5. The Jumps Model for the Short Run

Changes of the *P*-index are the jumps. The Main sample contains 515 jumps, of which 199 are larger, i.e. larger than 3 *P*-points. Section (s1) explains the 515 jumps by the tensions, and shows that the explanation only works for the larger jumps (s2). Some of the jumps are sequences that may be planned changes – they are more positive than discrete jumps (s3). A grievance asymmetry means that the political effect of a negative shock is stronger than the effect of a positive shock – the asymmetry is large for system jumps (s4). Finally, a range of regression estimators are used to show that they give random results (s5), and it is explained why (s6).

5.1 Explaining jumps

Table 1 tries to explain the jumps by the following five variables: Initial tension, $\Theta^P_{(-)}$,[1] initial income, $y_{(-)}$, annual growth rate, g; and average growth rate over the preceding five years, g5. In addition, fixed effects for countries and years are included in some of the regressions. These variables are taken to measure the transition and development. Only one of the five explanatory variables works. It is the tension, Θ^P, 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 Θ^P has some covariance with y; but income is statistically insignificant when Θ^P is omitted in column (4). The two growth variables have no effect.

Table 1. OLS regressions explaining the jumps, *J*, in the *MAIN* sample

N = 515	(1)	(2)	(3)	(4)	(5)
Initial tension, $\Theta^P_{(-)}$	**0.583** (14.6)	**0.849** (17.4)	**0.965** (19.2)		**0.579** (14.6)
Initial income, $y_{(-)}$	−0.070 (−0.2)	0.512 (0.7)	−3.053 (−3.5)	0.487 (1.4)	
Growth, g	0.021 (0.6)	0.008 (0.2)	0.028 (−0.6)	−0.015 (−0.3)	
Growth 5 years, $g5$	−0.079 (−1.1)	−0.129 (−1.6)	−0.014 (−0.2)	−0.071 (−0.9)	
Constant	1.573 (0.7)	−3.221 (-0.4)	**26.665** (3.2.4)	−2.497 (−1.0)	**0.995** (4.1)
FE for countries	No	Yes (121)	Yes (121)	No	No
FE for years	No	No	Yes (49)	No	No
R^2 net of FE	0.297	0.297	0.297	0.005	0.295
R^2 of FE		0.214	0.254		

See also the parallel Tables 3 and 6.1. The effect of the fixed effect is reached by running the regression in the column without the other variables. The fixed effects, has some collinearity to income and the tension. Thus, 0.75 as the best estimate of the effect of the tension. The number of fixed effects are added in brackets after the 'yes'.

[1] Recall that the tension is the difference $\Theta^P = P - \Pi(y)$ between the actual P and transition path from Figure 4a.

The estimated effects of the tension are all positive and highly significant with a size of about 0.75. When Table 3 below looks at the larger jumps only, it finds an effect that is twice as large. Both tables find that the average change is towards the *Π*-curve, and getting to the 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.

Chapter 6 compares the explanation of the jumps in Table 1 with the parallel Table 6.1 explaining the events. The important point is that the same variables do not explain the events. The tension is the key variable in Table 1, but it explains nothing in Table 6.2. The jumps happen close to randomly.

This is the core of the Jumps Model. It does not explain when the system changes, but once it does, it moves towards the transition path. Thus, the transition path is an attractor for the jumps that happen randomly. The randomness evidence follows in Chapter 6.

5.2 The importance of the size of the jump

The next step is to analyze the direction of the jumps as a function of their size. Table 2 counts the number of jumps that are towards and away from the *Π*-curve, so the *right* jumps are in the direction predicted by the tension, and the *wrong* jumps are in the opposite direction.

