)
adj_warend				-0.020** (-2.22)
Observations	1731	1731	1731	1543
Collapses	19	19	19	17
Pseudo-R^2	0.033	0.046	0.052	0.087

12 A complete report can be found in the do file.

Table 9: Predicting growth collapses. Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Number in parenthesis are robust t-statistics. * p <0.01, ** p < 0.5, *** p < 0.01. All regressions include year dummy variables.

Given the limited number of collapses in the sample, however, the interpretation should be treated with care. The number of sustained collapses is even smaller, with only two robust collapses in the original sample period. This renders inference too problematic, and the correlates of sustained collapses will therefore not be discussed.

3.6 Cross-sectional estimation

The data can be easily simplified to a conventional growth regression. Whereas the filter converts continuous data on growth rates into censored and binary data, an extended check is conducted by regressing 8 year average growth rates instead of a binary indicator for accelerations. Because such an exercise essentially asks a different question than a turning point analysis, the estimates can complement the previous findings. Any differences between the cross-country estimates and acceleration results could suggest that the correlates of turning points are different from drivers of (average) growth. Let:

$$git,t+7 = \beta 0+\beta 1tot\_thresh90it+\beta 2econlibit+\beta 3poschangeit+\beta 4negchangeit+ X1T+X2Z \quad (8)$$

where denotes the average growth from *t* to *t*+7 and the other regressors capture the usual policies and shocks at period *t*. Because the results are almost identical when using alternative datasets, only the estimates of PWT 6.3 are reported below.

Unlike before, terms-of-trade shocks and economic reforms both turn up highly significant (Table 10, Column I). Whereas terms-of-trade shocks had a positive (albeit insignificant) association with growth accelerations, even positive shocks tend to generally lower the subsequent average growth rate by about 0.7-1% points. Economic reforms, on the other hand, do not only correlate with sustained accelerations but raise average post-reform growth by 0.6-1.1% points. Positive regime changes show up marginally significant (yet too small and negative) in the baseline, but including additional controls renders the regime changes altogether insignificant.

In terms of magnitude, terms-of-trade shocks exhibit a roughly symmetric effect on subsequent growth: Both directions of terms-of-trade shock lower subsequent average growth by around 1% points (Column II). Combined with the insignificant accelerations estimates, the evidence suggests that terms-of-trade shocks could contribute to average volatility but do not drive shifts or turning points in growth trends.

As expected, reform reversals have a negative effect of similar magnitude as positive economic reforms. Symmetry, however, is again not the case for the Solow controls. A sudden increase in investment ratio (population growth) tends to raise (lower) subsequent growth by 0.3% (0.5%) points, but a sudden decrease does not have a statistically significant effect (Column IV). Contrasted to the puzzling finding that *decreases* in investment ratio were associated with growth accelerations, this would further support the argument that drivers of turning points differ from those of average growth. Finally, the controls for shocks are significant and have the "correct" sign (Column V).

Dependent variable: Least squares average growth (PWT6.3)					
	Base (I)	Sym (II)	GDP (III)	Pop (IV)	Shock (V)
adjposchange	-0.002*	-0.002	-0.001	-0.000	-0.001
	(-1.81)	(-1.18)	(-0.42)	(-0.25)	(-0.38)
adjnegchange	0.002	0.004**	0.006**	0.006***	0.007***
	(0.89)	(2.11)	(3.01)	(3.15)	(3.52)
adjtot_thresh90_ pos	-0.007***	-0.01***	-0.008***	-0.008***	-0.008***
	(-5.75)	(-8.03)	(-6.08)	(-6.21)	(-5.85)
adjeconlib_pos	0.006***	0.006***	0.007***	0.007***	0.011***
	(3.64)	(4.00)	(4.47)	(4.26)	(5.16)
adjeconlib neg		-0.007***	-0.006***	-0.008***	-0.007***
		(-2.85)	(-2.68)	(-3.01)	(-2.65)
adjtot thresh90 neg		-0.010***	-0.008***	-0.009***	-0.010***
		(-8.40)	(-6.96)	(-7.00)	(-6.73)
log rgdp			0.003***	0.003***	0.003***
			(8.58)	(8.42)	(6.73)
di_pos				0.003***	
				(2.68)	
di_neg				0.002	
				(1.55)	
dn_pos				-0.005***	
				(-4.60)	
dn_neg				-0.002	
				(-1.49)	
adj_lead_death				0.004**	
				(2.14)	
adj_civilwar				-0.008***	
				(-3.50)	

adj_warend				0.006*** (3.60)	
Observations	3365	3351	3330	3330	2655
R^2	0.121	0.14	0.15	0.16	0.19

Table 10: Cross-sectional specification. Notes: Estimated by OLS. Coefficients shown are marginal probabilities evaluated at the sample means. Number in parenthesis are robust t-statistics. * p <0.01, ** p < 0.5, *** p < 0.01. All regressions include year dummy variables.

