Jumps into democracy

Integrating the short and long run in the Democratic Transition

Martin Paldam and Erich Gundlach [1]

I. INTRODUCTION

A large literature deals with the relation between incomes and political regimes of countries. Section 2 gives a brief overview of this literature. It identifies an important gap in our knowledge: A *Democratic Transition* does appear in the long-run data, but recent empirical studies find that the short-run mechanism generating this transition is weak and causality dubious. However, a long-run relation between income and democracy can only occur if there is a short-run relation as well. This paper presents a simple new mechanism to bridge the gap.

We claim that the key to understanding the missing short-run relation is the observation that the two time series – for income and regime – have statistical properties that together violate the assumptions of standard regression analysis. Annual changes in income are much smaller than the level of income, which happens to be log-linear for many countries over time. By contrast, political regime indices have a bounded range with clustering at the top and are stepwise constant, with highly variable spells that last about 14 years on average. Regime indices change by jumps that sometimes cover a large fraction of the range.

Figure 1 sketches our new model, which has six variables, where two are special: One is the *transition curve*, which we estimate by kernel regression, and the other is the *tension*, which is the distance between the actual regime and the transition curve. The key mechanism in the model is that even when the jumps to a new regime are occurring (almost) randomly, the tension explains their *sizes* rather well. Thus, the transition curve is an attractor for the randomly occurring jumps. In this sense, the transition curve is a political equilibrium path.

[1]. MP: Department of Economics and Business, Aarhus University, Fuglesangs Allé 4, DK-8210 Aarhus V, Denmark. E-mail: mpaldam@econ.au.dk. URL: http://www.martin.paldam.dk. EG: Universität Hamburg and GIGA, Department of Economics, Von-Melle-Park 5, D-20146 Hamburg. Germany, E-mail: erich.gundlach@wiso.uni-hamburg.de. URL: http://www.erichgundlach.de

Figure 1

A sketch of the model
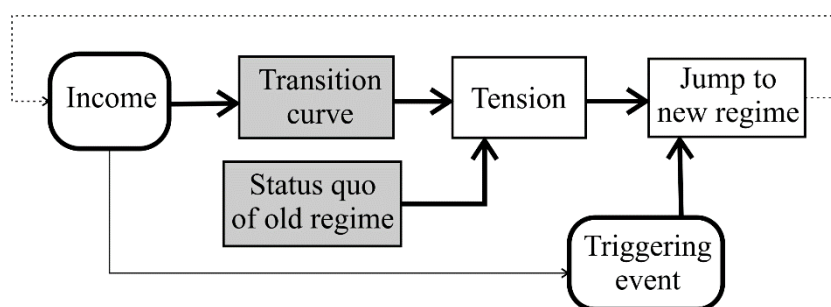
Note: The bold arrows are the main causal links. The thin arrows are marginal links.
Tables 2 and 3 below analyze the marginal link from income to the events.

In the model, income is exogenous and the triggering events occur randomly.[2] The old regime and the transition curve are predetermined. The two endogenous variables are the tension and the jump to the new regime. Thus, income is the exogenous variable that affects the regime. It does so indirectly through the tension, and in a way that is consistent with the transition curve. Consequently, the model explains the positive long-run correlation between income and democracy by allowing for both positive and negative income shocks as triggering events and for long spells of regime stability despite rising levels of income.

The model has two potential problems of simultaneity: One is the possibility that the new regime has an effect on income. This effect has been found to be small (see section 2.2), so it is unlikely to bias our analysis. The second possibility for bias occurs if the estimated transition curve depends upon current and future values of income and the political regime for the country where the jump occurs. The last paragraph of section 3.2 argues that the possible bias is negligible as well.

The data used are from the Maddison and Polity projects (see references). They overlap for 155 countries. To get a fairly balanced panel, we consider the 51 years from 1960 to 2010. Some data are missing, but our full sample has 6,997 annual pairs for income and the political regime.[3] Two subsamples are made, the *Main* sample (with 6,211 observations) and the OPEC sample. In the *Main* sample, the correlation between income and the Polity index is 0.56 and a kernel regression of the Polity index on income gives a perfect transition curve (see Figure 2).

The paper proceeds as follows: Section 2 is a brief survey of the related literature. Section 3 derives the transition curve, using kernel regressions. Section 4 presents a

---

[2]. All variables are explained in more detail below and in Table A1 in the online Appendix.
[3]. Missing values in the Maddison data for some countries in 2009 and 2010 are updated with values based on annual growth rates of per capita income taken from the World Bank Development Indicators (WDI).

comprehensive set of panel regressions on these data, confirming that the income coefficient is fickle. Bridging the gap between the two regression results, section 5 finds that triggering events happen almost randomly in an economic perspective in the sense that they are largely unaffected by income and its changes. In contrast, section 6 finds that if an event triggers a jump to a new regime, its direction and size correlate with the tension, i.e., with the distance between the actual regime index and its equilibrium value on the transition curve. Supplementary material and results are provided in an online Appendix to this paper.

II. EMPIRICAL RESEARCH ON INCOME AND DEMOCRACY

Most of the large literature on income and democracy deals with the long run, and tries to explain the strong correlation between the levels of two variables. We also know that the relation between the political regime indices and the growth rate is much weaker, see Docouliagos and Ulubaşoğlu (2008) for a meta-study covering 84 papers, until 2004. Below we concentrate on newer studies. It uses two approaches: The growth and development approach and the institutional approach – both come in several versions.

2.1. Growth and development theory: does income cause democracy?

Growth theory has found two basic steady states determined by technology: The traditional and the modern. Development theory deals with the *Grand Transition* from the traditional LICs (low-income countries) to the modern HICs (high-income-countries): All socio-economic variables we know about change substantially, and in much the same way, across countries and over time from the traditional to the modern. The systematic components in these changes are known as transitions (see Paldam and Gundlach 2008).

In this approach, technology causes income, and then income causes a democratization in the long run. Even if technology is made endogenous, the theory still predicts that the causal direction between the two variables goes from income to democracy. This is confirmed by Gundlach and Paldam (2009), and Paldam and Gundlach (2012), analyzing long-run causality by cross-country IV-regressions, using instruments for the long-run development potential of countries from Hibbs and Olsson (2004). The connection found is highly significant but leaves about half of the variation unexplained, so simultaneity cannot be completely ruled out.

Lipset (1959) was the first to note that most LICs are authoritarian, while most HICs are democracies. This observation led Lipset to formulate the modernization hypothesis, which is another name for the Democratic Transition. Many writers have reported empirical evidence for this transition (Burkhart and Lewis-Beck 1994, Helliwell 1994, Inglehart 1997, Barro 1999, Borooah and Paldam 2007) in line with our mentioned IV results.

The causal effect from income to democracy was attacked by Acemoglu *et al*. (AJRY) (2008, 2009), who used a dynamic panel specification with country- and time-fixed effects to demonstrate that income has no effect on the Polity index. Thus, they claim to have rejected short to medium run causality from income to democracy. They used this finding as discussed in section 2.2. This dramatic break has greatly influenced the subsequent literature.

Heid *et al*. (2012) report a positive income effect on democracy, using system GMM estimation, which assumes the unlikely independence of the (additional) instruments from the country-fixed effects. Moral-Benito and Bartolucci (MBB) (2012) add a quadratic term in log income to the AJRY specification to account for nonlinearities and find a positive income effect on democracy in low-income countries, but no effect in high-income countries.[4]

Fayed *et al*. (2012) report a negative effect of income on democracy based on Pooled Mean Group (PMG) estimation augmented by cross-sectional averages of all variables to control for cross-sectionally dependent residuals. This result is contrary to the long-run positive correlation. Brückner and Ciccone (2011) claim a window of opportunity for short-run democratic change after a negative income shock, based on their estimates for a sample of sub-Saharan African countries. Burke and Leigh (2010), Gassebner *et al*. (2012), and Dorsch *et al*. (2015) also present empirical evidence in favor of a negative income shock as a trigger for changes toward democracy (see also sections 5 and 6).

Benhabib *et al*. (2013) note – as we do – that the statistical structure of the two variables violates the assumptions behind the regression models used in most papers. With estimation methods that consider the different statistical properties of the variables, they find a positive income effect that is robust to the inclusion of country-fixed effects. Lundberg *et al*. (2016) explicitly test the linear specifications used by AJRY, CJSV, and MBB and reject them against alternative nonlinear specifications. Their results point to education as a variable that mediates the income-democracy relation, as has been claimed before by Armellini (2012) and by Murtin

---

[4]. Cervellati *et al*. (CJSV) (2014) find heterogeneous income effects by interacting the income variable with a dummy for former colonies:A negative income effect for (relatively poor) former colonies and a positive income effect for (relatively rich) non-colonies. The latter is in line with the hypothesis that rich countries remain democracies, while the former is in stark contrast to the modernization hypothesis.

and Wacziarg (2014).

