Do Educated Leaders Matter? *

Timothy Besley
LSE and CIFAR

Jose G.Montalvo
Universitat Pompeu Fabra and IVIE

Marta Reynal-Querol
Universitat Pompeu Fabra-ICREA, CEPR and CESifo

February 5, 2011

Abstract

This paper uses data on more than one thousand political leaders between 1875 and 2004 to investigate whether having more educated leaders affects economic growth rates. We exploit an expanded set of random leadership transitions due to natural death or terminal illness and first show that the individual characteristics of leaders matter for growth. We then provide evidence supporting the view that heterogeneity among leaders' educational attainment is important and that growth is enhanced by having leaders who are more highly educated.

*Paper prepared for a conference in honour of Angus Deaton. We are grateful to the conference participants, and especially Angus Deaton and Steve Pischke for useful suggestions. Anne Brockmeyer provided helpful comments on an earlier draft of the paper. We thank Florencia Abiuso for excellent research assistance. Besley thanks the Economic and Social Research Council and CIFAR for funding. Reynal-Querol is grateful for funding from the European Research Council under the European Community's Seventh Framework Programme (FP7/2007-2013)/ERC grant agreement n. 203576.Reynal-Querol and Montalvo acknowledge the financial support of the grant SEJ2007-64340 from the Spanish Ministerio de Educación, the Barcelona GSE Research Network and the Government of Catalonia.

1 Introduction

One of the most robust findings in empirical research is the importance of education in explaining economic outcomes. There is overwhelming evidence that education affects earnings – see, for instance, the summary in Card (1997). It has also been shown that education has an impact on charitable giving and other measures of citizenship – see, for example, Dee (2004) and Milligan, Moretti and Oreopoulos (2004). This paper examines this issue in a new context. It investigates how the educational level of a political leader affects aggregate economic growth in the country during his time in office.

The core data set for his study is a sample of more than one thousand political leaders who have been in office between 1875 and 2004. We also use educational data for these leaders which has been collected by Besley and Reynal-Querol (2009). Drawing on a variety of empirical tests, we begin by showing that the individual characterisics of leaders matter in the sense that there are significant leader fixed effects on growth. For this exercise, we follow Jones and Olken (2005) by expanding their data set on leaders who left office due to natural death or terminal illness. Our expanded time frame gives 217 such exits from office compared to the 77 exits in the post-WWII sample used by Jones and Olken. Exploiting the expanded data, we find a strong negative effect on growth of a random exit from office. We then look for heterogeneity according to educational attainment, using information on whether or not the leader has obtained a post-graduate qualification. We find robust evidence that growth in general is higher when leaders are more highly educated. When looking at heterogeneity among random exits according to educational attainment, we find some evidence to support the view that the decrease in growth is larger / growth falls by more when an educated leader leaves office.

The remainder of the paper is organized as follows. In the next section, we discuss the background and related literature. Section three discusses the data and section four the empirical methods. Section five presents the results and section six concludes.

2 Background and Related Literature

This paper contributes to a growing body of literature on how the characteristics of policy makers matter for policy outcomes. From a theoretical

point of view, this can be motivated using the citizen-candidate type approach as presented in Besley and Coate (1997) and Osborne and Slivinski (1996), who model political competition as a game between citizens competing to hold public office. In such a context, and with limited commitment, selection based on policy preferences, talent or virtue can affect policy outcomes. This approach has provided the motivation for studies on the effect of political reservation (i.e. reserving political office for particular groups in the population) by Pande (2003) who studies caste reservation and Chattopadhyay and Duflo (2004) who study reservations for women. Both argue that reservation matters by changing the identities of those elected to office. Lee, Moretti and Butler (2004) use U.S. data on close elections to argue that political affiliation matters.

The quality dimension in political selection has been studied in a citizen-candidate framework by Caselli and Morelli (2002) and Poutvarra and Takalo (2003). Caselli and Morelli (2002) argue that the key issue is to understand the factors which affect the supply of bad politicians, such as the rents that they can earn while in office. Imperfect information may also affect the incidence of bad politicians by making it difficult to spot candidate quality. Poutvarra and Takalo (2003) develop a model in which the value of holding office impinges on candidate quality via its effect on election campaigns. Gehlbach, Sonin and Zhuravskaya (2009) ask under what circumstances economic elites (such as businessmen) decide to run for political office.

