Aarhus and Hamburg
September 28th, 2010

The agricultural, demographic and democratic transitions

Two estimation models giving reverse results

Erich Gundlach, GIGA and Hamburg University, Germany[1]
Martin Paldam, Aarhus University, Denmark[2]

Abstract:
The agricultural transition, the demographic transition and the democratic transition explain the development paths of the share of agriculture, the population growth rate, and the standard democracy indices. We demonstrate that two related estimation models give contradictory results when applied to data for these transitions over half a century and 150 countries. One model shows that the long-run change in the transition variable is caused by income. The other model shows that the relation between income and the transition variable is spurious. The contradicting results lead to reflections on appropriate approaches for identifying causality in development.

Keywords: Long-run growth, development, transitions, causality and spuriousness
JEL: O1, P5, Q1

Acknowledgement: Parts of the paper were presented at seminars in Kiel, Hamburg, and Esbjerg. An earlier version of the paper was presented at the Silverplana 2009 workshop, where it benefitted from the lively discussion and notably from comments by Toke Aidt and Guiseppe Russo.

1. Address: GIGA German Institute of Global and Area Studies/Institute of Asian Studies, Rothenbaumchaussee 32, 20148 Hamburg, Germany.
E-mail: gundlach@giga-hamburg.de. URL: http://www.erichgundlach.de
2. Address: School of Economics and Management, Bartholins Allé 10, DK-8000 Aarhus C, Denmark.
E-mail: mpaldam@econ.au.dk. URL: http://www.martin.paldam.dk

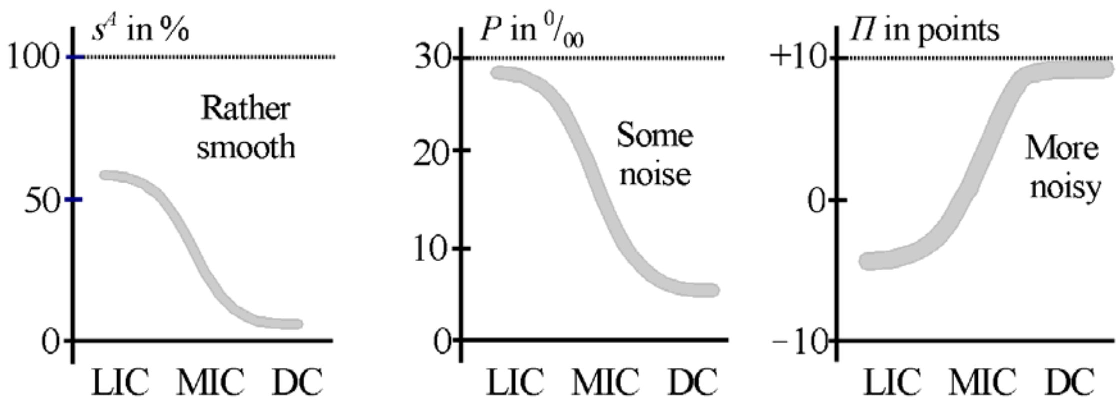
1. Introduction: two methods two results

The identification of causality is the hallmark of applied economics, and different methods exist to achieve that goal. We discuss two basic estimation models, which are termed the DPIV and the AJRY model. These models produce a consistent pattern of results when applied to a class of macro development processes known as *transitions*: one model predicts that the transitions are caused by the level of income; the other model predicts that the level of income does not have any causal effect on the transitions.

Transitions are changes in the long-run level of variables during the processes of development when a country goes from being an LIC to becoming a DC.[3] These processes are parts of the *Grand Transition* that consists of interacting transitions in many socioeconomic fields. Most textbooks on economic development deal with the *agricultural* and the *demographic* transitions; and in political science one finds the *democratic* transition. Stylized paths of the three textbook transitions are sketched on Figure 1.

Figure 1. The stylized paths of the three transitions

1a. The agricultural 1b. The demographic 1c. The democratic



Note: Income is on the horizontal axis. On the vertical axes are: s^A the share of agriculture; *P* population growth per 1000 inhabitants; and *Π* the Polity Index. See section 2.1. The precise paths in the data used below are shown in Gundlach and Paldam (2010a and c) and Paldam (2010).

Development is proxied by per capita income. Each transition is measured by one transition variable: the share of agriculture, the population growth rate and a democracy index (see Table 1 overleaf). The paths drawn imply that these variables are highly correlated with

3. The World Bank statistics uses the terms LIC, MIC and DC, which refer to *low income country*, *middle income* country and *developed country*, respectively.

income. Hence, when a high correlation is observed between a variable and income it suggests a transition in the variable. From the suggestion it needs to be tested if the correlation is due to causality from income to the variable. This is precisely where the findings from the two estimation models *normally* differ:[4] The DPIV model shows *causality* from income to the transition variable, while the AJRY model shows the relation to be *spurious*.

Abundant data-pairs are available for income and each of the three transition variables. The high correlation between the variables of each pair has been demonstrated in cross-country and in time series studies of many countries. A large theoretical literature claims to explain the main mechanisms behind the three transitions. Consequently, we use these textbook transitions for our empirical demonstrations, which come in two parts.

The ***A-tables*** present estimates of the ***DPIV model***. It is an IV method using an extreme set of DP (**d**evelopment **p**otential) variables as instruments, and it was introduced by Gundlach and Paldam (2009a) to analyze causality in the democratic transition. The A-tables show long-run causality from income to each of the transition variables.

The ***B-tables*** present estimates of the ***AJRY model***. It was used by Acemoglu, Johnson, Robinson and Yared (2008) to reject the hypothesis that income is causal for democracy. This result is reached by adding three controls to the DIPV estimation equation, namely the lagged endogenous variable and fixed effects for countries and time. The B-tables show that all three transitions are spurious.

The next section presents the data and the two estimation models. Section 3 provides the A-tables for the three transitions, which report detailed results for a cross-section of countries for one year. Also included is a figure for each transition with summary results for all cross-sections in 1960 to 2008. Section 4 provides the corresponding three B-tables, which report results based on 5-year averaged panel data in 1960-2008. We briefly mention results – published elsewhere[5] – for three *extra* transitions in sections 3.5 and 4.6, namely the transition of corruption, the religious transition, and the transition in the preference for capitalism/socialism. Section 5 discusses the conflicting results in the A- and B-tables and reflects on the appropriate approach for identifying causality in the cases at hand. Section 6 concludes.

4. The word *normally* indicates that we have studied six transitions and found no exception. However, we have not tried everything.
5. Documenting everything claimed in this paper is a rather extensive exercise. We refer to three published papers and a set of six background papers, which are made available on the home pages of the authors. They are listed in an additional reference list, which also explains what each paper contains.

2. The data and the two models

Section 2.1 defines the data; section 2.2 discusses the DPIV model; section 2.3 justifies the extreme DP instruments; and section 2.4 discusses the AJRY model.

2.1 The data for 1960 to 2008

The Grand Transition view sees development as an interacting net of transitions in (virtually) all fields of society. This view takes the *equivalence assumption* as the *default*, which is assumed till disproved: The long-run time-series (t-s) pattern in the transition variable is broadly equivalent to the cross-country (c-c) pattern.[6]

All countries started at comparable income levels about 200 years ago. Hence, the present c-c income differences reflect international differences in long-run growth rates. To study a transition requires t-s data that extend over a couple of centuries or c-c data that cover a broad income range. Long t-s data are scarce, but it is common to find broad ranging c-c-sets. Our estimates use a broad c-c panel that includes t-s data for half a century, which allow us to control the c-c results for t-s effects, if they are different.