Many authors have argued that though the main causal direction is from income to democracy, there is some simultaneity. The first theory in this vein is from Cheibub (1996), Przeworski and Limongi (1997), and Przeworski *et al.* (2000). While they argued that most causality is from income to democracy, they did argue that while high levels of development contribute to upholding democracy, rising levels of development do not elicit new democracies. Section 3.3 finds little evidence to support this hypothesis, and a number of authors have pointed out that it only holds for specific sample periods (Boix and Stokes 2003, Inglehart and Welzel 2005, Epstein *et al.* 2006, and Boix 2011).

This all suggests that the evidence is weakly in favor of a causal relation from income to democracy. It would greatly strengthen the credibility of this relation if we had a simple short-run model with a solid empirical base, such as we claim to have found.

2.2. Institutional theory: does democracy cause income?

Institutional theory sees institutions as the causal factor in development. Institutions are a wooly concept, but the political system must be an important institution. The relevant version of the theory is that democracy causes growth that determines income in the long run.

Acemoglu *et al.* (2005) have emphasized the primacy of institutions. AJRY (2008, 2009) argued that both income and democracy are driven by a common third factor, namely the (unobserved) deep power structure of a society. Thus, the strong correlation between income and democracy was held to be spurious. However, Acemoglu *et al.* (2016) now claim that democracy has a significant and robust positive effect on GDP per capita, even after controlling for country-fixed effects. This result appears to re-establish that the positive correlation between income and democracy is non-spurious.

Other authors have also pursued the idea that causality runs from democracy to income: Basso (2015) reports that democracy favors the fertility transition that is positively correlated with rising levels of income; Knutsen (2015) presents empirical evidence for a positive effect of democracy on income growth, mainly through its effect on technological change.[5]

This all shows that evidence in favor of a causal long run relation from democracy to income does exist. However, it is a small effect in the short-run, so it is unlikely to bias our

---

[5]. Nur-tegin (2014) suggests that new politically unstable regimes that replace stable autocracies may experience a worsening of the business environment, at least initially, which would tend to generate a negative link between democracy and income in the post-revolutionary period.

short-run estimates in sections 5 and 6.

2.3. What do fixed effects do?

A related literature sees 'culture' as the deep institutional factor that may matter for both long-run income growth and the political regime. Recent empirical contributions have tried to disentangle the complex web of endogeneity between measures of income, factor inputs, culture, and institutions.[6] However, culture is a slow-moving variable that cannot produce a significant omitted variable bias is the short run.

In the absence of better measures, slow-moving cultural factors may be controlled for with country-fixed effects, and intermediate variables with transitions may be controlled for with time-fixed effects. This explains why the inclusion of country-fixed effects has become the standard practice in the applied growth literature.

But, fixed effects estimation comes with a price: Sometimes the explanatory variable, in this case income, may be driven by country- and time-fixed effects itself (Hauk and Wacziarg 2010). Thus, there is a trade-off: including fixed effects may be necessary to control for omitted variables, but it comes at the danger of eliminating the variation of interest in the data.

Along these lines, Barro (2015) argues that missing statistical support for a positive income-democracy correlation in the presence of country-fixed effects may reflect econometric problems that arise in panels with a moderate time dimension. Barro finds positive effects of income (and schooling) on democracy without country-fixed effects in post 1960 data that do not disappear after the inclusion of country-fixed effects in a sample with a longer time dimension (post 1870 data).

The inclusion and exclusion of fixed-effects in the regressions in Table 1 in section 4 may explain some of the amazing variation of the reported income effects, notably the difference between estimates (1) and (2). We check how fixed effects for countries and time affect the relations in Tables 2 and 4 in sections 5 to 6. For the causal interpretation of our model, it remains important that the effect of the tension variable in Table 4 is robust to the inclusion of fixed effects.

---

[6]. For instance, Nikolaev and Salahodjaev (2017) test the hypothesis that the prevalence of infectious diseases influenced the formation of personality traits, cultural values, and even morality at the regional level, which then shaped economic institutions across countries. Davis (2016) studies the effect of individual vs. collective responsibility on long-run development by acknowledging that a taste for collective responsibility may have been adaptive in some preindustrial societies but not in others, and such cultural attitudes may persist to the present day, thereby simultaneously influencing social norms and economic outcomes.

III. KERNEL REGRESSIONS OF THE TRANSITION

Section 3.1 defines the variables, while section 3.2 introduces the kernel technique and argues that the *Main* kernel is a fine estimate of the Democratic Transition. Section 3.2 looks at the robustness of the transition curve; section 3.3 considers the skewness of the transition in the MENA/OPEC countries. Section 3.4 analyzes the variation around the *Main* transition curve.

3.1. Events, jumps and tensions

The paper deals with the dynamics of political regimes, but political regimes are unchanged in most years. Of the 6,997 Polity-scores, *P*, only 721 differ in value from the preceding year's value. We say that an *event* happens when the Polity score changes. The size of the change is the *jump*, with two exceptions. (1) Observations with $P = 0$ refer to anarchy (no regime), so changes to/from zero are not counted as a jump (this is 9% of all events); and (2) sequences of changes in the same direction over consecutive years are counted as a single jump.

(1) $J_{i,t} = P_{i,t} - P_{i,t-1}$ a *discrete* change of Polity in country *i* at time *t*

         $J_{i,t} = P_{i,t+k} - P_{i,t-1}$ a *sequence* of *k* jumps in the same direction in consecutive years[7]

These definitions give 555 jumps in our full sample and 515 jumps in our *Main* sample. Thus, about three quarters of all events *trigger* a jump to a new political regime. Section 5 studies how economic variables can explain *when* events occur. Section 6 studies how the same economic variables explain the *size* of the jumps caused by the triggering events.

     The hypothetical transition curve, *Π*, is an equilibrium path in the sense that if income would become constant at a certain level *y**, the regime would be predicted to converge to *Π(y*)*; i.e., the vertical distance between $P_{i,t}$ and $\Pi(y_{i,t})$ would drop to zero. Consequently, the distance between the actual regime and the equilibrium regime is termed the *tension*:

(2) $T_{i,t} = \Pi(y_{i,t}) - P_{i,t-1}$

     The tension is negative if the country has 'too much' democracy at its level of income.

---

[7]. A discrete jump requires that $J_{i,t}$, $P_{i,t}$, $P_{i,t-1} \neq 0$, and that $J_{i,t-2}$, $J_{i,t+1} = 0$. A sequence is counted as one jump in the year of the first jump. A sequence requires that $J_{i,t-1}$, $J_{i,t+k+1} = 0$ (or jumps in different directions). An event is a binary (0, 1) variable, while a jump is an integer in the interval from −20 to +20.

If income increases and the regime is constant, the tension decreases. If a triggering event occurs, we expect a negative jump, which is a jump towards less democracy. Conversely, the tension is positive if the country has 'too little' democracy at its level of income. If income increases and the regime is constant, the tension increases. If a triggering event occurs, we expect a positive jump, which is a jump towards more democracy.

Equations (1) and (2) look alike, but the correlation between the jumps, $J_{i,t}$, and tensions, $T_{i,t-1}$, is −0.13 in the *Main* sample. It increases to −0.50 if only the change-years are considered. Sections 5 and 6 discuss this difference in more detail.

3.2. Kernel regression and the path of the Democratic Transition

A kernel regression is a smoothed moving average process, with a fixed bandwidth, *bw*. The estimate is based on pooled data sorted by income, which scrambles the country and time dimensions. Table A4 (online Appendix) shows that the scrambling is satisfactory except at the two ends of the distribution, which are dominated by a few countries only. The scrambling allows the analysis to concentrate on the 'general' long-run pattern, and the kernel technique imposes no functional form on the data, so it allows us to see the shape of the transition curve.

Figure 2

A kernel regression on the *Main* sample (*N* = 6,211) and the OPEC sample (*N* = 561)
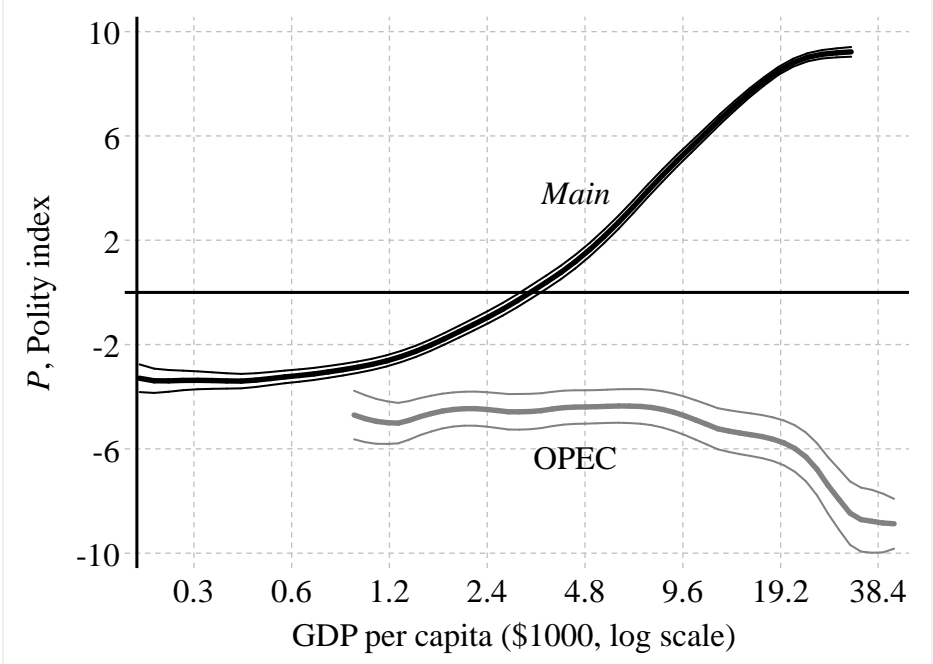
Note: Cross-country panel data, 1960-2010 (Maddison Project 2013; Marshall *et al*. 2016). Epanechnikov kernels with degree zero and bandwidth 0.5 (thick lines) with 95% confidence intervals (thin lines). GDP per capita in 1990 international Geary-Khamis dollars.