4 Discussion and conclusion

The results of the replication challenge some findings of Hausmann et al. (2005). By extending the dataset up to 2000, the dissertation provides evidence of fragility: Both positive and negative terms-of-trade shocks are not robustly associated with any type of growth acceleration or growth collapse. Even if the effect of terms-of-trade shocks is underestimated using the imperfect extension of , the evidence suggests that the correlates of turning points differ from those of average growth: While favourable terms-of-trade shocks had a positive effect in the original specification, even positive shocks are negatively associated with subsequent average growth.

Nonetheless, some evidence in favour of policies remains. Economic reforms, proxied as the beginning of trade openess is significantly associated with sustained growth accelerations. Surprisingly, sharp increases in investment ratio appear to only affect average growth but not turning points. While this is against the theoretical predictions of standard growth models, the finding is similar to Jones and Olken (2008) who argued for an "efficiency story", with growth accelerations driven by productivity gains through reallocation of resources across sectors. The (inconclusive) evidence that lowering investment rates in countries with a high initial rate increases the probability of a growth acceleration could corroborate this hypothesis.

If 17 years proxy long-run, a sustained effect of policy changes could suggest that they do not induce transition but a permanent shift in growth, as in endogenous models (Jones, 1995). In contrast, if 17 years still proxy transition growth, the evidence could speak for exogenous models. But even neglecting issues of endogeneity for policies, the evidence is insufficient to argue for either story: For endogenous growth, 17 years appear too short for capturing long-run. Similarly, the effect of economic reforms do not appear to taper off within 17 years (Appendix 4), a main prediction of convergence growth in exogenous models. The fact that strong changes in standard policy variables such as investment ratio and population growth are not associated with trend shifts, however, suggests that the explanatory power of both model classes is somewhat limited[13].

13 For example, both AK and Solow model a relationship between investments and transitory/long-run growth. Similarly, both endogenous and exogenous models predict a relationship between population and growth (market size/capital dilution).

4.1 Explaining the effect of autocratic transitions

The most robust finding is that negative regime changes are associated with all types of growth accelerations. This effect remains across all specifications and is large. A regime change towards autocracy increases average growth rate in the subsequent 7 periods by around 0.6% points. While the "zero-effect" of democratic transitions is in line with findings such as Rodrik and Wacziarg (2005), the positive effect of autocratic transitions has not gained much attention. Hausmann et al. (2005) did not offer any explanations after arguing that the effect disappears once distinguishing between sustained and unsustained accelerations. As sustained accelerations mostly occur in high income countries, however, this is likely due to an omitted variable bias: Controlling for the level of income, autocratic transitions turn up positive and significant again.

The result is not an artefact of the Polity IV index: When exchanging the Polity IV index with alternative indices such as the Freedom House index, the results do not change substantially (Appendix 5). Furthermore, it is not a result of a mis-specification described in Easterly (2001), whereby regressing a stationary variable (dummy for acceleration) on a non-stationary variable (initial conditions proxied as GDP) results in biased estimates: When controlling for the level of income using a simple dummy denoting low or high income, the results become even stronger (Appendix 6).

One plausible explanation is the developmental state hypothesis, whereby an autocratic and dirigiste regime is more able to mobilize resources and facilitate the economic transformation whenever market forces fail (Woo-Cummings, 1999). Qualitatively, the 1967 Brazilian acceleration, for instance, is often explained as the result of reforms and stabilization policies enforced by the military government following the 1964 coup d’etat. The Chilean 1974 acceleration coincides with the rule of Pinochet (and the Chicago Boys) following years of macroeconomic instability. These examples, however, remain selective and there are many alternative explanations: The Nigerian 1967 acceleration, for instance, coincided with a military coup, a civil war and a confounding oil shock in the early 1970s. Given problems of omitted variables and endogeneity of regime changes (Przeworski, 2004), it is inherently difficult to draw any useful conclusions from this (possibly even spurious) relationship between autocratic transitions and accelerations.

