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# The agricultural and the democratic transitions. Causality and the Roundup model

## by Erich Gundlach and Martin Paldam

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**Abstract:**
Long-run development (in income) causes a large fall in the share of agriculture commonly known as the agricultural transition. We confirm that this conventional wisdom is strongly supported by the data. Long-run development (in income) also causes a large increase in democracy known as the democratic transition. Elsewhere we have shown that it is almost as strong as the agricultural transition. Recently, a method has been presented to weed out spuriousness. It makes the democratic transition go away by turning income insignificant, when it is supplemented by a set of formal controls. We show that the same method makes the agricultural transition go away as well. Hence, it seems to be a method that kills far too much, as suggested by the subtitle. This suggestion leads to a discussion of the very meaning of long-run causality.

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# 1. Introduction

It appears that nobody doubts the *agricultural transition*:[1] In low income countries (LIC), the share of the agricultural sector is large; as countries grow into middle income countries (MIC), the share falls; and it becomes small in wealthy developed countries (DC). This is sketched on Figure 1a. It has been demonstrated in cross-country studies and in time series of many countries. We believe that we have a good understanding of the main mechanics of the fall – though of course there are still many variants of the process in need of explanation.

The agricultural transition is the least controversial of the many transitions that take place during development. The demographic, the urban and the human capital transitions are almost as uncontroversial. The *democratic transition* is also well-known, though the data measuring democracy are more difficult to handle,[2] and the weight of other variables in the relation is larger, etc. However, most would agree that it looks as depicted on Figure 1b.

Figure 1. The stylized story of the two transitions

| 1a. The agricultural transition | 1b. The democratic transition |
|---|---|
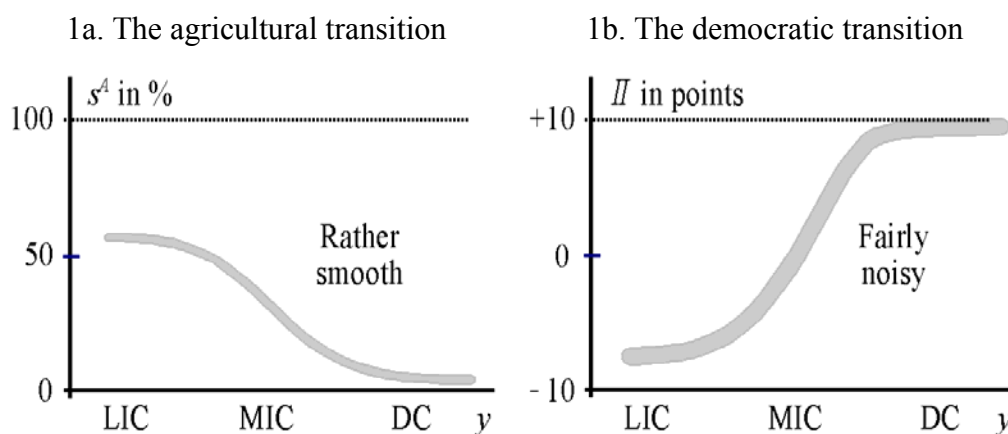


Note:    The horizontal axis is the income axis, where income is the logarithm to GDP per capita. $s^A$ is the share of value added of agriculture in GDP, and $\Pi$ is the Polity Index.

Recently, Acemogly, Johnson, Robinson and Yared (2008) have used a method – the AJRY-method – that makes the democratic transition go away. It is done by showing that income becomes insignificant in a model explaining the standard democracy index, when three formal controls are added. Below we show that the method also makes the agricultural transition go away.

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1. In this paper the term *transition* is reserved for the changes – in a certain field – caused by the *Grand Transition*, where the country makes the full development from an LIC to become a DC.
2. The data for the share of agriculture and income have a similar statistical structure. However, the democracy indices have a different structure as discussed in Section 2.4.

Before we apply the method, Section 2 shows how strong conventional wisdom is in the field: The transition explains the share of agriculture rather well, and the main direction of long-run causality is clear. This is parallel to our findings about the democratic transition (Gundlach and Paldam, 2009a), though the fit in the latter case is a little weaker.