The kernel for the *Main* sample on Figure 2 is our core estimate of the Democratic transition, $\Pi = \Pi(y)$, where y is income. The Π-curve is a perfect transition curve that diverges from the stable political regimes at the low end (where Polity is −3) and converges to a new stable level at the high end (where Polity is 9.5). On average across all data points, a political regime changes by 3 Polity-points per log-point of income, which corresponds to the effect of income on the Polity index reported in regression (1) of Table 1 below.

The curve is robust to the smoothing formula – we use the Epanechnikov's kernel that is the default in Stata. The sensitivity of the curve to the choice of the bandwidth is analyzed in section 3.2. The amazingly narrow confidence intervals are calculated from the point variance, using a pilot interval. With a high number of observations ($N = 6{,}211$), the confidence intervals become narrow, even when the observations from the individual countries scatter widely. While the *average* distance between the Polity index and the transition curve, namely the tension T, is zero by construction), the standard deviation of T is no less than 6.11, so the scatter around the transition curve is substantial.

The introduction mentioned a second simultaneity problem. It occurs when the data for the present and future Polity index for a country are used in the estimate of the Π-curve that is used to calculate the present tension. Since our sample covers 155 countries, each country has a weight of $1/155 = 0.6\%$ in the estimate of the Π-curve. On average, half of these observations are before the point in time of interest, so only the other half gives a potential simultaneity problem. This reduces the relevant weight of each country to 0.3%. The scrambling and the averaging spread this problem over a considerable interval in the data, so we are dealing with a negligible problem.

3.3. Robustness of the transition curve

Figure 3 reports the transition curve when the bandwidth, *bw*, is varied from 0.2 to 0.8. If it is set too small, the curve becomes wobbly, and if it is set too large, the curve becomes linear and the slope falls until it becomes zero at the mean. The two black curves for *bw* = 0.4 and 0.6 are close to the curve for 0.5 on Figure 2. The curve for *bw* = 0.2 is wobbly at the low end, where the data are thin, and the curve for *bw* = 0.8 becomes rather linear and misses the convergence at the top. However, the basic form of the curve remains rather stable and for most of the range, the confidence intervals (not shown) overlap nicely.

Figure 3

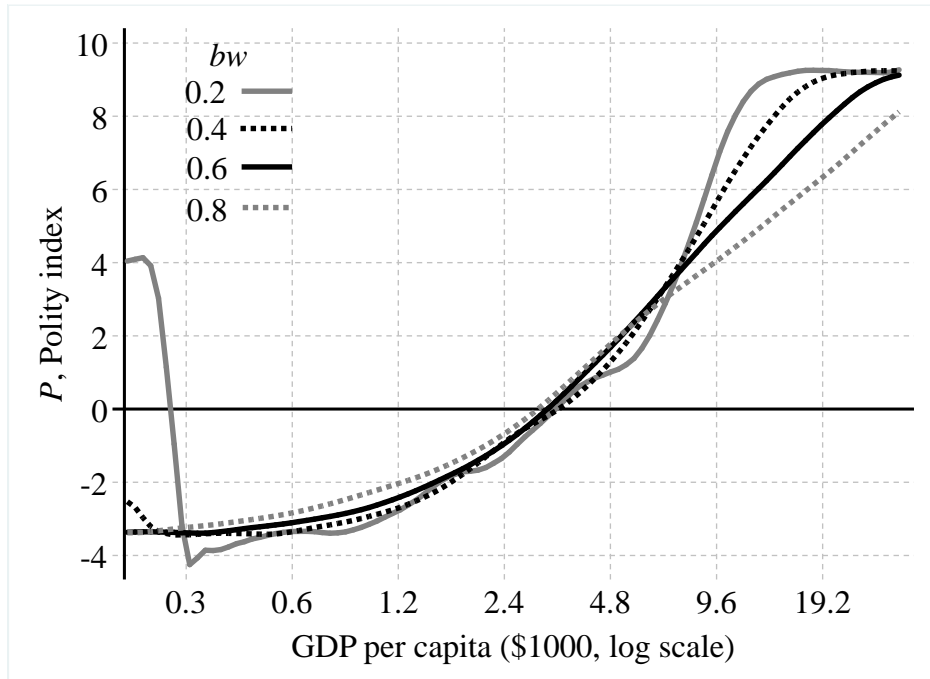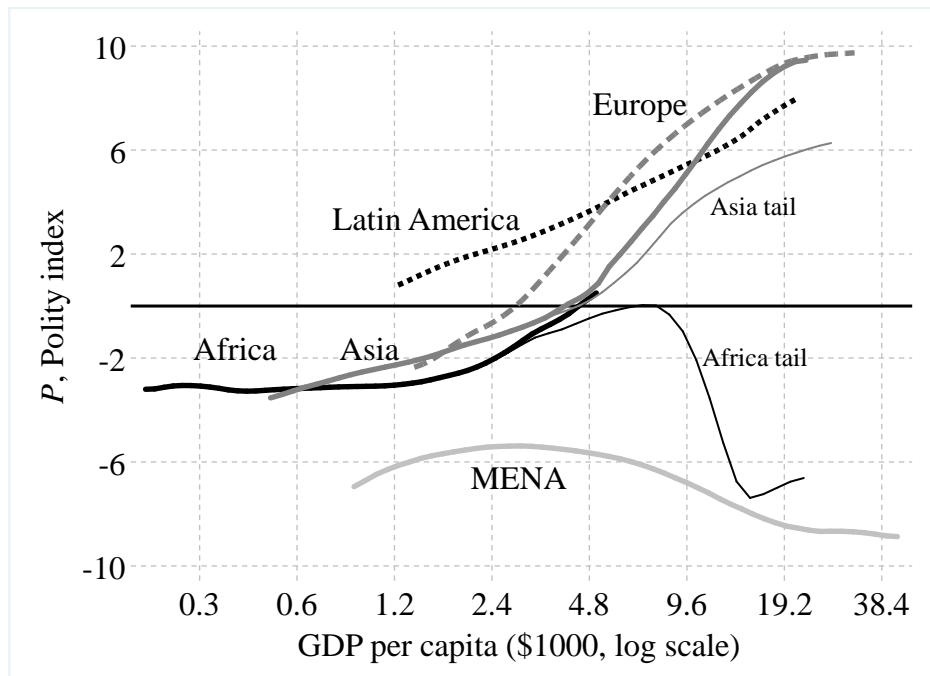The kernel-curve for the *Main* sample on Figure 2 for bandwidths of 0.2 to 0.8

Figure 4

Kernels for the transition in five country groups (OPEC members included)

Note: See Figure 2. Two country groups have one outlier each: Africa has Equatorial Guinea (that has become an oil country) and Asia has Singapore (that has a very unusual regime that is difficult to score in an index like Polity). The main curves are without the outliers and depicted as thick. The thin 'tail' curves include the outliers.

Figure 4 reports what happens when the 155 countries are divided in five standard groups: *Africa* (Sub-Saharan), *Europe* (incl. four overseas, Caucasus and Israel), *Latin America*, *MENA* (Middle East and North Africa), and *Asia,* which are all other countries (incl. Mauritius). For each country group, the sample size is reduced. Consequently, the confidence intervals become wider, and they overlap for most of the range for four groups: Africa, Asia, Europe and Latin America. Thus, the general transition curve from Figure 2 is a fine generalization of the Democratic Transition in the countries in these four groups. Only the MENA group – that has much overlap to the OPEC sample – is different.

3.4. The nexus of the MENA, OPEC and the Muslim country samples

MENA is a comparatively homogenous group of countries in terms of culture and religion.[8] Many are also OPEC countries or linked to OPEC in many ways: The kernels of the OPEC sample from Figure 2 and of the MENA group from Figure 4 are rather similar – and at the high-income end, the data are actually the same. These kernels indicate that the wealthier these countries get, the more authoritarian they become. The kernels for the *Main* and the *OPEC/MENA* samples have no overlap of confidence intervals. Both kernels may start at about the same Polity-level, for low incomes. But when income grows, the slope of the *OPEC/MENA* curve flattens out and turns negative. This is the opposite of the result for the *Main* sample.

