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Growth Accelerations Revisited

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LINK TO ABSTRACT

This paper comments on the *Journal of Economic Growth* article "Growth Accelerations" by Ricardo Hausmann, Lant Pritchett, and Dani Rodrik (2005), a seminal piece that seeks to identify significant determinants of growth accelerations. In this paper I respectfully refer to Hausmann, Pritchett, and Rodrik (2005) as HPR.

The contributions of this comment are threefold: First, this comment stresses some methodological issues of turning-point studies by reviewing the empirical strategy of HPR. Second, it corrects the original dataset as well as extends it from 1992 up to 2000, substantially increasing the sample size. Finally, it re-estimates the results using the improved dataset. Based on the evidence from the replication, the paper argues that the results in HPR are fragile to changes in sample and measures. Of 83 growth accelerations originally identified by HPR, only 45 are found robust using two updated GDP datasets. In contrast to the original finding, external shocks and positive regime changes are not significantly associated with growth accelerations. If any robust evidence is found, it is that economic reforms are correlated with sustained accelerations, while negative regime changes are associated with both unsustained and sustained growth accelerations. All the data are provided in the file linked at Appendix 1 at the end of this paper.

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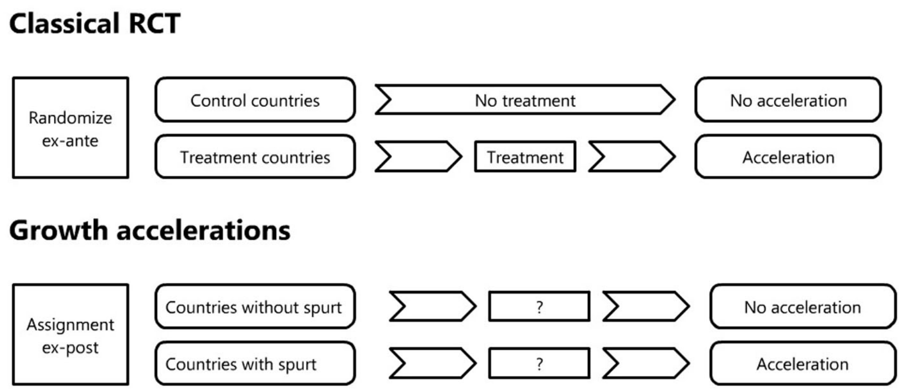
Methodological Issues

HPR use an unconventional approach to identify drivers of differential growth. Instead of running cross-sectional or panel estimations as in Barro (1991) or Islam (1995), HPR first employ a filter rule to identify sudden periods of growth accelerations. By then examining changes in policies and plausible variables around these turning points, the authors seek to isolate robust relationships between changes in policy and growth trajectory. Since publication of HPR, this novel approach has influenced related articles such as Ostry et al. (2007), Dovern and Nunnenkamp (2007) and Jones and Olken (2008). As of September 2010, the article had accumulated more than 50 citations in the Web of Science.

While the longitudinal approach of HPR appears particularly appealing for testing theories beyond averages, it faces familiar methodological weaknesses, such as omitted variables, endogeneity, and measurement errors. Ideally, these concerns 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 manipulate only a certain policy variable in the treatment group. It is hoped that, given the exogenous *ex-ante* group assignment, shocks and other unobserved confounds would be balanced across both groups. Any differential in growth performance across groups would then be causally attributed to the treatment.

Figure 1: A conventional randomized controlled trial (RCT) and the "pragmatic" growth accelerations approach



Even if such macroeconomic experiments are impossible, the growth accelerations article can be interpreted as a pragmatic version of the randomized controlled trial approach (see Figure 1). Similar to a randomized controlled trial (RCT), the strategy in HPR is to isolate effects of policies and shocks by comparing a treatment to a comparison group. The comparison is constrained in several ways, however. First, there are no exogenously created treatment and control groups. Instead, HPR 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, is 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.