4.2 Implications for further research

This dissertation, updating and revising the findings of Hausmann et al. (2005), highlights a few areas for further research. First, the exercise has once more shown that replication should be taken seriously. In growth literature, there is a temptation to data mine and run "kitchen sink" regressions. By doing so, *"the choice of period, of sample, and of proxies will often imply many effective degrees of freedom where one might always get what one wants if one tries hard enough"* (Bhagwati and Srinivasan, 2002). Examining the original Hausmann et al. (2005) dataset alone, one finds a vast variety of controls and alternative proxies that have been arguably regressed but not reported14. Although replication is often considered as tedious nitpicking, it is a defining feature of scientific research and progress (Kuhn, 1996). The coding errors found in the paper alone justify an extensive replication.

Second, turning point studies are prone towards poor data. Unlike cross-sectional studies, turning point studies require long time-series which are often unavailable. If most of the missing values are either dropped or coded zero (as is done in Hausmann et al. (2005)), selection biases could occur, as missing values are often corelated with country characteristics. Turning point studies focusing on rare events are particularly prone to missing values as this often implies valuable observations lost. In the original article, the regressions included only 51 (60%) of the growth accelerations at most, with important cases such as China 1978 even dropped in the extended specifications. While utmost effort has been put in to fill the gaps, further research could focus on compiling longer and more complete indices. As current proxies such as Sachs and Warner (1995) are crude at best, it is even possible that many policies were simply not picked up.

Finally, more research is needed to explore the surprisingly robust relationship between autocratic transitions and growth accelerations. The current state of literature does not come to a conclusive result on the relationship between democratization and growth, and such a robust correlation between political transition and turning point growth could be worth exploring. Given the small number of autocratic transitions, however, quantitative tests could be complemented by qualitative case studies.

14 Pace Hausmann et al. (2005), most of these proxies show up insignificant and do not deliver any robust effects.

4.3 Concluding remarks

Despite countless cross-country regressions, researchers have yet been unable to isolate the drivers of growth and explain the persisting income gap. While a turning point study such as Hausmann et al. (2005) proved promising in answering the question on which policies to pursue for growth, this dissertation suggests that even these findings are fragile upon changes in period, sample, measures and inclusion of controls. Although the original idea was to test hypotheses of competing growth models suggested in Easterly et al. (1993), the discouraging fragility of the results complicates a rigorous test and prohibits a study that goes beyond replication. Nonetheless, the dissertation once more illustrates the (known but often neglected) pitfalls of macroeconomic growth empirics and contributes to falsifying - or at least challenging - some existing findings.

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5 Appendix

Table 1: Growth accelerations in three datasets

		Original			PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
SubS Africa	NGA	57	4.3	3	57	3.6	2.5			
	MUS							57	4.8	4.1
	LSO							58	5.4	3.2
	NER							58	4.1	2.3
	SWZ							58	9.7	8.3
	CIV							58	5.1	3.6
	AGO							59	4.3	2.5
	STP							59	6.2	6.3
	COM							59	5.9	4.1
	NAM							59	6.5	5
	GNQ							59	11	7.6
	MRT							59	6.5	4
	TGO							60	6	4.1
	ERI							60	4	3.3
	GMB							60	3.6	2.2
	GNB							61	6.1	2
	ZMB				62	5.8	5.4			
	BDI							64	4	5
	BWA							64	7.9	6.4
	MWI							64	3.6	4.3
	RWA							64	4.7	6.2
	ZWE	64	7.2	6.5				66	6.6	6
	GHA	65	8.3	8.4	65	14.6	16			
	MOZ							65	5	4.4
	DJI							66	3.9	3.8
	MWI							66	3.7	3.5
	NGA	67	7.3	9	67	8.9	11.9	67	10	10.2
	ZWE				67	8.5	8.5			
	COG+	69	5.4	4.5	67	11.3	8	68	4.6	2.9
	BWA	69	11.7	8.8	67	11.3	8.9			
	GAB							68	9.1	7.6
	MWI	70	3.9	2.5	70	3.7	2.2			
	SEY							70	4.8	4.4
	BFA				71	3.7	2.8			
	GNB	69	8.1	8.4	71	10	15			
	LSO	71	5.3	4.6	71	7.1	8	71	8.1	8.7
	MUS+	71	6.7	8.5	71	6.8	10.1	70	7.2	9.9
	SDN							71	4.4	3
	GNQ							72	6.8	9.6
	CMR	72	5.3	5.9	72	5.4	5.4			
	MLI	72	3.8	3	72	3.7	2.5	73	4.9	4.2
	TCD	73	7.3	8						
	STP							73	5.7	3.9
	RWA	75	4	3.3				74	4.4	2.3
	CPV							76	10.8	13.5
	RWA							76	5.1	3.2
	UGA	77	4	4.6						