The OPEC/MENA exceptionalism may be explained by culture, or by a political economy version of the Dutch Disease mechanism.[9] An oil sector is an enclave in the economy that has few links to the rest of the economy except through the inflow of resource rent to the treasury, which is controlled by the government. Thus, an authoritarian ruler can afford both an adequate armed protection of his regime and a distribution of rents to purchase a solid coalition in support of his regime. This gives a drift toward a still more authoritarian regime. While the Polity-score goes to +10 with rising levels of income in other countries, it tends to go to −10 in the richest OPEC countries. This may be termed the political resource curse. Other resource-rich countries such as Norway, which have gone through the Democratic Transition long before the oil resource was exploited, apparently do not get trapped by a political resource curse and remain democracies.

---

[8]. MENA are all Arab countries, which share the same language, plus Turkey and Iran that both differ substantially from the Arab group.

[9]. See Paldam (2013) for a survey on the political economy of Dutch Disease, and Paldam (2009) on the influence of Islam on the political regime. Borooah and Paldam (2007) find that both explanations work independently.

3.5. The regime variability over the transition for the Main sample

The variability of the Polity-score over the transition is analyzed by the income-sorted and stacked (P_j, y_j)-data of the sample used for Figure 2. Treating these data as if they represented an ordered sequence of observations, a running standard deviation of the Polity-score, $Sdt_k(P)$, is calculated for a moving sequence of $k = 51$ Polity-scores. Each $Std_{51}(P)$ is placed next to the mid observation of income in the relevant interval to give the $(Std_{51}(P)_j, y_j)$-dataset. The $(Std_{51}(P)_j, y_j)$-set is analyzed by kernel regressions as before, giving Figure 5. The underlying procedure is a double 'averaging', first over the k-sequence, and then by the kernel regression. This causes very narrow confidence intervals for the kernel curve.

Figure 5

Kernel of the $(St_{51}(P), y)$ relation for the *Main* sample
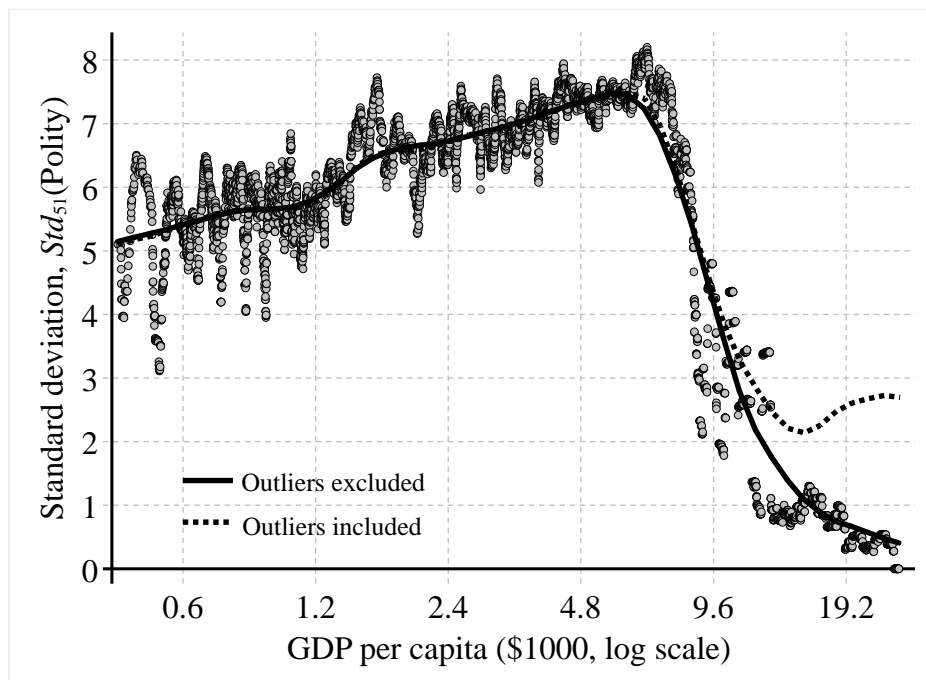
Note: See note to Figure 2. $N = 6,111$ and $bw = 0.25$. The two outliers are Equatorial Guinea and Singapore as in Figure 4.

The robustness of the kernel is analyzed by varying the bandwidth and k, the size of the moving sequence of Polity-scores. The result proves to be stable to a wide range of both parameters. The figure is drawn with and without the two outliers from Figure 4 (Equatorial Guinea and Singapore).

The key result is that the Polity-scores have a rather high and growing standard

deviation in the income range up to a GDP per capita of about $7,000, which points to a high degree of political regime volatility from low to medium income levels. When income increases beyond $7,000, the declining kernel-curve indicates a substantial decrease in political regime volatility, from a standard deviation of the Polity-score of over seven to well below one. Excluding the two outliers, it appears that from an income levels beyond $12,000 countries reach the modern steady state, where they become stable democracies, which is in line with the end-of-history hypothesis advanced by Fukuyama (1992).

IV. PANEL REGRESSION ESTIMATES OF THE TRANSITION

Section 4.1 introduces the empirical model, and Table 1 gives a set of 11 estimates. Section 4.2 discusses Part A of the table, which gives pooled parameter estimates, while section 4.3 turns to Part B of the table, which gives heterogeneous parameter estimates. Section 4.4 visualizes the common dynamic process that underlies the Polity-income data.

4.1. Panel regressions to identify the Democratic Transition

Our regression results are based on *pooled* and *heterogeneous* parameter models. A common feature of the pooled models is that the within-effects of the explanatory variable *income* and the effects of common shocks are restricted to be the same for all countries in the sample. By contrast, the heterogeneous models allow for country-specific income effects and for country-specific effects of common shocks. A dynamic specification of the Democratic Transition across countries i, over time t, with Polity, P_{it}, and income, y_{it}, can be written as

(3) $P_{it} = b_{1i} P_{i,t-1} + b_{2i} y_{i,t-1} + u_{it}$ with $u_{it} = \mu_i + \lambda_i f_t + \varepsilon_{it}$,

where $b_{2i}/(1-b_{1i})$ is the country-specific (heterogeneous) long-run parameter of interest and u_{it} is an error term that includes an unobserved country-specific effect μ_i and an unobserved common factor f_t with country-specific (heterogeneous) factor loadings λ_i.

The most popular panel estimators in the empirical growth literature (POLS, 2FE, Difference-GMM, System-GMM) impose the restriction of common within effects ($b_{ji} = b_j$) and identify μ_i and f_t with country and year dummies (or first-differencing and cross-sectional demeaning). However, common shocks may have different effects across countries (country-

specific factor loadings), and some variables may be nonstationary, leading to potentially biased pooled parameter estimates. More flexible *mean group* panel estimators have been developed by Pesaran and Smith (1995), Pesaran *et al.* (1999), Pesaran (2006), and Bond and Eberhardt (2013). We use a broad range of both types of estimators.

Table 1

Regressions using a range of estimators

	Part A. Pooled parameter models				
	(1)	(2)	(3)	(4)	(5)
	POLS-T	**2FE**	**AB**	**BB**	**CCEP**
Income per person	**3.21**	**−2.90**	−10.52	**1.88**	−0.30
[z-statistic]	[7.8]	[3.0]	[1.5]	[2.3]	[−0.3]
Observations	5,688	5,688	5,568	5,688	4,905
Countries	118	118	118	118	118
RMSE	1.73	1.70	1.66	1.81	1.57
Non-stat. residuals (CIPS *p*-val.)	0.00	0.00	0.00	0.00	0.00
Weak cross-sec. dependence (CD *p*-val.)	0.00	0.00	0.00	0.00	0.00
Instrument count			58	67	
AR1-*p*			0.00	0.00	
AR2-*p*			0.19	0.18	
Hansen test of overid. restrictions (*p*-val.)			0.29	0.04	
Diff.-in-Hansen test of IV subset (*p*-val.)				0.05	
	Part B: Heterogeneous parameter models					
	(6)	(7)	(8)	(9)	(10)	(11)
	PMG	**MG**	**CD-MG**	**CCEMG**	**AMG-D**	**AMG-S**
Income per person	−0.56	0.46	**−3.60**	0.77	−1.45	−1.27
[z-statistic]	[−1.7]	[0.5]	[−2.3]	[0.4]	[−1.7]	[−1.2]
Common dynamic process					**0.41**	**0.96**
[z-statistic]					[6.0]	[7.3]
Observations	5568	5568	5568	4905	4120	4120
Countries	118	118	118	118	103	103
RMSE	1.55	1.68	1.64	1.44	1.48	2.18
Non-stationary residuals (CIPS *p*-val.)	0.00	0.00	0.00	0.00	0.00	0.99
Weak cross-sec. dependence (CSD *p*-val.)	0.01	0.00	0.00	0.82	0.58	0.04

Notes: Cross-country time series data, 1960-2010. OPEC members and countries with less than 21 consecutive time series observations excluded. All estimates based on dynamic model, except AMG-S. Reported coefficients are long-run income effects. Bolded coefficients are statistically significant at the 5% level.