Closely related to our paper is work by Jones and Olken (2005) who show, using methods that we discuss below, that the quality of leaders matters for growth, although they do not provide evidence on the exact mechanism for this. One view suggests that some leaders are more competent than others and more able to make sensible economic policy choices which enhance economic performance. Related to this is the possibility that some leaders focus on broad based economic objectives rather than promoting narrow sectional interests which will tend to favor protectionist policies. Some leaders may also be more inclined to advocate the provision of public goods and infrastruture which has wide economic benefits. In all three cases, the return to having a more educated leader comes from the assumption that more highly education leaders are also better citizens and more likely to operate in the broader public interest.

We follow the lead of Jones and Olken (2005) in looking at leaders who exit from office randomly. They argue that in these cases, the timing of the transitions between leaders can be treated as exogenous, determined by the

death of the leader rather than by underlying country-specific conditions. By studying a fixed period of time before and after the transition, the method can also help to mitigate concerns about endogeneous selection into holding office.

Our view that educated leaders may be more publicly-spirited relates the paper to a growing literature which looks beyond the economic returns to education and seeks to investigating how education relates to citizenship. In this context, Dee (2004) finds that educational attainment has a large and statistically significant effect on voter participation and support for the freedom of speech. He also finds that additional schooling increases the quality of civic awareness as measured by the frequency of reading newspapers. Milligan, Moretti and Oreopoulos (2004) looks at the effect of extra schooling induced by compulsory schooling laws on the likelihood of becoming politically involved in the U.S. and U.K. They find that, in both of the countries, educational attainment is positively related to several measures of political interest and involvement.

Viewing leadership as an expression of responsible citizenship goes back to Plato and Aristotle. For Plato, a major concern is the possibility that leadership could degenerate if the leader does not separate his personal interests from the welfare of those he governs. In addition, intelligence is central to the Platonic view of leadership, so the idea that more educated citizens could be better leaders would come as no surprise. However, the link between education and leadership need not be causal. In the economics literature, there is a general concern with "ability bias" in the sense that more able people seek higher levels of education than less able people in order to signal their ability to future employers. In assessing the impact of education on earnings, there could be an analogous "pro-social bias" whereby individuals choose to become more educated as a conscious or unconscious manifestation of their concern for social welfare.

Whether it is through raw talent or concern for social welfare, these results would explain why more educated leaders are better leaders. The question is whether the impact of the leader's eduation is strong enough to show up significantly in explaing their performance.

3 Data

Our core data, based on the Archigos data set, identify the primary ruler in each country and year between 1875 and 2004.[1] In countries that have more than one head of state, the Archigos data identify the effective ruler based on the characteristics of the political system in place. Two rules are generally followed: (i) in Parliamentary regimes, the prime minister is coded as the ruler while in Presidential systems, it is the president; (ii) in communist states, the Chairman of the Party is coded as the effective ruler.

As described in Besley and Reynal-Querol (2009), we supplement the Archigos data with other sources, especially Ludwig (2002). Our final data set contains information on leaders' educational attainment for a core sample of 1654 leaders in 197 countries between 1848 and 2004. Following Ludwig (2002), educational attainment is measured using a discrete variable which can take one of eight values denoting the highest educational attainment of a leader: illiterate (no formal education); literate (no formal education); grade /elementary /primary school or tutors; high /finishing /secondary /trade school; special training (beyond high school), such as mechanical, nursing, art, music or military school; college; graduate or professional school (e.g. master's degree); doctorate (e.g. PhD). Our core variable measuring whether or not a leader is highly educated is a dummy variable equal to 1 if the leader is in either category 7 or 8, i.e. has a post-graduate qualification. We will refer to this variable as "graduate education".

When we control for democracy below, we use the Polity IV data base. It provides a definition of democracy that captures different dimensions: how competitive and open the recruitment of chief executives is; the extent to which the chief executive is constrained institutionally; and how competitive and regulated political participation is. This core variable ranges from –10 to +10. Following a long line of research by economists, e.g. Persson and Tabellini (2005), we define a country to be democratic if the variable POLITY2 is positive. Data on per capita income comes from the well-known

[1] Archigos has two datasets: the long one, which gives information on leader-year-country, and the short one, which gives information on leader-country. In the short dataset there are 95 leader-country points that do not appear in the long dataset. We include these 95 points in the long dataset, and in the long format. (leader-year-country). (These 95 country-leaders points correspond to the following countries: Barbados, Bahamas, Belize, Brunei, Cape Verde, Iceland, Luxemburg, Maldives, Malta, Montenegro, Solomon Islands, Suriname, Tiber, Transvaal, Zanzibar)

Maddison (2003) data set. This is the only widely available data which has coverage for a sufficiently long period of time.

