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A farewell to critical junctures:

Sorting out long-run causality of income and democracy

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Abstract

We consider the empirical relevance of two opposing hypotheses on the causality between income and democracy: The Democratic Transition hypothesis claims that rising incomes cause a transition to democracy, whereas the Critical Junctures hypothesis denies this causal relation. Our empirical strategy is motivated by Unified Growth Theory, which hypothesizes that the present international income differences have roots in the prehistoric past. Thus, we use prehistoric measures of biogeography as instruments for modern income levels, and find a large long-run causal effect of income on the degree of democracy. This result rejects the Critical Junctures hypothesis, which is an important part of the Primacy of Institutions view.

Keywords: Long-run growth, democracy, unified growth theory, biogeography

JEL: B25, O1

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1. Introduction

Income and democracy are highly correlated across countries for all years of the 20th century (see section 2), so there is an obvious relationship to be understood. However, the pattern of causality between the two variables has long been contested by economists and political scientists. It is difficult to sort out causality because it is easy to point to cases supporting one or the other direction, and there might well be simultaneity (see, e.g., Lipset 1959 and Moore 1966). We consider two testable and opposing hypotheses on the long-run causality between income and democracy.

The older hypothesis is the one of a *Democratic Transition*, which claims that the political development in the long run is a consequence of overall development. It is a part of the *Grand Transition* view, which goes back to Clark (1951) and Kuznets (1965, 1966). Long-run development is held to be an interacting set of transitions in many fields, and thus development is seen as a band of variations around a basic path.[3] We have demonstrated that such a path may be derived from the elementary micro theory of production possibility frontiers and indifference curves (Paldam and Gundlach 2008a).

The newer hypothesis is the one of *Critical Junctures*, which is a part of the *Primacy of Institutions* view. It goes back to Douglas North (1981, 1990) and has been developed in a series of papers by Acemoglu, Johnson, and Robinson (surveyed in 2005). Both income and democracy are held to be determined by the power structure of the political system. Countries that make different political decisions at critical historical junctures are predicted to embark on different paths of development. Consequently, the main direction of causality runs from the political system to economic development, and the observed correlation of income and democracy does not imply any causality from income to democracy.[4]

Hence, if we find a clear long-run causality from income to democracy, we content that the Democratic Transition hypothesis is confirmed. However, if this causal link is rejected, we content that the Critical Junctures hypothesis holds. Causality tests often give unclear and weak results. Fortunately, this is not the case in the present paper.

The causality analysis uses the set of extreme biogeography variables defined in Appendix A. They are compiled (mainly by Hibbs and Olsson 2004, 2005) to catch the long-run

3. The transitions of mortality, fertility, urbanization, human capital, and the sectoral composition of the economy are standard textbook material. The Grand Transition includes transitions in fields with no obvious connection to the economy, such as in gender roles, corruption, and religiosity; see e.g. Paldam (2007b).
4. See Acemoglu, Johnson, Robinson and Yared (2007, 2008). Gundlach and Paldam (2008b) argue that their empirical test is made so that the income-democracy relation must be rejected.

development potential of countries in accordance with the theories of Diamond (1997) and Galor (2005) discussed in section 2. The variables are biological and geographic factors that are exogenous in the perspective of recorded history.

The biological variables measure the conditions that prevailed in various regions of the world 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 annual perennial wild grasses (*plants*) known to have existed in prehistory, which goes from less than five for Sub-Saharan Africa to more than 30 for Europe.

The geographic variables measure the specific conditions that have constrained or enabled the spread of 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*).

We also use averages and first principal components of these measures 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 international trade.

Our main test compares two empirical explanations of the present cross-country pattern of democracy. (i) An OLS estimate explaining the pattern by present income. (ii) A two stage IV estimate, where stage one instruments the income pattern with the said biogeography variables, and stage two explains the democracy pattern by the generated *institution free incomes*. The key result of our analysis is that explanations (i) and (ii) of the democracy pattern are equally good. Hence the long-run causality appears to be driven exclusively from income to democracy, with critical junctures playing no role in the long run.

Our paper captures political institutions and economic development with one variable each. We believe that we have chosen the best available representations, and we can replicate our results with the main alternative measure of the degree of democracy (see below). Section

2 introduces our selected data series for democracy and income, and explains our empirical strategy in the context of theories of long-run development. Section 3 presents our basic result. Section 4 includes a number of robustness tests that leave our basic result intact. Section 5 concludes. The measures of biogeography and all other variables used in the paper are listed in Appendix A. Appendix B provides pairwise correlations of our selected control variables with our measures of income and democracy. Appendix C considers an alternative interpretation of the reduced form of our empirical model.

We should mention that at present we concentrate on the medium to long-run relations between income level and democracy. Paldam and Gundlach (2008a) survey the literature in more detail, including the large literature on the relation from democracy to economic growth.

2. An empirical strategy for the long run

We first present the income and democracy data and show how correlated they are.[5] We then discuss causality and provide a theoretical justification for using the extreme measures of biogeography as instrumental variables. On the face of it, it appears inconceivable that these variables can possibly work. Yet, it is shown by Olsson and Hibbs (2005) that they are significantly related to long-run economic development. This becomes reasonable if the cross-country pattern of development represents international differences in long-run growth and if our preferred instruments can be justified by a set of theories of long-run growth. We thus see these variables as pointing to the development potential of countries.

2.1 Measures of income and democracy

The two main data series used in this paper are income and the polity index:

(i) *Income, y*, is the natural logarithm of GDP per capita, measured in constant international dollars, and taken from the Maddison data set (see Maddison 2003).

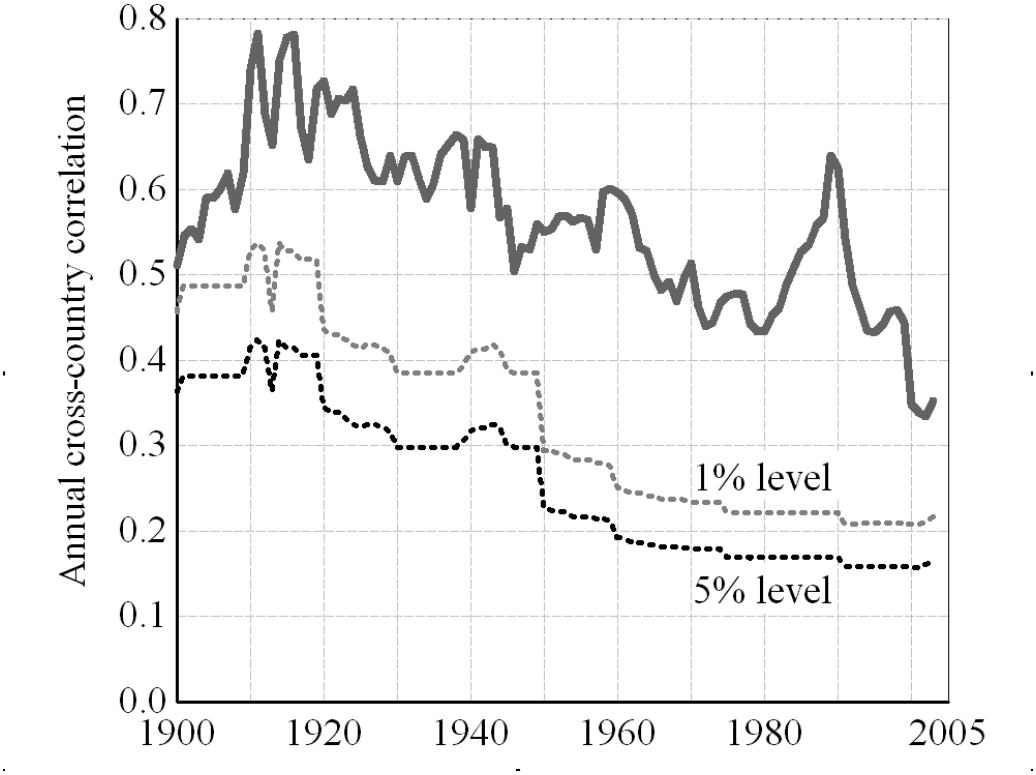
(ii) *The polity index, P*, measures the degree of democracy of the political system of a country by a scale ranging from -10 for a fully authoritarian regime to +10 for a fully democratic one (see Marshall and Jaggers 2006).[6]

5. The level of development (income) for a cross-section of countries at a given point in time reveals the differences in country-specific rates of long-run growth, given that all countries had similar income levels about 200 years ago. From the 15th century until about 1800, the West did grow by 0.1 to 0.15% per year according to Maddison (2003), while the rest of the world had zero economic growth. The West was thus ahead by about two times in income when modern economic growth started to take off.