The AJRY-method is applied in Section 3, which turns income insignificant in a model explaining the share of agriculture, when the same three controls are added. We also replicate AJRY's result about the democratic transition. We could have continued to show that the method also makes other transitions go away. However, instead we step back and ask: Could it be that this model has the Roundup property of killing *all* long-run connections? Section 4 looks at the way the AJRY-method treats the information content in the data. Finally, Section 5 discusses the meaning of causality in transitions.

## 2.    Long-run causality in the agricultural transition

The agricultural transition deals with two variables: $s_{it}^A = Y_{it}^A / Y_{it}$ is the value added *share of agriculture* ($Y_{it}^A$) in GDP ($Y_{it}$), while *income* ($y_{it}$) is the natural logarithm to GDP per capita. The sources for these data are the homepages of WDI and Maddison, respectively. The theory says that the path of $s_{it}^A$ is a function of development for which the best proxy is $y_{it}$. The path looks as Figure 1a, where the country goes from being an LIC to becoming a DC.[3]

The purpose of this section is to demonstrate just how strong conventional wisdom is. To analyze the long-run causality, we use a method that was introduced in Gundlach and Paldam (2009a) to study the democratic transition. It instruments income with a set of *DP-variables* that gives the long-run *development potential* of countries. The DP-variables are so extreme that we can be sure that they are exogenous, and still they work. Section 2.1 explains these variables, and they are documented in the Appendix. Section 2.2 looks at a cross section of countries for a single year (1995). Here we conclude that causality is one way only. Section 2.3 shows that the DP-variables also work for all other years with sufficient variation in the cross-country observations in our sample, which is from about 1970 onwards.

### 2.1    The extreme DP-variables: A very long-run perspective

Many theories have been presented to suggest what causes long-run development, but few of these theories are open to rigorous empirical investigation.[4] Diamond (1997) inspired Hibbs and Olsson (2004, 2005) to compile an amazing set of biological and geographical DP-variables for various regions of the world, which covers 112 present-day countries.

The biological variables measure the conditions that prevailed at the time of the Neolithic Revolution about 10,000 years ago, with Europe as the most favorable region and Sub-Saharan Africa as the least favorable. One measure 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.

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3. Obviously, geographic factors also count, so that agricultural production, by necessity, was always relatively small in countries as Greenland and Hong Kong, where arable land in scarce.
4. 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).

The geographic variables measure the specific conditions that have constrained or enabled the spread of the Neolithic 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 as the relation between the east-west distance and the north-east distance (*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, such as belonging to Eurasia vs. being a small island (*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 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 effect 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.

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 them as instruments for modern income levels.

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 O. Galor (and various coauthors) attempts to integrate the pre-take-off period with modern economic growth into one consistent theory, see Galor (2005) for a survey. Unified growth theory 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 our instruments in our empirical specifications.


*2.2    Long-run causality: Using the DP-variables for a single cross-section of countries*
The Grand Transition view argues that the long-run time-series (t-s) pattern in the transition variable is the same as the cross-country (c-c) pattern. The reason is that the c-c income pattern reflects international differences in long-run growth rates, given that all countries

started at comparable income levels about 200 years ago. To compare the two patterns requires t-s data extending a couple of centuries or c-c data for countries throughout a broad income range. Such long t-s data rarely exists even for a handful of countries, but it is common that several handfuls of countries in the LIC-group can be included in the c-c-set. We thus base our test of long-run causality on c-c-data.