Table 2. The size of the numerical jump and its direction relative to *Π*

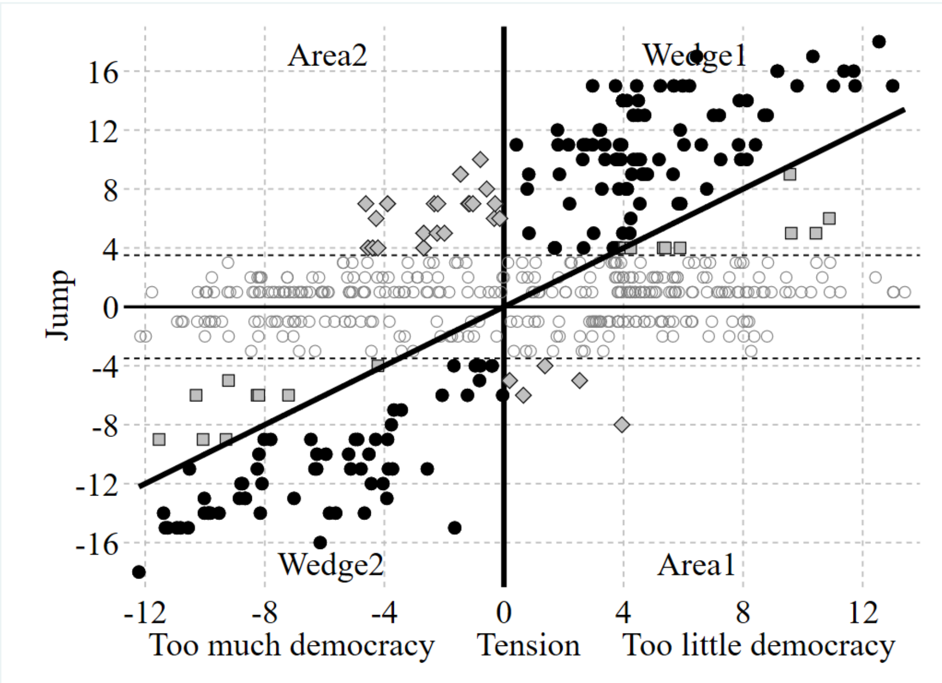
	(1)	(2)	(3)	(4)	(5)	(6)
	Jump	Direction relative to tension			Fraction	Binominal
	size	Both	Right	Wrong	Right	tests in %
(1)	1	187	91	96	0.487	66.95
(2)	2-3	129	74	55	0.574	5.63
(3)	4-6	50	36	14	**0.720**	0.13
(4)	7-9	43	32	11	**0.744**	0.10
(5)	10-12	50	49	1	**0.980**	0.00
(6)	12 up	56	56	0	**1.000**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

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 sum of the larger jumps in rows (3) to (6) is 199.

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. The test in column (6) reports that this is random. Row (2) shows that jumps of 2 and 3 are slightly more often in the right direction, but the difference is not statistically significant at the 5% level. However, jumps with larger sizes in rows (3) to (6) 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 at high income levels.

The two top rows show that small jumps $-4 < J < 4$ are random, with 165 jumps toward the transition curve, *Π*, 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* – and they are within the gray zone of measurement uncertainty – that can go either way, while *larger jumps* are system changes that mostly go in the direction of the *Π*-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 large jumps overshoot the *Π*-curve. Figure 1 and Table 3 confirm this.

Figure 1. Scatter of jumps over tensions



Explained in text. Overshooting is the black circles in the two symmetrical wedges. They are the areas between the 45-degree line ($J = T$) and the vertical line through (0, 0). Area1 and Area2 show cases of undershooting. The hollow grey circles are for small system adjustments, which may not be real, see section 4.6.

Figure 1 gives a (J, T)-scatter plot of the jumps and the tensions reported in Table 2. The hollow circles are 316 small jumps where $-4 < J < 4$, showing regime adjustments. The

199 larger jumps are for genuine regime changes. They are of three types: 26 are in the wrong direction (gray diamonds); 18 undershoot the *Π*-curve (gray squares). No less than 155 jumps are larger than the tension (black circles), so they overshoot the *Π*-curve. They are the points within the two symmetrical wedges. Wedge1 holds the (J, Θ^P)-points, where $J > \Theta^P > 0$. The positive tension means that the countries have too little democracy relative to their income level. They jump towards more democracy by more than Θ^P, so they overshoot the *Π*-curve. Conversely, in the negative Wedge2, where $J < \Theta^P < 0$, countries have too much democracy relative to their income level and overshoot the *Π*-curve to get too little democracy.

