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Socioeconomic transitions as common dynamic processes

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Abstract:

Long-run socioeconomic transitions can be observed as stylized facts across countries and over time. For instance, poor countries have more agriculture and less democracy than rich countries, and this pattern also holds within countries for transitions from a traditional to a modern society. It is shown that the agricultural and the democratic transitions can be partly explained as the outcome of dynamic processes that are shared among countries. We identify the effects of common dynamic processes with panel estimators that allow for heterogeneous country effects and possible cross-country spillovers. Common dynamic processes appear to be in line with alternative hypotheses on the causes of socioeconomic transitions.

Keywords: Long-run development, agriculture, democracy, socioeconomic transitions, mean group estimators, technology heterogeneity, cross-section independence

JEL: O1, P5, Q1

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1. Stylized facts and motivation

Socioeconomic transitions are long-run changes in the economic, demographic, social, and political structure of a society that happen along with rising levels of per capita income. The two probably best-known examples include the shift away from agriculture and the improvement of democratic institutions, which have been experienced in one way or another by most of today's rich countries over the last 100-200 years.

The same stylized pattern of socioeconomic transitions apparently also holds for a cross-section of countries. Moreover, transitions can be observed for many other socioeconomic variables as well. A typical low income country has not only more agriculture and less democratic institutions than the typical high income country, but also, say, a younger population, fewer city dwellers, more religiosity, more gender inequality, and more corruption. All this is just how the typical rich country of today looked like when it was poor, i.e., before it entered the transition from a traditional to a modern society.

The paper focuses on two of the best-known socioeconomic transitions, namely the agricultural and the democratic transitions. Both transitions can be studied over relatively long periods of time and across countries. In both cases, the long-run pattern for individual countries looks like the cross-country pattern. Figure 1 shows scatter plots of the value added share of agriculture and the Polity IV democracy score against per capita income for eleven averaged time periods (1960-64, ..., 2010-12). Depending on data availability, each graph includes about 130 countries and more than 1000 country-period observations.

In the pooled data, the agricultural transition looks like a rather smooth continuous process that levels out at a high per capita income level, whereas the democratic transition looks more like a discrete jump at an intermediate income range from being a non-democracy to being a democracy, with relatively few observations in between and a lot of noise in the data. The highlighted country observations in Figure 1 are typical cases illustrating these interpretations; counter examples of course exist. The overall impression from the pooled data is that the correlation between the share of agriculture and per capita income is stronger $(R^2 = 0.69)$ than the correlation between the degree of democracy and per capita income $(R^2 = 0.37)$.

Our reading of Figure 1 is twofold. Socioeconomic transitions look to be correlated with a certain range of per capita income, and they appear to be a common phenomenon shared by many countries. This suggests that socioeconomic transitions may be driven by income levels but also by spillover effects of common shocks, be they related to technological innovations like the green revolution or to power shifts in international relations like the demise of the Soviet Union. Especially political events in neighboring countries or in the relevant hegemonial power may have a larger triggering effect on the democratic transition than the income level per se, such that the long-run correlation between the level of income and the degree of democracy becomes pretty noisy.

It is obvious that long-run outcomes are the result of short run dynamic processes with noisy components that aggregate over time. It is also well known that developments in one country tend to spill over to others. So it is not trivial to identify a general long-run pattern in macroeconomic data with a limited time dimension because any long-run correlation between a transition variable and income may turn out to be spurious. The problem is that by reducing the possibility of spurious correlations one may actually eliminate the long-run information from the data. Even when great efforts have been made to develop estimators that account for complex dynamics and interactions, it remains an open question which estimator performs best in the presence of multiple data problems.

The paper uses a broad range of panel estimators to delineate the income effects from the spillover effects. Without taking into account that socioeconomic transitions are at least partly driven by spillover effects, estimated income effects are likely to be biased upwardly. We find that the unconditional income effects on the agricultural and the democratic transitions are either substantially reduced or become statistically insignificant if common dynamic processes are included as an additional regressor.

The next section briefly summarizes alternative hypotheses on the causes of socio-economic transitions. Section 3 elaborates on our empirical strategy. Alternative specifications of an AR1 panel model and an alternative modeling of the error term lead to estimators that allow for heterogeneous country effects and cross-country spillovers. Section 4 briefly discusses data, samples, and instrumental variables. Section 5 reports regression results for the agricultural and the democratic transitions based on "pooled" and "heterogeneous" estimators. The latter put fewer restrictions on the data than the standard panel estimators that dominate the empirical growth literature up to now. Section 6 concludes on the basis of our preferred estimation results.

2. Alternative hypotheses on the causes of socioeconomic transitions

The agricultural and the democratic transitions are covered by a large literature, and it has often been discussed if these transitions can be observed equally well across countries and over time. While the long-run facts have been well established for long,[3] it is more controversial how the short-run dynamics can be aggregated to produce the long-run results, especially in case of the democratic transition.

Selected recent examples of various aspects of this literature include Young (2012) and Eberhardt and Teal (2013) on empirical estimates of long-run sectorial shifts of the economy; Lucas (2009) on simulated growth paths of a two-sector model with agricultural and urban transitions; Young (2013) on the link between the rural-urban gap and the level of human capital (income); Hofmann and Wan (2013) on the direction of causality between income and urbanization; Michaels et al. (2012) on the links between the urban transition and other transitions; Murtin (2013) on the demographic transition; Welzel (2013) on the transition to universal freedoms, and not least the challenge by Acemoglu et al. (2008) that the observed long-run correlation between income and democracy is spurious.[4]

As usual it has proven difficult to identify the causal relation behind the correlations shown in Figure 1 and other transitions.[5] Without further evidence, one cannot decide on the relevance of alternative hypotheses on the causes and consequences of long-run income growth and socioeconomic transitions. The major controversies may be summarized as follows:

Our own view holds that the pattern of long-run development may actually follow a *grand transition* (Paldam 2002, Paldam and Gundlach 2008), where interacting transitions in many fields of society are said to be mainly driven by per capita income growth. A related formulation is that transitions are parts of a process of *sequential modernization* (Inglehart and Welzel 2005). These views claim by default that the innovations that drive the process of development and the direction of causality are located in the income-generating process. But as long as the details of the origins of such innovations are not explicitly modeled, the possibility of spurious correlations remains.

A second view holds that long-run development may be considered as a complex process where per capita income, bureaucratic capability, social equality and tolerance, and political representation may all change simultaneously and interactively, without a unidirectional

3. Documenting in detail what is claimed would be an extensive task. Seminal contributions include but are certainly not confined to Lipset (1959), Rostow (1960), Hobsbawm (1968), and Kuznets (1968). Earlier contributions that may have inspired these works are, e.g., Durkheim (1893) and Marx (1867).
4. This challenge has been addressed by Gundlach and Paldam (2009b), Benhabib et al. (2011), Murtin and Wacziarg (2011), Paldam and Gundlach (2012), Barro (2012), and Treisman (2013).
5. For instance, Stark and Iannaccone (1994) argue that the decline of religiosity due to rising levels of income is a "myth", and Lambsdorff (2007) claims that the correlation between income and corruption is driven by causality from corruption to income, and not the other way round as claimed by, e.g., Gundlach and Paldam (2009a).

causal structure. This view has been labeled *transformation trajectories in functional space* (Pritchett 2009). By default, it can account for patterns of reverse causality, but it remains silent on the question about where the innovations might come from that give the process of long-run development its relatively uniform pattern towards economic prosperity, good governance, low levels of violence, and democracy.