Development is measured by *income*, *y*, defined as the natural logarithm to GDP per capita in real PPP terms. The Maddison data set (in references) is used. The unit for the income variable is ***lp*** (logarithmic points), where 1 lp corresponds to a factor of 2.72. The full transition in our data set is about 4½ lp, which corresponds to an income difference of a factor of about 90.[7]

The three transition variables are defined and scaled as given in Table 1. In all three cases the transition covers most of the range observed in the variables. The three background papers Gundlach and Paldam (2010a, b) and Paldam (2010) describe the paths of the three transitions. These papers also supply the numbers given in columns (5)-(7) of the table.

Column (7) reports the size of the transition for 1 lp change in income as observed in the annual panel data in each of the three cases. Any empirical model that estimates the full transition path should by and large reproduce these numbers in order to provide a full account of the presumed effect of income on the transition variable.[8]

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6. The equivalence assumption is well-known to hold for the agricultural and the democratic transitions. The story is more complex as regards the demographic transition; see Paldam (2010).
7. There are some cases where the wealth of a country is due to resource rents from export, notably oil export. These countries have not passed through the Grand Transition.
8. For ease of comparison of the estimates, linear specifications are used throughout even when the form of the transition curve suggests convergence to a constant level at the two ends.

Table 1. The three transition variables

(1) Transition	(2) Variable $x = s^A, P, \Pi$	(3) Measure/scale	(4) Source [c]	(5) LIC level	(6) DCs level	(7) Per 1 lp [d]
Agricultural	s^A, share of agriculture	Value added in % of GDP	WDI	50%	Below 5%	-10
Demographic	P, population growth	Per 1,000 inhabitants [a]	WDI	$30^0/_{00}$	$3^0/_{00}$	-6
Democratic	Π, Polity index	Points from –10 to +10 [b]	Polity	−4 points	+10 points	+3

Notes: (a) The difference between the crude rates of fertility and mortality. (b) –10 is fully authoritarian and +10 fully democratic. (c) Homepages are given in references. (d) For one lp (logarithmic point) assuming a transition spanning 4½ lp. It is the coefficients on income needed to explain the full transition path.

The estimation models use either pure c-c data for a given year or panel data, where the variables are averaged over 5-year periods from 1960-64 to 2005-08, For s^A and Π data, a 5-year average is accepted if only four observations are available (as is the case for the last period).[9] For P many LICs report only every second or third year. These data move quite smoothly, so here a 5-year average is accepted even if only two observations are available.

Henceforth x refers to a transition variable such as $x = s^A, P, \Pi$. In the following, the indices i and t are for countries and time, respectively. For the pure c-c estimates, t is one of the 49 available years. For the panel estimates, t is one of the nine available 5-year averaged periods. The panels available for the three transitions are given in Table 2. To make all our panel estimates directly comparable, the same number of panels is used for specifications with and without the lagged endogenous variable.

Table 2. The three unbalanced panels used for 1960-2008

Transition, x	Sections	N_c [a]	Periods	Full panel	Available	Missing
Agricultural, s^A	3.2 & 4.2	151	9	1359	898	33.9%
Demographic, P	3.3 & 4.3	157	9	1413	1287	8.9%
Democratic, Π	3.4 & 4.4	153	9	1377	1199	12.9%

Notes: See Table 1. (a) Number of countries where (x_{it}, x_{it-1}, y_{it-1}) data combinations are available. The WDI data start in 1960, so the Polity data before 1960 are excluded.

2.2 The DPIV estimation models of the A-table: Estimates for individual years

The purpose of each A-table is to identify long-run causality from income to the transition variable under consideration. This is done by the DPIV model, which is estimated in two versions:

9. The background papers also report panel estimates using 3-year and 7-year periods.

(1) $x_i = \beta_0 y_i + \alpha + u_i$, OLS-estimate; β_0 catches the correlation between *x* and *y*.

(2) $x_i = \beta_0^{DP} y_i^{DP} + \alpha + u_i$, IV-estimate, second stage OLS estimate. The first stage instruments *y* with a set of DP-variables; β_0^{DP} catches the causal effect from *y* to *x*.

The extreme DP instruments in the DPIV model are biogeographical variables motivated in the next section and defined in the Appendix; see also Gundlach and Paldam (2009a). These variables are only available as one cross-section for a maximum of 112 countries; alternative instruments, also based on geography, are available for a larger sample of countries.

The A-table answers two questions:

(Q1): Is income causal to *x*?

(Q2): Does the causal effect found explain the full path of the transition?

Given that the relevant test criteria for the quality of the IV estimates are met, Q1 simply asks: Is $\beta_0^{DP} \neq 0$? If the answer is yes, we proceed to the more difficult question.

The answer to Q2 is assessed to be positive if three (somewhat overlapping) conditions are met: (c1) the correlation between *y* and *x* in equation (1) is the same as the causal effect from *y* to *x* in equation (2), i.e., $\beta_0 \approx \beta_0^{DP}$. If $\beta_0 \neq \beta_0^{DP}$, other causal links are involved. (c2) The joint β is equal to the value needed to explain the full transition from column (7) of Table 1. (c3) The chosen instruments do not identify reverse causality from *x* to *y*.[10]

To condense the reporting, each of the three transitions is covered by one A-table, which includes alternative combinations of DP-instruments for one year, and one A-figure, depicting the pattern of the estimates for all 49 cross-sections (years). The construction of the A-table and the A-figure is explained in more detail in section 3.1.

2.3 The extreme DP-variables

Many theories have been presented to suggest what causes long-run development, but few of these theories are open to rigorous empirical investigation.[11] However, Diamond (1997) inspired Hibbs and Olsson (2004, 2005) to compile an amazing set of *biogeographical* DP-

10. If (c1) and (c2) are accepted, it follows logically that (c3) should be fulfilled; but it is possible to construct theoretical cases where an exception may occur. Think, e.g., of cases where $\beta_0 \approx \beta_1$ by random variation, and not because models (1) and (2) explain different sample variations. Such cases are more improbable if (c3) holds.

11. The most suggestive empirical approaches are probably Boserup (1965) with a focus on agricultural development and Diamond (1997) with a focus on geographic and biological constraints. Other influential studies are Hall and Jones (1999), Pommeranz (2000), Acemoglu, Johnson, and Robinson (2001), Hansen and Prescott (2002), Williamson (2006), and Clark (2007).

variables for various regions of the world at the time of the Neolithic Revolution about 10,000 years ago. These variables are available for 112 present-day countries.

There are two biological variables. One is the number of *domesticable* big mammals (*animals*) that are believed to have existed in prehistory, which goes from zero for Sub-Saharan Africa to nine for Europe. The other is the number of *arable* wild grasses (*plants*) known to have existed in prehistory, which goes from less than five for Sub-Saharan Africa to more than 30 for Europe.

The geographic variables measure the specific conditions that have constrained or enabled the spread of prehistoric innovations to neighboring regions. One measure is based on a ranking of climates according to how favorable they are to agriculture (*climate*). A second measure captures the degree of east-west orientation (*axis*) of a country, which eases the flow of early agricultural innovations. A third measure calculates the size of the landmass to which a country belongs (*size*).

Averages and first principal components of these measures are used as instrumental variables. Moreover, we use an alternative set of geography-related variables that are expected to affect the income level of a present-day country through various channels. For instance, the number of frost days per winter (*frost*) may affect the productivity of agriculture, the potential for malaria transmission (*maleco*) may affect the accumulation of human capital, and the proportion of a country that is close to the open sea (*coast*) may affect the possibilities for trade.

Diamond (1997) discusses development in the world until about the year 1500; that is, before the medium-term growth rate reached 0.2% in any country. A take-off to modern economic growth (Rostow, 1960) occurred from about 1800, when an increasing number of countries acquired medium-term growth rates in excess of 1%. The unified growth theory by Galor and various coauthors (see Galor 2005) attempts to integrate the pre-take-off period with modern economic growth into one consistent theory. It claims that development becomes inevitable once technological change starts back in prehistoric times, and human capital is being accumulated until a critical mass is reached that allows the economy to take off from Malthusian stagnation to a modern growth regime. Thus, unified growth theory provides a theoretical justification for the use of the extreme DP-variables as instruments in our empirical specifications.