Notes: Robust accelerations found in all three datasets starred. Sustained accelerations with +. t is year (1900+t), $g_{t,t+7}$ is the (least squares) average growth for a 7 year episode starting at t. $\Delta g_{t,7}$ is the difference between the 7 year episode starting at t and $t-7$.

Table 1: Growth accelerations in three datasets (continued)

		Original			PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
	MWI				78	3.9	7			
	CMR							78	6	4.6
	BEN				79	4.3	6.1			
	COG	78	8.2	5.1				77	8	5.3
	MDG				82	3.9	7.5			
	SWZ				83	5	4.9			
	MUS+	83	5.5	4.4	84	5.5	4.6	84	6.4	6.5
	BWA				85	7.1	2.7			
	UGA	89	3.6	4.4						
	LSO							85	4.3	5.7
	SEY							86	4	4.8
	GNB	88	5.2	5.9	90	9	12			
	MWI	92	4.8	5.6	92	3.9	7			
SubS Africa	LSO				93	4	3.8			
	CPV							94	5.9	5.4
	SDN				94	6.4	6.1			
	GNQ							94	26.5	23.5
	BWA							96	3.7	3.1
	MLI							96	3.9	3.3
	MOZ				97	6.9	8.2			
	SDN							97	4.4	3
	BWA				99	3.7	3.1			
	AGO				99	4.9	2.4			
	NGA				00	8.8	8.4	01	4	3.4
	TCD				00	10	11.6	99	8.7	8.6
	TZA				00	4.5	3.6	00	4.1	4
	ERI							01	5	3
N America	CAN+	62	3.6	2.9	62	3.6	2.9	62	3.6	2.4
	USA+	61	3.9	3	62	3.7	2.9	62	3.7	2.5
	DNK+	57	5.3	3.5	57	5.1	3.3	57	4.2	2.1
	IRL+	58	3.7	2.7	57	4.7	3.6	58	3.9	2.6
	FIN	58	5	2.2	58	5	2.2			
	PRT+				58	5.9	2.3	60	5.9	2.6
	BEL+	59	4.5	2.4	58	4.6	2.3	58	4.2	2.1
	SWE							58	4.6	2.3
	ESP+	59	8	3.5	59	8.3	3.8	59	7.9	4.6
	AUS	61	3.8	2.3	61	3.9	2.3			
	PRT				66	7.9	2.1			
W Europe	FIN	67	5.6	2.2	68	5	2.1			
	GBR+	82	3.5	2.5	83	3.6	2.7	82	3.5	2.3
	ESP+	84	3.8	3.7	84	4.3	4.3	84	4	2.9
	PRT+	85	5.4	4.3	84	6	4.6	84	5.1	3.1
	IRL	85	5	3.4				85	4.6	3
	NOR	91	3.7	2.2	92	4.2	3.1	92	3.5	2.1
	AUS				92	3.5	2.6			
	IRL				93	8.6	4.6	93	7.9	3.4
	FIN	92	3.7	2.8						
	FIN				95	4.3	6.6	95	4	5.3
	ESP				96	4.1	2.9	96	3.8	2.3
	PAK	62	4.8	7.1	61	4.4	4.8	60	3.5	3.4
S Asia	PHL				70	3.7	2.4			
	KHM							72	3.7	6.3
	KHM							74	5	6.2

Table 1: Growth accelerations in three datasets (continued)