Table 3. Explaining the 199 larger jumps

N = 199	(1)	(2)	(3)	(4)	(5)
Initial tension $\Theta^P_{(-)}$	**1.567** (26.2)	**1.737** (25.5)	**1.786** (22.8)		**1.427** (23.5)
Initial income, $y_{(-)}$	**−2.721** (−5.8)	−1.470 (-1.0)	−2.818 (-1.4)	**1.896** (2.0)	
Growth, g	0.027 (0.5)	0.006 (0.1)	0.018 (0.2)	−0.007 (-0.1	
Growth 5 years,	−0.068 (−0.7)	**−0.297** (−2.0)	−0.306 (−1.6)	−0.270 (-1.2)	
Constant	**21.832** (6.3)	15.567 (1.3)	22.967 (1.3)	−11.773 (1.7)	
FE for countries	No	Yes (87)	Yes (87)	No	No
FE for years	No	No	Yes (47)	No	No
R^2 net of FE	0.785	0.785	0.785	0.023	0.738
R^2 of FE		0.112	0.180		

See also the two parallel tables: Tables 1 and 6.1. There is some multicollinearity between the fixed effects, income and the tension. Thus, I take 1.5 as the best estimate of the effect of the tension.

The many cases of under- and especially overshooting explain why the full convergence to the *Π*-curve tends to be slow, even if income would stay constant. Table 3 employs the regression specification used in Table 1 for the sample of the larger jumps. The results have a fine fit and an average overshooting by about 50% of the initial tension. Thus, it rises to (damped) cyclical movements of the *P*-index as discussed in Chapter 2.

5.3 The difference between discrete jumps and sequences

Table 4 compares the discrete jumps and the sequences. The sample includes all 262 larger jumps. While the standard deviations are roughly similar, the means are significantly different as shown by a *t*-test.

Jumps toward a more authoritarian regime normally are fast. A military coup typically takes one day, and the preparations are secret, for good reasons. Most coups are rather peaceful, and *The Economist* often reports that people first note that a coup has taken place when they wake up in the morning and see tanks in the streets.

Table 4. A comparison of jumps: discrete versus sequences

Size of jump	Discrete Negative	Discrete Positive	Sequences Negative	Sequences Positive
4-5	16	25	8	8
6-7	11	12	1	11
8-9	15	17	3	9
10-11	13	21	1	13
12-13	12	10	1	6
14 up	14	13	5	17
Sum	81	98	19	64
Average	0.65		5.88	
Std	9.83		8.87	
t-test = 4.13 for equal means, rejects for $p < 0.005\%$				

Numbers in the gray cells are in ΔP-points, while the remaining numbers are counts of cases.

Jumps towards democracy normally require a sequential process, which often contains four steps: (i) A government of national conciliation is appointed; (ii) it proposes a new constitution; (iii) it is approved by a referendum; and finally (iv) a general election takes place. The process normally takes two years, but it may take as many as four years.

5.4 The grievance asymmetry for system changes

The literature on vote and popularity often finds a grievance asymmetry: A negative event causes a loss of government popularity that is about twice the gain the government experiences from a positive event of the same size (see Nannestad and Paldam 1994, 1997).

Table 5. Number of events at different growth rates

		(c1)	(c2)	(c3)	(c4)	(c5)	(c6)	(c7)	(c8)
		Growth rates		Observations		Fraction	Binominal test (%)		Excess
		From	To	Events	All	(c3)/(c4)	(c5) $\geq x$	(c5) $\leq x$	events
Low	(r1)	$-\infty$	-6	61	343	0.178	0		**31.9**
	(r2)	-6	-2	81	565	0.143	0		**33.0**
	(r3)	-2	0	97	702	0.138	0		**37.3**
Avr	(r4)	0	2	107	1259	0.085	51.1	52.5	0.0
	(r5)	2	4	119	1404	0.085	52.6	51.1	−0.3
High	(r6)	4	6	80	905	0.088		67.0	3.1
	(r7)	6	8	29	424	0.068		12.6	−7.0
	(r8)	8	∞	40	514	0.078		31.3	−3.7

The gray cells are used to calculate the normal frequencies of events. It is: (107+119)/(1,259+1,404) = 0.085. Columns (c6) and (c7) report one-sided binominal tests for x = 0.085. Significant test results are bolded. The excess events are calculated as (c3) – x(c4). The zeros in (c6) are p-values below 0.005%.