A third view holds that the innovations that drive the process of long-run development are located in the political power structure of a society (Acemoglu et al. 2005). According to this view, changes of the fundamental structure of political power at *critical junctures* in economic history set countries on idiosyncratic and persistent paths of development. Hence what looks like a positive link from per capita income to more democracy may in fact reflect a spurious correlation that is driven by the deep institutions of a country that may have been shaped at distinct power shifts in the distant past (Acemoglu et al. 2008).

Taken all three views together, it remains unclear whether the observed correlations here labeled as socioeconomic transitions represent one-way causality from income to the transition variables, two-way causality, or a spurious relation that results from omitted factors driving both income and the transition variables. The previous literature has addressed these problems with panel estimators that allow for endogenous regressors and omitted variables. The next section introduces an empirical model that additionally allows for country-specific income effects and cross-country spillovers, which are aspects that have often been ignored in the empirical literature.

3. An empirical model with parameter heterogeneity and cross-section dependence[6]

A most parsimonious empirical model of a socioeconomic transition would describe a relation between the explanatory variable x, represented by log income per person, and a transition variable z, represented by an index variable that is bounded from above and below like the value added share of agriculture in GDP or the degree of democracy. Both x and z vary across countries i and over time t,

(1) $$z_{it} = \beta_i x_{it} + u_{it} \quad \text{with} \quad u_{it} = \mu_i + \lambda_i f_t + \varepsilon_{it},$$

6. The presentation of the empirical model closely follows Eberhardt (2012), Bond and Eberhardt (2013), Eberhardt et al. (2013), and Eberhardt and Teal (2014).

where β_i is the country-specific (heterogeneous) long-run parameter of interest and u_{it} is an error 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 explanatory variable x_t is modeled as a linear function of unobserved common factors f_t and g_t, each with country-specific factor loadings (δ_i and ϑ_i) and allowing for nonstationary input factors (and thus for nonstationary output),

(2) $$x_{it} = \pi_i + \delta_i f_t + \vartheta_i g_t + \upsilon_{it},$$

(3) $$f_t = \varphi f_{t-1} + e_t \qquad \text{and} \qquad g_t = \kappa g_{t-1} + e_t,$$

where the error terms ε_{it}, υ_{it}, and e_t in equations (1)-(3) are assumed to be normally distributed with mean zero and constant variance ($\sim N(0,\sigma^2)$). Given that the unobservable factor f is a determinant of x_{it} in equation (2) and is also part of the error term u_{it} in equation (1), the model generates cross-section dependence in the observables and unobservables. Hence x_{it} will be correlated with u_{it} in (1), and the parameter of interest β_i may not be identified independently from the country-specific factor loadings δ_i and ϑ_i of the common factors f_t and g_t.

This model setup contains three important elements that differ from the standard panel model setup of the empirical growth literature: (i) the potential heterogeneity in the impact of observables and unobservables across countries ($\mu_i, \beta_i, \lambda_i$), (ii) the potential non-stationarity of observables and unobservables (z_{it}, x_{it}, f_t, g_t), and (iii) the potential cross-section dependence of the residuals that would reflect the endogeneity of the regressor. Put differently, estimating equation (1) without accounting for the unobservables μ_i and f_t may lead to inconsistent estimates of β_i.

The most popular panel estimators in the empirical growth literature (POLS, 2FE, Difference-GMM, System-GMM) identify μ_i and f_t with country and year dummies (or with first-differencing and cross-sectional demeaning), but the latter assumes that global shocks have identical effects on each country, which seems quite restrictive. In any case, panel data would not provide any additional information compared to pure time series data without assuming some commonality across groups. Such cross-section dependence is actually a core element of our *grand transition* hypothesis, which claims that there is a common pattern of socioeconomic transitions across countries and over time. The presence of common shocks with possibly different effects across countries (country-specific factor loadings) and the possibility of nonstationary variables violate the implicit assumptions of cross-section independence and stationarity that are required for the standard panel estimators.

2FE estimation gets rid of the cross-section variation but ignores the main motivation for using panel data in the first place, which is the presumed common pattern across countries. POLS (and random effects) estimation maintains the cross-country variation but ignores that the time series effects may differ from the cross-section effects, which will result in biased estimates. Heterogeneous parameters, variable nonstationarity, and cross-section dependence of the residuals pose additional estimation problems that have been more or less ignored in most of the empirical growth literature.[7]

Following Pesaran (2006), the unobserved factor f can actually be captured if the number of sample countries (*n*) becomes large, namely by using cross-section averages of the dependent and independent variables ($\overline{z}_t$ and $\overline{x}_t$) as additional regressors. Substituting for u_{it} in equation (1) gives

$$(1') \qquad z_{it} = \beta_i x_{it} + \mu_i + \lambda_i f_t + \varepsilon_{it} .$$

Given that $\overline{\varepsilon}_t \to 0$ as $n \to \infty$, equation (1') can be solved for f when written in cross-section averages,

$$(1'') \qquad \overline{z}_t = \overline{\beta}\overline{x}_t + \mu + \overline{\lambda} f_t \qquad \Leftrightarrow \qquad f_t = (\overline{z}_t - \overline{\beta}\overline{x}_t - \mu) / \overline{\lambda} .$$

Substituting for f in equation (1') gives

$$(1''') \qquad z_{it} = \beta_i x_{it} + \lambda_i / \overline{\lambda}(\overline{z}_t - \overline{\beta}\overline{x}_t - \mu) + \mu_i + \varepsilon_{it} \qquad \Leftrightarrow \qquad z_{it} = \beta_i x_{it} + \omega_{1i}\overline{z}_t + \omega_{2i}\overline{x}_t + \omega_{3i} + \varepsilon_{it} .$$

The parameters ω_{ji} cannot be interpreted because they include the unknown averaged parameters $\overline{\lambda}$ and $\overline{\beta}$. But since they account for the cross-section dependence caused by the

7. E.g., see the Monte Carlo study of growth regressions by Hauk and Wacziarg (2009), which does not mention these estimation problems.

simultaneous presence of f in equation (1) and equation (2), equation (1''') can be used to derive an unbiased estimate of the (heterogeneous) long-run parameter of interest, β_i. This is the CMG (Common correlated effects Mean Group) estimator proposed by Pesaran (2006), which estimates β_i as an average of country-specific regressions and treats the effects of the common factors as nuisance parameters, i.e., merely as something to be accounted for that is not of particular interest for the empirical analysis.[8]

Bond and Eberhardt (2013) have added a further dimension to the CMG estimator. The AMG (Augmented Mean Group) estimator accounts for cross-section dependence by including a "common dynamic effect" instead of cross-section averages of all variables. In the present context, this additional regressor is constructed from the coefficients on the (differenced) year dummies (D) of a pooled OLS regression of equation (1) in first differences (FD-OLS),

$$(4) \qquad \Delta z_{it} = b\Delta x_{it} + \sum_{t=2}^{T} c_t \Delta D_t + \tilde{u}_{it}, \qquad \Rightarrow \hat{c}_t = \hat{\eta}_t^{\bullet}$$

where the collected coefficients $\hat{c}_t$ are relabeled as the "common dynamic process" $\hat{\eta}_t^{\bullet}$. In the second stage of the AMG estimator, the constructed variable $\hat{\eta}_t^{\bullet}$ is included in each of the n country-specific regression equations, either in restricted or in unrestricted form, which may also include a country-specific time trend (t),

$$(5) \qquad z_{it} = b_i x_{it} + \mu_i + c_i t + d_i \hat{\eta}_t^{\bullet} + \tilde{u}_{it} \qquad \Rightarrow \hat{b}_{AMG} = 1/n \sum_i \hat{b}_i .$$

Like the CMG estimates, the AMG estimates of the parameter of interest are derived as averages of the individual b_i estimates, thereby following the approach introduced by Pesaran and Smith (1995), which is the seminal paper in the literature on Mean Group estimators.