6. The *Gastil index* from Freedom House is an alternative to *P*, but it is only available from 1972. We have replicated most of the analysis in this paper using the Gastil index; see Gundlach and Paldam (2008b).

Both data series are available for a large number of countries and over a time horizon of about 200 years for some countries. We take *y* and *P* to be the best aggregate measures available for the economic and political development, though both have weaknesses at the conceptual level as well as at the measurement level.[7]

Figure 1. The annual cross-country correlation between income and democracy, 1900-2003

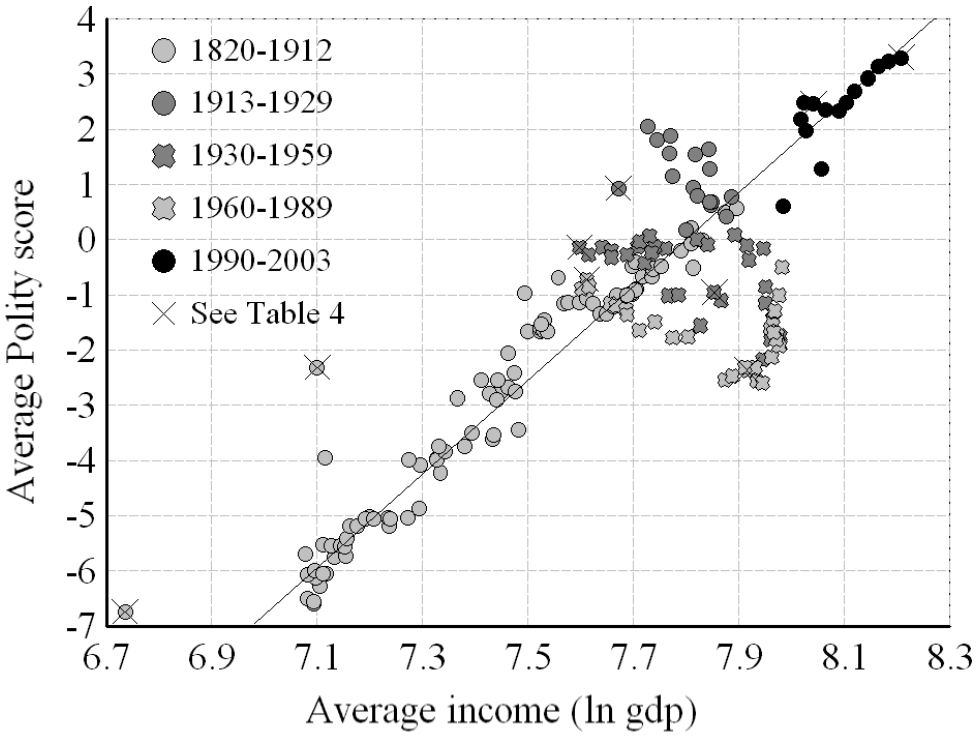


Notes: There are only about 20-30 country observations in the first years compared to about 150 country observations in the last years of the 20th century. The two broken lines indicate the level of statistical significance for a two-sided test of the correlation coefficient being different from zero.

Figure 1 shows the annual correlation of the two variables throughout the 20th century. For each year, all available observations are used. The result of this mechanical exercise is abundantly clear: The cross-country correlation has been statistically significant at the one percent level for each and every year of the 20th century. The declining trend in the correlation coefficient is overcompensated by the increase in sample size, such that the statistical significance of the correlation increases over time.

7. See Munck and Verkuilen (2002) for a critical discussion of democracy indices, and the appendix to Jensen and Paldam (2007) for a comparison of the Gastil and Polity indices.

Figure 2. The development over time for income and the Polity index



Another way to look at the data is Figure 2, which also includes the 19th century. For every year, the averages for *P* and *y* are calculated for all countries with available data. The number of countries goes up steeply especially in the period from 1955-95. The figure shows a strong relation that looks like a straight line. In certain periods, deviations happen, but the deviations are transitory and the long-run path re-emerges. The deviations occur precisely in the well-known periods of large political and economic changes. The figure thus suggests that the long-run pattern is clearer than the medium-run pattern, and that the long run may be fairly independent of the short run. This is very much what our statistical analysis will show.

2.2 Two problems for the Democratic Transition hypothesis[8]

The first problem is what long-run causality really means. A theoretical answer may be that long-run causality refers to the steady state. This is not an operational answer for the relation between income and democracy. The democracy variable, *P*, is constant over fairly long periods. When it moves, it does so in a discrete step. The income variable, *y*, changes every year, though normally only by 1-3%.

8. The Democratic Transition is also known as Lipset's Law or the Modernization hypothesis (Lipset, 1959).

Imagine that *P* adjusts to *y* in the long run, but that *y* can be temporarily kept back by a slow adjustment of *P*. Since the adjustment of *P* is stepwise, some of the steps will be large, and before the adjustment of *y* will be held back. After the adjustment of P, *y* will catch up with its long-run trend. In this case, we say that income is the primary variable because all of the long-run causality is from *y* to *P*. However, in the short to medium run a complex pattern of simultaneous interactions may occur between the two variables. This pattern may sometimes look like reverse causality from *P* to *y*. In Paldam and Gundlach (2008b) we provide evidence that the long-run causality actually appears to work through short-run simultaneity.

Our analysis aims at catching long-run causality by concentrating on the cross-country pattern. This does not mean that we reject studies that find a complex interaction process (see, e.g., Persson and Tabellini 2006, and Paldam and Gundlach 2008b), or case studies that show that growth is slowed down by the lack of political reform, and that it picks up after the reform. But it means that we are critical of studies that claim there is no causality at all from income to democracy.

The second problem is that the Democratic Transition hypothesis uses income as the primary variable. Income is an aggregate that appears problematic in that role. It works as a catch-all-variable – a "kitchen sink" – for the whole set of transitions that together constitute development. This is not satisfactory because of mutual simultaneity, and hence we go one – very long – step further by using measures of biogeography as primary variables for development. This identification strategy should help to disentangle the long-run effect of income on the degree of democracy from the complex interactions of the two variables in the medium and short run. Our primary variable goes back to a time long before all existing modern institutions came about, although they have roots going down more than 500 years.

2.3 A theoretical approach to long-run development

To justify this modeling strategy, we need a theoretical framework that explains development both in the era of the Malthusian stagnation that preceded the Industrial Revolution and in the subsequent era of modern economic growth. Such a theory should be able to explain the present large income differences between countries without invoking changes in the institutions of a country as explanatory factors, and in addition it should point to measures of biogeography as possible primary variables that can be used to identify the exogenous long-run variation in income.

The Unified Growth Theory of Galor (see his survey 2005) has many of the elements needed.[9] Unified Growth Theory holds that the apparent stability of the last few centuries of the Malthusian era saw a slow steady growth of the population and subtle but important changes in the composition of the population that ultimately led to the Industrial Revolution (Galor and Moav 2002). The methodology should be consistent with the interpretation of the Industrial Revolution as a gradual process; see Mokyr (2002) and Clark (2007).