POLS-T: Pooled OLS with time-fixed effects. 2FE: Two-way Fixed Effects. AB: Difference-GMM (Arellano-Bond) with restricted instrument count BB: System-GMM (Blundell-Bond) with restricted instrument count. CCEP: Common Correlated Effects Pooled including year fixed effects and 3 lags of the cross-section averaged variables. PMG: Pooled Mean Group using 4 lags of cross-section averaged variables. MG: Mean Group. CD-MG: Cross-sectionally Demeaned Mean Group. CCEMG: Common Correlated Effects Mean Group. AMG-D/S: Augmented Mean Group; dynamic model/static model.

CIPS: Correlated-Im-Pesaran-Shin panel unit root test for non-stationarity of residuals. CSD: Test for weak cross-sectional dependence of the residuals.

4.2. Part A: Pooled parameter models

The estimates reported in the first and the second columns of Part A of Table 1 should reveal a reasonable range of the effect of income on the degree of democracy. Due to the inclusion of the lagged endogenous variable, pooled OLS (POLS) and two-way fixed effects (2FE) are known to produce biased results, though in different directions. This suggests that the true income effect is expected to be somewhere within the range given by the two reported estimates – which is of little help in the present case because the range includes zero.

In the same way, the AB (Arellano-Bond) and the BB (Blundell-Bond) estimators give results with different signs, while the CCEP (Common Correlated Effects Pooled) estimator gives a statistically insignificant coefficient close to zero. Thus, the results for the pooled parameter models do not provide convincing empirical evidence for a positive effect of income on democracy, in line with results of the recent literature.

The residual diagnostics for all pooled estimators suggest that the null hypothesis of non-stationary residuals is rejected, which allows for the possibility of a cointegrating equilibrium relation between the degree of democracy and per capita income.[10] However, the null hypothesis of weak cross-sectional dependence of the residuals is rejected for all estimators, which implies that there is strong cross-sectional dependence in the residuals, thereby violating the conditions for unbiased estimates.

4.3. Part B: Heterogeneous parameter models

Part B of Table 1 reports the results for estimates of the Democratic Transition that are based on heterogeneous parameter models. All estimators run country-specific regressions to allow for individual income effects (which are reported as unweighted cross-country averages), but differ with respect to the modeling of common shocks and weak cross-sectional dependence of the residuals. Four variants are considered.

The Pooled Mean Group (PMG) estimator (Pesaran *et al*. 1999) allows for short-run country-specific effects, but imposes the restriction that the long-run effects are the same for all countries. Like the PMG estimator, the mean group (MG) estimator (Pesaran and Smith 1995) does not control for cross-sectional correlation with a year dummy, but when it is

---

[10] The Correlated-Im-Pesaran-Shin (CIPS) unit root test for non-stationarity is implemented with the Stata module `pescadf` (Lewandowski 2007). The CSD test for weak cross-sectional dependence Pesaran (2015) is implemented with the Stata module `xtcd2` (Ditzen 2016a).

estimated on cross-sectionally demeaned data (CD-MG), it implies that a common shock has the same effect in each country (like the pooled estimators that include a year dummy). The Common Correlated Effects Mean Group (CCEMG) estimator (Pesaran 2006) augments the country-specific regressions with panel cross-section averages of the dependent and independent variables to allow for unobserved country-specific effects of common shocks, but treats the implicit estimates as nuisance parameters that cannot be interpreted.

The Augmented Common Correlated Effects Mean Group (AMG) estimator (Bond and Eberhardt 2013) goes a step further by explicitly identifying a common dynamic process (CDP) that is caused by otherwise unobservable variables.[11] The idea is to run a first-stage regression of (3) in first differences and to collect the estimated coefficients on the (first-differenced) year dummies (f_t), which are held to capture the common evolution of unobservables in the level of *P* across countries and over time. This common dynamic process is plugged back into equation (3) as an additional covariate and yields, in the second-stage regression, an explicit estimate of the mean effect of unobservables on the degree of democracy.

Part B of Table 1 reveals that allowing for country-specific effects in combination with a more sophisticated modeling of the error term (see equation (3)) apparently does not help to find statistically significant positive effects of income on the degree of democracy. The only exception is the CD-MG estimator, where a negative income effect comes with a rejection of the null of weak cross-sectional dependence of the residuals. For all other heterogeneous models, the coefficient on income is statistically insignificant with favorable residual diagnostics in the sense that the null of non-stationary residuals is rejected, which is required for a possible cointegration between income and democracy. However, only CCEMG and the dynamic version of AMG (AMG-D) do not reject the null of weak cross-sectional dependence of the residuals. Hence, even the two statistically preferred estimators do not identify a robust direct effect of income on the degree of democracy.

The main positive result of part B is that the two AMG estimators confirm the presence of a common dynamic process as a statistically significant driver of the transition from an authoritarian to a democratic regime. Figure 6 reveals that the common evolution of the unobservables has about the same shape as the stylized transition path from Figure 2 (which is why the reported regression coefficient in part B of Table 1 is statistically significant). Our interpretation is that the kernel regression and the common dynamic process identified by the

---

[11]. PMG, MG, and CCEMG are implemented with the Stata module `xtdcce2` (Ditzen 2016b); AMG is implemented with the Stata module `xtmg` (Eberhardt 2012).

AMG estimator both point to the existence of a long-run pattern in the degree of democracy.
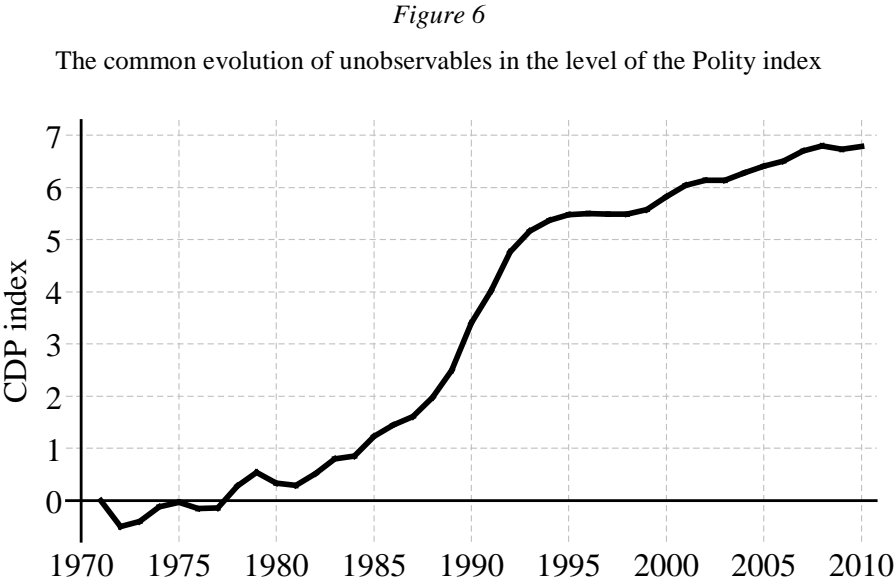
Note: Covariate derived from the first stage regression (in first differences) of the AMG estimator; based on static model.

The kernel regressions in section 3 show a clear link between income and democracy, but they cannot control for omitted variables. The panel regressions in Table 1 do not show a comparable link between income and democracy for a broad range of pooled and heterogeneous estimators. The introduction claimed that the statistical properties of the two variables income, y, and Polity, P, are so different that it is unlikely that y can explain P within a standard regression model. Nevertheless, it is evident that rich countries are more democratic than poor countries.

The main statistical problem appears to be that the Polity-variable is a bounded step-wise stable variable, where infrequent jumps of variable size interrupt substantial periods of stability. Also, the kernel estimates and Figure 6 reveal that there are nonlinearities involved at both ends of the range: in the evolution of the Polity index relative to the income level (pooled panel data) and in the common evolution of the unobservables that drive the Polity index according to the AMG estimates (cross-country averaged data).

Sections 5 and 6 model the link between income and democracy, taking these properties of the data into consideration. The logic of the modeling has already been set out in Figure 1. In our view, the key point is to distinguish between the almost fully random timing of the events that trigger a regime change and the much more predictable direction and size of the jump to a new regime once a triggering event has happened.