Table 1. The long-run effect of income on the share of agriculture

| Time $t$ is 1995 | Main model | Robustness of model to instrument variation | | | |
|---|---|---|---|---|---|
| Dependent variable: $s_{it}^A$ | (1) | (2) | (3) | (4) | (5) |
| No. of obs. (countries) | 97 | 102 | 97 | 97 | 135 |
| OLS estimates | | | | | |
| Income, $y_{it-1}$ | -10.82 (0.78) | -10.61 (0.72) | -10.82 (0.78) | -10.82 (0.78) | -11.03 (0.78) |
| Centered R² | 0.66 | 0.68 | 0.66 | 0.66 | 0.60 |
| IV estimates: $y$ is instrumented | | | | | |
| Income, $y_{it-1}$ | -9.16 (1.22) | -9.99 (0.99) | -8.91 (1.25) | -9.98 (1.09) | 10.64 (1.12) |
| 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 | 0.36 | 0.04 | 0.26 | 0.62 |
| Tests of validity of the IV-procedure | | | | | |
| First stage partial $R^2$ | 0.43 | 0.53 | 0.42 | 0.52 | 0.49 |
| Sargan test (p-value) | 0.71 | 0.32 | 0.28 | 0.15 | 0.03 |
| Cragg-Donald test for the strength of the instruments in the IV estimate | | | | | |
| Presumed causality: $y \Rightarrow s^A$ | 35.56 | 55.59 | 33.45 | 33.18 | 41.56 |
| CD critical value (size) | 19.93 | 19.93 | 19.93 | 22.30 | 22.30 |
| Reverse causality: $s^A \Rightarrow y$ | 12.18 | 23.55 | 11.17 | 13.64 | 18.01 |

Notes: All observations for income and the share of agriculture in GDP for 1995; standard errors in parentheses. All specifications include a constant term (not reported). A Cragg-Donald (CD) statistic *above* the critical value (10 percent maximal test size) indicates the rejection of weak instruments. The Sargan test for overidentification tests the joint null hypothesis that the instruments are valid and correctly excluded from the estimated equation.

Our causality test compares two c-c regressions for a single year: (R1) is an OLS-regression explaining the c-c pattern in the share of agriculture by the c-c pattern in income. (R2) is the same regression except that it is run in two stages, where the first stage instruments income

with the DP-variables. Thus, the first-stage estimate generates a set of incomes for the second stage regression that are unaffected by the present share of agriculture. The Hausman-test compares the coefficient to income in (R1) and (R2) to reveal if the potential endogeneity problem biases the OLS estimates.

Table 1 presents five regressions with different combinations of DP-variables – all are run for as many countries as possible. The bottom half of the table shows that the IV-regression makes sense. The Sargan test indicates that the DP-variables are correctly excluded from the estimation equation, except for column (5). The Cragg-Donald tests show that the DP-variables are strong instruments; i.e. the test rejects the null hypothesis of weak instruments by a wide margin.

Consequently we can directly compare the OLS with the IV estimate of the income effect: It is always rather close to −10, even in columns (1) and (3) where the Hausman test indicates a statistically significant difference between the OLS and the IV estimate. Note that the $R^2$-scores are about two thirds for a simple cross section with one explanatory variable, and that the standard errors of the income coefficients are rather small and imply a level of statistical significance higher than 1 percent in all five regression sets. The similarity of the OLS and IV estimates implies that it cannot be rejected that all causality is from income to the share of agriculture in the long run. So it is probably not surprising that the profession takes the agricultural transition to be uncontroversial.

To further analyze the causal direction, we run the reverse regression where the instrumented share of agriculture is used to explain the level of income. Hence, in this case (R1) would explain the c-c-pattern in income by the c-c pattern in the share of agriculture, and (R2) would be the two-stage regression where the first stage instruments the share of agriculture with the DP-variables. The bottom line of Table 1 shows that the DP-variables are weak instruments for the share of agriculture, except in column (2).

The two papers by Hibbs and Olsson differ in handling the difficult problem of the proper classification of the Neo-European countries – Australia, Canada, New Zealand, and the USA. In the sample of column (1) they are not included, but in the sample of column (2) they are included and coded with European biogeography, just because the European food and technology package was wholly transferred by the colonizers.[5] Relative to their income levels, these countries have a comparatively high share of agriculture that explains the cor-

