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Technical Change, Political Leaders and Foreign Education

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Abstract

I use the random timing of natural and accidental deaths of political leaders while in power to study the relationship between political leadership and technical change, since the timing of deaths is uncorrelated with underlying economic conditions. Using event study methodology, I present evidence that political leadership affects technical change, shedding some light on the relationship between leaders and economic outcomes. This relationship is neither driven by differences in development nor by human capital or technical shocks. Bootstrap sampling results indicate that foreign-educated leaders have positive effects on technical change.

JEL Classification: D72, O33

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1 Introduction

Although it is well-known that technical change plays a key role for economic growth, surprisingly little is known about the effect of the country's chief executive on country-level innovation. The novel contribution of this paper is to show that country-level technical change is affected by individual leaders in general, and foreign-educated leaders in particular. At the firm level, technical change has been shown to be dependent on individual chief executives. Marianne Bertrand and Antoinette Schoar (2003) show that CEOs have large, robust effects on research and development (R&D) expenditures. This finding is a key departure from models with homogenous CEOs where decision-making is dictated by profit-maximization motives, leaving no room for leaders to exert idiosyncratic impacts. The country-level analog to the models with homogeneous CEOs is the deterministic view of history held by Karl Marx and Leo Tolstoy, which contends that individual leaders are incidental to history and have little or no effect on economic outcomes (Benjamin Jones and Benjamin Olken 2005).

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The motivation behind the study of the nexus between country-level leadership and technical change is two-fold. First, although the political economy of technical change has received some scholarly attention, the potentially crucial role of political leaders has so far remained largely unexamined. Ricardo Hausmann (2014) writes:

> "(T)he argument in Daron Acemoglu and James Robinson's book *Why Nations Fail* is essentially that technology does not diffuse because the ruling elite does not want it to. (...) (B)ecause technology may upset their control over society, they choose to do without it. As a Venezuelan who is seeing his country collapse at this very moment, I do not doubt that there have been many instances in human history during which those in power have prevented progress."

I present empirical evidence that political leaders, who are ultimately the economy's agenda-setters, affect technical change, supporting Hausmann's view. While the political economy of technical change is a growing area of research (see for example Philippe Aghion, John van Reenen and Luigi Zingales 2013; Daron Acemoglu, Philippe Aghion, Leonardo Bursztyn, and David Hemous 2012; Julien Beccherle and Jean Tirole 2011), previous research in this area has not explicitly examined the political leader – technical change nexus. This paper contributes to the growing body of evidence refuting Tolstoy's deterministic view that leaders are incidental to socioeconomic outcomes. Individual leaders affect economic growth (Jones and Olken 2005), and growth is higher under more educated leaders (Timothy Besley, José García Montalvo and Marta Reynal-Querol 2011). Leaders have also been shown to exercise regional and ethnic favoritism (Roland Hodler and Paul Raschky 2014; Raphaël Franck and Ilia Rainer 2012) and to affect the emergence of political norms (Chris Bidner and Patrick Francois 2013), while earlier research uncovered links between the educational and employment backgrounds of leaders and the likelihood of economic reforms (Axel Dreher, Michael Lamla, Sarah Lein and Frank Somogyi 2009). I add to this literature by showing that leaders affect technical change, the main driver of economic growth. The identification strategy in this paper relies on the use of natural and accidental deaths of country leaders while in power as randomly timed events: a leader's natural death is essentially a randomly timed leadership transition. I build on the work of Jones and Olken (2005), who first introduced this idea. Randomly timed transitions are uncorrelated with any underlying socioeconomic conditions; it therefore becomes possible to identify the causal effects of political leadership on economic variables of interest.

The second motivation for this paper is to contribute to the sizable literature on return migration and development, by studying the impact of foreign-educated leaders on technical change. Return migrants have been shown to affect several facets of economic development, including democracy (Antonio Spilimbergo 2009), fertility choices (Michel Beine, Frédéric Docquier and Maurice Schiff 2013), investment

(Yaohui Zhao 2002), attitudes towards social stratification (Francesco Cerase 1974), social and political values (Shaul Kimhi and Nancy Gourash Bliwise 1992), and civic engagement (Catia Batista and Pedro Vicente 2011). While return entrepreneurs (Siping Luo, Mary Lovely and David Popp 2013) and return star scientists (Lynne Zucker and Michael Darby 2007) have been shown to exert positive impacts on domestic innovation, there is yet no existing evidence that foreign-educated political leaders promote technical change. This is another gap in the literature I attempt to fill in this paper.[1] Why should foreign education matter? Amending some of Spilimbergo's (2009, p. 539) stylized facts helps provide some perspective. In many countries, foreign-educated technocrats may be a scarce resource, such that they may be able to pursue innovation-enhancing policies as a matter of preference. Moreover, foreign-educated leaders are motivated to keep up with the more developed countries where they studied. Foreign-educated individuals also remain connected to their country of study and are therefore more likely to become aware of new ideas abroad and to encourage their dissemination domestically. The expectation that foreign-educated leaders display attributes that are likely to favor technical change is also supported by recent research in applied psychology (William Maddux, Eliza Bivolaru, Andrew Hafenbrack, Carmit Tadmor and Adam Galinsky, 2014; Christine Lee, David Therriault and Tracy Linderholm 2012; Angela Leung, William Maddux, Adam Galinsky and Chi-yue Chiu 2008).