V. EVENTS ARE ALMOST RANDOM

Table A3 in the online Appendix reports that the average regime lasts 14 years. A regime typically develops into a *status quo equilibrium* after a couple of years. It is stable for four reasons: (i) All regimes try to build a protective apparatus and (ii) some legitimacy. (iii) They also develop schemes that distribute rents to create stakeholders who support the regime, as they may lose their stake if the regime changes. (iv) Regimes are protected by the first mover disadvantage: Activists who want to change an authoritarian regime run a high personal risk – they may even be shot.[12]

Table 2

OLS regressions explaining the 675 events, *E*, in the *Main* sample

$N = 6,211$	(1)	(2)	(3)	(4)	(5)
Initial tension, $T_{(-)}$	0.000 (−0.1)	0.000 (0.1)	**0.002** (2.1)		0.000 (0.4)
Initial income, $y_{(-)}$	**−0.032** (−9.2)	**−0.025** (−2.3)	**−0.081** (−5.3)	**−0.032** (−9.3)	
Growth, g	**−0.002** (−3.5)	**−0.003** (−3.7)	**−0.003** (−3.7)	**−0.002** (−3.5)	
Growth 5 years, $g5$	**−0.004** (−3.7)	**−0.004** (−3.6)	**−0.003** (−2.1)	**−0.004** (−3.7)	
Constant	**0.371** (13.3)	0.206 (1.1)	0.544 (1.5)	**0.370** (13.4)	**0.099** (26.2)
FE for countries	No	Yes	Yes	No	No
FE for years	No	No	Yes	No	No
R^2 net of FE	0.024	0.006	0.009	0.024	0.000
R^2 of FE		0.060	0.073		
N	6,211	6,208	6,208	6,211	6,211

Note: See Table 1. For easy reference, the variables are defined in Table A1 (Appendix). The effect of the fixed effect is reached by running the regression in the column without the four economic variables. The difference between the R^2 of 0.024 in columns (1) and (4) and the R2's in columns (2) and (3) is a measure of the collinearity of the 4 variables, $T_{(-)}$, $y_{(-)}$, g and $g5$ and the dummies. Stata deletes some degrees of freedom when all the dummies are included. The tension variable above the dashed line is for comparison with the results in Table 4.

The data for the *Main* sample contains 675 events, where the Polity-score changes. The international media (like The Economist) normally report these events, so they are easy to look up. Most are the results of political conflict, such as an internal fight within the regime, a corruption scandal, the succession after the death of the ruler for natural or other reasons, etc.[13]

---

[12]. In non-democratic regimes, there is a trade-off between the loyalty of citizens and the repression needed to obtain regime stability (Wintrobe 1998). Most of the rich Western countries have a perfect polity score of 10, which indicates a high level of political legitimacy and the absence of political oppression.

[13]. In work in progress (Paldam and Gundlach 2018), we try to identify the determinants of the triggering events of 245 larger system jumps in 170 countries. Preliminary results suggest that triggering events are dominated by political shocks, while economic shocks play a marginal direct role at best.

Consequently, events do not follow a common pattern and they are hard to predict from the perspective of economics. Table 2 reports OLS regression results. The corresponding probit regressions (see Table A5 in the online Appendix) give similar results.

We try to explain the variation of the events with the following five variables, which are explained in more detail in Table A1 of the online Appendix: the initial income, $y_{(-)}$; the initial tension, $T_{(-)}$; the annual growth rate, g; and the average growth rate over the preceding five years, g5. In addition, fixed effects for countries and years are included in some of the regressions.

With $N = 6,211$, 'everything' is normally statistically significant. This is also the case in Table 2, even when the regressions explain only a small fraction of the variation. The country- and year dummies provide about 85% of the explanatory power – such as it is. The main result is that events are largely independent of the included explanatory variables. The most important observation from Table 2 is that the tension variable, which plays a key role in section 6 below, turns out to explain none of the variation in the events. Hence, the probability of an event does not depend on the distance of the *Polity*-score from its equilibrium value on the transition curve.

The coefficients to both growth variables are negative and statistically significant, but they are tiny. Consider the averaged estimated coefficient of growth g of 0.0025 in the regressions (1) to (4). Imagine a boom where the economy grows by 3 percentage points faster than it usually does. Taking the estimated coefficient at face value, such a boom would reduce the chance of a political regime change by no more than 3 x 0.0025% ≈ 0.0075 (percentage points). For the averaged 5-year growth rate g5, the estimated effect is a bit larger, but still small.

Governments and regimes that are successful in generating high economic growth may become popular and hence more stable, so that the coefficients on growth should be negative, but it has also been noted that high economic growth is disruptive for old political structures, so that the coefficients on growth should be positive. We do find that a positive change in the growth rate has a negative effect on the probability of an event, but the size of the effect is close to zero.

Table 3 studies the effect of income in another way. It considers *triggering* events that actually lead to a regime change: The 675 events discussed in Table 2 give only 515 jumps. The count data find that the number of jumps falls with a rising income level. The fall is not so strong in the beginning, but then it becomes substantial. This is as expected from section 3. At high-income levels, the countries are already democracies and the populations want no further

changes in the political regime. The expected stability (absence of events) of the political regimes at low-income levels is not confirmed, but then there are only few, if any, countries left in the traditional steady state where modern development has not (yet) started.

<div align="center">*Table 3*</div>

<div align="center">The number of jumps in six equally large intervals of the sorted data of the *Main* sample</div>

Interval of 1/6	Counts		Frequency of jumps	Binominal tests of fall [a)]			Income interval	
	N	Jumps		1-step	2-step	3-step	from	to
First	1035	120	0.116	n.a.	n.a.	n.a.	5.319	6.748
Second	1035	112	0.108	23.3	n.a.	n.a.	6.749	7.261
Third	1035	102	0.098	15.1	3.3	n.a.	7.261	7.952
Fourth	1035	98	0.095	38.5	9	1.6	7.952	8.517
Fifth	1035	71	0.069	0.3	0	0	8.517	9.215
Last	1036	12	0.012	0	0	0	9.217	10.363
All	6211	515						

a. The test is the probability that *n* or less of 1035 draws with the frequency of the preceding 1, 2 or 3 cells occurs by chance. The tests show a significant downward trend.

The result in Table 3 reflects that many LDCs have political regimes built around a single person. When an event triggers a change of that person, there is often a regime jump. This is not the case in developed countries, where widely respected institutions secure that rulers can change without a system change.

VI. JUMPS ARE EXPLAINED BY THE TENSION

Section 6.1 compares statistical explanations of jumps and events. Sections 6.2 and 6.3 analyze the direction and the overshooting of the jumps. While small jumps are unpredictable system adjustments, the tension predicts the direction of the larger jumps rather well, which normally overshoot giving irregular cycles around the transition curve.

6.1. The 515 jumps in the Main sample

Table 4 reports that the five explanatory variables from Table 2 explain a much larger fraction of the variation in the jumps than in the variation of the events. Now the tension variable, *T*, is the dominating variable, as seen from regressions (1) to (3) and (5). When it is excluded in

regression (4), the R^2-score drops to 0.005. The tension variable is a function of Polity, $P(y)$, hence T has some covariance with y; but income is statistically insignificant when T is omitted in column (4). The two growth variables have no measurable effect. The estimated effects of the tension, T, are all positive with a size between 0.5 and 1.0. Therefore, the average change is towards more democracy, but getting to the transition curve normally requires several jumps. A main result is that the inclusion of both fixed effects in column (3) generates a large negative income effect, but the effect of the tension does not fall – it rather rises.

Table 4

OLS regressions explaining the jumps, *J*, in the *Main* sample

$N = 515$	(1)	(2)	(3)	(4)	(5)
Initial tension, $T_{(-)}$	**0.583** (14.6)	**0.849** (17.3)	**0.989** (19.9)		**0.579** (14.6)
Initial income, $y_{(-)}$	−0.070 (−0.2)	−0.036 (−0.0)	**−5.307** (−5.2)	0.487 (1.4)	
Growth, g	0.021 (0.6)	0.005 (0.1)	−0.049 (−1.1)	−0.015 (−0.3)	
Growth 5 years , $g5$	−0.079 (−1.1)	−0.124 (−1.5)	0.013 (0.2)	−0.071 (−0.9)	
Constant	1.573 (0.7)	2.889 (0.5)	**32.036** (3.6)	−2.497 (−1.0)	**0.995** (4.1)
FE for countries	No	Yes	Yes	No	No
FE for years	No	No	Yes	No	No
R^2 net of FE	0.297	0.385	0.378	0.005	0.295
R^2 of FE		0.126	0.301		
N	515	515	515	515	515

Note: See note to Table 2. Note that the two sets of dummies – notably the time dummies – do cause some collinearity with income. However, the effect of the tension variable is highly significant throughout.

Table 5

A comparison of the fit of estimates in Tables 2 and 4

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			R^2		Marginal R^2 from Model (3)		
Explaining	Table	*N*	Model (1)	Tension, $T_{(-)}$	Income, $y_{(-)}$	Growth, g	Growth, $g5$
Events, *E*	2	6,211	0.024	0.001	0.004	0.003	0.006
Jumps, only $J \neq 0$	4	515	0.295	0.348	0.008	0.000	0.000
Jumps, incl. $J = 0$	A6	6,211	0.026	0.068	0.011	0.000	0.000

Note: The marginal R^2 is calculated from model (3) in the two tables by deleting one variable at a time.

Table 5 compares the results in the two parallel tables 2 and 4. The key new finding is the difference between the explanations of the events, *E*, and the jumps, *J*. Column (3) compares the fit – it is strikingly different. Columns (4) to (7) compare the marginal R^2 of the four explanatory variables. The key difference is the contribution of the *T*-variable in column (4): It gives no contribution in Table 2, but it is the only variable that counts in Table 4. We conclude

from the comparison that events happen randomly while the size of the jumps is explained by the tension.

The reader may think that this comparison is 'unfair' as Table 2 is calculated for all 6,211 observations, while Table 4 uses data for the 515 jumps only. However, Table 4 has also been re-calculated using all 6,211 observations (see Table A6 in the online Appendix) including jumps of size zero. This reduces the difference between the results, but there is still a large difference in the explanatory power of the tension.