		Original			PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
	MMR							75	4.3	3.5
	AFG				77	3.8	3.8	77	4	3.3
	PAK+	79	4.6	3.2	78	3.8	2.1	77	3.8	2.5
	LKA							76	4	3.3
	LAO				79	5.1	4			
	LKA	79	4.1	3.2	79	4.3	2.7			
S Asia	IND	82	3.9	2.4						
	IND				95	3.8	2			
	BGD				99	3.7	2.7			
	MMR							99	11.2	5.9
	PAK				00	4.3	3.4	01	3.6	3.2
	IND							01	3.6	3.2
	KHM				99	6.8	4.6	00	11.8	9
	LAO							01	4.3	2
	BGR							58	6.1	2.8
	ROM							60	5.8	2.2
	GRC							60	6.8	2
	USR							63	4.2	2
	GRC							62	6.7	2.3
	TUR							62	3.8	2.6
	TUR				64	3.6	3	64	3.9	2.2
	ROM				71	9.7	2.5			
	ROM	79	12.4	5.8						
E Europe	POL	92	5	5.8	93	5.6	7.4	93	5.6	8.6
	SVK				94	3.5	8.5			
	ALB				94	4.5	9.8	94	5.5	11.4
	HUN							94	4.1	8
	GRC				96	3.8	3.4	96	3.8	3.3
	BGR				98	5.8	7.6	99	6.1	6.4
	HUN				98	4.5	2.9			
	ROM				00	6	5.5	01	6.3	6.4
	CZE							01	5.4	3.1
	TUR							01	5	3.7
	RUS							00	7.4	8.6
	THA	57	5.3	7.8	57	4.6	8.2	59	4.6	3.6
	NZL	57	3.8	2.4	57	3.7	2.2			
	JPN+	58	9	3.2	58	9.1	2.5	59	8.4	2.4
	HKG							59	7.7	4.2
	KOR+	62	6.9	6.3	64	7.3	6.3	64	7.8	6.2
	PRK							63	7.3	6
	IDN+	67	5.5	6.2	67	7.6	8.3	67	7.4	8.9
	SGP							66	11	7.1
	SGP+	69	8.2	4	67	9.4	7.2			
E AsiaP	TWN+	61	7.1	3.8	63	7.3	3	62	6.8	3.9
	MYS				67	7.5	4.2			
	CHN+	78	6.7	5.1	78	8.4	4.8	77	5.7	3.9
	MYS	70	5.1	2.1				70	5.6	2.6
	PNG				72	7.7	4.6			
	SGP+				89	6.4	3.3	87	5.6	2.2
	MYS+	88	5.7	4.6	88	6.7	4.9	89	6.8	4.8
	JPN				85	4.2	2.1			
	THA	86	8.1	0.6	86	8.3	4.3	86	8.5	5
	PNG	87	4	3.7	89	4.4	4.3			
	KOR+	84	8	3.7	84	9	4.7	84	8.2	3.8

Table 1: Growth accelerations in three datasets (continued)

		Original			PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
	TWN				85	7	2.1			
	IDN	87	5.5	2.1	87	5.8	3	89	6	2.8
	CHN	90	8	3.8	92	7.6	2.1			
E AsiaP	VNM				91	5.3	2.7	91	6.5	4.2
	HKG				00	4.3	3.7	01	5	3.6
	THA				00	4.2	4.2	01	4.4	4.6
	CHN				00	10.5	3.8	01	8.2	3.5
	IDN							01	4	4.3
	IRN							57	4.8	5.2
	ISR	57	5.3	3.1	57	5.7	3.2			
	MAR	58	7.6	8.8	58	9.2	10.6			
	EGY							58	3.7	3.1
	TUN							59	4.3	4.3
	LBY							59	21.5	14.1
	SAU							60	7.8	3.7
	QAT							62	4.4	5
	OMN							64	23.1	18.9
	IRN				66	10	4.7	65	9	4.3
	QAT							65	4.2	7.8
	DZA							66	4.2	5.8
	ISR	67	7.2	4.4	68	5.6	2.6	67	6.7	2.3
	TUN	68	6.6	4.5	68	5.5	3.1	70	4.8	2.7
	SYR	69	5.8	5.5	69	5.8	5.9	69	7.2	8.3
MeNa	YEM							70	7.4	5.2
	DZA							71	4.8	2
	IRQ							72	9.5	8.7
	JOR	73	9.1	12.7	75	4.6	6.5	73	9.2	13.6
	EGY							73	7.2	5.4
	SYR	74	4.8	2.2						
	DZA	75	4.2	2.1						
	EGY	76	4.7	6.3	76	6.3	7.3			
	JOR				77	4.6	6.5			
	LBN				80	7.7	12.5			
	MNG				81	5.1	2.1			
	OMN							82	4.5	3.9
	SYR	89	4.4	7.3	90	4.4	7.4	89	4.6	7.2
	BHR							01	5.1	2.9
	IRN							01	6.1	3.3
	JOR							01	4.6	4.5
	PER	59	5.2	4.4	59	5.1	4.3	60	3.6	2.1
	NIC	60	4.8	3.8	60	6.2	6.6	60	5.5	5
	PAN	59	5.4	3.9	60	4.8	3.9			
	BOL							61	3.6	5.7
	MEX				62	4.2	2.3			
	ARG	63	3.6	2.7				63	3.6	2.9
	GTM				65	4.2	2.5			
L America	COL	67	4	2.4	67	3.8	2.3	67	3.8	2.3
	BRA	67	7.8	5.1	68	7.2	4.1	68	6.7	5.7
	DOM	69	5.5	6.6	69	5.8	6.4	69	6.1	6.2
	ECU	70	8.4	6.8	70	7.8	6.2			
	CUB							71	3.6	3.9
	HND							72	4.8	4.5
	HTI				73	4.7	5			
	PRY	74	6.2	3.7	74	6.9	4.5	74	7.4	4.8