Table 1 provides summary statistics of our variables. On the left hand side, we find the mean of the variables for all those observations for which we have data on growth. In this sample, the leader has a post-graduate qualification in 26% of the years and a college education in 66% of the years. The average age of a leader is 57 years, and the average number of days in office is 2449, which is about 6.7 years. In 46% of the country-year observations the regime is democratic. On average the population has 4.8 years of education.

On the right hand side of table 1, we report economic growth averaged over countries and years according to different types of leaders and political regimes. On average, countries had growth rates of 1.5% per annum. Democracies have grown faster than autocracies, and countries in which the leader had a post-graduate qualification have also grown faster than countries whose leaders did not have a post-graduate graduate qualification.

Figure 1 shows the distribution of leaders according to their educational attainment. The modal leader has a college education with education skewed towards having a graduate education. Figure 2 looks at the variation over time in the proportion of leaders who have a college education and a post-graduate qualification. College education has clearly grown over time, but the trend in post-graduate education is less discernible. Recall that our main variable of interest is graduate education (i.e., more than college education).

For all leaders, we collected information on how each of them lost power using the Encyclopedia of Heads of States and Governments, Oxford Political Biography: Who is Who in the Twentieth Century World Politics and the Encyclopedia Britannica. It is important for the identification strategy that the timing of these leadership transitions is unrelated to underlying economic and political conditions. The leaders that we focus on are those that left office due to natural death, accident or serious illness such as a stroke. We will refer to such cases as random leadership transitions. Among the 2075 leaders in the Archigos list, 217 of them lost power by one of the above mentioned causes. There are 32 leaders that died very shortly after the death of a previous leader. For such cases, we follow Jones and Olken (2005) and drop the second leader when estimating the leader's impact on growth because we can safely assume that the second leader was in office for too short a time to have had an impact on economic growth. We therefore focus on 185 leaders

in our empirical analysis when using only random transitions. The data appendix lists the random transitions and their reasons for leaving office. It also discusses the differences between our sample and the one used by Jones and Olken (2005).

We have information on the education of 158 out of the 185 leaders who leave office randomly. Among these, 27 (17%) have a post-graduate qualification. However, once we take account of missing values in the average growth rate before and after the random transitions, our sample is reduced to 115 leaders. Among this group, 23 leaders (20%) have a post-graduate qualification while 68 have at least a college degree. In the full sample, 26% of the leaders have a graduate degree, so the sample based on random transitions does not seem to be biased towards high or low educational attainment among leaders.

4 Estimating Leader Quality

The aim of our exercise is to try to measure the contribution of a leader to a country's performance and to assess how this relates to the leader's educational attainment. Our empirical approach is essentially based on two methods. First, we estimate leader fixed effects. Second, we estimate the impact of random leadership transitions on economics outcomes.

To obtain leader fixed effects, we estimate the following model:

$$\Delta y_{i\ell t} = \zeta_\ell + \theta_i + \gamma_t + X_{it}\beta + \epsilon_{i\ell t} \tag{1}$$

where $\Delta y_{i\ell t}$ is the growth rate in country i at date t when leader ℓ is in office. On the right hand side, we include year fixed effects γ_t, country fixed effects θ_i and other time varying characteristics X_{it}that we think could affect the growth rate which we discuss below. The key variables are the leader fixed effects ζ_ℓ. For Charles De Gaulle, for instance, this variable takes value 1 in France from 1958 to 1968, and value 0 in all other countries and years. The estimation of the leader fixed effect is not possible for leaders who have been in office for only a single year. We also cannot identify the leader fixed effects in countries that had only one political leader during the time period that the country enters our data, since the fixed effect for this leader cannot be separated from the country fixed effect.

In a well-known paper, Bertrand and Schoar (2003) estimate (1) to evaluate the effect of CEOs on firm performance. In their case, i refers to a

firm. To allow for the identification of λ_ℓ and θ_i, the data used to estimate this model need to cover CEOs who have worked in more than one firm. In our case, this would mean having leaders who have served in more than one country, which can obviously not be the case. Instead, our identification strategy relies on the analysis of political leaders who stay in power for only a subset of the sample years available for each particular country.