When comparing episodes with accelerations to episodes without, a crucial assumption is that the 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 many factors that could possibly have driven the acceleration, posing a degrees-of-freedom problem when trying to find any drivers of growth (Durlauf et al. 2005). Even worse, there are 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 following HPR suffer the same methodological issues as typical cross-country regressions, complicating identification.

Measurement and Coding Errors

Extending the GDP estimates

HPR identify growth spurts using three criteria. Let $g_{t,\ t+n}$ denote the least squares average growth rate from t to $t+n$ and $\Delta g_{t,\ t+7}$ the change in average growth rate at t over horizon n. By definition, a growth acceleration has occurred if and only if:

$g_{t,\ t+7}$	$\geq$ *3.5ppa Growth is rapid*	(1)
$\Delta g_{t,\ t+7}$	$\geq$ *2ppa Growth accelerates*	(2)
y_{t+7}	$\geq$ *max(yi), i* $\leq$ *t Post-growth output exceeds pre-episode break*	(3)

A growth acceleration is *sustained* if the (least squares) average growth in $g_{t+7,t+17} \geq 2ppa$. Otherwise the acceleration is *unsustained*. If several subsequent periods qualify as a growth acceleration, HPR 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 Penn World Table 6.1 (PWT), a "surprisingly large number" (HPR 2005, 307).

Here I apply the same conditions to the newly available 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 de Haan 2008). If the original results are not artefacts of the filter, such small differences should not cause any significant differences in results.[2]

Based on PWT 6.3, 128 growth accelerations were found for the years 1957-2001. 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 for 1957-2001. Limited to a comparable sample, however, the number of acceleration

2. Considerable effort has been put in to reverse engineer the original rule. Professors Hausmann, Pritchett, and Rodrik did not respond to my queries about the timing rule.

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 (see Appendix 2). If taken seriously, this would suggest that more than half of the original 83 growth accelerations could be artefacts of measurement error.

It is discouraging that such errors even show up after heavy averaging (Johnson et al 2009).[3] 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 throughout the same period an actual *negative* average growth of -1.2% (PWT 6.3) and -4.5% (Maddison). 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). These selective examples constitute the largest discrepancies, but the sorts of measurement errors behind them are common.

To account for these errors, a synthesis of all datasets is used to obtain robust cases. I define a growth acceleration as *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 yields 51 robust accelerations for 1957-1992 and 19 for the extended period 1993-2000 (see Table 1).

Table 1: Growth accelerations by decades and dataset: Episodes/sustained episodes.

Growth accelerations				
Decade	**PWT6.1**	**PWT6.3**	**Mad**	**Robust**
1950	13/12	13/12	24/13	7/6
1960	23/11	29/16	45/20	18/7
1970	23/7	27/8	33/7	11/4
1980	16/7	21/10	16/10	11/9
1990	8/0	29/0	20/1	15/0
2000	Na	9/0	23/0	8/0
Total	83/37	128/46	161/51	70/26
Countries	110	125	137	121

3. Johnson, Larson, Papageorgiou, and Subramanian (2009) 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.

Finally, a sustained acceleration is robust if the average growth of a robust acceleration is $g_{t+7,t+17} \geq 2ppa$ 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.

Extending the regressors

The regressors are extended to prepare the subsequent probit replication. The variables of interest are *tot_thresh90*, *econlib*, *poschange* and *negchange*. The variable *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. All variables are extended up to 2000.

Polity IV: The variables *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. HPR, however, misled by faulty data description in Polity IV, have coded *any* change in Polity IV as a regime change, thus interpreting small scale transitions as fundamental changes —the problem pointed out and corrected for by Jong-A-Pin and de Haan (2008).[4] For example, Ghandi's interupted rule in 1977, a one unit point change towards democracy, is coded in HPR 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 occurred. In light of the numerous errors, I decided to recode the Polity IV index 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

4. Note, however, that the corrected index of Jong-A-Pin and Haan (2008) itself had some miscoded observations.

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, overall, 55 new regime changes in the extended sample between 1993-2001, 17 negative and 38 positive.