The DP-variables measure exogenous geographical facts and biological preconditions before the start of recorded history, so these variables are truly exogenous conditions for long-run development. The studies that first used these DP-variables demonstrate a statistically significant correlation with modern cross-country levels of income. These statistical properties allow us to use the DP-variables as instruments for modern income levels.

One problem with the DP-variables is how the four Neo-European countries – Australia, Canada, New Zealand and the USA – should be treated. Initially they had less favorable biogeographic conditions, but they gained substantially in terms of biological variables by European immigration. In column (2) of the A-tables below, the four countries are included as transferred West European countries, i.e., with average European biological measures. The other columns exclude the Neo-Europeans.

2.4 The AJRY estimation model and the B-table: Panel estimates

The B-table employs the AJRY model for an alternative causality test. The relation between the DPIV and the AJRY model is best seen by re-writing equation (1) in the panel version as:

(3) $x_{it} = \beta_0 y_{it-1} + \alpha + u_{it}$, *Base model* – corresponds to (1),

where u_{it} are the residuals, and y_{it-1} is income lagged by one time unit.[12] Since only one c-c-set of DP-instruments exists, equation (2) has no panel version, but the coefficient estimates of (1) and (3) can be compared.

The results of the estimates of (1) and (3) show why many textbooks on development cover the agricultural, the demographic and the democratic transitions. However, all spuriousness should be weeded out before a causal relation like (2) is accepted. This idea has led Acemoglu, Johnson, Robinson and Yared (2008) to reconsider if income really counts for the explanation of the transition by adding *three formal controls* to relation (3), namely the lagged endogenous, x_{it-1}; fixed effects for countries, α_i; and fixed effects for time, α_t,: [13]

(4) $x_{it} = \beta_1 y_{it-1} + \gamma x_{it-1} + \alpha_i + \alpha_t + u_{it}$, *AJRY model* – with adjustment mechanism.

The AJRY model tests if the effect of income is (still) statistically significant when the three controls are included. If H_0: $\beta_1 = 0$ cannot be rejected, causality from income to the transition variable is considered as spurious if predicted by (1) to (3).

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12. The AJRY model uses initial values, while (3) and (4) use lagged values. We have tried both and found only marginal differences in the results.
13. Many researchers believe that adding the three controls is a good way to weed out spuriousness in micro-econometric studies. The innovation of the AJRY team is to apply this approach to macro-relations in development.

Estimates of β_0 in pure c-c models without the lagged endogenous variable reflect long-run relations. Estimates of β_1 from models with a lagged endogenous variable such as in (4) reflect short-run relations, but the long-run effect can be inferred from the relation between β_1 and γ. That is, the two βs should satisfy the relation:

(5) $\beta_0 \approx \beta_1 / (1-\gamma)$, Implied long-run income effect.

The AJRY team applied their model to the democratic transition. Below we demonstrate that their result generalizes to other transitions. The B-table presented in section 4 reports estimates of equations (3) and (4) and estimates of some intermediate models, including results for equation (5) where appropriate. The construction of the B-table is explained in more detail in section 4.1.

3. The A-table with the DPIV model for the three transitions

Section 3.1 explains the construction of the A-table, and the A-figure. Section 3.2 covers the agricultural transition, section 3.3 the demographic transition, and section 3.4 the democratic transition. Section 3.5 surveys results, published elsewhere, for the three extra transitions.

Both the A-tables and the B-tables use a few *conventions*: The parentheses hold t-ratios, calculated from robust standard errors. If the t-ratio indicates statistical significance at the 5% level, the coefficient estimates are bolded, and if the significance is between 5% and 10%, they are bolded in italics. Bolded test-statistics point to a problem, as will be explained.

3.1 The construction of the A-table and the corresponding A-figure

Tables 3-5 are the three A-tables. The two β-rows give the OLS and the IV estimates of equations (1) and (2). The five columns use different combinations of the instruments.

Two tests of the validity of the IV method are reported:[14] The Sargan test for over-identification rejects the joint null hypothesis that the instruments are valid and correctly excluded from the estimated equation. Since we want the p-value of the test statistic to be above 0.05, test values below that limit are bolded. The Cragg-Donald test rejects the hypothesis of *weak* instruments if the test statistic is above the critical value (10 percent maximal test size). Since we want our instruments to be *strong*, test values below that limit are also bolded. Once we accept that the IV-estimates are valid, it makes sense to interpret the results by assessing the statistical significance and the size of the estimate of β_0^{DP}.

The Hausman C-test for parameter homogeneity tells us if $\beta_0 = \beta_0^{DP}$ can be rejected, so the p-value of the test statistic is bolded if it is below 0.05.

The last row of the A-tables reports the Cragg-Donald statistic for the *reverse causality test*; i.e., a test of $x \Rightarrow y$, where x is instrumented. Our main results are easier to interpret if the instruments are weak when used in the reverse. This is normally the case; see however Table 4. We bold these results if they show that the instruments are strong or if they are larger than the corresponding ones for the presumed main direction of causality.

Column (1) is our preferred instrumentation because it is most parsimonious and comprehensive, but at least in principle all five estimates should be equally good. If only one of the five tests in a row is problematic, we still think that our causality hypothesis is

14. The estimates are done by Stata. It is our impression that the two tests encompass the many other tests available in the Stata output, at least for the data of this project.

acceptable. This would certainly be the case if the five tests were independent, but they are of course not, so the aggregate strength of the five tests in a row is open to debate.

The A-tables are for one year only, which is chosen to be the year 1995. The corresponding A-figures – Figures 2-4 – present the estimation results for equations (1) and (2) with the instrumentation of column (1) of the A-tables for each year in 1960-2008. The 49 estimates of β_0 and β_0^{DP} are shown as two curves, where each curve is surrounded by a significance interval of two standard errors. This allows a second test of $\beta_0 \approx \beta_0^{DP}$. It is the case if the two curves are within the significance intervals of the two estimates.

The A-figures have two "table-lines": *N* is the number of countries used for each coefficient estimate, and *CD* is the Cragg-Donald test statistic. A low *N* typically reflects missing data for poor countries. In these cases the significance interval broadens substantially, and the CD-test statistic falls because the sample variation is reduced.

3.2 The agricultural transition

The first A-table with the DPIV model is Table 3. It appears that the conditions for the statistical validity of the instruments are met. Only one of the Sargan-tests is below the 10% limit, and all the CD-tests are strong. The reverse causality CD-tests are all smaller, and all but one are weak.

All five IV-estimates of the income effect are statistically significant with sizes of about -10 and t-ratios of about 9. We conclude that increasing income by one logarithmic point *causes* a fall in the share of agriculture of about 10 percentage points.

The five OLS estimates are about -11 and hence about as large as the IV estimates. The Hausman-tests reject parameter homogeneity in one case and are indecisive in another, but given the high t-ratios we conclude that the estimates of the income effect do not differ in an economically relevant way. Moreover, the size of the estimated income effect is sufficient to explain the full transition path of the share of agriculture (see column (7) of Table 1).