The static model outlined above also has a dynamic representation, which will be used in our regression analyses in Section 5. The reason is that studying a long-run equilibrium relation with a static model like equation (1) may not appropriately capture the dynamic

8. Monte Carlo simulations by Coakley et al. (2004) confirm the robust performance of the CMG estimator in rather general non-stationary settings where regressors and errors share common factors and their factor loadings are possibly dependent.

adjustment of the system and may mistake short-run deviations for long-run effects. A simplified dynamic representation is derived by assuming a static model with a country-fixed effect and an autocorrelated error term ζ_{it} but without the common factor f_t, introduced in equation (1). Hence the simplified dynamic version of equation (1) becomes

(6) $$z_{it} = \beta_i x_{it} + \mu_i + \zeta_{it} \qquad \text{with} \qquad \zeta_{it} = \rho_i \zeta_{i,t-1} + e_{it}.$$

A simple AR1 model follows for $\rho_i \neq 0$ as

(7) $$z_{it} = \rho_i z_{i,t-1} + \beta_i x_{it} - \rho\beta_i x_{i,t-1} + (1-\rho)\mu_i + e_{it}.$$

The AR1 model implies that the coefficient of the lagged explanatory variable equals the product of the coefficient of the contemporaneous explanatory variable and the coefficient of the lagged endogenous variable. The *common factor restriction* $\rho_i\beta_i = \rho_i \cdot \beta_i$ (not to be confused with the common factor f_t discussed before) can be evaluated as a test of dynamic misspecification, which is discussed in Blundell and Bond (2000) in a comparison of production function estimates based on alternative (pooled) panel estimators.

Equation (7) can also be related to an error correction model (ECM) that underlies the Pooled Mean Group (PMG) estimator (Pesaran et al. 1999). In this panel model, the intercepts, short-run coefficients, and error variances differ freely across countries, while the long-run parameter is restricted to be the same (pooled) across countries. The ECM is derived by subtracting z_{it-1} from both sides and by adding and subtracting $\beta_{i2}x_{it}$ on the right-hand side. Collecting terms then gives

(8) $$\Delta z_{it} = (\gamma - 1) z_{it-1} + (\beta_{i1} + \beta_{i2}) x_{it} - \beta_{i2}\Delta x_{it} + \mu_i + e_{it},$$

which leads to the standard ECM representation

(9) $$\Delta z_{it} = -\phi(z_{it-1} - \theta x_{it}) - \beta_{i2}\Delta x_{it} + \mu_i + e_{it},$$

where the short-run error-correcting dynamics is captured by the parameter $\phi = 1 - \gamma$. The long-run income effect can be recovered from a PMG estimation of equation (9) as an estimate of

$$(10) \qquad \theta = \left(\bar{\beta}_{i1} + \bar{\beta}_{i2}\right) / \left(1-\gamma\right) = \bar{\beta}_i ,$$

which can be directly compared with other estimates of the long-run coefficient of interest from pooled and heterogeneous parameter models.

4. Data, samples, and instrumental variables

Data and samples

All our estimates of the effect of income on a transition variable are based on the panel data that are used for Figure 1. Our raw data include observations for more than 130 countries in 1960-2012, though with gaps since not all included countries existed in all of the sample years, and for some countries observations are missing for some sample years.

Figure 1 uses five-year averages of the available annual data, which gives us a maximum of eleven five-year periods in 1960-2012 (with the last period based on three years). Countries with less than one million inhabitants in 2010 and countries with oil production as the dominant industry are excluded.

Income per capita (in logs) is our main explanatory variable, taken from PWT 8.0 (Feenstra et al. 2013). In our sample with 5-year averaged data, income varies by a factor of more than 200 across countries and time. As is highlighted by Figure 1, this variation in income is used to explain the fall in the value added share of agriculture in GDP (*agriculture*) (World Bank 2013) and the rise of the degree of democracy (*democracy*) (Center for Systemic Peace 2013). We refer to these two trends as the agricultural and the democratic transitions.

The two transition variables *agriculture* and *democracy* are bounded from above and below. Hence these variables can be normalized to an index with scale [0, 1], which turns them into a proportional dependent variable. A linear regression of a (bounded) proportional dependent variable on income will lead to inconsistent predicted values of the transition variable in the presence of steady income growth. This problem is addressed by a logit transformation of the transition variables (Baum 2008).

A logit transformation implies that index values of 0 and 1 are returned as missing values; hence such observations will be dropped from the sample. With regard to the estimation of socioeconomic transitions, this does not appear to be a problem. E.g., countries without agriculture or full dictatorships or full democracies should indeed be excluded from the samples, as is guaranteed by the logit transformation.

Further limitations of the samples result from the data demands of some of the estimators and of some of the test statistics to be used below. For instance, the Pesaran CD-test (Pesaran 2004) of cross-section dependence requires a sufficient number of overlapping time series observations of all sample countries, though it allows for gaps in the time series observations. But gaps in the time series observations are not allowed in case of estimation in first differences, which is implicit with the PMG and the AMG estimator. In addition, the AMG estimator was found to run on balanced samples only.

For direct comparability of all panel models considered in the next section, each transition is estimated on a fixed balanced sample, which necessarily has fewer countries and a shorter time dimension than is available in the raw data used in Figure 1. For instance, the available time series observations for all rich countries for *agriculture* do not start before 1970, and for many countries the series ends in 2009. Hence using 1960 as the initial year (or 2012 as the final year) to maximize the time dimension would confine the sample to a small number of countries. To keep a balance between the number of sample countries and sample years, 1970-2010 is used as the default time dimension for a balanced panel, which implicitly determines the number of countries to be included.

Instrumental variables

The instrumental variables used to identify the presumed causal effect of income on transition variables measure alternative geographical properties of countries which are correlated with but are independent from present income levels. So the minimum conditions of instrument relevance and exogeneity appear to hold. But these geography-based variables are of course only available for a single cross section of countries, and one may question whether they can actually identify exogenous variation in income that is related to the transition variable, which remains an untestable assumption.[9]

We motivate our identification strategy with reference to unified growth theory (Galor 2005), which attempts to integrate into one consistent theory, the Malthusian era, before the Industrial Revolution and the subsequent period of modern economic growth. Unified growth theory claims that persistent development becomes inevitable once technological change starts in prehistoric times, and human capital is accumulated until a critical mass is reached that allows the economy to take off from Malthusian stagnation to a modern growth regime.