Two other key issues arise when estimating this model. The first issue is the plausibility of our identification condition. If we want to interpret the leader fixed effects as the leader's quality, we need to assume that there are no other unobservable country-time specific effects.[2] To avoid the omission of variables that could act as unobservable time-varying factors, we introduce controls such as GDP per capita. It is unlikely that this identification strategy allows for the estimation of the causal effect of the identity of the leader on the outcome of the country. At most, we can asses if there is evidence that a country's performance is systematically associated with the identity of its leader.

The second possible issue is sample selection. Let $\sigma_{i\ell t}$ be an indicator variable denoting whether a leader ℓ is selected in country i at date t and assume the set of leaders $L(i)$ in each country i is picked according to the following process:

$$\Pr ob\left\{\sigma_{i\ell t}=1\right\}=F_{it}\left(Z_\ell\right)+\eta_{i\ell t}$$

for some selection function $F_{it}(\cdot)$, where Z_ℓ are leader "qualities" and $\eta_{i\ell t}$ is a stochastic element. When we estimate

$$E\left(\lambda_\ell:\sigma_{i\ell t}=1\right)=G_{it}\left(Z_\ell,\eta_{i\ell t}\right)$$

our concern is that $\eta_{i\ell t}$ is correlated with $\varepsilon_{i\ell t}$ in (1). In other words, leadership transitions may not be exogenous.

If we analyze a subsample of leaders who randomly abandon power, we may have a chance to attenuate the selection problem and, at the same time, mitigate the identification problem since we will not be comparing all the leaders in all the countries. For this reason, we will estimate our initial model both on the full sample and on the subsample of leaders who exogenous exit office.

The regression on the full sample of leaders will provide the benchmark case. Next we consider only random leadership transitions but without sepa-

[2] Bertrand and Schoar (2003) note that using this identification strategy with their data yields the same results as when using workers who have moved across firms.

rate fixed effects for the pre and post exit periods. Third, we use the sample of random transitions. Finally, we will estimate the effect of a random death by comparing the outcome variable before and after the transition. In this final exercise we follow the method outlined in Jones and Olken (2005), based on averaging the outcome variable over the T years before the death of the leader (PRE period) and the T years after the death (POST period). The subindex z represents a particular random transition.

$$\begin{aligned} \overline{PRE}_z &= 1/T \sum_t Y_{zt}^{PRE} \\ \overline{POST}_z &= 1/T \sum_t Y_{zt}^{POST} \end{aligned}$$

Under the null hypothesis that the identity of the leader does not have any effect on outcomes, we have

$$\overline{POST}_z - \overline{PRE}_z \sim N\left[0, \frac{2\sigma_{\epsilon i}^2}{T}\right]$$

where the variance $\sigma_{\epsilon i}^2$ is country specific.

To implement this econometric test, we estimate the following model:

$$\Delta y_{izt} = \alpha_i + \lambda_z^{PRE} PRE_{zt} + \lambda_z^{POST} POST_{zt} + \gamma_t + \epsilon_{izt} \tag{2}$$

where z indexes the random transitions and annual growth is measured from the Maddison data. For each leaders' random transition there is a separate set of dummies, denoted by PRE_z and $POST_z$. PRE_z is a dummy equal to 1 in the five years prior to leader z's random transition in that leader's country. $POST_z$ is a dummy equal to 1 in the five years after leader z's random transition. The procedure estimates a separate coefficients λ_z^{PRE} and λ_z^{POST} for each random transition z. PRE_z and $POST_z$ are defined so that the actual year of random transition is not included in either dummy.

After estimating (2), we construct a chi-squared test of the equality of the mean of the outcome variable in the PRE and the $POST$ period for all the random transitions, using the Wald statistic:

$$W = \frac{1}{N_z} \sum_{z=1}^{N_z} \frac{(\overline{POST}_z - \overline{PRE}_z)^2}{2\widehat{\sigma}_{\epsilon i}^2/T} \tag{3}$$

where $N_z = \sum_{\ell=1}^{L} 1(\ell = z)$. Under the null hypothesis, the product $N_z * W$ follows a $\chi^2_{N_z}$.

We will compare the results obtained using leader fixed effects in different samples with the specification in Jones and Olken (2005). Notice that testing the null hypothesis that leader identity does not matter is equivalent to testing whether the leader fixed effects are equal to 0. In the specification of Jones and Olken (2005) the null hypothesis is described as a zero difference of the outcomes before and after each random death. Therefore, the test verifies the equality of the effect of two consecutive leaders before and after a random transition in a particular country.