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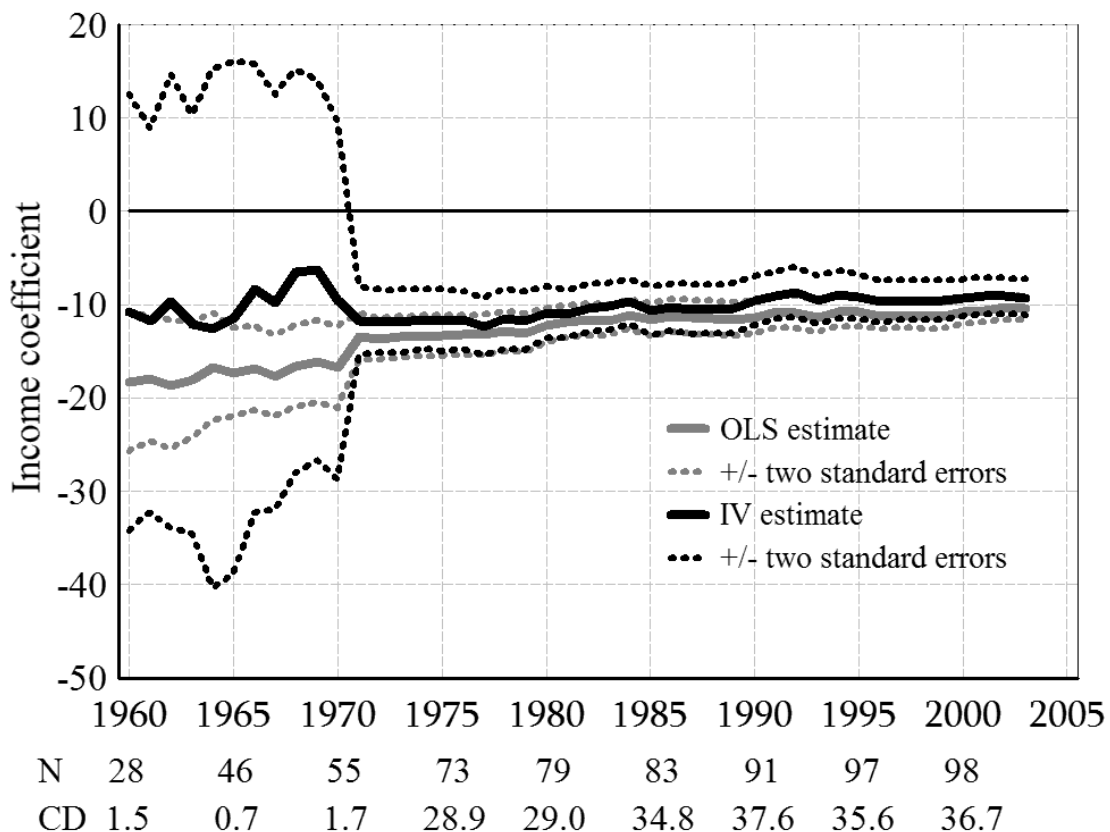5. Germany has also been added to this sample.

relation with the instruments *bioavg* and *geoavg* in column (2).[6] Fortunately the overall results do not depend upon the classification of these countries, as is shown by the results in the other columns: The main direction of causality is from income to the share of agriculture, anyhow.

*2.3    Long-run causality: Running model (1) from Table 1 for 1960-2003*

The test in Table 1 is for one year only. Figure 2 covers every year for which we have comparable cross-country data on the share of agriculture and income, namely 1960 to 2003. The graph shows the two estimated income coefficients (OLS and IV) for our main model (1) from Table 1, which appears to be the most parsimonious comprehensive specification. The graph reveals that the DP-instruments are strong only from the early 1970s when the data cover enough LICs.

Figure 2. OLS and IV estimates of the agricultural transition, 1960-2003



| | 1960 | 1965 | 1970 | 1975 | 1980 | 1985 | 1990 | 1995 | 2000 | 2005 |
|---|---|---|---|---|---|---|---|---|---|---|
| N | 28 | 46 | 55 | 73 | 79 | 83 | 91 | 97 | 98 | |
| CD | 1.5 | 0.7 | 1.7 | 28.9 | 29.0 | 34.8 | 37.6 | 35.6 | 36.7 | |

Note: N is the number of observations in the calculations, and CD is the Cragg-Donald-test. It shows that the instruments are strong from 1972 onwards.

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6. The move of countries explains the comparatively high CD-test statistic of 55.6 in model (2).

We find that the estimated income coefficients for both the OLS and the IV estimate are very similar for all years with a sufficient number of country observations. Also, they are close to the estimates for the year 1995. Hence, an income coefficient of about -10 is confirmed for alternative instrumentations in Table 1 and for alternative years on Figure 2.