9. The alternative assumption is that the instrument picks up exogenous variation in a third variable that affects both income and the transition variable.

According to this theory, geographical conditions that have largely remained unchanged over more than one millennium may explain why some regions developed stable agrarian societies earlier than others. If stable agrarian societies are a precondition for accumulating a critical mass of human capital, it would become apparent why these societies became rich earlier than others.[10] In turn, such a sequence of different stages of development would explain an indirect effect of unchanged geographic conditions on modern levels of per capita income, in line with the observed correlation in the data.

Along these lines, the number of frost days per winter (*frost*) (Masters and McMillan 2001) may affect the productivity of agriculture and hence the potential to generate surplus leading initially to population growth and later to human capital accumulation. The potential for malaria transmission (*maleco*) (Kiszewski and Sachs et al. 2004) may affect the accumulation of human capital through various channels, and the proportion of a country that is close to the open sea (*coast*) (McArthur and Sachs 2001) may affect the possibilities for trade and hence technology import, which in turn is held to be a key determinant of human capital accumulation (Lucas 2009).

This line of reasoning does not guarantee that our instruments identify exogenous variation in income and not in some other determinant of long-run development, such as institutions. This possibility is discussed in Gundlach and Paldam (2009b), where an alternative set of instruments is used based on pre-historic bio-geographic conditions coded by Olsson and Hibbs (2005). Our results in the next section are only marginally affected by using this alternative set of bio-geographic instrumental variables.

Detailed definitions and sources of all variables used in the regression analyses of the next section are given in Table A1 in the appendix. All estimates are done with Stata 13. Stata code, data files, and detailed results are available upon request.

In the following, we first try to establish causality with IV estimation, which implicitly controls for omitted variables and other econometric problems. One remaining problem of this approach is that it only uses the cross-country variation to estimate income effects, just because reasonable external instruments with a time dimension are missing. That is, an effective IV strategy will address some estimation problems, but it may produce upwardly biased estimates since it cannot control for country heterogeneity and the presumed spillover effects of common shocks.

10. See Boserup (1965) for the role of agriculture in long-run development.

Then alternative panel estimators are used in an attempt to replicate the cross-section estimates, which should work in the absence of spillover effects. The general result is that this does not work out. Our interpretation is that standard panel estimators (POLS, 2FE, BB) are too restrictive in the sense that they do not allow for county-specific transition functions and for county-specific effects of common shocks. Based on more flexible mean group estimators, it is found that the agricultural and the democratic transitions are driven by a common dynamic process, while income as such apparently does not directly affect the democratic transition.[11]

5. Empirical results

Our empirical results for the agricultural and the democratic transitions are based on a benchmark cross-country model and two sets of panel models, namely "pooled" and "heterogeneous" parameter models. A common feature of the pooled models is that the within-effects of the explanatory variable *income* are restricted to be the same for all countries, while the heterogeneous models allow for country-specific income effects. The latter are not to be confused with level effects, which are captured by country-fixed effects in both types of panel estimators.[12] Table A2 in the appendix gives an overview of the differences between pooled and heterogeneous parameter models with regard to fixed effects and the modeling of unobservables, as discussed in Section 3.

The agricultural transition: Pooled parameter models of Table 1

The first column of Table 1 reports an estimate of the agricultural transition on the basis of instrumented time-averaged OLS (BE-IV), which serves as our benchmark estimate of the long-run relation.[13] The long-run income coefficient is statistically significant (robust standard errors in parentheses), and it implies that a 1% increase in per capita income is associated with a 0.86% decline in the logit-transformed value added share of agriculture in GDP, which translates into a 0.7% decline in the untransformed value added share of agriculture.

11. This result is in line with the findings of a recent extreme bounds analysis by Gassebner et al. (2012), who report a positive long-run income effect on the persistence but not on the onset of democracy.
12. In the context of production functions, "technology heterogeneity" refers to country-specific parameters (production elasticities), and "technology differences" refer to differences in the level of technology that are captured by country-fixed effects.
13. The IV regressions are implemented with the Stata module `ivreg2` (Baum et al. 2010).

The first-stage test statistics reject the null of weak instruments (Partial R^2) for a test size slightly larger than 10% (*F*-statistic; the critical value for a 10% test size is 22.3) and do not reject the null of (conditional) instrument exogeneity (Sargan *p*-value). The IV estimate of the income effect does not statistically significantly differ from the OLS estimate of the income effect (Hausman *p*-value), which indicates that potential reverse causality cannot have a large effect on the estimated income effect.

The question is whether the favorable cross-sectional statistical evidence for a long-run income effect, which is based on a static model like equation (1), also shows up in the panel estimates, which are based on dynamic models like equation (7) that differentiate between short-run and long-run effects.

The estimates reported in the second and the third columns set a reasonable range of the panel estimates of the agricultural transition. Due to the inclusion of the lagged endogenous variable in the dynamic panel model, pooled OLS (POLS) and two-way fixed effects (2FE) are known to produce biased effects, though in different directions. This suggests that the true income effect is expected to be somewhere within the range of -0.98 and -0.69, which also supports the BE-IV estimate. However, some diagnostic tests of the residuals point to possible misspecification problems. For instance, the null of the common factor restriction (see Section 3) is rejected (*p*-value) [14] and the null of cross-section independence of the residuals (CD *p*-value) is also rejected.[15] By contrast, the null of nonstationary residuals is rejected (CIPS *p*-value) for both POLS and 2FE, which can be considered as a rejection of the null of no cointegration and hence as a rejection of a possible reverse causality bias.[16]

The latter also holds for the System-GM (BB) estimator, where the instrument count has been confined to a number close to the number of cross-section observations (countries). The statistically significant income effect lies within the expected range and the null of the common factor restriction is not rejected, but the null of cross-section independence of the residuals is again rejected. The standard BB test statistics reject the null of second order autocorrelation of the residuals and do not reject the null of instrument exogeneity for the instruments in levels and in first differences (Hansen *p*-values), but these tests do not speak to the crucial moment condition of the independence of the instruments in differences from the country fixed effects, which is unlikely to hold. If the moment condition does not hold,

14. The test of the common factor restriction is implemented with the Stata module `md_ar1` (Söderbom (2009).
15. The Pesaran CD-test is implemented with the Stata modul `xtcd` (Eberhardt 2011).
16. The Correlated-Im-Pesaran-Shin (CIPS) unit root test is implemented with the Stata module `pescadf` (Lewandowski 2007).

System-GMM does not provide an advantage over Difference-GMM (AB, not shown), which in turn is known to suffer from a weak instrument problem in cross-country panel data.

The common correlated effects pooled (CCEP) estimator (Pesaran 2006) aims to control for possible cross-section dependence of the residuals but maintains the pooling restriction of identical within-income effects across countries. Here the common shocks, modeled as unobservables that are proxied by cross-section averages of all dependent and independent variables, are allowed to have different effects across countries, while using time dummies to proxy for common shocks would imply identical effects across countries. The estimated income coefficient is smaller (in absolute value) than expected from the POLS-2FE interval, and the common factor restriction does not hold. Somewhat surprisingly, the null of cross-section independence of the residuals is also rejected, which could reflect that the pooling restriction on the income coefficient is not appropriate.