Unified Growth Theory hypothesizes that the transition from stagnation to growth can be captured by a single dynamical system, where the stability of the set of steady-state equilibria is altered in the process of development due to latent state variables such as human capital formation. According to this view, changes in the quality of the institutional framework (or their absence) may support or hinder the transition from one steady state to another. However, they are neither necessary nor sufficient to explain why there has been sustained growth of per capita income in the world economy over the last 200 years, but not before in all of human history. In this model, long-run growth is an inevitable consequence of changes in the size and the composition of the population, which may have biological (Galor and Moav 2002; Clark 2007)[10] and geographical (Diamond 1997)[11] roots. Thus, instrumenting the level of income with measures of biogeography should produce a measure of the exogenous (institution-free) long-run variation in income that is needed in order to explain the Democratic Transition.

2.4 The biogeographic data: Do they catch the development potential?

Prehistoric biogeographic conditions may explain why the transition from a hunter-gatherer society to an agrarian society started earlier in some regions of the world than in others, and why it sometimes did not start at all. One condition for a sustainable early transition is the availability of plants and larger mammals that can be domesticated.

The availability of suitable plants is believed to have varied widely across the prehistoric world, with at least five and probably nine areas with independent agricultural development (Diamond 1997, p. 99). The Near East or Fertile Crescent of Southwest Asia has been identified as the earliest site for a string of further developments beyond agriculture, such as cities, writing, and empires (Diamond 1997, p. 135).

9. Another theory of the very long run is suggested by Hansen and Prescott (2002).
10. Clark (2007) builds on Unified Growth Theory, especially on the version of Galor and Moav (2002).
11. Diamond (1997) identifies prehistoric biogeographic conditions that have been conducive for the development of stable agricultural societies, but he is in conflict with Unified Growth Theory in his assessment of the effects of population growth on agricultural technical change. For an early theory of agricultural development that is in line with Unified Growth Theory, see Boserup (1965).

The availability of domesticable large mammals as a source of food and as a means of transport and warfare also differed substantially across the world in prehistoric times. For instance, no large mammal has ever been domesticated in Sub-Saharan Africa, and most large mammals became extinct in the Americas when the continent was invaded by human hunter-gatherers from Asia. It makes a difference for military success whether the major domesticated animal is the turkey or the horse. More generally, areas with domesticated plants and mammals could support larger populations, and larger populations developed into more complex societies.

An additional factor that has spurred or hindered such developments is geography. Regular climatic variation with dry and wet seasons and moderate temperatures are obviously beneficial for agricultural development, as is a large landmass that spreads horizontally rather than vertically because climatic zones change faster along a north-south axis than along an east-west axis. This implies that all else constant, agricultural innovations like new domesticated plants could spread more easily across Eurasia than across the Americas or Sub-Saharan Africa, and they could not spread at all to small isolated places like New Guinea. Hence, the prehistoric potential for developing rather stable complex agrarian societies with large populations was much stronger in some regions of the world than in others.

In addition, the co-evolution of domesticated plants that were partly used to feed domesticated mammals brought a further advantage for prehistoric societies with favorable biogeographic conditions. Their populations developed at least partial resistance against a number of diseases that may have spread from domesticated animals to humans. Diamond (1997) argues that Europe (Eurasia) conquered the Americas and not the other way round just because of plants, animals, germs, continental axis, and size, despite comparable levels of per capita income on both continents.

The question not resolved by Diamond (1997) refers to the ultimate factors that explain persistent economic growth since about 1800, first in Europe and its offshoots and later in East Asia and other parts of the world. All available facts point to a more or less constant per capita income that may have differed in levels across the world, but remained trendless throughout all of human history until 200 years ago. Galor and Moav (2002) suggest that natural selection on humans during the seeming income stability of the Malthusian era provides an answer for the emergence of the Industrial Revolution.

Their theory is supported by the historical facts presented by Clark (2007), who suggests that a long history of an institutionally more or less stable society with settled agriculture, as exemplified by England from about 1200 to the beginning of the Industrial

Revolution, initiated behavioral changes in the population that ultimately led to the demise of the Malthusian straightjacket of diminishing returns. According to Clark (2007), the main mechanism for this latent development was that the rich had persistently more surviving descendants than the poor. So there was strong and permanent downward social mobility, which may have helped to spread middle class behavior throughout the society, either genetically or by cultural transmission.[12]

Many of the arguments in sections 2.3 and 2.4 are speculative, but the authors cited provide a wealth of illuminating detail and a good deal of formal modeling to back up their arguments. Hence, we feel that there is both theoretical and empirical justification behind our use of these variables as instruments to generate the institution-free incomes demanded by our tests.

3. Specification and main result

Our basic equation is given by

$$P_i = \alpha + \beta y_i + \mathrm{X}_i'\gamma + \varepsilon_i, \qquad (1)$$

where P is the degree of democracy in country i in a given year, y is the natural logarithm of GDP per capita in constant international dollars, X_i' is a matrix of other covariates, α is a regression constant, ε is an error term, and β is the coefficient of interest that measures the long-run effect of income on democracy.

3.1 The underlying structure

The two major problems with estimating equation (1) are omitted variables, which we address in the next section, and reverse causality. As motivated by our empirical strategy, we use prehistoric measures of biogeography to identify the exogenous part of the variation in actual cross-country incomes. More formally, we have the additional equations

$$y_i = \lambda_1 + \eta_1 \, popchange_i + \mathrm{X}_i'\gamma_1 + u_{1i}, \qquad (2)$$

12. This Malthusian mechanism appears to have changed the average behavior towards less violence, more working hours, and more investment in human capital, with effective institutions adjusting to the changes in behavioral norms. Hence, slow but steady changes in the composition of the population over a long time span of at least 500 years may help us understand why an Industrial Revolution occurred at all, and why in England and Europe. It appears that beneath the seeming stagnation of the Malthusian era, economically unsuccessful behavior literally died out under the stable conditions provided by settled agriculture.

$$popchange_i = \lambda_2 + \eta_2 agristab_i + \mathrm{X}_i'\gamma_2 + u_{2i}, \text{ and} \quad (3)$$

$$agristab_i = \lambda_3 + \eta_3 biogeo_i + \mathrm{X}_i'\gamma_3 + u_{3i}, \quad (4)$$

where λ_j are regression constants, η_j are parameters, and u_{ji} are error terms; *popchange* is a measure of the changes in the composition of the population that developed slowly but steadily in the Malthusian era before the Industrial Revolution, *agristab* is a measure of the long-run institutional stability of agrarian societies in the Malthusian era, and *biogeo* is a measure of biogeographic conditions that prevailed at the time of the Neolithic revolution.

Although we lack information to estimate equations (2)-(4), we can nevertheless employ measures of biogeography as instruments for the actual level of income if such measures have no direct impact on the observed degree of democracy and if they are sufficiently correlated with modern cross-country income levels. Both conditions appear to be satisfied, as argued above.

3.2 The main result (Table 1): Comparing OLS and IV estimates

All specifications in Table 1 refer to a single cross-country regression for the year 1995.[13] These regressions include no control variables. The top section of the table presents the OLS results. The adjusted (centered) R-squared of the OLS regression indicates that 20-30 percent of the cross-country variation in the degree of democracy in 1995 is associated with the cross-country variation in (log) GDP per capita.

The next section of the table presents the IV results, where we present specifications with alternative sets of instruments. With a partial R^2 of about 0.5 for the first stage regressions, the instruments are statistically satisfactory. More formally, the Cragg-Donald test statistic shows that the instruments are *strong* in all five columns of Table 1 (the hypothesis of weak instruments is rejected) because it is always above the stated critical values. In addition, all specifications pass the Sargan test for overidentifying restrictions at conventional levels of statistical significance.