Table 5 shows that the grievance-hypothesis generalizes to regime jumps. It gives the number of events at each of eight intervals for the growth rate, with one lag. The gray area, in rows (r4) and (r5), represents normal growth. The top panel, in rows (r1) to (r3), shows the effect of below-average growth. There, countries have too many events, as they should if the regime is held responsible for the poor growth performance. In all cells, the excess instability is significantly positive, but it sums only to 102.2 (= 31.9 + 33.0 + 37.3) over 1,610 (= 343 + 565 + 702) observations. That is 6.3%, so the effect is moderate.

The bottom panel, in rows (r6) to (r8), displays the effect of above-average growth. More than half are negative, as they should if the regime is rewarded for good growth performance, but the 'excess' stability sums only to −7.6 (= 3.1 − 7.0 − 3.7) for N = 1,843 (= 905 + 424 + 514), which is −0.4%. The positive effect of high growth is small, and insignificant. The grievance asymmetry is larger for system stability than for government popularity.

5.5 Regression models (for the econometrician)

The most important counter-argument to the analysis above is presented in Acemoglu *et al.* (2008), who used the L2FE panel regression (also known as a GDPM):

(7) $P_{it} = a_{1i} + a_{2t} + b_1 P_{i,t-1} + b_2 y_{i,t-1} + u_{it}$, where the a's are fixed effects for countries and time, and u is the residuals. The steady state income effect is $b^* = b_2/(1 - b_1)$. It should be equal to the income effect in Table 1.

The surprising result in estimates of (7) is that b_2 became small and insignificant, which of course carries over to b^*. Thus, there is a contradiction. I interpret equation (7) as representing the Granger-causality idea, as it analyzes if income adds anything when P is explained by fixed effects and P lagged. Thanks to the long spells of constant Ps, it is not surprising that equation (7) finds that y explained nothing. Below, this result is replicated, and in addition it is shown that a whole spectrum of 11 regression models give rather fickle results.

These results are reported in Table 6. The 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:

(8) $P_{it} = b_{1i} P_{i,t-1} + b_{2i} y_{i,t-1} + u_{it}$ 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.

Table 6. Regressions using a range of estimators

	Part A. Pooled parameter models				
	(1)	(2)	(3)	(4)	(5)
	POLS-T	2FE	AB	BB	CCEP
Income	**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	−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	5,568	5,568	5,568	4,905	4,120	4,120
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

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.

The regressions are: 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.

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).

Common shocks may have different effects across countries, 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).

A broad range of both types of estimators is used. *Part A: Pooled parameter models.* The estimates reported in the first and the second columns of Part A of Table 8 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.[2] 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.

Part B: Heterogeneous parameter models: Part B of Table 6 reports the results for estimates of the Democratic Transition using 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.

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

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 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.[3] 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 6 studies country-specific effects in combination with a more sophisticated modeling of the error term. This 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

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

authoritarian to a democratic regime. It appears that the kernel regression and the common dynamic process identified by the AMG estimator both point to the existence of a long-run pattern in the degree of democracy.

The kernel regressions in Section 4 show a clear link between income and democracy, but they cannot control for omitted variables. The panel regressions in Table 6 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.

5.6 Concluding remarks on the problematic regression estimates

When the whole set of 11 regression estimates of the income effect is considered, it is clear that something strange is going on. Normally, a sequence of regressions that increasingly adjust for more and more potential problems should show an improvement in the results, indicating a convergence to the true result. Thus, the coefficient estimate should move in a predictable way and the t- or z-ratios should increase. This is not the case in Table 6. The income effect jumps up and down in a seemingly random way.

My interpretation is that this shows that *the regression tools of the profession are inappropriate for the problem at hand*. 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, while income is an almost linear variable (i.e. the *gdp* is almost log-linear). In addition, the kernel estimates reveal that there are nonlinearities involved at both ends of the range. Maybe some sort of a hazard model could be developed for the problem, but linear regression models are the wrong tool, even if it contains many refinements.

Another way to understand why short-run panel models are likely to fail is to look back on Figure 4.11. There are no signs that any lags/leads between the two variables have a peak giving a choice. While a rise in income will cause a rise in the P-index, it cannot be predicted when it happens within (at least) a ten-year period. However, the average correlation is significantly positive throughout – we are clearly dealing with a long-run connection. Though the relation is strong in the long run, it is fuzzy in the short run.