Table 1: Growth accelerations in three datasets (continued)

		Original			PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
	CHL+				74	3.7	2.9	74	3.9	3.9
	TTO	75	5.4	3.5				74	5.4	2.6
	URY	74	4	2.6	73	4.6	3.6	74	4	3.1
	PAN	75	5.3	2.7	75	6.4	4.1	76	4.3	2.5
	CUB				77	5.7	2.1			
	PRI+				84	5.4	5.4	85	3.9	3.5
	CHL+	86	5.5	6.7	86	5.9	8.1	86	5.9	7.5
	URY	89	3.8	2.1	88	3.7	3.8			
	ARG	90	6.1	9.2	90	3.8	5.9	90	4	5.8
L America	HTI	90	12.7	15						
	DOM	92	6.3	5.8	92	4.7	3.4	94	5.4	4.5
	TTO				93	7.1	7	95	4.7	3.8
	PRI				95	4.4	2.1			
	CUB				99	4.3	3.8	01	6.7	2.7
	PAN				00	3.8	2.2			
	PER							01	5	3.6
	ARG							01	5.6	5.2
	URY							01	5	4.4
	COL							01	3.6	4.5

Table 2: Growth collapses in two datasets

		PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
S Asia	AFG	85	-7.7	-11.2	85	-6.2	-9.6
E AsiaP	VNM				63	-3.7	-5.8
	PRK				91	-14.7	-14.7
	ALB	86	-5.8	-6.2	86	-6.2	-6.7
	BGR				86	-4.6	-5
E Europe	ROM			86	-6.7	-7	
	HUN			87	-3.9	-4.9	
	USR			89	-9.8	-11	
	YSR			87	-11	-11.5	
	BOL	79	-3.8	-6.1	79	-3.6	-6.2
	CUB	88	-7.1	-9.6	88	-8.3	-9.1
	JAM	73	-4.1	-8.1	73	-3.7	-7.8
	ECU				96	-4.5	-5.8
	HTI				88	-5.9	-3.9
S America	NIC+	75	-5.7	-7.7	75	-7.4	-9.3
	NIC	86	-8.8	-12.1	86	-5.5	-3.5
	PER	85	-4.8	-2.9	85	-4.8	-2.6
	SLV	77	-4.9	-8	77	-4.3	-6.9
	TTO	80	-6.5	-9.8	82	-4.2	-8.5
	VEN	77	-5.1	-7.7	78	-4.2	-5.5
	ARE	78	-5	-30.8			
	ARE				81	-10.8	-10.5
	IRN	74	-12.3	-21.5	75	-8.4	-16
MeNa	IRQ+	79	-7.4	-17.27	79	-8.6	-18
	IRQ	88	-13.7	-13			
	IRQ	98	-6.6	-11			
	IRQ				01	-3.7	-7.3

Notes: Robust collapses found in PWT6.3 and Maddison starred. Sustained collapses with +. t is year (1900+t), $g_{t,t+7}$ is the (least squares) average growth for a 7 year episode starting at t. $\Delta g_{t,7}$ is the difference between the 7 year episode starting at t and $t-7$.