Economic reforms: The variable *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 openness. *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 openness 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.

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 data,[5] 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, HPR's reconstruction appears poor: 18% of the observations are coded differently across the variables, with the tendency that the new index reports more shocks than the old index. However, there is also evidence that the old variable had some coding errors: Even though the article reports the inclusion of lags, that does not seem to be the case when examining the data.

Nonetheless, the imperfect extension is a serious problem as it will complicate commensurability and possibly downward bias the estimated effect of shocks. Despite my investing a great deal of time in attempting to reverse-engineer the variable, I was unable to reconstruct a more precise variant. For pragmatic reasons, this variable will be used to extend the time series and the direction of bias

5. The naming of the file (*etot_thresh90*) bears similarity to variable names in Easterly's regressions. Professors Hausmann, Rodrik, and Pritchett did not respond to queries about the source of the data.

will be given attention when interpreting estimates. Some descriptive statistics for the new dataset are shown in Table 2.

Table 2: Portion of episodes preceded or accompanied by adjusted regressors.

	PWT6.1		PWT6.3		Maddison	
(a) Growth accelerations	57-92	93-00	57-92	93-00	57-92	93-00
Economic liberalization	12%	na	8%	33%	8%	36%
Positive regime change	10%	na	7%	7%	6%	27%
Negative regime change	13%	na	16%	7%	14%	0%
Positive ToT shock	21%	na	12%	13%	14%	18%
(b) Sustained accelerations	57-92	93-00	57-92	93-00	57-92	93-00
Economic liberalization	15%	na	13%	0%	15%	0%
Positive regime change	12%	na	8%	0%	9%	0%
Negative regime change	8%	na	8%	4%	15%	0%
Positive ToT shock	18%	na	13%	0%	12%	0%

Fragility of Regression Estimates

Overall, the data-gathering exercise increases the sample size by up to 50%, improving the statistical power of the inference. The replication strategy is as follows: The estimation is first confined to the old sample period and the original baseline is evaluated by plugging in the updated GDP datasets and adjusted regressors. The equations are then re-estimated using the full sample size, increasing the sample period to 2000. If the results in HPR are robust, correcting and extending the dataset should not yield any substantial differences.

Basic replication

In line with HPR, the general specification for all models is:

$$prob(episode_{it}=1)=\Phi(\beta_0+\beta_1 tot\_thresh90_{it}+\beta_2 econlib_{it}+\beta_3 poschange_{it}+\beta_4 negchange_{it}+T\gamma) \qquad (7)$$

where $episode_{it}$ is 1 if there is a growth acceleration within $[t-1, t+1]$ in country i and 0 otherwise. $tot\_thresh90_{it}$, $econlib_{it}$, $poschange_{it}$ and $negchange_{it}$ are 1 in $[t, t+4]$ following an event at t. T are time dummies to capture shocks common to all countries and Φ is the cumulative distribution function of the standard normal distribution. All specifications are estimated using a probit model, but the results do not change substantially when employing a linear probability model. I compute heteroscedasticity robust standard errors.

The replication results are presented in Table 3. Column I forms the original baseline, with terms-of-trade shocks and regime changes as significant predictors of growth accelerations. This original result, however, is fragile once alternative GDP data are used: Even with the original regressors unchanged, the effect of positive terms-of-trade shocks swings from significant to insignificant only by updating the PWT dataset (Column III). This sample dependence becomes even more apparent when replacing the PWT with the Maddison dataset (Column V), where the effect of positive regime changes likewise turns insignificant.

Table 3: Original sample size with different GDP datasets.