5 Results

We discuss our results in two parts. We first investigate whether the identity of the leader matters at all, contrasting different ways of looking at this. We then examine whether growth performance differs by the educational attainment of leaders.

5.1 Does leader identity matter?

Although the objective of the paper is to discuss the importance of leader's education on the economic performance of countries, it is convenient to compare the basic results obtained with our new dataset with previous findings in the literature. For this reason in this section we deal with the question of the relevance of leaders in explaining the economic performance of countries. Our first results are presented in table 2. Our objective here is to explore a series of specifications, moving from a broad sample to a more conservative specification to investigate whether the identity of political leaders matters. For each specification, we report two sets of estimates depending on whether or not we account for country-specific heteroscedasticity. **We assume that there is country-specific heteroscedasticity**[3] **and country-specific autocorrelation**[4]. The estimated variances-covariance matrices are used to obtain GLS estimators.

[3] The homoskedasticity assumption is rejected in all the specification.

[4] In a previous version of the paper we consider only country-specific heteroskedasticity.

We begin by discussing the results in panel A of table 2. The first two columns in table 2 uses the full sample of leaders to estimate leader fixed effects. We report an F-test of the hypothesis that all leaders are equally good, i.e. the leader fixed effects are equal to zero. The results show that we cannot reject the hypothesis that leader identity matters. However, interpreting these results as an indication of the importance of leader quality would abstract from the endogeneity and sample selection problems that we discussed in the previous section. Columns (3) and (4) in table 2 therefore focus only leaders who die or leave office due to random causes.[5] **However, tha transition point is still endogenously determined and, therefore, we offer these results as a matter of comparison.** The F-statistic once again allows us to test whether all leader fixed effects are equal to zero. We still find that leader identity matters. Interpreting the leader fixed effects as measures of leader quality is more persuasive in this second model, since here leaders do not leave office for reasons associated with a country's growth performance. **We should notice that none of the exercises in the initial columns of table 2 are econometrically sound. In all the cases there is an endogeneity problem of some kind or another.**

In columns (5) and (6) of table 2, we focus again on the restricted sample of random transitions. However, we now use dummies for a fixed number of years before and after the random transition as in Jones and Olken (2005). We hope that this will help us solve both the endogeneity and sample selection problem. In both cases, the F-test on the leader fixed effects does suggest that leader identity matters. Thus, regardless of which sample we look at, we find that leader identity matters. Restricting our attention to the subsample of leaders who left office due to random causes does not appear to affect this conclusion.

In column (7) **and (8)**, we use the Jones and Olken (2005) methodology to estimate the effect of a random leadership transition on growth. For each transition, the test compares the growth rates in the pre and in the post transition periods. **Column (7) uses only a correction for heteroskedasticity while column (8) corrects also for first-order country-specific autocorrelation.** The results indicate a 0.02% reduction in the annual growth

[5]When we refer to the sample of random transitions we consider only the effect of the two leaders that correspond to those transitions but we include all of the leaders to estimate the country and year fixed effects.

rate during the five years following a random leadership transition. This small negative effect is in line with the results on the post-war sample in Jones and Olken (2005).

Panel B assesses the robustness of the results to controlling for the beginning-of-period value of GDP per capita. **This lagged dependent variable is introduced purely as a control for unobservable time-varying factors. In columns (7) and (8) we should interpret the test as the difference in the pre-transition period versus the outcome of the post-transition period conditional on the initial level of GDP per capital.** The results are essentially identical to the ones we already describe for panel A. They show that leader identity matters.

5.2 Education and Leader Quality

Having established that leader identity matters, we now assess the evidence on whether leaders' educational attainment affects the quality of leadership.[6] We estimate the impact of leaders' education on growth by including our key education variable as a regressor. Following Bertrand and Schoar (2003), we check the robustness of our findings to controlling for other leader characteristics such as age and the length of tenure of the leader, and country characteristics such as democracy and the average level of education of the population. We then look at the impact of leaders' education on economic growth using evidence from random leadership transitions.