Table 2: Growth collapses in two datasets (continued)

		PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
MeNa	JOR	66	-4.7	-8.4	65	-5	-10
	JOR	86	-3.9	-12.3	85	-5.1	-8.4
	KWT				70	-10.4	-10.6
	KWT	79	-9.7	-2.3			
	KWT	95	-3.8	-12	94	-4.1	-12.2
	LBN				83	-9	-6.7
	LBY				68	-8	-28.5
	LBY				81	-8.4	-10.9
	LBN	86	-15.4	-19.7			
	LBY	78	-12.27	-20.9			
	MNG	88	-5.9	-11	87	-5.8	-9.6
	ROM	86	-5.6	-5.9	86	-6.7	-7
	SAU	77	-8.1	-17.8			
	SAU				80	-8.4	-10.6
	QAT				58	-3.5	-4
	QAT				80	-19	-13
SubS Africa	AGO				71	-11.3	-13
	AGO	88	-5.4	-9.4	89	-5.7	-5.7
	BDI	90	-4.8	-7.7	91	-5.6	-7.3
	CAF	76	-3.5	-4.1			
	CAF	86	-3.6	-2.2			
	CDR				73	-5.2	-6.2
	CDR				88	-10.5	-10
	CIV				82	-3.7	5.2
	CMR	86	-6.1	-9.6	86	-7.4	-13.5
	COG				83	-3.6	-10.2
	COM				71	-9.3	-11.6
	DJI				74	-4	-7.6
	DJI				87	-3.9	-4.5
	COG	87	-4.1	-11.4			
	ETH	80	-4.1	-3.4			
	GAB				77	-4.5	-13.9
	GAB				96	-5	-5.2
	GMB				79	-5.9	-7.9
	GNQ				67	-4.6	-14.3
	GHA	72	-3.8	-18			
	GNB	81	-3.8	-4.8			
	GNB	97	-3.7	-12.7	96	-4.2	-4.6
	LBR				71	-3.8	-6.8
	LBR				94	-4.2	-8.9
	LBR	79	-4.3	-2.1			
	LBR	88	-35	-31			
	LBR	00	-4.9	-21.78			
	MDG	75	-3.6	-4.4			
	MDG				79	-4.5	-2.5
	MOZ				72	-7	-12
	MOZ	80	-4.7	-4.8			
	NER	67	-4.6	-6.3	67	-3.9	-6.5
	NER+	79	-4.2	-5.1	80	-5.7	-8.9
	NGA	62	-4.5	-7.6			
	NGA	75	-4.1	-12	77	-5.1	-8.4
	NGA	80	-4	-2			
	RWA	81	-3.7	-6.2			
	RWA	87	-4.5	-2.4	86	-3.7	-5.4

Table 2: Growth collapses in two datasets (continued)

		PWT63			Maddison		
Region	ISO	t	$g_{t,t+7}$	$\Delta g_{t,7}$	t	$g_{t,t+7}$	$\Delta g_{t,7}$
	RWA	90	-4.1	-2.2			
	SDN	79	-6.6	-9			
	SLE	92	-12.7	-10.7	92	-12.7	-10.4
	SOM	77	-4.6	-4.5	77	-4.6	-6.3
	SOM	91	-6.3	-7.1			
	STP				79	-3.7	-7.6
	TCD	75	-7.8	-7.7	75	-8.1	-8
SubS Africa	TGO	78	-6	7.2	78	-5.4	-4.9
	UGA				72	-4.2	-5.8
	UGA	75	-6.8	6.7			
	ZAR	73	-5.3	-5.9			
	ZAR	89	-11.9	-10.8			
	ZMB	70	-4.5	-8.7			
	ZMB	89	-4.8	-4.8	90	-3.6	-3.1
	ZWE	99	-14.4	-17.7	99	-5.6	-8

Table 3: Original regressors and sample period, with updated GDP data

	Dependent variable: episode based on different datasets			
	PWT61 (I)	PWT63 (II)	Mad (III)	Robust (IV)
poschange	0.028	0.023	-0.001	0.017
	(1.97)	(1.16)	(-0.09)	(1.49)
negchange	0.107***	0.08***	0.129***	0.075***
	(5.80)	(3.53)	(6.24)	(4.90)
econlib	0.022	0.019	0.051*	0.049**
	(1.10)	(0.68)	(1.87)	(2.38)
tot_thresh90	0.04***	0.039*	0.024	0.004
	(2.62)	(1.74)	(1.25)	(0.33)
Observations	2140	2140	2089	2113
Accelerations	51	49	40	26
Pseudo-R^2	0.059	0.025	0.082	0.056

otes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample eans. Numbers in parenthesis are robust t-statistics. * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$. All gressions include year dummy variables.