The pooled mean group (PMG) estimator (Pesaran et al. 1999) is an intermediate estimator between two extreme panel estimation techniques, namely *pooling* and *averaging*. The 2FE estimator is the workhorse model of estimation by pooling. Here the intercepts are allowed to differ across countries (country-fixed effects), but all other coefficients and error variances are constrained to be the same across countries. By contrast, the mean group (MG) estimator (Pesaran and Smith 1995, see below) allows *all* parameters to vary across groups, not only the intercepts. By running individual regression equations for each group (country), the MG estimator proceeds by averaging the estimated individual time series coefficients.

The PMG estimator tries to fill the gap between estimation by pooling and averaging. It allows the intercepts, short-run coefficients, and error variances to differ freely across groups (countries), but the long-run coefficients are constrained to be the same (pooled). Applied to our transition regressions, the PMG estimator allows that each country's transition variable may react differently to all sorts of shocks in the short run or along the adjustment to a common steady state path, but the steady state path itself is held to be governed by parameters that are common to all countries.

The PMG estimator includes country-fixed effects since it allows for cross-country differences in the intercepts. Time-fixed effects are not included because the underlying model is estimated for each country separately to allow for heterogeneous short-run parameters.[17] We find a larger (in absolute value) income effect than expected from the POLS-2FE interval. The error correction term is negative as predicted and statistically

17. The PMG regressions are implemented with the Stata module `xtpmg` (Blackburne and Frank 2007).

significant, but the residual diagnostics are poor: the null of non-stationary residuals is not rejected, while the null of cross-section independence of the residuals is rejected.[18] This disappointing result could reflect that allowing only for some parameter heterogeneity may not be enough to address the individual cross-country differences in the transition process.

The agricultural transition: Heterogeneous parameter models of Table 2

Table 2 reports the results for estimates of the agricultural transition that are based on heterogeneous parameter models. All estimators allow for country-specific income effects (which are reported as unweighted averages) but differ with respect to the modeling of common shocks and cross-sectional independence of the residuals (see again Table A2 for an overview). Three variants are considered.[19]

The mean group (MG) estimator (Pesaran and Smith 1995) does not explicitly control for cross-sectional correlation but can be estimated on cross-sectionally demeaned data (CD-MG), which imposes the restriction that a common shock has the same effect in each country. The common correlated effects mean group (CMG) estimator (Pesaran 2006) allows for unobserved country-specific effects but treats them as "nuisance parameters" that cannot be interpreted (see Section 3). The augmented common correlated effects mean group (AMG) estimator (Bond and Eberhardt 2013) goes one step further by explicitly identifying a common dynamic process (see also Section 3). All three MG estimators allow for the inclusion of a country-specific time trend (T), which is meant to capture the individual effect of an omitted variable that evolves in a linear fashion (like human capital).

A general result of Table 2 is that the income effects derived with heterogeneous panel models are substantially smaller (in absolute value) than the income effects derived with the pooled parameter models of Table 1. In two cases (MG-T and CMG), the income effects are only marginally statistically significant. The common factor restriction is rejected for all models, which speaks against the underlying AR1 specification. Otherwise, the two AMG models that directly control for common shocks perform best in terms of residual diagnostics. Since the inclusion of a linear trend is not supported by the results, our preferred estimate of the agricultural transition is based on the AMG estimator in the fifth column. The estimated income effect is only about one third or one half as large as suggested by the POLS-2FE interval estimates reported in Table 1. However, even a zero income effect would not imply

18. The test of the common factor restriction is not applicable because the estimated equation deviates from the required AR1 specification.

19. All mean group regressions are implemented with the Stata module `xtmg` (Eberhardt 2012).

the absence of an agricultural transition in the presence of a statistically significant effect of a common dynamic process.

Figure 2 visualizes the evolution of the common dynamic process that drives the agricultural transition. The negative slope explains why Table 2 reports a positive correlation between the common dynamic process and the (logit transformed) value added share of agriculture. As it stands, the estimated common dynamic process looks like a summary of the arbitrarily selected country-specific correlations between income and the value added share of agriculture that are highlighted in the upper panel of Figure 1.

The democratic transition: Pooled parameter models of Table 3

Table 3 and Table 4 report estimation results for the democratic transition which are based on the same specifications of the regression equations as the ones in Tables 1 and 2.

The estimates for BE-IV in Table 3 are in line with our previous results on the effect of income on the degree of democracy (Gundlach and Paldam 2009b), which were based on an untransformed democracy index, a different cross-section sample, and different instrumental variables. The estimated income effect is statistically significant and implies that a 1% increase of per capita income is associated with a 0.72% increase in the logit transformed PolityIV index of democracy, which translates into a 0.67% change in the normalized [0,1] index of democracy. Some reservations about the robustness of this result remain because the null of (conditional) instrument exogeneity is almost rejected at the 5% level (Sargan *p*-value) and the null of weak instruments cannot be rejected for a 10% test size (*F*-statistic; the critical value for a 10% test size is 22.3).

The results for the pooled parameter models confirm that the evidence for a statistically significant positive effect of income on democracy is not overly convincing.[20] For instance, the 2FE estimates replicate the result by Acemoglu et al. (2008) that there is no statistically significant income effect on democracy. The 2FE and the (also insignificant) BB result are backed by favorable residual diagnostics: the null hypotheses of the common factor restriction and cross-section independence of the residuals are not rejected, while the null of non-stationary residuals is rejected. However, absence of evidence for an income effect does not necessarily imply that there is no common pattern of a transition from non-democratic to

20. Brückner and Ciccone (2011) claim a "window of opportunity" for democratic change after a *negative* income shock for a sample of sub-Saharan African countries. However, their study does not consider a lagged endogenous variable in its estimation equation and may, therefore, suffer from misspecified dynamics. Brückner and Ciccone (2011) may actually estimate the short-run trigger rather than the long-run cause of a change in the political regime. For negative income effects that trigger a change in the political system toward democracy, see also Burke and Leigh (2010) and Dorsch et al. (2015), who focus on a sample of non-democracies.

democratic political systems with rising levels of development. Statistically significant positive income effects are found for POLS, which ignores country-fixed effects, and for CCEP, which suffers from inconsistent residual diagnostics (rejection of cross-section independence).

The PMG results reveal a statistically significant error correction term but a rather small and statistically insignificant income effect; the null of cross-section independence of the residuals is rejected. Fayed et al. (2012) report a negative income effect on democracy derived with the PMG estimator once they include cross-sectional averages of income and of the democracy score as additional regressors to control for cross-sectionally dependent residuals. As Fayed et al. (2012) point out, a negative PMG income coefficient would imply a steady state relation that produces a widening gap between income and democracy. In our reading, such a result is in conflict with the stylized fact that whereas all countries started as non-democracies when they were poor more than 200 years ago, almost all countries that have become rich also have become democracies ever since, apart from the notable exception of countries with oil production as the dominant industry.

The democratic transition: Heterogeneous parameter models of Table 4

Turning to the heterogeneous parameter models in Table 4, we do not find any statistically significant income effects on the degree of democracy. For all models, the residual diagnostics are favorable in the sense that they do not reject the null of the common factor restriction but they do reject the null of non-stationary residuals, which is required for a cointegrating relation. However, the null of cross-section independence of the residuals is rejected with the exception of the AMG estimator in the fifth column. Hence as in Table 2 for the agricultural transition, this is again our preferred estimator. As before, the AMG estimator confirms a common dynamic process as a statistically significant driver of the transition from authoritarian to democratic political systems.