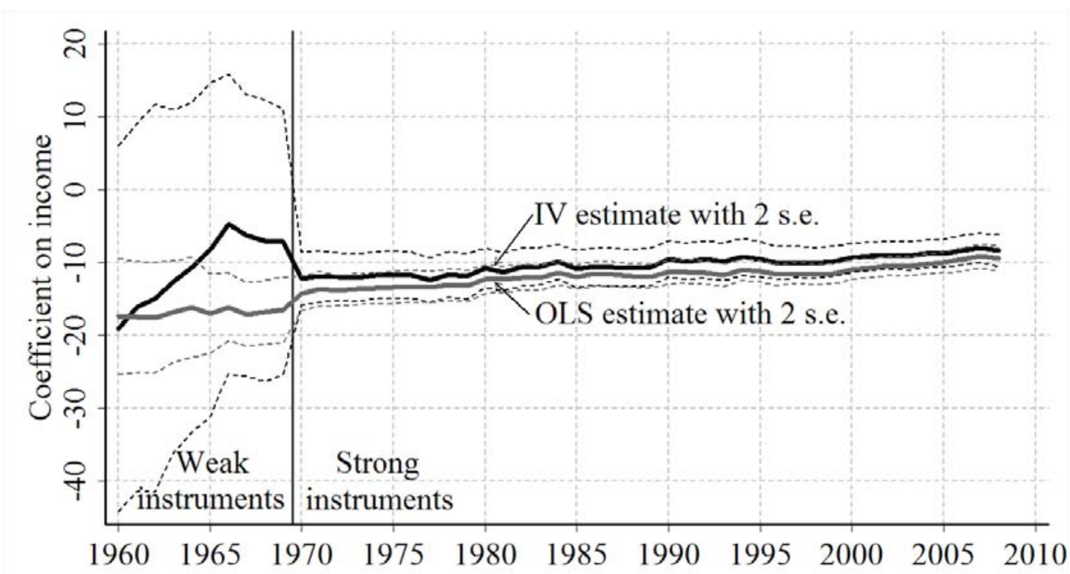
The A-figure confirms the c-c results for 1995 for all years after 1970, when the sample of countries becomes large enough to cover a broad income range. After 1970, the two β-curves are fairly flat at -10, and within an interval of two standard errors of each other. Thus, the A-figure confirms the robustness of the result of the A-table.

We conclude that income fully explains the transition path of the share of agriculture.

Table 3. The A-table for the agricultural transition

Estimated for 1995	Main model	Robustness of model to instrument variation			
Dependent variable: s_i^A	(1)	(2)	(3)	(4)	(5)
No. of obs. (countries)	97	102	97	97	136
			OLS estimates		
β_0 on income, y_{it-1}	**-11.19**	**-10.96**	**-11.19**	**-11.19**	**-11.24**
	(-14.2)	(-15.0)	(-14.2)	(-14.2)	(-14.2)
Centered R²	0.67	0.69	0.67	0.67	0.60
			IV estimates: *y* is instrumented		
β_0^{DP} on income, y_{it-1}	**-9.54**	**-10.39**	**-9.25**	**-10.39**	**-10.84**
	(-7.9)	(-10.6)	(-7.4)	(-9.7)	(-9.7)
Instruments	*biofpc, geofpc*	*bioavg, geoav*	*animals, plants*	*axis, size, climate*	*coast, frost, maleco*
		Hausman test for parameter consistency of OLS and IV estimate			
C-statistic (p-value)	***0.06***	0.38	**0.03**	0.27	0.61
			Tests of validity of the IV-procedure		
First stage partial R^2	0.45	0.56	0.43	0.55	0.50
Sargan test (p-value)	0.56	0.28	0.35	0.16	***0.05***
	Cragg-Donald test for the strength of the instruments in the IV estimate				
Presumed causality: $y \Rightarrow s^A$	37.82	61.79	35.01	37.55	43.68
CD critical value (size)	19.93	19.93	19.93	22.30	22.30
Reverse causality: $s^A \Rightarrow y$	13.25	**26.37**	11.75	15.34	18.38

Figure 2. The A-figure for the agricultural transition, 1960-2008



N:	33	50	68	78	96	96	83
CD:	1.3	2.2	28.3	30.7	46.5	37.4	27.8

Note: All CD statistics before 1970 point to weak instruments, but from 1970 instruments are strong.

3.3 The demographic transition

The pattern of results for the demographic transition is more complex, beginning with the conditions for the validity of the instruments. The Sargan-tests are remarkably variable, but our preferred specification (1) suggests that the instruments are correctly excluded. The CD-tests all indicate that the instruments are strong. However, the last row of the table shows that the instruments are even stronger in the case of reverse causality. We thus have to conclude that there is two-way causality between income and the demographic transition.

All the IV-estimates are around -10 with a t-ratio of about 10½, so there is no doubt that rising income *causes* a fall in the population growth rate. But given that there is two-way causality, the path caused by income differs from the observed long-run path. The OLS-estimates are around -7 in all five cases, which is numerically smaller than the IV estimate. Hence the Hausman-test rejects parameter homogeneity in all five cases. The coefficient needed to explain the full transition path is -6 (see column (7) of Table 1), which is fairly consistent with the estimates from the OLS-regressions, but it is substantially less in absolute value than the IV estimate. The effect of instrumented income on the path of the demography transition would thus be substantially larger than the one actually observed.

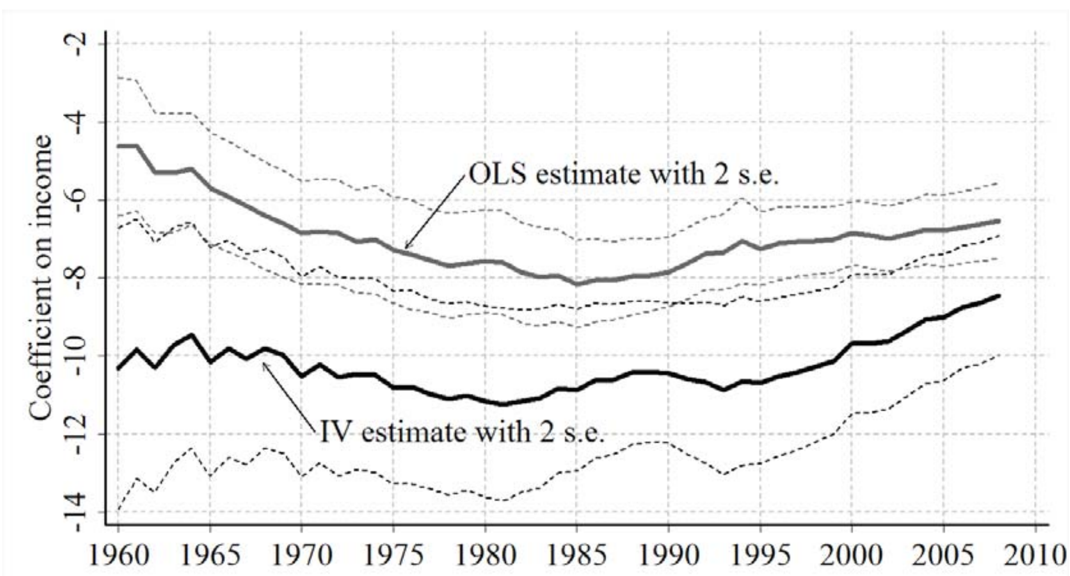
The A-figure confirms these results for all other sample years. The IV-estimates are around -10, and the OLS estimates are around -7. The difference is statistically significant as the two β-curves are outside the two standard error intervals of each other. Since the IV-estimates are too large to explain the observed transition path, it follows that the reverse causality acts as a "brake". That is, population growth obviously *causes* the level of income to *rise,* but this effect is smaller than the negative causal effect from income to population growth. This is an important long-run result – and it is worth discussing in greater detail elsewhere.[15]

15 In unified growth theory (Galor 2005), population growth is an engine of development before a critical mass of human capital is reached; i.e., as long as a country remains relatively poor. There is also evidence that fertility increases in present-day rich countries (Myrskyla et al. 2009), which is in line with a return of population growth to a moderate constant level, as discussed in Paldam (2010) in the context of a hump-shaped development of the rate of population growth over the democratic transition.