What this means in quantitative terms can be illustrated by an example. For the year 1995, Ghana is close to the 25th percentile of the log income measure in our sample (7.05), and Thailand is close to the 75th percentile (8.79). The income difference between Thailand and Ghana predicts a $-10 \cdot (8.79 - 7.05) = -17.4$ percentage point difference in the value added share of agriculture in GDP between the two countries. The actual difference in 1995 is 29.3 percentage points, so our estimate explains more than half of the observed difference in the share of agriculture between Ghana and Thailand.

*2.4     Other transitions*

Many other transitions have been studied, but the causality-test used above has only been applied to the *democratic transition* for the Polity index in Gundlach and Paldam (2009a) and for the Gastil index in Paldam and Gundlach (2009), where tables parallel to Table 1 and figures parallel to Figure 2 show very much the same pattern. We have also looked at the *transition of corruption* in Gundlach and Paldam (2009a), using the same set of instruments. In these cases long-run causality is found to be from income to the transition variable.

The most interesting data to use in the study of the democratic transition is the Polity2 index. It brings time series extending 200 years for about 15 countries, and we can hence confirm that the t-s and the c-c patterns both show a strong transition. However, as the statistical properties in the time series for income are very different from the one for the Polity index, the short to medium run is quite tricky to analyze. The Polity democracy index is an integer between $-10$ and $+10$, which stays constant more than 92% of all years, and when it jumps it does so by an average amount that corresponds to 1/3 of its range for a full century. Once it reaches "full" democracy it normally ceases to move. In a comparison with the main alternative democracy index, it is also shown that the index has measurement errors that are larger than the one of the usual economic time series (Paldam 2009).

## 3. Making the agricultural transition go away

Recently, many economists have argued that before a causal relation is accepted all spuriousness should be weeded out. This idea has led Acemoglu, Johnson, Robinson and Yared (2008) to reconsider if income *really counts* in the democratic transition. The method used is the following specification:

$$(1) \qquad TV_{it} = \beta y_{it-1} + \gamma TV_{it-1} + \alpha_i + \alpha_t + u_{it},$$

where $TV_{it}$ is a transition variable such as the share of agriculture $s^A$ or the degree of democracy $\Pi$, for country $i$ at time $t$, while $u_{it}$ are the residuals. $TV$ is explained by lagged income, $y_{it-1}$, and three formal controls: the lagged endogenous, $TV_{it-1}$; fixed effects for countries, $\alpha_i$; and fixed effects for time, $\alpha_t$. The model tests if the effect of income is (still) statistically significant when the three controls are included. If $H_0 : \beta = 0$ cannot be rejected, it would follow that the bilateral correlation between the transition variable and income is spurious.

### 3.1 Are the share of agriculture and the Polity index related to income?

The AJRY-test is applied in column (1) of Tables 2 and 3. Column (1) of Table 2 shows that the effect of income on the value added share of agriculture is insignificant when the three controls are included in the regression. By the logic of AJRY, we would conclude that the presumed agricultural transition is spurious. If the reader agrees, this is surely a sensational new result.

Column (2) of Table 2 is the panel version of the c-c-model in Table 1. Once again it shows what it is that makes people think that there is such a phenomenon as the agricultural transition. What is truly impressive here is that the AJRY-method eliminates an income effect that explains 2/3 of the variation and obtains a t-ratio of almost 40. Surely, it takes a very powerful method to kill the strongest and most uncontroversial of all transitions.

The next columns (3) – (6) reveal that what takes care of most of the coefficient to income are the lagged endogenous variable and the fixed effects for countries – the fixed effects for time just take the last little roots. Including two of these controls is not enough to eliminate the income effect, and the inclusion of one or both of the fixed effects without the lagged endogenous variable does not even affect the size of the income effect.