13. The cross section for the year 1995 used in Table 1 and the cross sections for the other years to be used in Section 4.3 have been constructed to maximize country observations for measures of income and democracy conditional on the available observations of the instrumental variables. The biogeography data provided by Olsson and Hibbs (2005) include 112 country observations. If income data or democracy data are missing for 1995 (or another of the selected cross section years), we use the next observation within a time interval of +/- 10 years. Hence, to include Ethiopia in the 1995 sample, we use the 1993 observation for *polity*. Belize, Cap Verde, Hong Kong, Iceland, Luxembourg, Maldives, Malta, and Samoa are not included in the Polity IV database. Fiji, Papua New Guinea, and the Solomon Islands are not included in the Maddison database. The estimation results are not statistically significantly affected by the additional observation on Ethiopia (see Table 3a below).

Table 1. The estimated effect of income on the degree of democracy

	Main model	Robustness of model to instrument variation			
Dependent variable: *P*	(1)	(2)	(3)	(4)	(5)
No. of obs. (countries)	101	106	101	101	142
			OLS estimates		
Income, *y*	2.83 (0.45)	2.89 (0.41)	2.83 (0.45)	2.83 (0.45)	2.67 (0.44)
Centered R^2	0.29	0.31	0.29	0.29	0.21
			IV estimates: *y* is instrumented		
Income, *y*	2.75 (0.67)	3.41 (0.57)	2.57 (0.68)	2.96 (0.61)	3.11 (0.64)
Instruments	*biofpc, geofpc*	*bioavg, geoav*	*animals, plants*	*axis, size, climate*	*coast, frost, maleco*
First stage partial R^2	0.44	0.53	0.43	0.54	0.48
CD F-statistic	37.98	57.49	37.44	37.95	42.12
CD critical value (size)	19.93 (10%)	19.93 (10%)	19.93 (10%)	22.30 (10%)	22.30 (10%)
Sargan test (p-value)	1.73 (0.19)	3.33 (0.07)	0.04 (0.85)	1.49 (0.47)	0.59 (0.75)
			Hausman test for parameter consistency of OLS and IV estimate		
C-statistic (p-value)	0.03 (0.87)	1.80 (0.18)	0.27 (0.61)	0.09 (0.76)	0.91 (0.34)

Notes: All observations for 1995 or the next available year; standard errors in parentheses. All specifications include a constant term (not reported). The Cragg-Donald (CD) statistic should be *above* the critical value (10 percent maximal size) given for the instruments to be strong. The Sargan test for overidentification tests the joint null hypothesis that the instruments are valid and correctly excluded from the estimate.

The instrumented measure of income has a large and statistically significant effect on the degree of democracy in all regressions presented. The estimated coefficient on income is about 2.8 using both the OLS and the IV estimator.[14]

The estimate of our *main model* is given in column (1). It uses the first principal components of four measures of geographic conditions and two measures of biological conditions. We use this model in the further analysis as it is the most parsimonious set of variables that catches the largest amount of variation in the alternative measures of biogeography.

Columns (2) to (5) show that the result in (1) is robust to variations in the instruments. The plain averages in (2) include observations that may bias the estimates.[15] The instruments

14. What this means can be illustrated by an example. Ghana is close to the 25th percentile of the 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 $(8.79 - 7.05) \cdot 2.8 \approx 4.9$ Polity-point difference between the countries. The actual difference in 1995 is 10 Polity points, so the estimate explains about half of the observed difference in the said democracy index of the two countries.

15. Column (2) includes the Western offsprings Australia, Canada, New Zealand, and the United States (plus Germany). These countries are recoded by Hibbs and Olsson (2004) as having European biogeography because the full European food and technology package was imported by the colonialists. Since the inclusion of the recoded data would potentially bias the results in favour of the hypothesis of a democratic transition, we use the original observations of *bioavg* in column (2), but do not find different results relative to the other columns.

in columns (3) and (4) refer either to biology or to geography, and the instruments in column (5) provide only a limited amount of biological variation.

The key finding in the paper is that we cannot reject at conventional levels of statistical significance that the OLS estimates of the income coefficient are the same as the IV estimates, as indicated by the p-value of the C-statistic of the Hausman test for parameter consistency. This finding implies that the OLS estimate is not upwardly biased by a potential reverse causality from democracy to income. Hence, whatever the institutional history of the sample countries and irrespective of the critical junctions passed on the way, the long-run outcome is essentially the same, and it is explained fairly well by income.[16]

3.3 Stability of the main model over time: 1820-2003

As we are dealing with a long-run effect, we expect that our main result should hold for a great many years. The stability of the main result over time is analyzed in two ways:

(i) By using panel data, which allows us to economize on the smaller cross-country samples of the earlier periods. The panel-data analysis follows in 4.4 below.

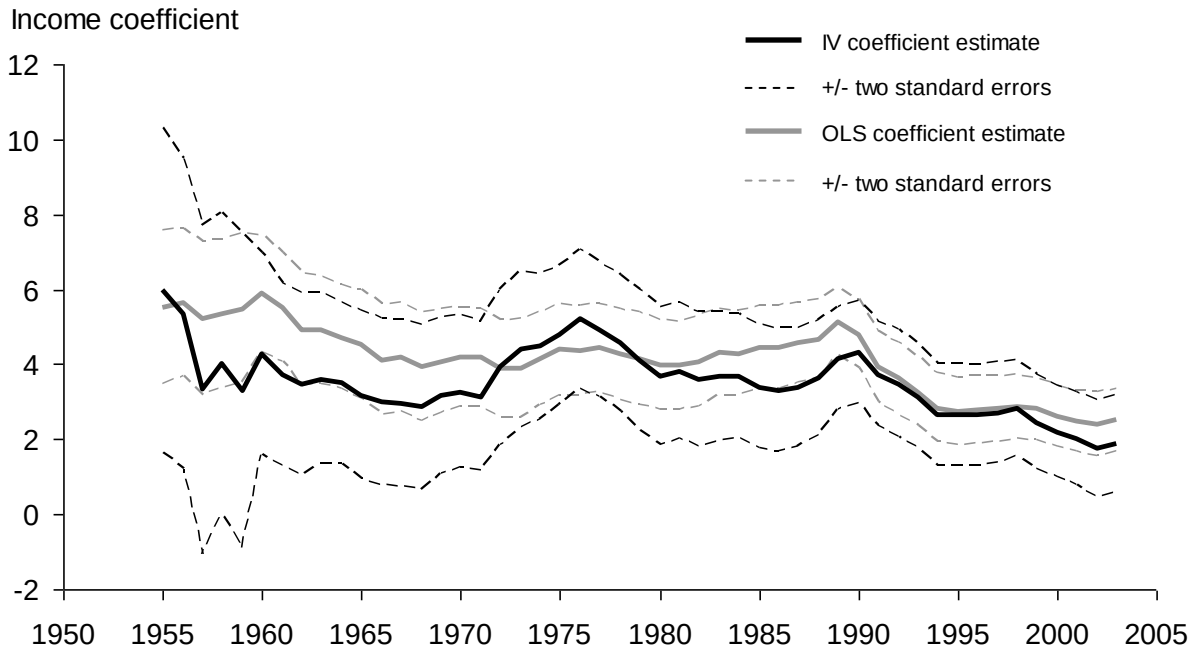
(ii) By estimating our main model for each of the 184 years from 1820 to 2003. Figures 3a and 3b show the OLS- and the IV-estimates of the income coefficient, surrounded by 95 percent confidence intervals for all these years.

To catch the Grand Transition, the sample must include a range of poor *and* rich countries. This is only the case after 1960. As we go back an additional 140 years to 1820, the data on income and democracy concentrates on two and then one dozen of today's rich countries. This is indicated by the Cragg-Donald F-statistic (CD-F) that examines the strength of the instruments. They are only strong after 1960, and we therefore separate the time period 1820-2003 in two figures with a short overlap.