6.2. The direction of the jumps

The next step is to analyze the direction of the jumps as a function of their size. Table 6 counts the number of jumps that are towards and away from the transition curve, so the *right* jumps are in the direction predicted by the tension, and the *wrong* jumps are in the opposite direction. Row (1) of the table reports 187 jumps of a numerical size of 1, where 91 are in the *right* direction, while 96 are in the *wrong* direction. This appears random, and column (6) reports that it is. Row (2) shows that jumps of the numerical sizes of 2 and 3 are slightly more often in the right direction, but the difference to jumps in the wrong direction is not statistically significant at the 5% level. However, jumps with larger sizes (row (3) and below) are significantly more likely to be in the right direction; jumps with a numerical size of 12 and up are all in the right direction. Part of this is an artefact as the Polity index is limited to the interval $[-10, 10]$. While this barely limits jumps in the right direction at low levels of income, it does limit jumps in the wrong direction.

Table 6

The size of the numerical jump and its direction

	(1)	(2)	(3)	(4)	(5)	(6)
	Jump	Direction relative to tension			Fraction	Binominal
	size	Both	Right	Wrong	Right	tests in %
(1)	1	187	91	96	0.487	66.95
(2)	2-3	129	74	55	0.574	5.63
(3)	4-6	50	36	14	0.720	**0.13**
(4)	7-9	43	32	11	0.744	**0.10**
(5)	10-12	50	49	1	0.980	**0.00**
(6)	12 up	56	56	0	1.000	**0.00**
	All jumps	515	338	177	0.656	**0.00**

Note: Table explained in text. The test is a one-sided binominal test for H0: The number of right jumps is random with the probability 0.5. All bolded test results reject randomness.

The two top rows show that small jumps $-4 < J < 4$ are random, with 165 jumps toward the transition curve and 151 jumps away from it. However, the 199 larger jumps have 173 right and only 26 wrong. This suggests that small jumps may be considered as *regime adjustments* that can go either way, while *larger jumps* are system changes that mostly go in the direction of the transition curve. The correlation between the jumps and the initial tension is 0.54, but the correlation between the jumps and the resulting tension (i.e., after the jump) is −0.34. This suggests that most large jumps overshoot the transition curve.

6.3. Overshooting of the transition curve

Figure 7 gives a (J, T)-scatter plot of the jumps and the tensions reported in Table 6. There are 316 jumps where $-4 < J < 4$ (white circles: regime adjustments). The other 199 (larger) jumps are of three types: 26 are in the wrong direction (gray diamonds); 18 undershoot the transition curve (gray squares). No less than 155 jumps are larger than the tension (black circles), so they overshoot the transition curve. They are the points within the two symmetrical wedges.

Figure 7

Scatter of jumps over tensions
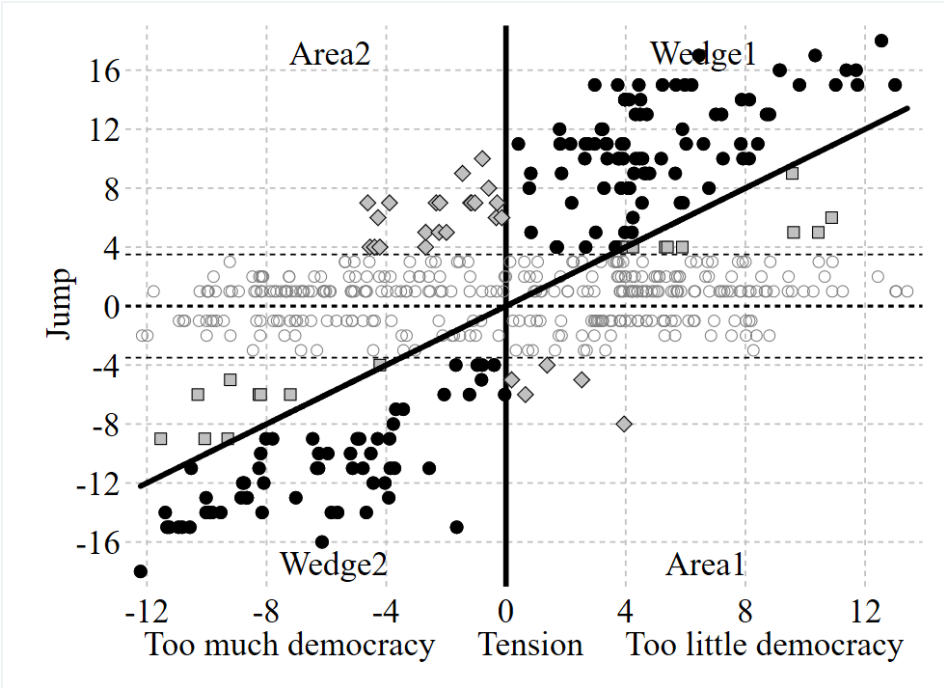
Note: Explained in text. The two symmetrical wedges are the areas between the 45-degree line ($J = T$) and the vertical line through origo (0, 0). Area1 and Area2 show cases of undershooting.

Wedge1 holds the (J, T)-points, where $J > T > 0$. The positive tension means that the countries have too little democracy relative to their income level. They jump towards more democracy by more than T, so they overshoot the transition curve. Conversely, in the negative Wedge2, where $J < T < 0$, countries have too much democracy relative to their income level and overshoot the transition curve to get too little democracy.

Table 7

Explaining the larger jumps

$N = 199$	(1)	(2)	(3)	(4)	(5)
Initial tension $T_{(-)}$	**1.567** (26.2)	**1.570** (26.4)	**1.439** (24.0)	**1.427** (23.5)	
Initial income, $y_{(-)}$	−2.721 (−5.8)	−2.829 (−6.5)			**1.896** (2.0)
Growth, g	0.027 (0.5)		0.043 (0.7)		−0.007 (−0.1)
Growth, $g5$	−0.068 (−0.7)		−0.286 (−2.8)		−0.270 (−1.2)
Constant	**21.832** (6.3)	**22.585** (7.0)	**1.931** (5.2)	**1.760** (4.7)	−11.773 (−1.7)
R^2	0.785	0.784	0.748	0.738	0.023

The many cases of under- and especially overshooting explain why the full convergence to the transition curve tends to be slow, even if income would stay constant. Table 7 employs the regression specification used in Tables 2 and 4 for the sample of the larger jumps. The results reveal a rather fine fit and an average overshooting by about 50% of the initial tension.

VII. CONCLUSIONS: THREE RESULTS

The Grand Transition is the change from a traditional society to a modern one. It is a process of deep changes, which also affects the political regime, giving a Democratic Transition. *Our first result* is that the Democratic Transition is strong in the data. The *Main* kernel regression on Figure 2 is a perfect transition curve. After a few years, most regimes tend to reach a status-quo equilibrium that sticks for some time. Time series measures of political regimes, such as the Polity index, reveal the stepwise stability. A regime change may only occur after an event. *Our second result* is that events happen (almost) randomly in the sense that standard economic variables explain very little of their variation across countries and time.

Some events result in a period of anarchy followed by a return to the old regime, but most events trigger a regime *jump*. *Our third result* is that most of these jumps are proportional to the tension, which is defined as the vertical distance between the curve and the observed

democracy score of a regime. The conceptual distinction between the randomness of triggering events and the size and the direction of the jumps has not been discussed in the literature up to now. The empirical relevance of the tension for the jumps to a new regime is a new empirical result. It integrates the short and the long run of the Democratic Transition.

Consequently, we have demonstrated that the estimated transition curve acts as an *attractor* for the jumps that are triggered by random events. This suggests that if income would stall at some intermediate income level, the political regime would converge to the position on the curve for that income level. However, there are probably no steady states at an intermediate income level. Hence, with the possible exception of the oil countries and conditional on persistent economic growth, all countries are predicted to reach stable democracy in the modern steady state.

Summary

When countries get wealthy, they become democracies. Using kernel regressions, we show that the long-run path in the *Polity* index looks exactly as a transition curve. However, the literature lacks a short-run model that can generate this path. We note that the main political regime index is constant for most years. However, the stability is interrupted by infrequent jumps that are often quite large. We argue that periods of constancy represent political status quo equilibria that need to be broken by a triggering event. We find that such events occur randomly, i.e., they cannot be explained by economic variables. But if an event causes a change in the regime, the jump is normally in the direction of the transition curve. Hence, the curve acts as an attractor for the jumps. This is a new finding that integrates the short and the long run of the Democratic Transition.

References:

Acemoglu, Daron, Simon Johnson and James A. Robinson (2005). Institutions as the Fundamental Cause of Long-run Growth, in: Philippe Aghion and Steven Durlauf (eds.), Handbook of Economic Growth. Vol. 1 A. Amsterdam: North-Holland: 385–472.