For the purposes of this exercise, it is not entirely clear what level of educational attainment may matter. We first use the core variable "graduate education" from Besley and Reynal-Querol (2009). This variable takes value 1 if the leader has a post-graduate education, i.e. beyond college, 0 otherwise. Using this measure, Besley and Reynal-Querol (2009) showed that leaders selected in democracies are more likely to have a post-graduate qualification. In many ways, our measure of education parallels Bertrand and Schoar (2003)

[6]Notice that separating the sample according to educational groups or including an explanatory variable to measure education may yield biased results. The difference between the effect of educated and uneducated leaders contains a sample selection term: the difference between the expected growth rate that an educated leader would have obtained had he been less educated minus the expected growth rate of a leader that is less educated. An uneducated person that becomes a leader probably has some unobservable skills which make her more able to produce better outputs than what would happen with an educated leader had she been less educated. Under this interpretation, if there is sample selection then the causal effect of graduate leaders will be smaller than our estimate.

who use a dummy equal to 1 if a CEO has an MBA. We then assess the robustness of our results by replacing the graduate education dummy with a college education dummy, which takes value 1 if the leader has a college or post-graduate education, 0 otherwise.

We now discuss evidence on the importance of education using random transitions in leadership due to the death or illness of the leader. We will disaggregate the effects of such transitions on growth by education sub-groups. Specifically, we estimate model (2), but allow for the pre and post growth effects to be heterogeneous according to the leader's educational attainment. Once again, we will use the post-graduate dummy as our measure of education. In each case, we will report the Wald statistic corresponding to (3) to see whether the difference between pre and post growth effect is significant. We will also explore whether the point estimates differ between education sub-groups.

The core results are presented in table 3 panel A. The left part of panel A shows that there is a negative and significant effect on growth when a leader with a post-graduate qualification leaves office due to death or illness. This effect is larger in size than that in column (7) of Table 2. On average, the departure of an educated leader leads to a 0.713 percentage point reduction in growth. This contrasts with the reduction of just 0.05 percentage points after the death of leader who does not have a post-graduate qualification. The right hand part of panel A reproduces these same statistics using the college education dummy. Here, we find that the growth effect of a college educated leader leaving office is similar to that of a leader with post-graduate qualification leaving office. Note, however, that growth now appears to increase when a non-college educated leader leaves office. This last result can be interpreted in the following way: a non-college education leader is even less educated than a leader who has not obtained a graduate education (right hand side of panel A). The exit from office of a little educated leader leaves room for a new leader who is likely to be better educated and able to achieve higher growth rates.

In panel B, we analyze the four possible cases of leadership transition, again using the graduate education dummy. The first case is a transition from a leader with a post-graduate qualification to another with the same level of education. In this case, we find no significant effect on growth. A transition from a leader with post-graduate qualification to a leader without post-graduate qualification yields an average reduction in growth of around 2.1 percentage points per year over the five year post-transition window.

Hence, the growth reduction due to the loss of a highly educated leader, which is on average 0.7 percentage points (see panel A), is larger if the next leader is less well educated. The next entry in panel B records the estimated effect of a transition from a leader without a post-graduate qualification to a highly educated leader. Here, we find that growth falls by 0.15 percentage points on average Finally, we look at transitions between two leaders who do not have a post-graduate qualification. Here, the effect on growth is on average positive.

Panel C repeats the results of panel B using college education instead of graduate education as the key variable. The results are similar to those in panel B, except that we now observe a reduction in growth after the transition from one college educated leader to another. As in panel B, the transition from an educated to a non educated leader yields an average reduction in growth of around 1.7 percentage points per year over the five year post-transition window. This result is now smaller in magnitude but still indicates that the more educated is the leader we loose, the larger is the negative impact on economic growth. In contrast to the results in panel B, panel C shows that the effect of a transition from a non educated to an educated leader increases growth, which is what we would intuitively predict.

6 Robustness checks

Jones and Olken (2005) find heterogeneous effects with respect to democracy. They find a positive and significant effect on growth when an autocrat leaves office. In addition, it is well known that autocracy is correlated with low educational levels. Therefore, the results in table 3 may be caused by the autocratic nature of the leader and not by his/her level of education. To investigate this point further we divided the sample in four groups, considering the interaction between education and political system. Table 4 summarizes the results. Panel A shows that the effect of a random transition of a democratic leader is significantly negative while the effect of a transition after the death of an autocratic leader is positive and also significant. Jones and Olken (2005) find also a positive and significant effect for autocracts. However, they do not find a significant effect for transtions from a democratic leader. We should notice that the sample size of random transitions of de-

mocratic leaders in Jones and Olken (2005) is less than half our sample.

Panel B of Table 4 shows the effects of random transtions of leaders with different levels of education separating by democrats and autocracts. As argued by Besley and Reynal-Querol (2009) democracies are more likely to select educated leaders. When we allow for the effect of education to be different in autocracies and democracies, we find a negative coefficient when a democratically elected leader dies. The point estimate is larger for highly educated leaders (-2.02 for those with a post-graduate qualification and -0.59 for those without). For autocrats the effect is positive whether or not the leader is highly educated.