Figure 3a starts in 1955. For every year after 1960, the CD-F statistic is larger than 22, so the instruments are strong. Both the OLS and the IV estimates have a fairly stable average of about 3 as in Table 1 and in Tables 2 and 3 below. The confidence intervals of the two estimates have considerable overlap every year, so they do not differ from each other. Thus, the results for the 43 years where the instruments are strong are all very much like the ones reported in Table 1.

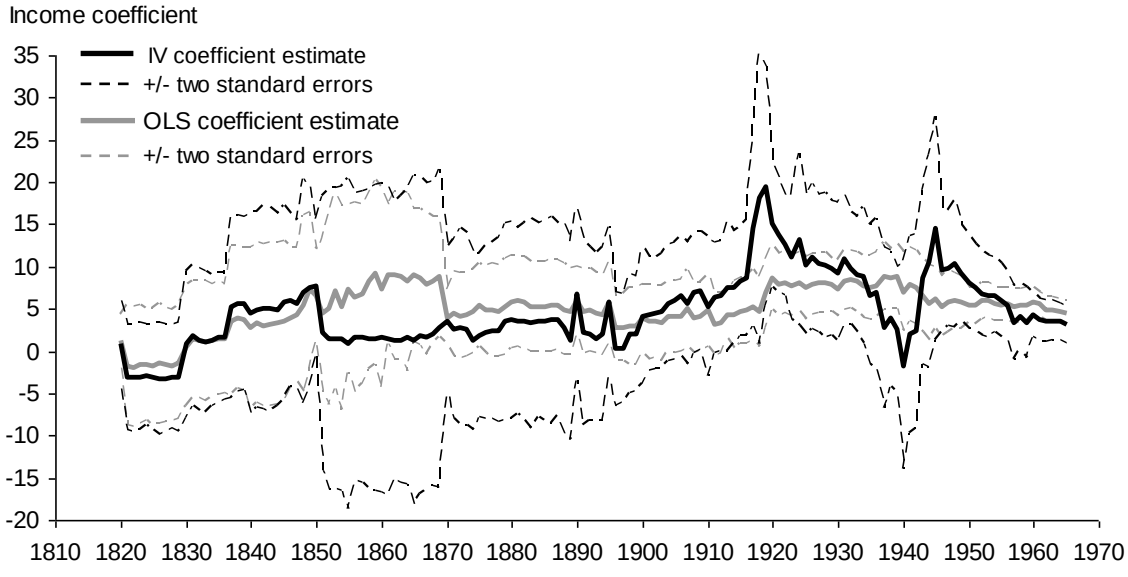
16. We have also recalculated Table 1 in the reverse to see if *Polity* instrumented with the measures of biogeography can explain income as well as an OLS estimate. We find that in the reverse specification our preferred instruments are rejected by the Cragg-Donald test, so our instruments only work one way from income to democracy. These results are available upon request.

Figure 3a. The OLS and IV estimates of the income coefficient, 1955-2003



obs:	56	75	85	91	96	96	96	97	100	99
CD-F:	7.2	18.7	30.5	33.2	32.8	34.5	40.9	40.4	36.8	37.1

Figure 3b. The OLS and IV estimates of the income coefficient, 1820-1965.
Note change of scale on the vertical axis



obs:	9	16	26	24	23	33	53
CD-F:	3.4	4.5	1.8	1.7	3.1	2.9	4.9

Figure 3b covers the years from 1820-75. The CD-F statistic shows that the instruments are weak till 1960. Thus, the results have to be taken as unreliable, and they do vary much more than the reliable results of Figure 3a. However, the results in Figure 3b are still trendless, and the OLS and IV estimates are rather similar.[17]

Taken together we conclude that as much as the country sample permits us to test our main result, the tests do confirm that the results hold in the long run.

4. Further analysis of robustness and consistency of the main result

A possible objection to the main result in Table 1 is that somehow the OLS results are skewed due to omitted variables. We check the robustness of the results by adding 10 controls, one at the time to avoid multicollinearity. Our controls are either socio-political (see 4.1) or ethno-cultural (see 4.2) variables. They are chosen to have an effect on the degree of democracy that is independent of the income effect or may even dominate the presumed income effect.[18]

Appendix B shows the pairwise correlations between our control variables and our measures of income and democracy. The selected variables are significantly correlated to democracy or income in all but one case. Given that our instruments for income are extreme, it is possible that the inclusion of the control variables may change the estimated income effects. Since we have data for two centuries, we can also use the within-estimator to take the possibility of an omitted-variable bias to its limits (see 4.4), but we find that our main result for the long run is consistent with the pattern both within and between countries.

4.1 Four socio-political control variables (Table 2)

The four socio-political control variables included in Table 2 are the share of mining in GDP (*mining*), the Gini coefficient (*gini*), and the relative numbers of deaths by homicide (*homicavg*) and by suicide (*suicide*). These variables vary widely across countries, and they are all significantly correlated to either polity or income (Appendix B). They may be inter-

17. Going back further in time beyond 1820, income data are available for limited country samples for the years AD 1, 1000, 1500, 1600, and 1700. We find that the correlation between per capita income and our preferred instruments is statistically significant for the years 1500, 1600, and 1700, but not for the years 1 AD and 1000. These results are based on less than 25 observations, but they are in line with the view that our preferred instruments are correlated with per capita income since the onset of modern economic growth about 500 years ago. For the earlier data points that refer to the Malthusian regime of stagnating long-run per capita income, one would probably have to look for a correlation between population size (as a proxy for technology) and our measures of biogeography.

18. For our purpose, it is less important whether the additional control variables are actually exogenous. We are mainly interested in the robustness of our estimated income coefficient.

preted as measuring the availability of resource rents, the degree of income inequality, the prevalence of violent conflict among individuals, or the disposition for psychic depression. We speculate that each of these measures may affect the degree of democracy in ways that are independent of our income measure.[19]

Table 2. The effect of adding socio-political variables to the main model

Dependent variable: *P*	(1)	(2)	(3)	(4)
No. of obs. (countries)	93	72	61	39
Control used in column	*Mining*	*Gini*	*Homicavg*	*Suicide*
	OLS regressions, including one control			
Income, *y*	2.79 (0.45)	3.01 (0.56)	3.27 (0.69)	2.85 (0.81)
Control (of column)	5.87 (7.81)	0.02 (0.06)	0.04 (0.05)	0.03 (0.06)
Centered R^2	0.29	0.30	0.27	0.31
	IV estimates: *y* is instrumented with *biofpc* and *geofpc*			
Income, *y*	2.53 (0.66)	2.81 (0.97)	3.68 (1.18)	2.84 (1.36)
Control (of column)	5.60 (7.84)	0.02 (0.07)	0.05 (0.05)	0.03 (0.07)
First stage partial R^2	0.47	0.33	0.34	0.35
CD F-statistic	39.02	16.83	15.00	9.60
CD critical value (size)	19.93 (10%)	11.59 (15%)	11.59 (15%)	8.75 (20%)
Sargan test (p-value)	1.22 (0.27)	0.90 (0.34)	0.64 (0.42)	1.01 (0.32)
	Hausman test for parameter consistency of OLS and IV estimate			
C-statistic (p-value)	0.27 (0.60)	0.07 (0.80)	0.18 (0.67)	0.00 (1.00)

Notes: See Table 1. In the IV regressions, *y* is instrumented as in column (1) of Table 1.

We find that conditional on the level of instrumented income, none of the controls is statistically significantly correlated with the degree of democracy. Moreover, the inclusion of each of the controls does not significantly affect the size of the estimated income effect. The Cragg-Donald test for weak instruments does not perform as well in these specifications as in Table 1, but the first stage partial R^2 remains relatively high, and the Sargan test statistic does not reject the exclusion restriction.