Figure 3 reveals that the evolution of the common dynamic process that drives the democratic transition also looks like a summary of the arbitrarily selected country-specific correlations between income and democracy that are highlighted in the lower panel of Figure 1. Here the slope is positive but less linear as in the case of the agricultural transition. The non-linearity underlines that the democracy index may be best described as a jump variable (Paldam and Gundlach 2016). The non-linearity may also explain why it has been difficult to estimate statistically significant income effects with linear regression equations. In any case, the absence of a statistically significant income effect does not imply that there is no democratic transition: our results suggest that the democratic transition is driven by a dynamic process that is shared among countries and apparently correlated with the *average* level of per capita income.

The democratic transition: Are the techniques appropriate for the problem at hand?
The alternative estimation approaches employed above are an attempt to try a full spectrum of regression techniques on two transitions. The results are rather clear and consistent for the agricultural transition, but less so for the democratic transition. The latter may be due to the particular structure of the political data that have been used (Polity IV). They measure the character and stability of political systems. Perhaps the dynamics of political systems differ fundamentally from the dynamics of economic systems? In particular, it should be noted that the pooled Polity index is stable for an average period of no less than one decade. However, the average stability period comes with a large standard deviation due to the discrete jumps that interrupt the stability. In combination, these features of the data may limit what can be identified with linear regression techniques.

An alternative approach may recognize that political systems are stepwise stable and that there is a difference between the triggering events that generate a change of the political system and the change itself, which is a system jump (Paldam and Gundlach 2016). While the triggering events are largely random, most system jumps are found to be in the direction of the transition path indicated by Figure 3. How such a two-step process can be caught by the regression techniques used above is not obvious, but perhaps this explains why the coefficient of the common dynamic process is found to be statistically significant while the income coefficient is found to be statistically insignificant.

This leaves a difficult question for further research. Spillover effects and short-run dynamics are obviously important determinants of socioeconomic transitions, which can be handled by the estimators used in the present paper. But it is not clear whether these (and other linear) estimators can handle processes with stepwise stability and discrete jumps, which are typical for political systems.

6. A common pattern of socio-economic transitions

Is it possible to demonstrate a general pattern of socio-economic transitions that holds both across countries and over time? Our answer is a conditional yes.

The long-run results are rather clear. We use instrumented time-averaged OLS (BE-IV) estimates as our point of reference for a long-run income effect. Our IV results suggest that the observed correlations can be interpreted as long-run causal relations from income to the transition variables. The general pattern is also visible in the raw data shown in Figure 1. This is our *grand transition* hypothesis, which we want to replicate with the dynamic panel estimates that include long-run and short-run effects. Essentially our paper is about aggregation over time – something that is known to be difficult, so more than tentative replications of the cross-section estimates with the dynamic panel estimates cannot be expected.

The pure cross-section IV specification may suffer from ignoring dynamic adjustment processes and cross-country spillover effects, which is likely to lead to upwardly biased income effects. Panel specifications can control for country- and time-fixed effects and allow for some modeling of dynamic adjustment processes. A dynamic panel model (AR1) may guard against not rejecting a false positive (spurious correlation). However, standard panel estimators employ a number of implicit restrictions which increase the probability of not rejecting a false negative (missing the signal in the noise).

Avoiding the acceptance of spurious correlations is like using the weed killer Roundup. One wants to rid the garden of weeds, but also to preserve the flowers. If the dose of Roundup is too strong, the outcome will be a garden without weeds but also without flowers. So rather than applying a generous dose of Roundup in the form of panel estimators that do not allow for individual country effects and cross-country spillovers, we consider more flexible (mean group) panel estimators that help us to identify a common signal in the noise.

Our preferred results suggest that the agricultural and the democratic transitions are each driven by common dynamic processes and not necessarily by the level of per capita income per se, notwithstanding a close correlation between the common dynamic processes and the average level of per capita income over time.

But before one draws overly ambitious conclusions, it may be worthwhile to consider a number of robustness tests that are left for further research. For instance, one may include other socioeconomic transitions that have been debated in development economics, such as the demographic and the human capital transitions or trends in urbanization and female labor force participation, to see if a common dynamic process emerges from the data in these cases as well. One may also consider a generic dynamic panel model (instead of an AR1 model) in combination with 5-year periods (instead of annual data), which has been the standard panel specification in the empirical growth literature (but is less appropriate for mean group estimators). Especially with regard to the democratic transition, one may study the apparent non-linearity of the income-democracy relation in more detail. As always, some concerns about reverse causality remain in the context of mean group estimators, but diagnostic tests that point to stationary residuals indicate that this cannot be a large problem.

If we take our present results at face value, it looks that they are compatible with all three hypotheses mentioned in the introduction. Maybe this is actually the main message of our paper. For instance, our own *grand transition* hypothesis can be defended by pointing to the correlation between average per capita income and the estimated common dynamic process, in the sense that our cross-section estimates use per capita income as a proxy for the true drivers of the transitions that are common to all countries in the process of development. The hypothesis of *transformation trajectories in functional space* accepts that there is a systematic pattern in the data, but it is agnostic with regard to the direction of causality, which is not directly addressed by our preferred estimator (AMG).

Finally, the hypothesis of *critical junctures* assumes that the deep power structure of a society sets countries on an individual economic and political development path, which is compatible with our modeling of country-specific (heterogeneous) effects of rising income levels and of common dynamic shocks. However, the implied effects of "technology" heterogeneity and cross-section dependence have not explicitly been addressed in previous empirical studies of socioeconomic transitions. Our results show that there is an important signal in the data that has been overlooked up to now.

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Table A1. Definitions and sources of variables

1. Explanatory variable (*x*)

income	Natural logarithm of Gross Domestic Product per capita in international dollars, expenditure-side income data (PWT 8.0), annual data. Source: Feenstra et al. (2013).

2. Transition variables (*z*)

agriculture	Share of agricultural value added in Gross Domestic Product, in percent, annual data. Source: World Bank (2013), rescaled to [0, 1] and logit transformed.
democracy	Polity2 index of democracy, ranges from -10 (full autocracy) to +10 (full democracy), annual data. Source: Center for Systemic Peace (2013), rescaled to [0, 1] and logit transformed.

3. Instrumental variables

coast	Proportion of land area within 100 km of the sea coast [0, 1]. 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 [0, 1]. Source: Masters and McMillan (2001).
maleco	Index 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 [0, 31.5]. Source: Kiszewski and Sachs et al. (2004).