Methodologically, the role of particular leader characteristics has been difficult to pin down: Besley, Montalvo and Reynal-Querol (2011) note that the identification of the effects of leader education is complicated by sample selection issues. To identify the causal impact of foreign education on technical change, I take an experimental approach and overlay bootstrap samples over event-study estimates of leader effects for each individual leader. Specifically, I estimate each leader's effect on technical change then resample leaders across a treatment and a control group, where the treatment group receives a higher share of the foreign-educated leaders than the control group, and I allow this share to vary in small increments across different treatments. This method allows for the mitigation of identification concerns that arise due to unobserved leader heterogeneity and permits a *ceteris paribus* interpretation of the effect of leaders' foreign education. Increasing the share of the foreign-educated leaders in treatment widens the gap in leader effects across groups, suggesting a causal effect of foreign education on technical change. To enrich the analysis with some historical perspective, I also collect patent data prior to 1883 and biographic data for individual leaders, including educational and professional background characteristics going back to the 17th century for the United Kingdom. The remainder of this paper is organized as follows. In Section 2, I discuss some stylized facts and historical evidence. Section 3 introduces the empirical strategy and main results. Section 4 deals with potential biases due to cross-country differences in development

[1] To date, two studies (Amelie Constant and Bienvenue Tien 2010; Marion Mercier 2016) examine the effects of foreign-educated leaders on foreign direct investment and democracy respectively. While both these studies make interesting contributions to the literature, their identification strategies suffer from endogeneity and sample selection issues.

and to human capital-altering shocks and plausibly exogenous technical shocks. In Section 5, I turn to the study of the leaders' foreign education. Section 6 concludes.

2 Stylized Facts and Historical Evidence

Do individual leaders have varying attitudes to technical change? Casual observation into history and current affairs can be revealing. In 2009, United States President Barack Obama stated:

> "(H)istory should be our guide. The United States led the world's economies in the 20th century because we led the world in innovation. Today, the competition is keener; the challenge is tougher; and that is why innovation is more important than ever. (. . .) That's how we will ensure a high quality of life for this generation and future generations. With these investments, we're planting the seeds of progress for our country."

This could be interpreted as cheap talk; however, the White House did launch several innovation-focused initiatives, including Educate to Innovate in 2009. On the other hand, vested interests, ideology and political objectives may explain why some leaders, even in advanced economies, exhibit negative attitudes to technical change and science, in line with Hausmann's (2014) view. A current example is Australian Prime Minister Tony Abbott. A climate change skeptic, Abbott disbanded a ministry dedicated to science shortly after his election and enacted aggressive budget cuts to scientific research and innovation, which "have the potential to threaten (Australia)'s global edge in research and industry innovation" (Special Broadcasting Service 2014).

Under the premise that individual leaders have idiosyncratic effects on technical change, we should be able to observe distinctive within-tenure movements and discontinuities or trend reversals near tenure boundaries. Historical evidence of such patterns is abundant; I present some cases in this section. Iran under Ruhollah Khomeini provides an interesting case study of some of the ways leaders can affect technical change. Figure 1 plots innovative activity in Iran before, after and during Khomeini's tenure, which is indicated by the shaded area. Innovation drops discontinuously at his entry and begins to rise after his exit. Khomeini's regime is likely to have affected technical change in many ways. First, many Iranians migrated (Michael Axworthy 2013, p. 147), thereby shrinking the talent pool. Second, Iran under Khomeini underwent large-scale nationalization of many key industries, including banking, insurance, power, transportation, major mining sites, dams, major irrigation systems and foreign trade (John Rosser and Marina Rosser 2004, p. 499). This wave of nationalization and the attending generalized uncertainty over property rights are likely to have acted as a deterrent to domestic entrepreneurship and innovation. Third, foreign direct investment (FDI), a known determinant of innovation (see for example