## Table 2. The effect of income on the value added share of agriculture

| Dependent variable: $s^A$ | AJRY-model | Simplified versions | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Lagged initial income, $y_{it-1}$ | -1.64 | **-12.32** | **-10.31** | **-2.16** | **-13.12** | **-1.52** |
| | (1.05) | (0.31) | (1.96) | (0.98) | (1.31) | (0.28) |
| Lagged dep. variable, $s^A_{it-1}$ | **0.69** | No | No | **0.70** | No | **0.87** |
| | (0.06) | | | (0.05) | | (0.02) |
| Country fixed effects | Yes | No | Yes | Yes | Yes | No |
| Time fixed effects | Yes | No | Yes | No | No | Yes |
| Number of observations | 695 | 695 | 695 | 695 | 695 | 695 |
| Number of countries | 143 | - | 143 | 143 | 143 | - |
| R² within | 0.66 | - | 0.34 | 0.65 | 0.28 | - |
| R² | - | 0.67 | - | - | - | 0.93 |

Note: OLS panel regressions, with robust standard errors in parentheses. Figures in bold indicate statistical significance at 5 percent level. The data sample is an unbalanced panel with annual data for 1960-2003, which was used to calculate 5-year average data (first 5-year averages: 1960-1964). The value added share of agriculture in GDP is in percent (source: WDI home page. Income is measured as the log of GDP per capita in constant international dollars (source Maddison home page).

## Table 3. The effect of income on the Polity democracy index

| Dependent variable: $\Pi$ | AJRY model | Simplified versions | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Initial income | -0.01 | **0.18** | -0.00 | **0.09** | **0.17** | **0.02** |
| | (0.03) | (0.01) | (0.05) | (0.02) | (0.04) | (0.01) |
| Lagged dependent, $\Pi_{-1}$ | **0.63** | No | No | **0.73** | No | **0.88** |
| | (0.04) | | | (0.03) | | (0.02) |
| Country-fixed effects | Yes | No | Yes | Yes | Yes | No |
| Time-fixed effects | Yes | No | Yes | No | No | Yes |
| Number of observations | 981 | 981 | 981 | 981 | 981 | 981 |
| Number of countries | 153 | - | 153 | 153 | 153 | - |
| R² within | 0.57 | - | 0.32 | 0.49 | 0.06 | - |
| R² | - | 0.27 | - | - | - | 0.85 |

Note: See Table 2. Data sample is an unbalanced panel with annual data for 1960-2003, which was used to calculate 5-year average data (first five year averages: 1960-1964). The degree of democracy is measured by the normalized Polity2 variable (Marshall and Jaggers, 2009). Same income data as in Table 2.

Hence, the statistically insignificant income effect of column (1) does not result from the inclusion of country-fixed effects per se. This is a remarkable result in light of the relatively short time dimension of our sample (nine observations of 5-year averages in 1960-2003, with one lost due to the lag structure). Section 3 reconsiders this pattern of results in more detail.

The democratic transition is analyzed in Table 3. It is constructed precisely as Table 2, but the share of agriculture is replaced by the Polity2 democracy index. As expected, the $R^2$-scores are lower in the regressions of Table 3 than in the corresponding models of Table 2, but otherwise the results are much the same.

Column (1) replicates the AJRY-result.[7] The statistical significance of the income effect goes away when the three controls are included. Column (2) again shows the statistically significant correlation that has made many researchers think that there is a democratic transition (also known as Lipset's Law). Here the AJRY-method manages to eliminate an income effect with a t-ratio of about 20.

Columns (3)-(6) reveal that it is slightly easier to make the democratic transition disappear than the agricultural transition. Including country- and time-fixed effects already generates a statistically insignificant income effect in column (3), but the income effect remains statistically significant if only one fixed effect is included, and the size of the estimated income effect is not affected if only country-fixed effects are included.


### 3.2    What has been shown?

Tables 2 and 3 tell the same basic story. The contrast between the models in columns (1) and (2) is dramatic. The AJRY-model with a lagged endogenous variable and both types of fixed effects makes the effect of income go away, and hence the transition vanishes. Columns (2) - (6) basically demonstrate that it takes the *full* AJRY-model to make the transitions go away. Section 4 discusses in more detail why this happens, but it is already clear that we are faced with two alternatives.