Table 4. The A-table for the demographic transition

Estimated for 1995	Main model	Robustness of model to instrument variation			
Dependent variable: *P*	(1)	(2)	(3)	(4)	(5)
No. of obs. (countries)	100	105	100	100	144
		OLS estimates			
β_0 on income, y_{it-1}	**-7.25**	**-7.17**	**-7.25**	**-7.25**	**-6.95**
	(-11.9)	(-12.7)	(-11.9)	(-11.9)	(-11.6)
Centered R²	0.59	0.61	0.59	0.59	0.48
		IV estimates: *y* is instrumented			
β_0^{DP} on income, y_{it-1}	**-10.68**	**-9.83**	**-10.64**	**-9.58**	**-9.78**
	(-10.3)	(-11.7)	(-10.1)	(-10.8)	(-10.3)
Instruments	*biofpc, geofpc*	*bioavg, geoav*	*animals, plants*	*axis, size, climate*	*coast, frost, maleco*
		Hausman test for parameter consistency of OLS and IV estimate			
C-statistic (p-value)	**0.00**	**0.00**	**0.00**	**0.00**	**0.00**
		Tests of validity of the IV-procedure			
First stage partial R^2	0.45	0.55	0.43	0.54	0.46
Sargan test (p-value)	0.99	***0.06***	0.74	0.28	**0.00**
		Cragg-Donald test for the strength of the instruments in the IV estimate			
Presumed causality: $y \Rightarrow P$	39.61	61.26	37.16	37.44	40.02
CD critical value (size)	19.93	19.93	19.93	22.30	22.30
Reverse causality: $P \Rightarrow y$	**65.13**	**89.99**	**59.10**	**41.50**	**49.22**

Figure 3. The A-figure for the demographic transition, 1960-2008



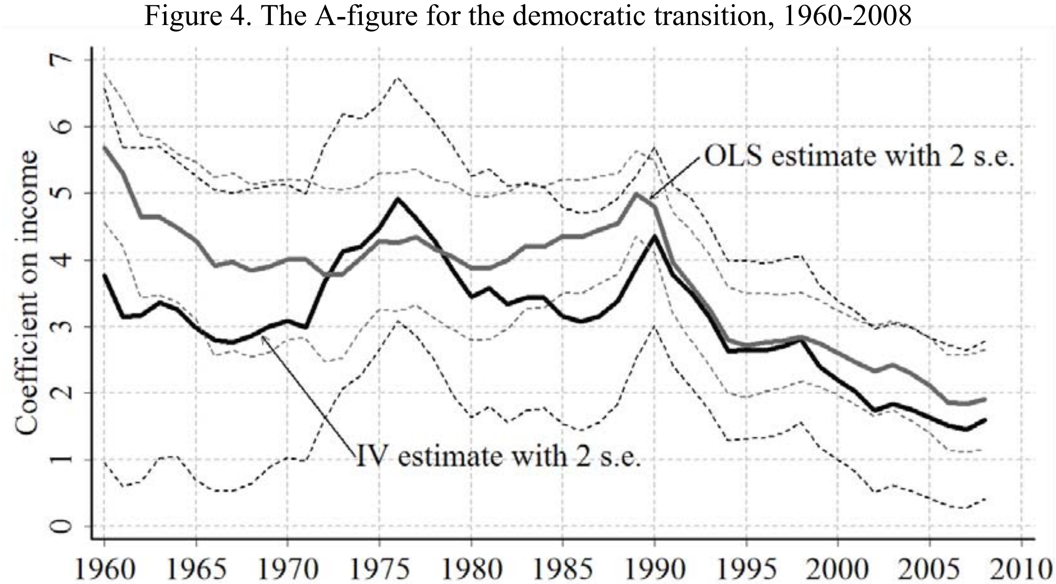
N:	97	97	97	100	100	100
CD:	26.8	31.2	35.8	47.3	39.8	41.6

Note: All CD statistics reject weak instruments.

Table 5. The A-table for the democratic transition

Time *t* is 1995	Main model	Robustness of model to instrument variation			
Dependent variable: Π	(1)	(2)	(3)	(4)	(5)
No. of obs. (countries)	101	106	101	101	143
			OLS estimates		
β_0 on income, y_{it},	**2.72**	**2.77**	**2.72**	**2.72**	**2.63**
	(6.2)	(6.8)	(6.2)	(6.2)	(6.1)
Centered R²	0.27	0.30	0.27	0.27	0.21
			IV estimates: *y* is instrumented		
β_0^{DP} on income, y_{it},	**2.65**	**3.26**	**2.48**	**2.81**	**3.02**
	(4.0)	(5.8)	(3.7)	(4.7)	(4.8)
Instruments	*biofpc, geofpc*	*bioavg, geoav*	*animals, plants*	*axis, size, climate*	*coast, frost, maleco*
		Hausman test for parameter consistency of OLS and IV estimate			
C-statistic (p-value)	0.89	0.20	0.63	0.82	0.40
			Tests of validity of the IV-procedure		
First stage partial R^2	0.44	0.53	0.43	0.54	0.47
Sargan test (p-value)	0.21	***0.07***	0.81	0.49	0.81
	Cragg-Donald test for the strength of the instruments in the IV estimate				
Presumed causality: $y \Rightarrow \Pi$	38.02	57.94	37.51	37.81	41.01
CD critical value (size)	19.93	19.93	19.93	22.30	22.30
Reverse causality: $\Pi \Rightarrow y$	6.97	16.64	5.37	6.52	6.92

Figure 4. The A-figure for the democratic transition, 1960-2008



N:	77	93	97	98	101	101
CD:	17.4	31.7	36.2	42.0	38.1	39.8

Note: The CD statistics for 1960 is at the borderline. All other CD statistics indicate strong instruments.

3.4 The democratic transition[16]

The pattern of results for the democratic transition is more like the pattern for the agricultural transition. The tests for instrument validity are rather satisfactory. Only one of the Sargan tests is in the dubious zone, all of the CD-tests show that the instruments are strong, and none of the reverse causality tests points to problems.

All the IV income effects are around 2.8 and statistically significant with t-ratios of about 4½, so there is no doubt that rising income *causes* a democratization. The OLS estimates are around 2.7, and the Hausman-tests do not reject parameter homogeneity in any of the five cases. Column (7) of Table 1 requests a coefficient of about 3 for an explanation of the full transition path, and this is very much what is found. Thus we conclude that the causality from income to the democracy index *fully explains* the transition path.

The corresponding A-figure shows that the two β-curves are largely within the interval of two standard errors of each other. We speculate that the apparent fall in the β-curves after 1990 or at least after 1995 might be due to the dissolution of the Soviet Empire. Overall, the A-figure confirms the robustness of the result of the A-table for the democratic transition.

3.5 The three extra transitions

Many other transitions have been studied, but the DPIV-test has only been applied for three additional cases.[17] These cases are noisier, with substantially fewer observations for the transition variable. Nevertheless, the resulting A-tables are much like the ones for the three cases presented above.

The *transition of corruption*. Here the transition variable is the corruption perception index from Transparency International, which is only available since 1995, starting with few countries. The DPIV-test shows that income is causal to the large change from high corruption in LICs to high honesty in DCs.

The *religious transition*. Here the transition variable "religiosity" is calculated by a factor analysis of 14 items related to religious behavior and motivations that are reported in the World Values Survey. The dominating factor appears to be a rather robust measure of religiosity, which is available for 240 polls covering 95 countries. These data show that the

16. Table 5 is an updated version of the A-table from Gundlach and Paldam (2009). For a parallel A-table for the Gastil index from Freedom House, see Paldam and Gundlach (2010a). Both show the same result. Note that Acemoglu et al. (1968) and Gundlach and Paldam (2009) transform the Polity data to a range of 0 to 1. At present the untransformed data are used, and one more period is added.
17. The references are Gundlach and Paldam (2009b), Paldam and Gundlach (2010b) and Bjørnskov and Paldam (2010).

religiosity score falls to less than half during the transition. The result from the DPIV-test is that income is causal to this fall.

The *transition in the preference for capitalism/socialism*. Here the transition variable is the score for the aggregate preferences for capitalism or socialism (CS score). It is also taken from an item reported in the World Values Survey that asks respondents about their preferences for public vs private ownership of business. The CS score is available for 200 polls in 92 countries. Preferences change substantially toward capitalism with rising levels of development. Once again the DPIV-test shows that income is causal to the observed rise in the CS score.