	Dependent variable: episode based on different datasets							
	PWT6.1		PWT6.3		Maddison		Robust	
	Orig. (I)	Adj. (II)	Orig. (III)	Adj. (IV)	Orig. (V)	Adj. (VI)	Orig. (VII)	Adj. (VIII)
poschange	0.029** (1.97)	-0.027 (-1.64)	0.026** (1.74)	-0.026 (-1.52)	0.021 (1.53)	-0.023 (-1.37)	0.030** (2.48)	-0.016 (-1.23)
negchange	0.108*** (5.80)	0.071*** (3.45)	0.076*** (4.13)	0.083*** (3.93)	0.099*** (5.42)	0.055*** (2.91)	0.089*** (5.38)	0.112*** (5.60)
econlib	0.022 (1.10)	0.04* (1.71)	0.008 (0.36)	0.026 (1.14)	-0.005 (-0.25)	0.005 (0.25)	0.003 (0.20)	0.017 (0.91)
tot_thresh90	0.045*** (2.62)	0.029** (2.29)	0.028 (1.55)	0.031** (2.37)	-0.005 (-0.33)	0.006 (0.51)	0.016 (1.23)	0.005 (0.53)
Observations	2140	2060	2026	1947	1853	1811	1793	1723
Accelerations	51	77	49	91	40	77	26	55
Pseudo-R^2	0.06	0.04	0.05	0.06	0.07	0.05	0.07	0.07

Notes: Estimated by probit. Coefficients shown are marginal probabilities evaluated at the sample means. Numbers 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 in the GDP data, Column VII reports a synthesis of the PWT 6.3 and Maddison datasets. Instead of using either dataset, *robust_episode* captures only those accelerations that are commonly identified in both. As before, an acceleration at t in PWT 6.3 is defined robust if the respective Maddison acceleration lies within $[t-2, t+2]$. Using the more reliable "average" of both datasets, positive regime changes turn up significant again but the effect of terms-of-trade shocks remains insignificant.

Column II, IV, VI and VIII repeat this exercise using the corrected regressors.[6] The results suggest that some original results could be driven by coding errors. Replacing the regime change variables with the corrected variants, the sign of positive regime changes swings, now 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). Negative regime changes remain robustly associated with growth accelerations in all specifications, but now the effect of economic reforms and external shocks is fragile depending on the underlying GDP dataset used.

Full sample

Table 4 reports the extended estimates based on different versions of the dependent variable. As a reference, the estimate in Column I is based upon the PWT 6.1 data and limited to the original sample size: As shown 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, 14 new accelerations are added. Now, positive regime changes exert a significantly negative effect. The positive effect of economic reforms and external shocks turns insignificant, leaving only negative regime changes highly significant (Column II). While the effect of negative regime changes persists when exchanging the PWT 6.3 data with the Maddison data, positive regime changes and economic reforms swing again in significance (Column III). Similar to last replication, Column IV reports a robust synthesis of the PWT 6.3 and Maddison data. Once more, the robust results suggest that the only reliable correlates of accelerations are negative regime changes, with economic reforms now insignificant.

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6. A stepwise replacement of the regressors is found in the Appendix 3.

Table 4: Full sample size with different GDP datasets.

Dependent variable: episode based on different datasets				
	PWT 6.1 (I)	**PWT 6.3 (II)**	**Mad (III)**	**Robust (IV)**
poschange	-0.027 (-1.64)	-0.024* (-1.69)	-0.011 (-0.78)	-0.010 (-0.92)
negchange	0.071*** (3.45)	0.046** (2.52)	0.034* (1.92)	0.066*** (4.10)
econlib	0.04* (1.71)	0.027 (1.62)	0.033* (1.99)	0.012 (0.97)
tot_thresh90	0.03** (2.29)	0.015 (1.21)	0.005 (0.40)	-0.003 (-0.36)
Observations	2060	3088	2817	2994
Accelerations	77	91	77	55
Pseudo-R^2	0.044	0.053	0.064	0.054

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

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 *tot_thresh90*, which was more sensitive in capturing shocks. While this cannot be completely ruled out, the results from the basic replication (see Table 3) suggest that it is unlikely that the extended results are driven by an imperfect extension: Even with regressors and sample period unchanged, replacing the PWT 6.1 with the new datasets causes terms-of-trade shocks to turn insignificant (see Table 3, Column VI and VIII). Based on the extension, the robust effect of negative regime changes remains the only reliable result, while the other estimates strongly depended on the sample period used.