The first alternative is to accept the AJRY-model at face value. Here the conclusion is that income has no effect on the share of agriculture or on the level of democracy. Since it is possible to reject the strongest and least controversial of all transitions, we assume that the AJRY-test can also be used to demonstrate that a great many other broadly accepted transitions are spurious. So the long-run fall in birth rates and death rates, and the growth of cities

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7. The Polity2 index is normalized as is done by AJRY. We use 5-year average data instead of 5-year interval data and the Maddison income data instead of PWT income data, but still the result holds.

may have nothing to do with the rise in incomes. The established theory of economic development will thus need a thorough revision!

The second alternative is that the AJRY-test has a much larger scope than just to weed out spurious relations. We suggest that the method has the Roundup property: In the effort to reveal Type II errors (acceptance of wrong models) a method has been made to generate Type I errors (rejection of true models) with a very high probability. Below we shall further elaborate on the second alternative.[8]

One reason to suspect equation (1) as misleading is the strong *long-run* causal relation between income and agriculture or democracy.[9] Given the causality, there must be a connection in the short run as well. It might be weak and volatile, but when aggregated over time, it has to become strong. A partial-adjustment model like columns (1), (4), and (6) implicitly predicts a long-run income effect ($\beta / (1 - \gamma$) based on the estimated coefficients for the lagged endogenous variable and lagged income. Thus, an alternative long-run effect can be calculated from columns (4) and (6), and it is close to the one estimated in column (2). But if lagged income is statistically insignificant as claimed by AJRY (and in column (1)), there is no long-run income effect at all.

Another reason to suspect the AJRY-model as misleading has to do with a catch-all effect. Model (1) uses the fixed effects for countries to explain why the share of agriculture varies across countries, but the transition hypothesis says that the level of *income* does just this. Hence, we suspect that the country-fixed effects may work at least partly like the income variable. Taking the results of the AJRY-model in column (1) seriously, we would end up suggesting that the observed decline of the share of agriculture and the observed increase of democracy should be explained by nothing but unspecified level-effects for countries. That is, country-fixed effects catch all of the variation in levels, which is mainly what the transition hypothesis is about.

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8. We suspect that the main reason for the choice of the model in the AJRY-paper was that the democratic transition hypothesis is in conflict with a theory of development that emphasizes the fundamental role of institutions. Acemoglu, Johnson and Robinson are well-known for their advocacy of the *Primacy of Institutions* view. The greater causal scheme of that view is laid out in Acemoglu et al. (2005, p 392). Here income does not cause democracy. See also the more cautious survey by Paldam and Gundlach (2008).
9. The AJRY-paper also finds a long-run relation between income and democracy.

## 4. What does model (1) do to the data?

This section argues that the AJRY-model treats the data to be explained as an onion, by peeling away one layer after the other until little remains. It makes no sense to peel away so much of the onion, but it can be done, as we have shown. The peeling process removes three layers before the income variable is allowed to play a role, so we have to consider the following progression of models:

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

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

(4)     $TV_{it} = \delta_3 TV_{it-1} + \alpha_i + \alpha_t + \varepsilon_{3it}$   the further layer of the fixed time effects.

Table 4 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 (2) already peels of 93% and 83% of the variation of the two transition variables (as measured by the $R^2$). What is left to be explained is further reduced by the two fixed effects, which represent 122 and 138 dummy variables respectively. The remaining variation in the dependent variables is 4% and 12% in the two cases. We know that there is measurement error (especially for the Polity2 index) and some other variables that are likely to influence the two transition variables,[10] so there are precariously little left for income to explain. It is no wonder that the effect of income does become insignificant when it has to compete with the 123 to 139 explanatory variables of model (4).

Table 4. How much variation remains?

| | Fraction of the explained variation in the dependent variable: | | | | | |
| | Agricultural transition | | | Democratic transition | | |
| | R-squared | Variables | Obs. | R-squared | Variables | Obs. |
|---|---|---|---|---|---|---|
| Equation (2) | 0.928 | 1 | 695 | 0.830 | 1 | 981 |
| Equation (3) | 0.958 | 116 | 695 | 0.854 | 108 | 981 |
| Equation (4) | 0.960 | 123 | 695 | 0.882 | 139 | 981 |

Note: R-squared based on OLS panel regressions. Same data samples as in Tables 2 and 3. *Variables* are a count of the number of explanatory variables included in the regressions.