Is the effect of educations caused mainly by leaders of African countries? To analyze this issue we estimate the effect of random transitions eliminating the leaders of African countries. Table 5 shows that the basic results are robust to reducing the sample in this way. A transition from a graduate leader to a graduate leader has no effect while a transition from a graduate to a non-graduate has a large negative and significant effect. These results are not very surprising since the reduction of the sample due to the elimination of the African leaders is small (8.8%) since most of the countries of Africa gained independence in the 50's or 60's and, for the initial years, there is no information on GDP.

As a final robustness exercise we check if the basic results that we report in Table 3 are econometrically sound by running a falsification test. We are going to move the random transitions five years back. These spurious transitions should, in general, lead to different results to the ones reported previously. Table 6 shows that this is the case. Using this spurious transtions we find that moving from a graduate leader to another graduate increases in a large and very signficant amount the average growth of the economy, opposite to the null effect we find using the real transitions. In addition, the spurious transitions from a non-graduate to a graduate generate a null effect while we find a significant negative effect using the real transitions.

7 Concluding Comments

This paper has used a new sample of political leaders between 1875 and 2004 to investigate whether the identity of a leader matters for economic growth and whether more educated leaders generate higher growth. We find evidence in favor of both hypotheses. The paper therefore adds to the emerging literature on the importance of leaders' characteristics in explaining policy outcomes.

But there is much that remains to be done. The exact mechanisms at work in explaining how leadership matters remain opaque. One important intermediate step is to understand how policy is affected by leaders. It is also important to take into consideration a wider set of outcome variables rather than focus only on economic growth. This provides a rich agenda of work for the future.

References

[1] Acemoglu, Daron, Georgy Egorov and Konstantin Sonin, (2008), "Political Selection and the Persistence of Bad Governments," unpublished typescript.

[2] Bennedsen, Morton, Francisco Perez-Gonzales and Daniel Wolfenzon, (2007), "Do CEOs matter?" Copenhagen Business School, Working Paper No. 13-2007.

[3] Bertrand, M. and A. Schoar (2003), "Managing with Style: the Effect of Managers on Firm Policies," *Quarterly Journal of Economics, 11*8 (4), 1169-1208.

[4] Besley, Timothy, [2005], "Political Selection." *Journal of Economic Perspectives*, 19(3), pp. 43-60.

[5] Besley, Timothy and Stephen Coate, (1997), "An Economic Model of Representative Democracy," *Quarterly Journal of Economics*, 112(1), 85-114.

[6] Besley, Timothy, and Marta Reynal-Querol, (2009), "Do democracies select more educated leaders?", unpublished typescript.

[7] Card, David, (1997), "The causal effect of education on earnings," in Orley Ashenfelter and David Card (eds), *Handbook of Labor Economics* Vol. 3, Elsevier.

[8] Caselli, Francesco and Massimo Morelli, (2004), "Bad Politicians," *Journal of Public Economics*, 71(3), 829-53.

[9] Chattopadhyay, Raghabendra and Esther Duflo, (2004a), "Women as Policy Makers: Evidence from a India-Wide Randomized Policy Experiment," *Econometrica* , 72(5), 1409-1443.

[10] Dee, Thomas S. [2004], "Are there civic returns to education?", *Journal of Public Economics*, 88 (9-10), 1697-1720.

[11] Dreher, A., Lamla, M., Lein, S. and F. Somogyi (2009), "The Impact of Political Leaders' Profession and Education on Reform," *Journal of Comparative Economics*, 37, 169-193.

[12] Gehlbach, Scott, Konstantin Sonin and Ekaterina V. Zhuravskaya, [2009], "Businessman Candidates," unpublished typescript.

[13] Göhlmann, S. and R. Vaubel (2007), "The Educational and Occupational Background of Central Bankers and its Effect on Inflation: an Empirical Analysis," *European Economic Review*, 51, 925-941.

[14] Jones, B. and B. Olken (2005), "Do leaders matter? National Leadership and Growth Since World War II," *Quarterly Journal of Economics*, 835-864.

[15] Kaplan, S., Klebanov, M. and M. Sorensen (2008) "Which CEO Characteristics and Abilities Matter?," mimeo.

[16] Lee, David S., Enrico Moretti, and Matthew Butler, (2004), "Do Voters Affect or Elect Policies? Evidence from the U.S. House," *Quarterly Journal of Economics*, 119(3), 807-860.