The next section deals with the AJRY model, and the three main transitions are used for illustrations, but the results are backed up by results from the three extra transitions. A summary of the results for the extra transitions is reported in Table 10 in section 4.5 below.

4. The B-table with the AJRY model for the three transitions

Section 4.1 explains the construction of the B-table. The agricultural transition is covered in section 4.2, the demographic transition is in section 4.3, and the democratic transition in section 4.4. Section 4.5 considers the explanatory power of the three controls in the AJRY model. Section 4.6 compares with the results for three extra transitions.

4.1 The construction of the B-table

Column (1) of the B-tables reports the estimates of the Base model (3), which is the panel version of equation (1) reported in the A-tables. Now it is estimated on about 1000 observations, and hence it obtains much higher t-ratios than in the A-tables. It is important that the income effects from equations (1) and (3) are very similar.

Column (2) gives the estimates of the AJRY model (equation (4)), which augments the Base model with the lagged endogenous variable and two fixed effects. The key point of the two causality tests is to compare the coefficient estimates of columns (1) and (2). They are amazingly different for all three transitions.

Columns (3) - (7) report estimates of five mixed models, which use alternative combinations of fixed effects and the lagged dependent variable. Two different income effects are estimated, depending on the inclusion of the lagged endogenous variable, namely β_0 and β_1. The two income effects are related by equation (5). The top line of the B-tables reports the long-run coefficient of interest, β_0, which can be directly compared to the estimates reported in the A-tables.

4.2 Is the agricultural transition strong or spurious?

Column (1) in Table 6 reports an income coefficient that is close to the OLS estimate in the corresponding A-table (section 3.2).The estimated income coefficient comes with a t-ratio of no less than 42. Usually it is not so easy to turn a coefficient with a t-ratio of that magnitude insignificant, but the AJRY model in column (2) can. In all of the five mixed models in the subsequent columns – where only one or two of the three AJRY controls are included – a statistically significant income effect remains. All three controls together are needed to make the income effect go away.

Table 6. The B-table for the agricultural transition

Dependent variable: s^A	Base model	AJRY model	Mixed model variants				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β_0 on income, $y_{it\text{-}1}$	**-11.87**	-3.25	**-9.76**	**-9.86**	**-6.67**	**-13.63**	**-9.86**
	(-42.9)	(-1.33)	(-12.9)	(-6.1)	(-3.2)	(-11.3)	(-12.2)
β_1 on income, $y_{it\text{-}1}$, adj.		-1.11	**-1.45**		**-2.11**		**-1.45**
		(1.2)	(-5.5)		(-2.6)		(-5.8)
Lagged dep. variable, $s^A_{it\text{-}1}$	No	**0.66**	**0.85**	No	**0.68**	No	**0.85**
		(16.4)	(36.0)		(17.7)		(38.0)
Country fixed effects	No	Yes	No	Yes	Yes	Yes	No
Time fixed effects	No	Yes	No	Yes	No	No	Yes
Number of observations	898	898	898	898	898	898	898
Number of fixed effects		151, 9		151, 9	151	151	9
R^2 adjusted	0.660	0.172	0.933	0.406	0.694	0.329	0.936
R^2 within		0.715		0.412	0.694	0.330	
R^2 between		0.977		0.672	0.968	0.654	
R^2 overall		0.933		0.677	0.932	0.661	

The first row reports the seven estimates of β_0, the long-run income effect. In columns (2), (3), (5), and (7) the effect is estimated by equation (5). Six of the seven results are statistically significant and around the value of -10 that explains the full transition path. They are all close to the income effect estimated with the DPIV model in section 3.2. It is no wonder that the profession seems to agree that the agricultural transition is a fact of development. The one exception is the income effect estimated by the AJRY model. It is worth considering how one estimate could be so different – we shall return to this question in section 5.

4.3 Is the demographic transition strong or spurious?

The B-table for the demographic transition tells a story that is similar to the one of section 4.2. The AJRY model reduces an income coefficient of -6.54 with a t-ratio of 32 to zero. It is interesting that the pattern of the estimates in columns (1)-(7) is the same as in the case of the agricultural transition, even though the A-table indicates that causality is more complex in the demographic case.

The seven estimates of the long-run income effect, β_0, are all significant, except the AJRY one, but they are somewhat inconsistent. The main statistical problem is that the coefficient on the lagged endogenous variable is close to 1, so the relation is too close to a unit root. Hence equation (5) for the estimate of the long-run effect from the adjustment models comes to contain $(1 - \gamma) \approx 0$ in the denominator. From Table 1 and section 3.4, we expect that β_0 is close to -6, but only two of the seven estimates come close to this value. Once again, especially the AJRY estimate is far from the expected value, and even its sign is wrong.

Table 7. The B-table for the demographic transition

Dependent variable: P_{it}	Base model	AJRY model			Mixed model variants		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β_0 on income, y_{it-1}	**-6.54**	0.07	**-13.99**	**-1.82**	**-5.93**	**-4.95**	**-11.20**
	(-31.8)	(0.0)	(-8.1)	(-2.1)	(-2.7)	(-5.8)	(-10.5)
β_1 on income, y_{it-1}, adj.		0.02	**-0.67**		**-0.91**		**-0.68**
		(0.0)	(-5.9)		(-2.3)		(-6.0)
Lagged dependent, P_{it-1}	No	**0.76**	**0.95**	No	**0.85**	No	**0.94**
		(22.7)	(82.0)		(25.2)		(77.2)
Country-fixed effects	No	Yes	No	Yes	Yes	Yes	No
Time-fixed effects	No	Yes	No	Yes	No	No	Yes
Number of observations	1287	1287	1287	1287	1287	1287	1287
Number of fixed effects		157, 9		157, 9	157	157	9
R^2 adjusted	0.430	0.789	0.952	0.423	0.742	0.130	0.957
R^2 within		0.791		0.427	0.742	0.130	
R^2 between		0.991		0.628	0.991	0.459	
R^2 overall		0.949		0.388	0.951	0.430	

Table 8. The B-table for the democratic transition

Dependent variable: Π_{it}	Base model	AJRY model			Simplified versions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β_0 on income, y_{it-1}	**3.33**	-1.19	**2.95**	-0.59	**5.26**	**2.87**	**2.19**
	(20.2)	(-1.1)	(4.3)	(0.7)	(4.2)	(3.9)	(3.2)
β_1 on income, y_{it-1}, adj.		-0.44	**0.33**		**1.39**		**0.26**
		(-1.1)	(3.1)		(4.0)		(2.5)
Lagged dependent, Π_{it-1}	No	**0.63**	**0.89**	No	**0.74**	No	**0.88**
		(19.6)	(61.8)		(25.8)		(61.6)
Country-fixed effects	No	Yes	No	Yes	Yes	Yes	No
Time-fixed effects	No	Yes	No	Yes	No	No	Yes
Number of observations	1199	1199	1199	1199	1199	1199	1199
Number of fixed effects		153, 9		153, 9	153	153	9
R^2 adjusted	0.240	0.612	0.830	0.343	0.534	0.045	0.846
R^2 within	-	0.615		0.348	0.535	0.046	
R^2 between		0.950		0.049	0.924	0.305	
R^2 overall		0.812		0.038	0.808	0.241	

4.4 *Is the democratic transition strong or spurious?*

Table 8 is the B-table for the democratic transition. The estimation results are much the same as for the two previous B-tables. The base model in column (1) estimates an income effect of 3.33 with a t-ratio of about 19.5. This is turned insignificant and even negative by the AJRY model. The estimate in column (2) is a replication of Acemoglu et al. (2008) on updated data. Our results are very much the same as in their analysis. The effect of income on democracy is found to be zero, so it is revealed to be spurious according to this model.