Table A2. Panel estimators compared

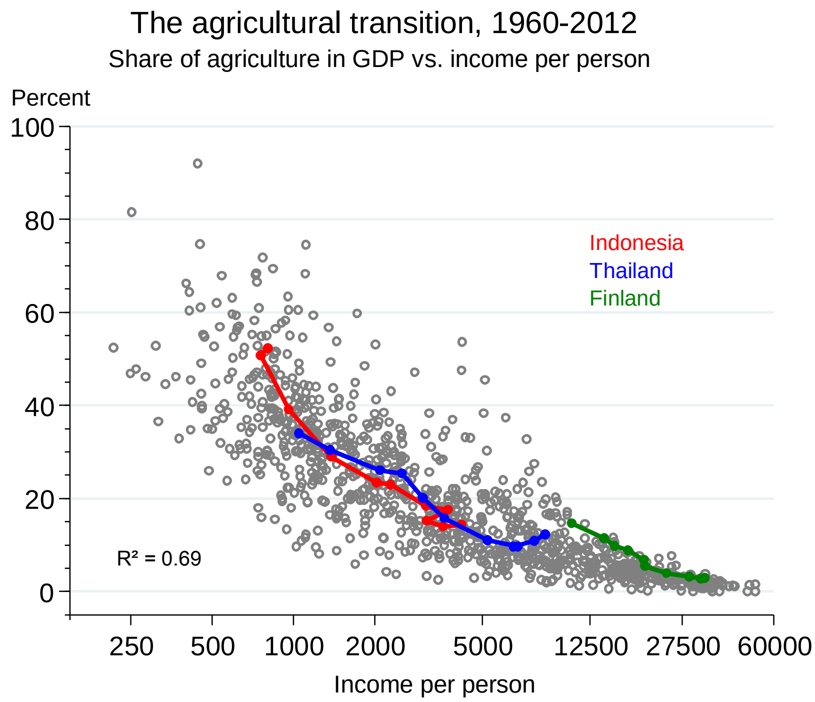
	Long-run slope coefficient	Fixed effects		Modeling of common shocks			Common dynamic process identified by:
					Evolution		
		Country	Time	Slope coefficient	No restriction	Linear	
				Pooled parameter models			
BE-IV	pooled	no	no				
POLS	pooled	no	yes	pooled	yes		
2FE	pooled	yes	yes	pooled	yes		
BB	pooled	implicit	implicit	pooled	yes		
CCEP	pooled	yes	no	heterogeneous	yes		
PMG	pooled	yes	no				error correction
				Heterogeneous parameter models			
MG-T	heterogeneous	individ.	no	heterogeneous		yes	
CD-MG	heterogeneous	individ.	implicit	pooled	yes		
CMG	heterogeneous	individ.	subst.	heterogeneous	yes		
CMG-T	heterogeneous	individ.	subst.	heterogeneous		yes	
AMG	heterogeneous	individ.	subst.	heterogeneous	yes		additional regressor
AMG-T	heterogeneous	individ.	subst.	heterogeneous	-	yes	additional regressor

Notes:
"implicit" indicates that country-fixed effects are eliminated by first differences and time-fixed effects are eliminated by cross-section demeaning. "individ." indicates that country-fixed effects are handled by individual estimates for each sample country. "subst." indicates that time-fixed effects are substituted by cross-section averages of the dependent and independent variables.

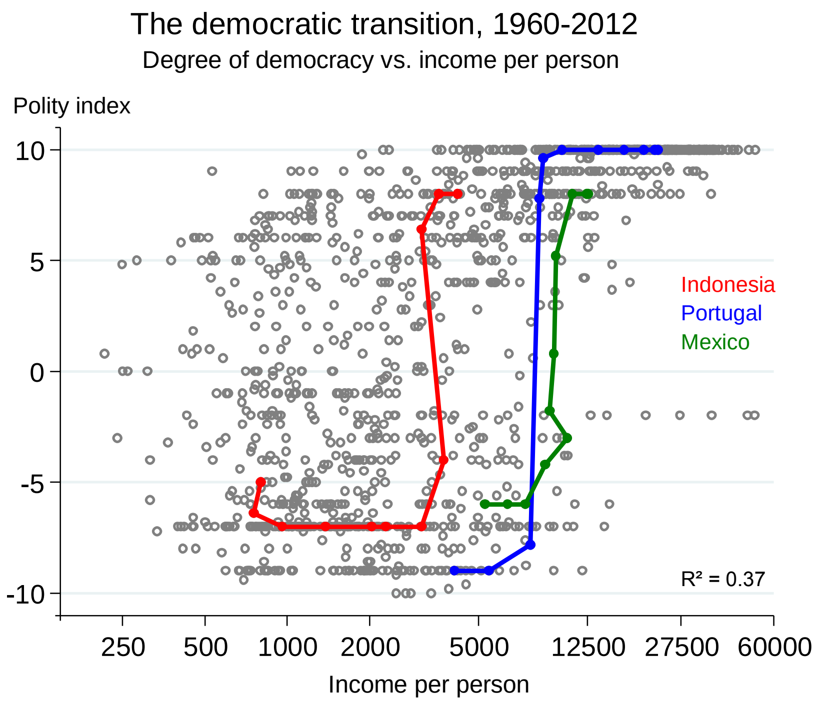
Pooled parameter models.
BE-IV: Instrumented Between Effects estimator (instrumented time-averaged OLS estimator).
POLS: Pooled OLS estimator.
2FE: Two-way Fixed Effects estimator.
BB: System GMM (Blundell-Bond) estimator.
CCEP: Common Correlated Effects Pooled estimator.
PMG: Pooled Mean Group estimator.

Heterogeneous parameter models.
MG-T: Mean Group estimator with linear time Trend.
CD-MG: Cross-sectionally Demeaned Mean Group estimator.
CMG(-T): Common correlated effects Mean Group estimator (with linear time Trend).
AMG(-T): Augmented common correlated effects Mean Group estimator (with linear time Trend).

Figure 1. Socioeconomic transitions, 1960-2012

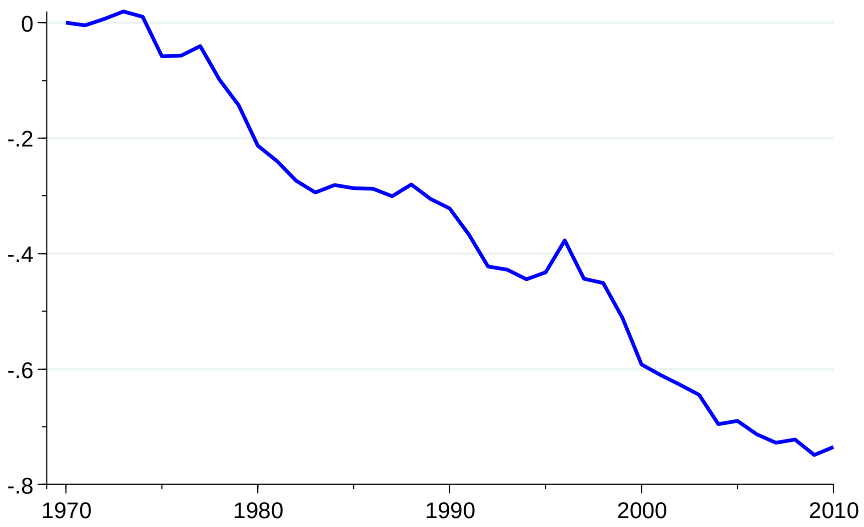


Share of agriculture in percent; income in international $ (log scale).
131 countries, eleven 5-year averaged periods, 1048 country-period observations.
Sources: WDI and PWT 8.0; own calculations.