[17] Maddison, Angus, (2003), *The World Economy: Historical Statistics*. Development Center of the Organization for Economic Cooperation and Development, Paris, France.

[18] Milligan, Kevin , Enrico Moretti and Philip Oreopoulos, (2004}, "Does education improve citizenship? Evidence from the United States and the United Kingdom," *Journal of Public Economics*, 88 (9-10), 1667-1695

[19] Morrisson, Christian and Fabrice Murtin, (2008), "The Century of Education", PSE Working Paper No. 2008-22.

[20] Osborne, Martin J. and Al Slivinski, (1996), "A Model of Political Competition with Citizen Candidates," *Quarterly Journal of Economics*, 111(1), 65-96.

[21] Pande, Rohini, (2003), "Minority Representation and Policy Choices: The Significance of Legislator Identity," *American Economic Review*; 93(4), pp. 1132-1151.

[22] Poutvaara, Panu and Tuomas Takalo, (2003), "Candidate Quality," unpublished typescript.

8 Data Appendix

Our data expands the set of observations used by Jones and Olken (2005) (JO hereafter) and it is useful, therefore, to compare our observations of random transitions with theirs. JO provide information on leader deaths only from 1945 onwards. The JO data set lists 77 deaths, 5 of which are dropped due to overlaps because the leader died very shortly after the death of a previous leader. In our list of 185 random deaths from 1875 to 2004, 79 cases correspond to deaths before 1945, and 106 fall in the period between 1945 and 2004. Appendix Table 1 gives a complete list of the 185 leaders whose random transition in office we exploit and compares in detail our 106 deaths after 1945 with the list of 72 deaths in JO.

In this table we list the year in which the leader randomly left office, the particular cause of death or incapacitating illness, the leader's education level and whether or not she appears in the JO list. The catalogue of causes of death and illnesses is as follows: 60 leaders had heart problems, 20 retired or died with cancer; 9 died following an accident; 7 due to complications after a surgery; 6 due to pneumonia; 5 due to brain hemorrhages; 4 due to strokes; 3 due to circulatory problems; 3 due to influenza; 2 due to injuries; 2 due to other natural causes; 2 due to renal failure; 2 due to Parkinson's Disease; with others dying from bronchial disorders, cirrhosis of the liver, gallstone problems, lung failure, syphilis, or Waldenstrom's disease. This leaves 52 cases for which the cause of death or illness was not specified. Among the 185 leaders, 154 actually died while 31 retired due to an unexpected serious illness.

It is useful to compare our 106 deaths after 1945 with the list of 72 cases in JO. During this period our dataset does include most of the JO deaths. In particular, we include 62 of the 72 deaths that they use. The 10 leaders that JO have in their data and we do not can explained as follows. For 3 of them, there is a death close in time to the death of a leader prior to 1945. These are John Curtin in Australia, and Ramon Magsaysay in the Philippines and Tribhuvan of Nepal. Here, we follow the JO selection rule, giving priority to the first leader that died. There are 3 cases where following the Archigos criteria, we do not follow the JO judgement of who is leader. So in Comoros in 1975 JO has Price Jaffar dying unexpectedly. However, Archigos considers that Soilih is the head of a revolutionary council. In Greece JO judge that George II is ruling the country and died in 1947 whereas Archigos considers Maximos, the Prime Minister, to be ruling the country in this period. Finally

in Portugal, JO considers that the Prime Minister, Francisco de Sa Carneiro, to be ruling the country when he died in 1980. However, Archigos assigns the leadership role to the President, Antonio Ramalho Eanes. The final 4 cases are those where the judgement is based on the numbers that a leader has been in power in a given year. We assign to each leader year observation, the leader who has spent a majority of months in power in that year. There are three cases where the leader who died in office was not assigned to the leader year observation on that basis. This is true of Peron in his second term in office, in 1974, Donald Sangster in Jamaica in 1967 and Domingo Diaz Arosemena in Panama in 1949. And, finally, Ahmed Ould Bouceif from Mauritaria. that died the month he raised in power. In the end, these are small differences of judgement and even if we include these 10 cases, the results do not change. However, more significantly, we do have 44 new cases during the post 1945 period that are not included in the JO list. Compared to the JO dataset, we agree in all but one case on the cause of death – Dupong in Luxemburg in 1952. We classify him as having died due to heart disease while the JO data code him as having died due to complications associated with having a broken leg.