4.2 Six ethno-linguistic control variables (Table 3)

The six ethno-cultural control variables included in Table 3 are the index of ethno-linguistic fractionalization (*ethnoel*), dummies for French or English legal origins (*lofre* and *loeng*), and

19. Sachs and Warner (1995) use the share of mining in GDP, and Borooah and Paldam (2007) use the share of oil production in GDP as a control for resource rent. Uslaner (2008) shows that a low gini coefficient furthers democracy.

the share of the population that has certain religious beliefs (*prot*, *romcat*, *muslim*). These variables have been used as controls in many other papers. We speculate that the degree of ethnic and linguistic diversity, the origin of the legal framework of a country, or the adherence to a large religious community may affect democracy independent of the measure of income.

Table 3. The effect of adding ethno-cultural variables to the main model

Dependent variable: *P*	(1)	(2)	(3)	(4)	(5)	(6)
No. of obs. (countries)	97	101	101	101	101	101
Control used in column	*Ethnoel*	*Lofre*	*Loeng*	*Prot*	*Romcat*	*Muslim*
	OLS regressions, including one control					
Income, *y*	2.64 (0.56)	2.82 (0.45)	2.82 (0.45)	2.71 (0.45)	2.57 (0.44)	2.25 (0.45)
Control (of column)	-1.05 (2.14)	-0.27 (1.05)	-0.21 (1.19)	3.32 (2.56)	3.76 (1.40)	-5.83 (1.62)
Centered R^2	0.28	0.29	0.29	0.30	0.33	0.37
	IV estimates: *y* is instrumented with *biofpc* and *geofpc*					
Income, *y*	2.16 (1.07)	2.69 (0.69)	2.72 (0.70)	2.61 (0.69)	2.92 (0.62)	2.67 (0.64)
Control (of column)	-2.10 (2.93)	-0.31(1.07)	-0.26 (1.22)	3.44 (2.63)	3.51 (1.44)	-5.28 (1.73)
First stage partial R^2	0.28	0.43	0.42	0.43	0.51	0.50
CD F-statistic	17.96	35.94	35.65	36.07	49.51	49.19
CD critical value (size)	11.59 (15%)	19.93 (10%)	19.93 (10%)	19.93 (10%)	19.93 (10%)	19.93 (10%)
Sargan test (p-value)	1.58 (0.21)	1.69 (0.19)	1.70 (0.19)	1.34 (0.25)	1.32 (0.25)	0.32 (0.57)
	Hausman test for parameter consistency of OLS and IV estimate					
C-statistic (p-value)	0.28 (0.60)	0.06 (0.81)	0.04 (0.85)	0.04 (0.84)	0.65 (0.42)	0.88 (0.35)

Notes: See Table 1. In the IV regressions, *y* is instrumented as in column (1) of Table 1.

We do not find any statistically significant direct or indirect effect of the ethno-linguistic or legal control variables. However, the share of the population with the two Christian beliefs is positively correlated with the degree of democracy, and the share of the population with Muslim belief is negatively correlated with democracy.[20] In both specifications with religious beliefs, the size of the estimated income effect is statistically not significantly different from the results in Table 1, and there is no evidence for weak instruments (CD F-statistic) or a rejection of the exclusion restriction (Sargan statistic).

The key result from both Table 2 and 3 is that the OLS estimates of the income coefficient are the same as the IV estimates, as indicated by the p-value of the C-statistic of the Hausman test for parameter consistency. We conclude from these findings that adding

20. For similar results, see Borooah and Paldam (2007). Note that even if the coefficient to *Prot* is insignificant, it is virtually the same as the one to *Romcat*, and both are statistically different from the one to *Muslim*.

alternative socio-political or ethno-cultural control variables to our basic specification[21] does not alter our statistically significant and quantitatively important estimate of the long-run effect of income on democracy: most of our estimates are in the range of 2.5-3.0.[22]

4.3 Panel data and the time dimension of the Democratic Transition

The paper deals with the long-run effect of income on democracy, and we contend that the long-run effect can be best estimated from a cross-section of countries. However, the long-run must somehow be composed of short-run adjustments, even when these adjustments might be small and have considerable variation, making them difficult to estimate.

Most recent contributions to the literature have concentrated on the shorter run and used panel data sets for the estimation – where each observation is an average over a fairly short time period such as five years. The empirical model used on these data is given by:

$$P_{it} = \alpha_{(i)} + \beta^s y_{it} + \delta P_{it-1} + \varepsilon_{it}, \qquad (5)$$

where $\alpha_{(i)}$ may or may not allow for country heterogeneity, β^s measures the short-run effect of income on democracy, and δ measures the autoregressive adjustment of democracy over time. In this model, the long-run effect of income on democracy is given by

$$\beta^{\infty} = \beta^s / (1 - \delta) \qquad (6)$$

A key result of the previous literature on income and democracy has been that estimates of β from a model like equation (1) are close to the estimates of β^{∞} from models like (5) with a sufficient time period that exceeds five years.[23] Thus, empirical results based on equation (5) can provide considerable evidence for the Democratic Transition.

The *P*-data contains relatively little information: It is a step variable, and most political systems have long periods of stability, so it is often constant. Consequently, estimates of the annual adjustment parameter δ are very close to 1.[24] Therefore, if the panel data are constructed with a short time horizon, such as five years, equation (5) becomes tricky to estimate

21. The same pattern of results holds if we run the specifications of Tables 2 and 3 by using the instrumental variables of columns (2) to (5) of Table 1. These results are available upon request.
22. We have also replicated Tables 1, 2 and 3 for an alternative measure of democracy, the Gastil index. The results are reported in Paldam and Gundlach (2008b). In terms of instrument validity and parameter consistency, they are virtually the same as the estimates for the Polity index.
23. Relations like (5) have been estimated on the Polity index and the Gastil index, with and without fixed effects for countries and for time, and by using different time periods. For a survey, see Paldam and Gundlach (2008a). More details are given in Borooah and Paldam (2007), Paldam (2007), and Jensen and Paldam (2007).
24. The estimate reached in Borooah and Paldam (2007) is δ = 0.965 (0.011).

because it almost contains a unit root. The dominance of the lagged adjustment variable becomes bigger the more the information content in the *P*-data is reduced. If the specification also includes fixed effects for countries and time, the effect of income on democracy becomes small and statistically insignificant, as reported by Acemoglu et al. (2007, 2008).

Consequently, we do not think that evidence based on model (5) with a short time horizon and with fixed effects can be used to reject the Democratic Transition hypothesis. The Grand Transition view and the Democratic Transition hypothesis are about long-run trends that can be best handled by pure cross-section estimates, not by a combination of fixed effects and lagged adjustment over a short time horizon.

4.4 Panel regressions of the cross-country pattern (Table 4)

Panel data increase the sample size by combining cross-sections of countries for different years. Panels also allow us to use fixed effects for countries to separate the pure cross section effects from the effects of adjustment over time. To stay as much as possibly within the long-run framework used till now, we estimate a simplified version of equation (5) *without* the lagged adjustment of democracy.[25] We generate two "symmetrical" sets of estimates, where set (a) uses the variance that is removed in set (b), and vice versa:

(i) Set (a) gives the *within-estimates* of the pure time series effect. These estimates are based on fixed effects for countries that remove the pure cross-country effects; hence our cross-country instruments cannot be used.

(ii) Set (b) gives the *between-estimates* of the pure cross country effect. These estimates use our instruments, but cannot employ fixed effects for countries since the time dimension is removed.