Sustained and unsustained accelerations

Predicting accelerations lumps different types of accelerations together. In line with HPR, accelerations can be classified into unsustained accelerations and sustained accelerations. If both types of growth accelerations 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.

Table 5, Column I presents the results from HPR for sustained growth accelerations. These results remain robust when accounting for measurement errors using the combined dataset (Column III). Increasing the sample size and correcting for the coding errors, however, both positive and negative regime changes turn insignificant (Column II and IV). While the adjusted terms-of-trade shocks exert a significant effect in the original sample (Column II), the effect remains insignificant in the extended sample (Column IV).

Table 5: Full sample, sustained and unsustained accelerations with different datasets.

Dependent variable: episode based on different datasets								
	Sustained accelerations				Unsustained accelerations			
	PWT61		Robust		PWT61		Robust	
	Orig. (I)	Adj. (II)	Orig. (III)	Adj. (IV)	Orig. (V)	Adj. (VI)	Orig. (VII)	Adj. (VIII)
poschange	0.051*** (3.74)	0.004 (0.32)	0.041*** (3.33)	-0.011 (-1.10)	-0.004 (-0.34)	-0.022 (-1.52)	(drop)	0.007 (0.71)
negchange	0.038*** (2.82)	0.002 (0.16)	0.053*** (3.72)	0.017 (1.30)	0.076*** (4.85)	0.044*** (2.96)	0.099*** (4.56)	0.061*** (4.23)
econlib	0.170*** (4.14)	0.049** (2.31)	0.225*** (3.51)	0.035** (2.13)	(drop)	(drop)	(drop)	-0.021 (-2.30)
tot_thresh90	0.01 (1.20)	0.042*** (3.03)	0.004 (0.51)	-0.003 (-0.47)	0.065*** (3.63)	0.009 (0.74)	0.081*** (2.60)	-0.006 (-0.67)
Observations	1197	1634	904	2040	1222	1700	555	2290
Accelerations	12	29	12	23	18	27	9	26
Pseudo-R^2	0.11	0.11	0.17	0.07	0.13	0.06	0.15	0.06

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

Similarly, exchanging the GDP dataset does not substantially change the original results for the unsustained growth accelerations (Column V and Column VII). Once regressors are corrected, however, positive terms-of-trade shocks are no longer significantly associated with unsustained accelerations. The effect of negative regime changes for unsustained accelerations, on the other hand, remains robust across all tests (Column V to Column VIII).

The result—that economic reforms produce sustained accelerations, while autocratic transitions produce unsustained accelerations—is in line with HPR and seems intuitive, but there is some evidence of an omitted variable bias: Since sustained accelerations occur mostly in developed countries, whereas negative regime changes never occur in high income countries (Przeworski 2008), it is likely that the effect of negative regime changes on sustained accelerations is downward biased as it also captured the effect of the income level. Indeed, once the level of

GDP per capita is controlled for, the effect of negative regime change turns significant, once again (See Appendix 4).

Discussion

Even though replication is often considered tedious nitpicking, the results of this replication challenge some findings of HPR. By correcting and extending the dataset up to 2000, the paper provides evidence of fragility: Neither positive terms-of-trade shocks nor regime changes are robustly associated with unsustained or sustained growth accelerations.

Nonetheless, some robust evidence remains. In line with HPR, economic reforms, proxied as the beginning of trade openness, are significantly associated with sustained growth accelerations. The arguably most robust finding, however, is that negative regime changes are associated with both unsustained and sustained growth accelerations. This effect remains robust across all specifications and is large. 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. HPR 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, there is some evidence of an omitted variable bias.