The seven estimates of the long-run income effect, β_0, are somewhat mixed. Three of these estimates are close to the value of about 3 from Table 1 that would be needed to explain the full transition path, as confirmed in section 3.4. Both fixed effects have to be included to turn the sign of the income coefficient and render it insignificant, otherwise the income effect remains significant and in the expected range.

4.5 *What does the AJRY model do to the data?*

We argue that the AJRY model treats the variation in the data in a way that can be explained by the onion metaphor - one layer after the other is peeled away until little to nothing remains. Consider the following progression of models:

(6) $x_{it} = \delta_1 x_{it-1} + \varepsilon_{1it}$ layer of the lagged endogenous variable,

(7) $x_{it} = \delta_2 x_{it-1} + \alpha_i + \varepsilon_{2it}$ additional layer of the country-fixed effects, and

(8) $x_{it} = \delta_3 x_{it-1} + \alpha_i + \alpha_t + \varepsilon_{3it}$ final layer of the time-fixed effects.

Table 9 reports the fraction of the variation in the dependent variable that is explained by the stepwise inclusion of the lagged endogenous variable and the two fixed effects.

Equation (6) already peels off between 83% and 95% of the variation of the transition variables (as measured by the R^2). What is left to be explained is further reduced by the two fixed effects, which represent 158, 164 and 160 dummy variables, respectively. Much the same happens if the peeling is done in reverse order - the fixed effects for countries already peel off between 70% and 86% of the variation in the dependent variable. Either way, there does not remain much to be explained by any further right-hand-side variable: only between 4% and 12% of the initial variation in the dependent transition variable is left.

Table 9. How much variation remains?

Corresponds to Equation		Fraction of the explained variation in the dependent variable: Table 5. Agriculture R^2	Nvar	Obs.	Table 6. Demography R^2	Nvar	Obs.	Table 7. Democracy R^2	Nvar	Obs.
(6)	lagged endogenous	0.930	2	898	0.950	2	1287	0.828	2	1199
(7)	+ FE for time	0.933	10	898	0.954	10	1287	0.848	10	1199
(8)	+FE for countries	0.961	160	898	0.968	166	1287	0.880	162	1199
	As equations (6) to (8), but the variables are added in reverse order									
(9)	FE for countries	0.862	151	898	0.847	157	1287	0.689	153	1199
(10)	+ FE for time	0.902	159	898	0.911	165	1287	0.797	161	1199
(11)	+ lagged endogenous	0.961	160	898	0.968	166	1287	0.880	162	1199
	Fixed effects for time alone									
(12)	FE for time	0.176	10	898	0.399	10	1287	0.369	10	1199

Note: R-squared based on OLS regressions. Same data samples as in the B-tables. *Nvar* is a count of the number of explanatory variables included in the regressions incl. the constant. The last period and the last country are deleted to keep a constant in the regressions, so that the R^2-scores are comparable.

Thus we conclude from the B-tables that the transition variables can be explained rather well either by income, by itself lagged, or by country-fixed effects, but less so by time effects. From the A-tables we learn that the three transitions can be explained rather well when income is instrumented with the extreme DP-variables. We think that our most interesting finding is the robustness of the A-table result in the presence of a country-fixed effect in the B-tables. This shows that the c-c results can be reproduced with the within-country variation, at least as long as not most of the within-country variation is also controlled for by a fixed time effect and a lagged endogenous variable.

4.6 The three extra transitions

The B-tables for the three transitions discussed in section 3.5 are reported in Paldam and Gundlach (2010a, b). The numbers of observations, *N*, are smaller in these cases. The panels are fairly incomplete, but the estimates still work.

The main results for both the A- and the B-tables are briefly summarized in Table 10. The results are very much as for the three transitions already discussed. The DPIV-test – in the A-table – gives results that indicate a highly significant long-run causality from income to the transition variable. The results in the A-table are consistent with the base model in the B-table, while the AJRY model is always very different and insignificant. The four columns to the right of Table 10 give the dimensions of the three panels.

Table 10. A brief summary of the A- and B-tables of the three extra transitions

	A-table (average of the five tests)					B-table		Panel for B-table			
Transition	OLS	IV	*Nc*	Sargan	CD-test	Base	AJRY	*Nc*	*Np*	*N*	Missing
Corruption	**1.39**	**1.43**	98	0.65	36.9	**1.62**	0.15	156	4	429	31.3%
	(13.5)	(9.6)				(24.1)	(0.5)		3-year		
Religiosity	**-11.96**	**-14.72**	65	0.32	23.6	**-11.69**	3.61	64	5	126	50.8%
	(-7.3)	(-6.1)				(-6.7)	(0.4)		Waves		
CS-score	**5.98**	**8.93**	63	0.41	19.6	**8.00**	15.40	50	3	90	40.0%
	(2.8)	(3.0)				(2.6)	(0.7)		Waves		

Note: See previous A- and B-tables. *Nc* is number of countries, and *Np* is number of periods. Missing is calculated from *N* relative to the product *Nc·Np*. The published A-table in Gundlach and Paldam (2009b) is presented differently from the A-tables discussed so far. To make the results comparable, it has been recalculated in the same form as the other A-tables and reported in Gundlach and Paldam (2010b).

5. Explaining the contradictory results

The DPIV and the AJRY model can be considered as two pure forms of causality tests that both test if $y \Rightarrow x$. The A-tables with the DPIV model show that the three textbook transitions are caused by development as proxied by income. The B-tables with the AJRY model show that the relation between development and the three transitions is spurious. The three extra cases confirm these conflicting results. Hence a contradiction remains.

5.1 The logic of the two estimation models

The key to the discussion is the distinction between *trends* and *innovations* in the series, as defined by Granger (1980). The trends are the parts of the series that can be explained by itself and fixed effects. The innovations are the idiosyncratic, exogenous parts of the series. In the shorter run, growth accelerations and decelerations are the idiosyncratic innovations of interest. But in the long run, the growth trends themselves are the idiosyncratic innovations that matter.

The DPIV model is a pure cross-section IV-test using truly exogenous instruments. By including countries across the widest possible range of incomes, any estimate looks at the long run of the full transition. The DP-variables used as instruments are also pertaining to the very long run. Thus the DPIV model shows that in *the long run* causality goes from income to the transition variable. This is probably uncontroversial, but it leaves open whether other factors than income play an important role in transitions in the medium and short run. However, ignoring the long-run relation in discussions of the medium and short run would be a mistake from this perspective.

The AJRY model is related to the family of Granger causality tests. As shown in section 4.5, it removes all systematic components from the dependent transition variable and leaves only the short-run innovations in the series. Then it analyzes if the short-run innovations matter for the transition variable: of course they do not.

5.2 The claim of the literature on the three transitions

A large literature discusses the relation between development and each of the three transitions. It appears to be rare indeed to see the claim that the short-run innovations in income is the crucial factor for the path of any of the three transition variables.

Consider the agricultural transition. Imagine that income rises in the short run due to a truly exogenous growth spurt. It will raise the share of agriculture if it happens in the agricultural sector, but it reduces the share if it happens in another sector. This variation does not reflect what the agricultural transition is about. What the transition literature claim is that if income raises permanently, the share of agriculture will gradually fall. The same logic applies to the demographic transition. Transitions are held to be slow processes of adjustment.

The democratic transition is more troublesome to analyze in this context since the political system often stays constant for long periods and then jumps due to a *triggering* event.[18] Even when the triggering event is known, it often appears rather random from the point of view of the economist.

A well-known case is the Spanish democratization.[19] In the 1950s and 60s, Spain converged economically to the West and had a military dictatorship that made the country a political fossil in the West. The DPVI model argues that the strong development of Spain was the underlying cause for the democratization. However, the triggering event for the change was the death of General Franco (in 1975); not a particular growth spurt, so the AJRY model would reject that the economy triggered the event and hence conclude that income is spuriously correlated to the democratization of Spain. Had the old general lived shorter or longer, the democratization would most likely have occurred in another year, but it is hard to imagine that it would have failed to occur at all. For the economist – and even the historian – what matters is the general phenomenon that drives a certain path of development, whereas the actual triggering event belongs in the noise term.