The (unbalanced) panel data set contains the cross-sections of countries for nine selected years: 1820, 1870, 1913, 1938, 1950, 1960, 1973, 1995 and 2003.[26] These years are marked with an X in Figure 2. The first two years – 1820, 1870 – stick out in Figure 2, but the two years are the only ones before 1913 with enough observations of the polity index for running our tests. The other seven years are fairly typical points in the scatter diagram. They are chosen to approximate important breaks in the economic history of the 20th century, such as the two world wars (1913, 1938), the begin of the cold war (1950), the end of colonialism (1960), the first oil crisis (1973), and the demise of the Soviet Union (1995).

25. For a discussion of the effects of the additional inclusion of a lagged adjustment variable and a time-fixed effect, see Gundlach and Paldam (2008b).

26. As noted in footnote 13, the cross sections for the selected years have been constructed by using observations within a time interval of +/- 10 years if income data or democracy data are missing.

Table 4. Within- and between-estimates of the effect of income on the degree of democracy

Dependent variable: *P*	(a) Within-estimates			(b) Between-estimates		
	(1a)	(2a)	(3a)	(1b)	(2b)	(2b)
No. of obs.	785	548	428	572	388	297
No. of countries	157	156	156	103	102	102
Income, *y*	3.15 (0.29)	2.19 (0.56)	2.76 (0.85)	2.85 (0.77)	2.75 (0.60)	2.82 (0.58)
Instruments	no	no	no	yes	yes	yes
Country-fixed effects	yes	yes	yes	no	no	no
Included cross sections	all nine years	1960/1973/ 1995/2003	1973/ 1995/2003	all nine years	1960/1973/ 1995/2003	1973/ 1995/2003
CD F-statistic	-	-	-	27.55	41.29	40.95
CD critical value (size)	-	-	-	19.93 (10%)	19.93 (10%)	19.93 (10%)
F-test (p-value)	3.96 (0.00)	3.63 (0.00)	3.01 (0.00)	-	-	

Notes: Standard errors in parentheses. All specifications include a constant term (not reported). For the within-estimates (country fixed effects), the F-test rejects the hypothesis that all country-specific constant terms are jointly zero (p-value in parentheses). In columns (4)-(6), we use our preferred instruments *biofpc* and *geofpc*, which pass the Cragg-Donald (CD) test for weak instruments.

Table 4 reports the pair-wise estimates of sets (a) and (b) for three panels: (1) includes all 9 cross-section years; (2) only looks at the period since 1960; and (3) is shortened to the years since 1973. The estimate of the income effect till now has been a little below 3 such as 2.8. None of the six estimates in Table 4 deviates significantly from that, and most are actually very close, which implies that the within-estimates and the between-estimates produce about the same effect of income on democracy. This result further strengthens the empirical support for the Democratic Transition hypothesis.

Appendix C discusses the possibility that our instruments identify exogenous variation in a broad measure of the institutional framework of a country that works through income, so that the relation we have estimated could turn out to be spurious. The problem is that we do not have two independent instruments for measures of income and institutions that would be needed for a direct test. However, we find that our instruments are better suited to identify the exogenous variation in income than the exogenous variation in institutions, which we read as indirect evidence in favor of the Democratic Transition hypothesis.

5. The Democratic Transition

Our analysis takes income and the Polity index as the best aggregate representations of the economic and political development. Hence, we have shown that the economy dominates

politics in the long run.[27] We have added a number of qualifications as regards the short to medium run, but in the long run institutions and critical junctions apparently do not influence the outcomes with respect to the political development. Our empirical results suggest that there is a large robust effect of the level of income on the degree of democracy.[28] As is also highlighted by Figure 1, income is certainly not the only determinant of democracy, but it appears to be a rather powerful predictor.

Overall, we confirm Lipset's original finding (see Lipset 1959 and Barro 1999), though by a new empirical strategy. We think that our strategy can also be applied in other fields where the direction of long-run causality is contested. For instance, we have shown that the long-run causality is exclusively from income to (less) corruption, and hence we add the Transition of Corruption along with the Democratic Transition to the set of transitions that constitute development (Gundlach and Paldam 2008a).

There are two strong and politically important conclusions from our results. The positive conclusion is that countries are likely to transform their political regime towards democracy with rising levels of per capita income. Of course this does not mean that all countries will always become democracies once they have reached a certain level of development, but a political change towards democracy becomes increasingly likely as countries become wealthy.

However, there is also a downside to our results: We should be pessimistic about attempts to impose a democratic regime in poor countries by outside force. We think that such a regime change cannot be expected to last if a self-contained growth process is missing. The Critical Junctures hypothesis would probably be more optimistic on this point if the change in democracy would reflect a genuine change in the power structure of the political system, but our aggregate results suggest otherwise. We would predict that the degree of democracy of a country is mainly determined by its income level, so the political regimes of poor countries are likely to revert to a level of autocracy that is compatible with their level of development.

At the end, we may turn from the specific hypothesis to the broader views: We conclude that we have found a large piece of evidence that the Grand Transition view provides a better understanding of the broad pattern of development than the Primacy of Institutions view.

27. This finding has a curious parallel to the theory of Economic Determinism that originated from Karl Marx.
28. An estimate of 3 is within the 95 percent confidence interval of most of our results. It predicts that the income difference between the 25th and the 75th percentile of our samples would result in a change of the degree of democracy of 5 (OLS) to 4.5 (panel data) score points. This is between about one half and one third of the observed difference in the degree of democracy between the 25th and the 75th percentile of our samples.

Appendix A: Definitions and sources of variables used in the tables

Dependent variable and main explanatory variable used in all tables	
P	Combined polity score. Source: Marshall and Jaggers (2006) and Polity homepage.
y	Natural logarithm of GDP per capita, measured in 1990 international Geary-Khamis dollars. Source: Maddison (2003) and Maddison homepage.
Biological instruments used in Table 1. Tables 2 and 3 use only *biofpc*	
animals	Number of domesticable big mammals, weighing more than 45 kilos, which are believed to have been present in various regions of the world in prehistory. 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 (malaria ecology); 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), here taken from www.earth.columbia.edu/about/director/malaria/index.html#datasets (data as of 27 October 2003).
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).
Geographic instruments used in Table 1. Tables 2 and 3 use only *geofpc*	
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).
geoav	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).
Socio-political control variables used in Table 2	
mining	Share of GDP in the mining and quarrying sector, approx. 1988. Source: Hall and Jones (1999).

gini	Gini coefficient, approx. 1990. Source: Deininger and Squire (1996).
homicavg	Total intentional completed homicides per 100,000 population, average for 1990-2000. Source: UNODC (2005).
suicide	Total number of suicides per 100,000 population, estimates for early 1990s. Source: Parker (1997).
Ethno-cultural control variables used in Table 3	
ethnoel	Average value of five different indices of ethnolinguistic fractionalization: the probability that two randomly selected persons from a given country: (i) will not belong to the same ethnolinguistic group, (ii) will speak different languages, (iii) will not speak the same language; (iv) the percentage share of the population not speaking the official language; (v) and the percentage share of the population not speaking the most widely used language. Source: La Porta et al. (1998).
loeng	Dummy for English Common Law legal origin of the Company Law. Source: La Porta et al. 1998.
lofre	Dummy for French legal origin of the Commercial Code: La Porta et al. 1998.
muslim	Share of the population with Muslim religious belief. Source: La Porta et al. (1998).
prot	Share of the population with protestant religious belief. Source: La Porta et al. (1998).
romcat	Share of the population with roman-catholic religious belief. Source: La Porta et al. (1998).
Additional variables used in Appendix C	
exprop	Index of protection against expropriation 1985-1995. Source: Taken from Acemoglu et al. (2001).
kaufavg	Unweighted average governance indicator for the year 1996, based on six survey measures: voice and accountability, political stability, government effectiveness, regulatory quality, rule of law, and control of corruption. Source: Kaufmann et al. (2004).
lnmort	Natural logarithm of settler mortality rates in European colonies in the early 19th century; fourth mortality estimate. Source: Acemoglu et al. (2001)
socinf	Index of social infrastructure, calculated from the years that a country is open to international trade, law and order, bureaucratic quality, corruption, risk of expropriation, and government repudiation of contracts. Source: Hall and Jones (1999).