Acemoglu, Daron, Simon Johnson, James A. Robinson and Pierre Yared (2008). Income and Democracy. *American Economic Review.* 98(3): 808-42

Acemoglu, Daron, Simon Johnson, James A. Robinson and Pierre Yared (2009). Reevaluating the Modernization Hypothesis. *Journal of Monetary Economics.* 56: 1043–58

Acemoglu, Daron, Suresh Naidu, Pascual Restrepo and James A. Robinson (2016). Democracy Does Cause Growth. MIT, November (forthcoming *Journal of Political Economy*)

Armellini, Mauricio (2012). The Democratic Factor in the Education-Growth Relationship. *Kyklos.* 65(3): 285–312

Barro, Robert J. (1999). Determinants of Democracy. *Journal of Political Economy.* 107(6): 158–83

Barro, Robert J. (2015). Convergence and Modernization. *Economic Journal.* 125(June): 911–942

Basso, Alberto (2015). Does Democracy Foster the Fertility Transition? *Kyklos.* 68(4): 459–74

Benhabib, Jess, Alejandro Corvalan and Mark M. Spiegel (2013). Income and Democracy: Evidence from Nonlinear Estimations. *Economic Letters.* 118(3): 489–92

Boix, Carles (2011). Democracy, Development and the International System. *American Political Science Review.* 105(4): 809–28

Boix, Carles and Susan Stokes (2003). Endogenous Democratization. *World Politics.* 55(4): 517–49

Bond, Stephen R. and Markus Eberhardt (2013). Accounting for Unobserved Heterogeneity in Panel Time Series Models. Mimeo, Oxford and Nottingham

Borooah, Vani. K. and Martin Paldam (2007). Why is the World Short of Democracy? Across-Country Analysis of Barriers to Representative Government. *European Journal of Political Economy.* 23(4): 582-604

Brückner, Markus and Antonio Ciccone (2011). Rain and the Democratic Window of Opportunity. *Econometrica.* 79(3): 923–47

Burke, Paul J. and Andrew Leigh (2010). Do Output Contractions Trigger Democratic Change? *American Economic Journal: Macroeconomics.* 2(October): 124–57

Burkhart, Ross and Michael Lewis-Beck (1994). Comparative Democracy: The Economic Development Thesis. *American Political Science Review.* 88(4): 903–10

Cervellati, Matteo, Florian Jung, Uwe Sunde and Thomas Vischer (2014). Income and Democracy: Comment. *American Economic Review.* 104(2): 707–19

Cheibub, Jose Antonio (1996). What Makes Democracies Endure? *Journal of Democracy.* 7(1):39–55

Ditzen, Jan (2016a) XTCD2: module to perform Pesaran's (2015) test for weak cross sectional dependence in panel data. Available for download in Stata

Ditzen, Jan (2016b). XTDCCE2: module to estimate heterogeneous coefficient models in dynamic panels with dependence between cross sectional units. Available for download in Stata

Dorsch, Michael, Karl Dunz and Paul Maarek (2015). Macro Shocks and Costly Political Action in Non-Democracies. *Public Choice.* 162(3-4): 381–404

Doucouliagos, Hristos, and Mehmet A. Ulubaşoğlu (2008). Democracy and Economic Growth: A Meta-Analysis. *American Journal of Political Science.* 52(1): 61-83

Eberhardt, Markus (2012). Estimating Panel Time Series Models with Heterogeneous Slopes. *Stata Journal.* 12(1): 61–71

Epstein, David, Robert Bates, Jack Goldstone, Ida Kristensen and Sharyn O'Halloran (2006). Democratic Transitions. *American Journal of Political Science.* 50(3): 551–69

Fukuyama, Francis (1992). *The End of History and the Last Man*. Many editions

Gassebner, Martin, Michael J. Lamla and James R. Vreeland (2012). Extreme bounds of democracy. *Journal of conflict resolution.* 57(2): 171–97

Gundlach, Erich and Martin Paldam (2009). A Farewell to Critical Junctures. Sorting out the Long-Run Causality of Income and Democracy. *European Journal of Political Economy.* 25(3): 340-54

Gundlach, Erich and Martin Paldam (2017). A Hump-Shaped Transitional Growth Path. P.t. working paper

Hauk, William R. and Romain Wacziarg (2009). A Monte Carlo Study of Growth Regressions. *Journal of Economic Growth.* 14(2): 103–47

Heid, Benedikt, Julian Langer and Mario Larch (2012). Income and Democracy: Evidence from System GMM Estimates. *Economic Letters.* 116(2): 166–69

Helliwell, John F. (1994). Empirical Linkages between Democracy and Economic Growth. *British Journal of Political Science.* 24(2): 225–48

Hibbs, Douglas A. Jr. and Ola Olsson (2004). Geography, Biogeography, and Why Some Countries Are Rich and Others Are Poor. *Proceedings of the National Academy of Sciences of the United States* (PNAS). 101: 3715–74

Inglehart, Ronald (1997). *Modernization and Post Modernization*. Princeton, NJ: Princeton University Press

Inglehart, Ronald and Christian Welzel (2005). *Modernization, Cultural Change and Democracy: The Human Development Sequence*. New York: Cambridge University Press

Knutsen, Carl H. (2015). Why Democracies Outgrow Autocracies in the Long Run: Civil Liberties, Information Flows and Technological Change. *Kyklos.* 68(3): 357–84

Lewandowski, Piotr (2007). 'PESCADF': module to perform Pesaran's CADF panel unit root test in presence of cross section dependence. Warsaw School of Economics, Institute for Structural Research, Poland. Available for download in Stata

Lewis Davis (2016). Individual Responsibility and Economic Development: Evidence from Rainfall Data. *Kyklos.* 69(3): 426–70

Lipset, Seymour M. (1959). Some Social Requisites of Democracy: Economic Development and Political Legitimacy. *American Political Science Review.* 53(1): 69-105

Lundberg, Alexander L., Kim P. Huynh and David T. Jacho-Chavez (2017). Income and Democracy; A Smooth Varying Coefficient Redux. *Journal of Applied Econometrics.* 32(3): 719–24

Madison Project. URL: http://www.ggdc.net/maddison/maddison-project/home.htm

Marshall, Monty G., Ted R. Gurr, and Keith Jaggers (2016). *Polity IV Project. Users' Manual*. University Center for Systemic Peace. URL: http://www.systemicpeace.org/polityproject.html. Latest issue

Moral-Benito, Enrique and Cristian Bartolucci (2012). Income and Democracy: Revisiting the Evidence. *Economics Letters.* 117(3): 844–47

Murtin, Fabrice and Romain Wacziarg (2014). The Democratic Transition. *Journal of Economic Growth.* 19(2): 141-81

Nikolaev, Boris and Raufhon Salahodjaev (2017). Historical Prevalence of Infectious Diseases, Cultural Values, and the Origins of Economic Institutions. *Kyklos.* 70(1): 97–128

Nur-tegin, Kanybek (2014). Entrenched Autocracy or New Democracy: Which Is Better for Business? *Kyklos.* 67(3): 398–419

Paldam, Martin (2009). An Essay on the Muslim Gap: Religiosity the Political System. Chapter 10 pp 213-42 in Ferrero, M. and R. Wintrobe, eds. *The Political Economy of Theocracy*. Palgrave Macmillan, New York

Paldam, Martin (2013). The Political Economy of Dutch Disease: A Survey. Chapter 10 pp 179-98 in Cabrillo, F., and M.A. Puchades-Navarro, eds. *Constitutional Economics and Public Institutions*. Edward Elgar, Cheltenham UK

Paldam, Martin and Erich Gundlach (2008). Two Views on Institutions and Development: The Grand Transition vs. the Primacy of Institutions. *Kyklos.* 61(1): 65–100

Paldam, Martin and Erich Gundlach (2018). Why Do Political Regimes Change? A Study of Triggering Events. First version available from: http://martin.paldam.dk/Papers/GT-Main/10-Reverse.pdf

Pesaran, M. Hashem (2006). Estimation and Inference in Large Heterogeneous Panels with a Multifactor Error Structure. *Econometrica.* 74(4): 967–1012

Pesaran, M. Hashem (2015). Testing Weak Cross-Sectional Dependence in Large Panels. *Econometric Reviews.* 34(6-10): 1089–1117

Pesaran, M. Hashem and Ron P. Smith (1995). Estimating Long-Run Relationships from Dynamic Heterogeneous Panels. *Journal of Econometrics.* 68(1): 79-113

Pesaran, M. Hashem, Yongcheol Shin and Ron P. Smith (1999). Pooled Mean Group Estimation of Dynamic Heterogeneous Panels. *Journal of the American Statistical Association.* 94(446): 621–34

Przeworski, Adam and Fernando Limongi (1997). Modernization: Theories and Facts. *World Politics.* 49(2): 155–83

Przeworski, Adam, Michael E. Alvarez, José Antonio Cheibub and Fernando Limongi (2000). *Democracy and Development. Political Institutions and Well-Being in the World, 1950-1990*. Cambridge UP, Cambridge UK

Wintrobe, Ronald (1998). *The Political Economy of Dictatorship*. Cambridge UP., Cambridge, UK

World Bank (WDI). URL: http://databank.worldbank.org/data/reports.aspx?source=worlddevelopmentindicators