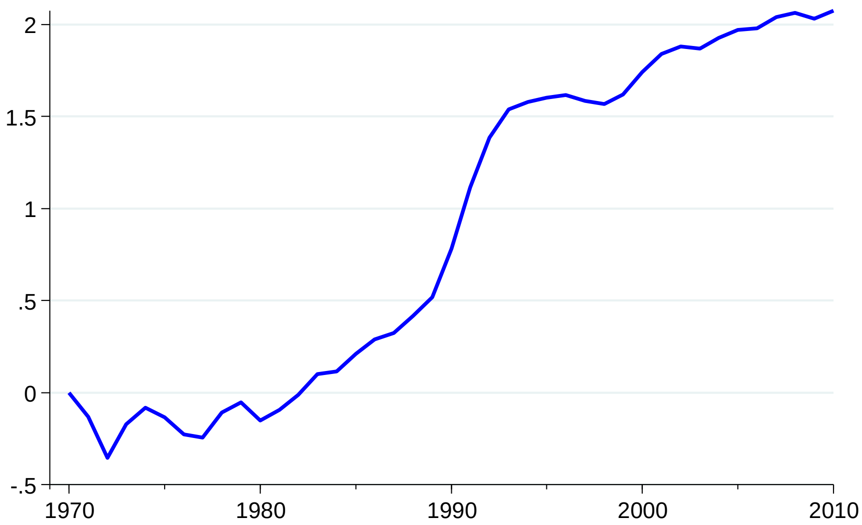
Degree of democracy measured by Polity IV index; income in international $ (log scale).
132 countries, eleven 5-year averaged periods, 1244 country-period observations.
Sources: Center for Systemic Peace and PWT 8.0; own calculations.

Figure 2. The evolution of the common dynamic process of the agricultural transition



Based on year dummy coefficients of an estimate of the agricultural transition regression in first differences.

Figure 3. The evolution of the common dynamic process of the democratic transition



Based on year dummy coefficients of an estimate of the democratic transition regression in first differences.

Table 1. The agricultural transition: pooled parameter models

	BE-IV	POLS	2FE	BB	CCEP	PMG
Income per person	-0.86*	-0.98*	-0.69*	-0.85*	-0.51*	-1.22*
	(0.07)	(0.09)	(0.15)	(0.11)	(0.16)	(0.05)
Time dummies	No	Yes	Yes	Yes	No	No
Observations	50	2000	2000	2000	2000	1950
Countries	50	50	50	50	50	50
RMSE	0.41	0.16	0.15	0.17	0.14	0.14
Common factor restriction (*p*-val.)		0.00	0.00	0.11	0.03	
Non-stat. residuals (CIPS *p*-val.)		0.00	0.00	0.00	0.00	0.75
Cross-sec. independ. (CD *p*-val.)		0.03	0.01	0.02	0.00	0.00
Instruments	*coast, frost, maleco*					
Partial R^2	0.59					
Sargan-*p*	0.18					
1st-stage *F*-stat.	21.91					
Instrument count				54		
AR1-*p*				0.00		
AR2-*p*				0.42		
Hansen-*p*				0.38		
Diff.-Hansen-*p*				0.52		
Error corr. term						-0.15
p-value						0.00
Hausman-*p*	0.91					

Notes:
Balanced cross-country time series data, 1970-2010. OPEC members and countries with a population of less than 1 million persons in 2010 excluded. All estimates based on dynamic model, except BE-IV estimates. Reported coefficients are long-run effects of the explanatory variable (in bold); robust standard errors in parentheses. BB estimates with restricted instrument count.

Table 2. The agricultural transition: heterogeneous parameter models

	MG-T	CD-MG	CMG	CMG-T	AMG	AMG-T
Income per person	-0.22 (0.13)	-0.53* (0.17)	-0.31* (0.15)	-0.22 (0.12)	-0.32* (0.14)	-0.25* (0.13)
Country trend	-0.01* (0.00)			0.00 (0.00)		0.00 (0.00)
Common dynamic process					0.33* (0.08)	0.48* (0.10)
Observations	2000	2000	2000	2000	2000	2000
Countries	50	50	50	50	50	50
RMSE	0.13	0.13	0.12	0.12	0.13	0.12
Common factor restriction (*p*-val.)	0.01	0.00	0.01	0.02	0.00	0.01
Non-stat. residuals (CIPS *p*-value)	0.00	0.00	0.00	0.00	0.00	0.00
Cross-sec. independ. (CD *p*-value)	0.00	0.09	0.01	0.01	0.96	0.52

Notes:
Balanced cross-country time series data, 1970-2010. OPEC members and countries with a population of less than 1 million persons in 2010 excluded. All estimates based on dynamic model. CD-MG based on cross-sectionally demeaned variables. Reported coefficients are long-run effects of the explanatory variable (in bold); robust standard errors in parentheses.

Table 3. The democratic transition: pooled parameter models

	BE-IV	POLS	2FE	BB	CCEP	PMG
Income per person	0.72* (0.22)	0.64* (0.15)	-0.46 (0.32)	0.17 (0.43)	0.42* (0.21)	0.06 (0.08)
Time dummies	No	Yes	Yes	Yes	No	No
Observations	62	2480	2480	2480	2480	2418
Countries	62	62	62	62	62	62
RMSE	1.00	0.51	0.50	0.53	0.47	0.50
Common factor restriction (*p*-val.)		0.00	0.30	0.73	0.04	
Non-stat. residuals (CIPS *p*-val.)		0.00	0.00	0.00	0.00	0.00
Cross-sec. independ. (CD *p*-val.)		0.23	0.16	0.54	0.00	0.00
Instruments	*coast, frost, maleco*					
Partial R^2	0.43					
Sargan-*p*	0.07					
1st-stage *F*-stat.	14.82					
Instrument count				54		
AR1-*p*				0.00		
AR2-*p*				0.78		
Hansen-*p*				0.11		
Diff.-Hansen-*p*				0.51		
Error corr. term						-0.10
p-value						0.00
Hausman-*p*	0.94					

Notes:
Balanced cross-country time series data, 1970-2010. OPEC members and countries with a population of less than 1 million persons in 2010 excluded. All estimates based on dynamic model, except BE-IV estimates. Reported coefficients are long-run effects of the explanatory variable (in bold); robust standard errors in parentheses. BB estimates with restricted instrument count.

Table 4. The democratic transition: heterogeneous parameter models

	MG-T	**CD-MG**	**CMG**	**CMG-T**	**AMG**	**AMG-T**
Income per person	-0.39 (0.88)	-0.52 (0.53)	0.25 (0.64)	0.26 (0.70)	0.05 (0.42)	0.22 (0.67)
Country trend	0.02* (0.01)			-0.01 (0.01)		-0.01 (0.01)
Common dynamic process					0.37* (0.07)	0.54* (0.13)
Observations	2480	2480	2480	2480	2480	2480
Countries	62	62	62	62	62	62
RMSE	0.46	0.47	0.41	0.40	0.45	0.43
Common factor restriction (*p*-val.)	0.42	0.28	0.98	0.68	0.59	0.55
Non-stat. residuals (CIPS *p*-value)	0.00	0.00	0.00	0.00	0.00	0.00
Cross-sec. independ. (CD *p*-value)	0.00	0.01	0.02	0.02	0.37	0.08

Notes:
Balanced cross-country time series data, 1970-2010. OPEC members and countries with a population of less than 1 million persons in 2010 excluded. All estimates based on dynamic model. CD-MG based on cross-sectionally demeaned variables. Reported coefficients are long-run effects of the explanatory variable (in bold); robust standard errors in parentheses.

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