Appendix B

Table A1. Correlation of control variables with democracy and income

	P (polity)			*y* (income)		
	Coefficient	p-value	No. of obs.	Coefficient	p-value	No. of obs.
Socio-economic control variables						
mining	-0.27	0.00	126	0.01	0.87	120
gini	-0.15	0.17	87	-0.31	0.00	85
homicavg	0.03	0.78	94	-0.20	0.05	91
suicide	0.37	0.01	50	0.43	0.00	48
Ethno-cultural control variables						
ethnoel	-0.28	0.00	128	-0.55	0.00	123
lofre	-0.09	0.27	157	-0.17	0.03	152
loeng	0.03	0.68	157	-0.01	0.86	152
prot	0.32	0.00	155	0.24	0.00	151
romcat	0.40	0.00	157	0.20	0.01	152
muslim	-0.57	0.00	157	-0.26	0.00	152

Note: Coefficient of correlation; the *p*-value gives the level of statistical significance. All observations for 1995 or the next available year.

Appendix C: The simultaneity of income and some other institutions

Income and institutions have developed simultaneously through economic history. One may therefore object that our main result reflects, at least partly, the effect of the institutional power structure on democracy rather than a pure income effect. For instance, if we delete the regression constants to simplify and modify our equation (1) as

$$P_i = \beta y_i + \gamma inst_i + \varepsilon_i, \tag{A1}$$

where *inst* is a measure of the institutional structure of a country, and subsequently rewrite equations (2) and (3) as

$$y_i = \eta inst_i + u_i \quad \text{and} \tag{A2}$$

$$inst_i = \mu biogeo_i + \nu_i, \tag{A3}$$

with η and μ as parameters and u and ν as error terms, then the reduced form follows as

$$P_i = \mu(\beta\eta + \gamma) biogeo + \omega_i \tag{A4}$$

with ω_i as a modified error term. The reduced form shows that our main result could reflect, at least in principle, that there is no direct effect of income ($\beta = 0$), but instead an effect of the institutional structure on democracy that works through income ($\gamma > 0$). Since we do not have two independent instruments that would disentangle the separate effects of income and institutions on democracy in equation (A1), we have to rely on indirect estimates to see whether our main result could imply that $\beta = 0$ and $\gamma > 0$, as in the Critical Junctures hypothesis.

If our instruments are too weak to identify the exogenous variation in the measure of institutions in equation (A2), we would have indirect evidence that our estimates in Table 1 cannot *only* be due to the effect of institutions on democracy. Put differently, finding that our instruments do not help to predict the effect of institutions on income in equation (A2) would imply that our main result in Table 1 must reflect, at least partly, a direct effect of income on democracy ($\beta > 0$), as in the Democratic Transition hypothesis.

The main empirical problem is that the two competing hypotheses should be estimated on different samples. But with different samples, different results may not reveal which hypothesis prevails. Acemoglu et al. (2001) emphasize that the effect of institutions on income (Critical Junctures) can only be estimated from a country sample that excludes the former colonial powers, which are all rich countries today. Yet the effect of income on democracy can only be estimated from a sample that includes poor *and* rich countries, as shown with Figure 3. Hence, it is a priori unlikely that we can estimate the Democratic Transition hypothesis from a sample of former European colonies that does not include rich countries as well.

To allow for at least some rich countries in the sample, we therefore use the instruments employed in specification (2) of Table 1, which includes observations for the former colonies Australia, Canada, New Zealand, and the United States. Another constraint for the sample of former European colonies is given by the measure of institutional quality. For instance, the measure employed by Acemoglu et al. (2001) includes only a few Sub-Saharan countries. Therefore, we use the measure of social infrastructure proposed by Hall and Jones (1999), which includes a regionally more balanced sample of former colonies.

The first column in Table A2 reproduces the result reported by Acemoglu et al. (2001), here based on different income data and on the Hall-Jones measure of institutional quality (*socinf*). There is a strong effect of the measure of institutional quality on income, which is identified by the instrumental variable (log) settler mortality in the early 19th century (*lnmort*)

as used by Acemoglu et al. (2001). There is no evidence for a weak instrument problem according to the Cragg-Donald statistic. The OLS estimate of the regression coefficient is statistically significantly smaller than the IV estimate, as indicated by the p-value of the Hausman C-statistic, which is also in line with the original results reported by Acemoglu et al. (2001). Hence, the results of column (1) support the Critical Junctures hypothesis, suggesting that there is a causal effect of institutions on income.

Table A2. Estimating the relative performance of the Democratic Transition hypothesis

	Dependent variable: *y*		Dependent variable: *polity*	
	(1)	(2)	(3)	(4)
No. of obs. (countries)	65	73	73	69
	IV estimates			
socinf (IV)	6.30 (0.87)	7.78 (1.39)	2.20 (1.02)	-3.28 (2.76)
Instruments	*lnmort*	*bioavg, geoavg*	*bioavg, geoavg*	*biofpc, geofpc*
First stage partial R^2	0.34	0.18	0.39	0.13
CD F-statistic	32.86	7.61	22.16	4.71
CD critical value (size)	16.38	19.93	19.93	19.93
Sargan test (p-value)	-	0.34 (0.56)	9.98 (0.01)	0.79 (0.38)
	OLS estimates			
socinf (OLS)	4.13 (0.43)	4.08 (0.40)	2.28 (0.63)	1.91 (0.75)
Centered R^2	0.58	0.59	0.15	0.09
	Hausman test for parameter consistency of OLS and IV estimate			
C statistic (p-value)	12.9 (0.00)	19.6 (0.00)	10.0 (0.01)	8.19 (0.02)

Notes: See Table 1 and text. The samples are restricted to former European colonies.

In terms of equations A1-A3, this finding implies that $\gamma > 0$, but this does not mean by itself that the Democratic Transition hypothesis ($\beta > 0$) is rejected. Column (2) tests whether our instruments also identify a causal effect of institutions on income, which would be a necessary condition for $\beta = 0$ in light of our results in Table 1. This is not the case for the selected specification. Our instruments (*bioavg*, *geoavg*) are rejected as weak by the Cragg-Donald statistic, which implies that our instruments are not well suited to identify a causal effect of a broad measure of institutions on democracy that works through income as in equations A1 and A2. Hence, the results in column (2) fail to reject the hypothesis ($\beta > 0$).

Column (3) replicates our main specification for the sample of column (2), i.e. a sample of former European colonies that includes the neo-Europes Australia, Canada, New Zealand, and the United States. Here our instruments are not rejected by the Cragg-Donald test, but the

Sargan test for overidentification rejects the exclusion restriction and the Hausman test rejects the hypothesis of the similarity of the OLS and the IV estimate, though the bias appears to be small. Taken at face value, the results of column (3) suggest that $\beta > 0$ and hence support our main results presented in Table 1. However, these results depend on the inclusion of the rich neo-Europes. This is shown by column (4), where we use our preferred instruments *biofpc* and *geofpc* (see Table 1, column (1)) and find them rejected for the sample that excludes Australia, Canada, New Zealand, and the United States.

Overall, these findings leave us with a somewhat unsatisfactory result. We do find that our instruments work for samples that include a sufficient number of rich and poor countries, but the robustness of this result cannot be tested because alternative measures of the explanatory variables and alternative instruments can only be employed for samples which lack a sufficient number of rich or poor countries. We interpret the results of this section as suggesting that there is indirect evidence in favor of $\beta > 0$. Hence, we maintain that $\beta > 0$, which is in conflict with the Critical Junctures hypotheses but in line with the Democratic Transition hypothesis.

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