Table 4: The effect of economic reforms on subsequent growth.

Dependent variable: Least square average growth with different lags				
	PWT63		Maddison	
	$g_{t,t+7}$(I)	$g_{t+7,17}$(II)	$g_{t,t+7}$(III)	$g_{t+7,17}$(IV)
adjposchange	-0.002*	0.002	-0.000	0.003***
	(-1.83)	(1.54)	(-0.61)	(2.97)
adjnegchange	0.002	-0.001	-0.000	0.001
	(0.99)	(-0.69)	(-0.08)	(0.62)
adjtot_thresh90_pos	-0.007***	-0.007***	-0.007***	-0.006***
	(-5.74)	(-5.36)	(-6.37)	(-4.98)
adjeconlib_pos	0.006***	0.008***	0.006***	0.008***
	(3.58)	(4.41)	(4.58)	(5.07)
Observations	3330	2582	3270	2627
Pseudo-R^2	0.12	0.09	0.14	0.11

Notes: Estimated by OLS. Numbers in parenthesis are robust t-statistics. * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$. All regressions include year dummy variables. If a fundamental policy change such as economic reforms would only produce transition growth, the lagged effect (Column II and IV) should be smaller. However, the statistical equality of the coefficient for *adjeconlib_pos* cannot be rejected.

Table 5: Replacing Polity IV with Freedom House Index

Dependent variable: episode based on different data versions					
	Original sample period				Sustained sample
	PWT 6.1 (I)	PWT 6.3 (II)	MAD (III)	Robust(IV)	Robust (V)
poschange	0.028*				
	(1.67)				
negchange	0.081***				
	(3.40)				
tot_thresh90	0.025				
	(1.27)				
econlib	0.010				
	(0.43)				
fdmhouse_pos		0.028*	0.023	0.014	0.022**
		(1.74)	(1.64)	(1.46)	(2.21)
fdmhouse_neg		0.082**	0.057**	0.078***	0.142***
		(2.54)	(2.14)	(3.73)	(4.53)
adjtot_thresh90_pos		0.047***	0.038	0.001	0.003
		(2.61)	(2.48)	(0.19)	(0.47)
adjeconlib_pos		0.008	0.054*	0.034	0.312***
		(0.26)	(1.71)	(1.59)	(4.56)
Observations	2410	1551	1533	1551	775
Accelerations	51	48	40	25	10
Pseudo-R^2	0.06	0.02	0.05	0.05	0.25

Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Numbers in parenthesis are robust t-statistics. * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$. All regressions include year dummy variables. Replacing Polity IV with the Freedom House index, the results for negative regime change remain robust. Given the small sample size, however, the results should be treated with care.

Table 6: Sustained and unsustained accelerations with income dummy

	Dependent variable: robust_episode					
	Sustained accelerations			Unsustained accelerations		
	Base (I)	GDP (II)	Dummy (III)	Base (IV)	GDP (V)	Dummy (VI)
adjposchange	-0.011	-0.009	-0.003	0.007	0.007	0.005
	(-1.10)	(-0.95)	(-0.26)	(0.71)	(0.72)	(0.50)
adjnegchange	0.017	0.026*	0.031**	0.062***	0.062***	0.057***
	(1.30)	(1.81)	(2.19)	(4.23)	(4.07)	(3.94)
adjtot_thresh90_pos	-0.003	0.001	0.005	-0.005	-0.005	-0.007
	(-0.47)	(0.18)	(0.64)	(-0.67)	(-0.64)	(-0.79)
adjeconlib_pos	0.035**	0.03*	0.021	-0.021**	-0.021**	-0.020**
	(2.13)	(1.91)	(1.46)	(-2.30)	(-2.29)	(-2.36)
log_rgdp		0.008***			0.000	
		(3.15)			(0.12)	
low_income			-0.037***			0.006
			(-4.90)			(0.98)
Observations	2040	2040	2040	2290	2290	2290
Accelerations	23	23	23	26	26	26
Pseudo-R^2	0.074	0.086	0.104	0.057	0.057	0.058

Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Numbers in parenthesis are robust t-statistics. * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$. All regressions include year dummy variable.