5.3 One problem for all transition models

Once it is accepted that the relations of interest are between long-run trends in the variables, it is clear that many other variables may be at play as well. Each transition is described by a highly reduced DPIV model, and the causality proofs in the A-tables cannot rule out the role of intermediate variables. The Grand Transition is a notoriously complex process, involving many variables. But by using valid IV estimation, we do not have to be worried about omitted variables bias.

The democratic transition is a case in point. Maybe an educational transition is needed before development will increase the level of democracy. But once development occurs, it

18. Paldam (2009) documents the stepwise properties of the Polity data.
19. A rather similar story can be told of Greece and Portugal, at about the same time, and of Chile 15 years later, though in these three cases the triggering was a more complex story.

inevitably generates a transition in the level of education. It is also possible that the agricultural transition is needed for the democratization. Our A-table result only proves that countries that have turned wealthy in the course of development normally become democracies.[20] However, countries that have turned wealthy without development (such as Saudi Arabia and Brunei) do not normally become democracies.

The B-tables can then be used to say that short-run processes are complex, even when long-run causality is an accepted fact. However, it would be wrong to use the short-run complexity to discard the long-run relation.[21] The AJRY model implies a long-run income effect according to equation (5), as discussed in sections 4.2-4.4. Given that the long-run effect is uncontroversial, it has to show up. But it does not simply because the short-run income effect β_1 is found to be zero. Too much variation must have been taken out of the data by the AJRY model to allow for a meaningful estimation of the long-run relation.

5.4 Type I and Type II errors made by the two models

An alternative way to compare the two models is to consider the tradeoff between Type I and Type II errors, where the first is a rejection of a true model and the second is acceptance of a false model. The purpose of the AJRY model is to handle Type II errors by rejecting spurious relations. However, the stronger one guards against Type II errors, the more Type I errors one makes. Avoiding Type II errors is like using weed killers. One wants to rid the garden of weeds, but also to preserve the flowers. If the weed killers – like Roundup – are too strong, everything gets killed. We suggest that the AJRY model has the roundup property when applied to models of long-run development.

Is it sufficient to show that the AJRY model has the roundup property by demonstrating how it works in six gardens? We think that it is because we use the three most uncontroversial transitions, where plenty of data are available, and check the robustness of our results with three less-known additional cases. In our view, all results together are so remarkably similar that they appear to represent a compelling set of evidence against using the AJRY model for the estimation of long-run transitions.

20. At present, Singapore appears to be the only exception to this rule.

21. The long-run relation is analyzed towards the end of Acemoglu et al. (2008), but this analysis is not integrated in the short-run analysis.

6. Conclusion on causality

Economists chase causality, which is a difficult business in the absence of controlled experiments. Theory and various descriptive data tools such as correlations provide hints for reasonable hunting grounds. The paper discusses the outcome of three such hunts, for the agricultural, the demographic, and the democratic transitions.

In all three cases both theory and correlations suggest that big game is present: Income may be causal to the transition variable. Our DPIV-causality shows that in two cases (agriculture and democracy) long-run causality is exclusively from income to the transition variables. Taken at face value, this implies that simple correlations (OLS) already provide an unbiased order of magnitude of the presumed income effects. In the third case (population growth) two-way causality occurs, which acts as a brake making the transition slower.

The transition hypotheses can be rejected if the observed correlation is spurious, so that both variables are caused by a third variable *C*, such that $C \Rightarrow y, x$. Normally a spurious relation would be revealed by the IV-test used to identify the direction of long-run causality. If important omitted variables determine the observed transition, our DPIV estimates should produce statistically insignificant income effects. But this is not the case for the three transitions at hand.

The AJRY model is meant as a strong test of spuriousness as it demands that a causal relation from y to x can only be accepted if x can be explained by the innovations in y. The AJRY model rejects income effects for all three transition variables. Consequently, it suggests that all three transitions are spurious.

However, we believe that the whole point of a long-run relation is that it is between the systematic parts of the series, notably between levels, and as shown above this is precisely what is weeded out from the data by the AJRY method. Consequently, the AJRY method cannot be applied as a test of a long-run relation.

The Grand Transition view claims that many economic, political, and cultural variables change their relative level when the level of income changes from LIC to DC, as sketched on Figure 1. For the examples of Figure 1, the long-run hypothesis has been abundantly confirmed. Given the long-run relation, a short-run relation that can be aggregated to the said long-run relation must exist. Aggregation is notoriously difficult, but it is better to search diligently than to apply a generous dose of Roundup.

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1 Paldam, M., 2009. The polity data: An analysis of their properties

2 Gundlach, E., Paldam, M., 2010a. The agricultural transition in a generic country. A graphical exposition.

3 Paldam, M., 2010. The demographic transition. An estimate of the typical path

4 Gundlach, E., Paldam, M., 2010b. The democratic transition in a generic country. A graphical exposition

5 Gundlach, E., Paldam, M., 2010c. The transition of corruption. Causality and two models

6 Paldam, M., Gundlach, E., 2010. The religious transition and the transition in support for capitalism. Causality and two models

(1) Documents a set of characteristics of the Polity data. The paths of the three transition variables are depicted in (2), (3) and (4). These papers also bring the B-tables for panels using 3-year and 7-year periods. (5) Deals with the transition of corruption. It brings the A-table in the form used in section 3 and the B-table. (6) Brings the B-tables for the religious transition and the transition in support for capitalism.

Appendix: Definitions and sources of the DP-variables

Instruments used in the A-tables. The A-figures only uses *biofpc* and *geofpc*.

animals	Number of domesticable big mammals, weighing more than 45 kilos, which are believed to have been present in prehistory in various regions of the world. Source: Olsson and Hibbs (2005).
bioavg	Average of *plants* and *animals*, where each variable was first normalized by dividing by its maximum value. Source: Hibbs and Olsson (2004).
biofpc	The first principal component of *plants* and *animals*. Source: Olsson and Hibbs (2005).
maleco	Measure of malaria ecology; combines climatic factors and biological properties of the regionally dominant malaria vector into an index of the stability of malaria transmission; the index is measured on a highly disaggregated sub-national level and then averaged for the entire country and weighted by population. Source: Kiszewski and Sachs et al. (2004).
plants	Number of annual perennial wild grasses known to have existed in various regions of the world in prehistory, with a mean kernel weight exceeding 10 milligrams. Source: Olsson and Hibbs (2005).
axis	Relative East-West orientation of a country, measured as east-west distance (longitudinal degrees) divided by north-south distance (latitudinal degrees). Source: Olsson and Hibbs (2005).
climate	A ranking of climates according to how favorable they are to agriculture, based on the Köppen classification. Source: Olsson and Hibbs (2005).
coast	Proportion of land area within 100 km of the sea coast. Source: McArthur and Sachs (2001).
frost	Proportion of a country's land receiving five or more frost days in that country's winter, defined as December through February in the Northern hemisphere and June through August in the Southern hemisphere. Source: Masters and McMillan (2001).
geoavg	Average of *climate*, *lat*, and *axis*, where each variable was first normalized by dividing by its maximum value. Source: Hibbs and Olsson (2004).
geofpc	The first principal component of *climate*, *lat*, *axis* and *size*. Source: Olsson and Hibbs (2005).
lat	Distance from the equator as measured by the absolute value of country-specific latitude in degrees divided by 90 to place it on a [0,1] scale. Source: Hall and Jones (1999).
size	The size of the landmass to which the country belongs, in millions of square kilometers (a country may belong to Eurasia or it may be a small island). Source: Olsson and Hibbs (2005).