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10. E.g. the share of agriculture is influenced by policies supporting prices and by fluctuations in the prices of the main crops of countries. The democracy index is influenced by e.g. political scandals and violence.

The next step is to check whether our income measures are actually correlated with the two fixed effects. If income works through the fixed effects, equation (1) cannot be used to falsify any transition hypotheses. Table 5 reveals that there is a statistically significant correlation between the country-fixed effects $\alpha_i$ and income, but no correlation between the time-fixed effects $\alpha_t$ and income. Hence, including country-fixed effects together with the lagged endogenous variable will necessarily reduce any income effect. Country-fixed effects obviously take out a fraction of the variation that can be explained by income. But the given time horizon of our panel data is already sufficient to estimate a statistically significant income effect once the fixed time effects are not included in the specification (see Tables 2 and 3, column (4)).

Table 5. Correlation between fixed effects and income

|  | Agricultural transition | | Democratic transition | |
|---|---|---|---|---|
|  | Coefficient | R² | Coefficient | R² |
| Country-fixed effects | -3.22 (0.14) | 0.38 | 0.059 (0.004) | 0.13 |
| Time-fixed effects | -0.13 (0.10) | 0.00 | 0.005 (0.004) | 0.01 |
| Combined fixed effects | -3.30 (0.14) | 0.39 | 0.065 (0.005) | 0.14 |

Note: OLS panel regressions, robust standard errors in parentheses. All regressions in the agriculture sample based on 695 observations; all regressions in the democracy sample based on 981 observations. For underlying data sources, see Tables 2 and 3.

In the end this leaves us with a choice. We can use income to explain as much as it can before we leave the field to the fully mechanical explanations provided by the two fixed effects. Or we can allow the fixed effects to take away a lot of the variation. This brings us to the nature of fixed effects in regression exercises. Strictly speaking, fixed effects explain nothing at all in a very complicated way using a large number of artificial variables. We think that the income variable provides a preferable explanation that is in line with the principle known as Occam's Razor.

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# 5. 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 two such driving hunts, for the agricultural and the democratic transitions.

In these cases both theory and the usual descriptive data tools suggest that big game is present. Consequently, we have analyzed causality by the method demonstrated in Section 2. We hypothesize that (c1) income is causal to the transition variable $y \Rightarrow TV$, or at least that (c2) the two variables are mutually causal, $y \Leftrightarrow TV$. We also look for reverse causality (c3), where the transition variable causes income, $y \Leftarrow TV$. In both cases we find that case (c1) is accepted, and that (c3) is rejected, so that (c2) becomes irrelevant. However, (c1) can also be rejected if the correlation is due to (c4) a spurious relation. For instance, both variables can be caused by a third variable $C$, $C \Rightarrow y, TV$. Normally, a spurious relation would be revealed in the form of perfect simultaneity in the IV-tests used to identify the direction of long-run causality. This is not the case for the two transitions at hand.

The AJRY-model is meant as a stronger test of spuriousness as it demands that we only accept (c1) if the *idiosyncratic* parts of the transition variable *TV* can be explained by income *y*. The AJYR-model rejects (c1) for both transition variables, so it suggests that there is neither an agricultural nor a democratic transition.

Our main point is that long-run relations are *not* between the idiosyncratic parts of the series. 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 by the two transition variables in Figure 1. In these cases 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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# Appendix A: Definitions and sources of the DP-variables used in Table 1

Dependent variable and main explanatory variable used in Table 1

$s_{it}^A = Y_{it}^A / Y_{it}$  The share of the GDP in agriculture, $Y_{it}^A$, of total GDP, $Y_{it}$. Source: WDI

$y$  Natural logarithm of GDP per capita, measured in 1990 international Geary-Khamis dollars. Source: Maddison (2003) and Maddison homepage: http://www.ggdc.net/maddison/

Instruments used in Table 1. Figure 2 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), (downloaded 27-10-03): www.earth.columbia.edu/about/director/malaria/index.html#datasets

*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).