The surprisingly robust result for negative regime changes 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 (see Appendix 5). Furthermore, the result is not likely to be caused by a misspecification 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 (see Appendix 4).

Implications for Further Research

This paper 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, 181). Examining the original HPR dataset alone, one finds a vast variety of controls and alternative proxies that have perhaps been regressed but not reported. 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 vulnerable to problems arising from the poverty of the 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 HPR), selection biases could occur, as missing values are often correlated with country characteristics. Turning-point studies focusing on rare events are particularly prone to missing values, as the approach often involves the loss of valuable observations. 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 possible that many policies were simply not picked up.

Concluding Remarks

Despite countless cross-country regressions, researchers have been unable to isolate the drivers of growth and explain the persisting income gap. While a turning-point study such as HPR proved promising in answering the question on which policies to pursue for growth, this paper suggests that even these findings are fragile upon changes in period, sample, measures, and inclusion of controls.

Even though not dismissing the utility of growth regressions altogether, the paper once more illustrates the pitfalls of macroeconomic growth empirics and contributes to falsifying—or at least challenging—some extant findings.

Appendices

Appendix 1: Zip file containing data description and all data used in this paper. Link

Appendix 2: Doc file of growth accelerations in three datasets. Link

Appendix 3: Baseline with corrected and extended regressors, stepwise replacement

Dependent variable: episode (PWT 6.1)				
	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*** (2.63)	
adj_poschange		-0.028* (-1.72)	-0.028* (-1.72)	-0.027 (-1.64)
adj_negchange		0.072*** (3.47)	0.071*** (3.46)	0.071*** (3.45)
adj_econlib			0.038* (1.65)	0.04* (1.71)
adj_tot_thresh90				0.03** (2.29)
Observations	2140	2060	2060	2060
Accelerations	51	50	50	77
Pseudo-R^2	0.059	0.044	0.045	0.044

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

Appendix 4: Sustained and unsustained accelerations with income controls

Dependent variable: robust_episode						
	Sustained accelerations			**Unsustained accelerations**		
	Base (I)	**GDP (II)**	**Dum (III)**	**Base (IV)**	**GDP (V)**	**Dum (VI)**
adj_poschange	-0.011 (-1.10)	-0.009 (-0.95)	-0.003 (-0.26)	0.007 (0.71)	0.007 (0.72)	0.005 (0.50)
adj_negchange	0.017 (1.30)	0.026* (1.81)	0.031** (2.19)	0.062*** (4.23)	0.062*** (4.07)	0.057*** (3.94)
adj_econlib	-0.003 (-0.47)	0.001 (0.18)	0.005 (0.64)	-0.005 (-0.67)	-0.005 (-0.64)	-0.007 (-0.79)
adj_tot_thresh90	0.035** (2.13)	0.03* (1.91)	0.021 (1.46)	-0.021** (-2.30)	-0.021** (-2.29)	-0.020** (-2.36)
log_rgdp		0.008*** (3.15)			0.000 (0.12)	

low_income			-0.037*** (-4.90)			0.006 (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. * $p < 0.01$, ** $p < 0.5$, *** $p < 0.01$. All regressions include year dummy variables.

Appendix 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* (1.74)	0.023 (1.64)	0.014 (1.46)	0.022** (2.21)
fdmhouse_neg		0.082** (2.54)	0.057** (2.14)	0.078*** (3.73)	0.142*** (4.53)
adj_econlib		0.047*** (2.61)	0.038 (2.48)	0.001 (0.19)	0.003 (0.47)
adj_tot_thresh90		0.008 (0.26)	0.054* (1.71)	0.034 (1.59)	0.312*** (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. * $p < 0.01$, ** $p < 0.5$, *** $p < 0.01$. All regressions include year dummy variables.

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