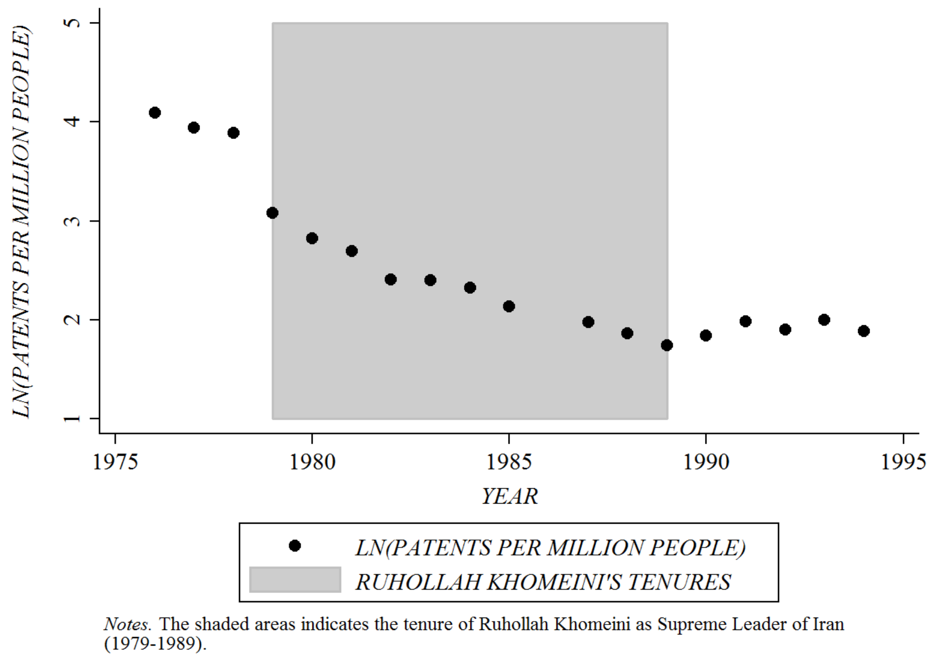


Figure 1: Iran under Ruhollah Khomeini.

Lee Branstetter 2006), was banned altogether (John Marangos 2013, p. 58).

Conversely, FDI was a major enabling factor for Singapore's technological take-off. Singapore's large inflow of FDI during the second half of the 20th century can largely be attributed to the leadership of one individual: Prime Minister Lee Kuan-Yew. The Cambridge-educated Lee "courted multinational investors to upgrade the economy from mass manufacturing to high-tech industry" (Terry McCarthy 1999). How did Lee decide to turn to foreign investors? In this case, foreign networks matter. In From Third World to First: The Singapore Story, 1965-2000, Lee Kuan-Yew (2000) recounts from his stay at Harvard University in the 1960s: "(Harvard Business School Professor Raymond) Vernon dispelled my previous belief that industries changed gradually and seldom moved from an advanced country to a less-developed one." It is through the lens of a distinguished foreign academic that Lee saw the growth opportunities attendant to bolstering FDI. Lee Kuan-Yew's Singapore illustrates the stylized fact that foreign-educated individuals maintain foreign networks, which can be key to attracting FDI. Many countries have experienced a similar pattern of higher FDI under foreign-educated leaders, in Africa in particular (Amelie Constant and Bienvenue Tien 2010).

Foreign education may also affect some of the leader's cognitive abilities in ways that are consistent with the fostering of innovation. Recent findings in applied psychology support this view: studying abroad has been found to enhance complex cognitive processes which facilitate creative thinking (Christine Lee, David Therriault and Tracy Linderholm 2012). William Maddux, Eliza Bivolaru, Andrew

Hafenbrack, Carmit Tadmor and Adam Galinsky (forthcoming) show that exposure to multicultural environments promotes improvements in integrative complexity, which is the extent to which one is able to adopt multiple perspectives when processing information, solving problems, and making decisions. Multicultural exposure has also been shown to increase creative performance and creativity-supporting cognitive processes (Angela Leung, William Maddux, Adam Galinsky and Chi-yue Chiu 2008). Lastly, foreign education may simply alter the leader's opinions, attitudes and preferences towards scientific research and economic policy in general. The network, cognitive, and preference channels are however not empirically distinguishable; in the empirical work, I treat them jointly as "virtues" of foreign education.

What are some other ways individual leaders may affect technical change? Transversal across all channels is the simple idea that innovation is affected by leader actions which modify its expected returns. Conversely to Iran's experience under Khomeini, skilled foreign migrants can be enticed to immigrate when favorable policies, such as a selective softening of migration law, are enacted. Property rights are also paramount: where leaders are expected to freely expropriate inventors, innovation will slump. Institutional action of the type pursued by Barack Obama and Tony Abbott also exerts significant effects. Research funding, subsidies, depreciation schedules and other tax incentives are all well-known determinants of technical change, and can be affected by political leaders. Finally, the cultural *zeitgeist* a particular leader creates may be powerful. In 1979, 3M CEO Lewis Lehr stated: "we learned to follow the fellow who follows a dream" (Jean-Philippe Deschamps 2003).

Of course, leaders could still be incidental to technical change. After all, what we have considered so far are simply individual tenures for individual leaders, which may coincide with long-term trends. Instead, we can gain some insight from leaders with multiple tenures: under the premise that leaders have idiosyncratic effects, then one leader's multiple tenures should be observably different from long-term trends and from the tenures of other leaders. Historical examples include President Julio Roca of Argentina and Prime Ministers Charles Ruijs de Beerenbrouck of the Netherlands and Olof Palme of Sweden. Figures 2-4 shows that technical change behaves idiosyncratically in each of these leaders' two tenures. Some outlying points notwithstanding, innovation is either systematically above or below the long-term trend and displays similar patterns in each leader's two tenures, suggesting a correlation between technical change and political leadership that goes beyond coincidence. It is not immediately apparent what causes these movements, but it is worth noting that Palme was an early and vocal proponent of investment in nuclear power, which may have contributed to some patenting activity while he was in power. While there is little historical record of which policies were carried out by Charles Ruijs de Beerenbrouck in the Netherlands and Julio Roca in Argentina, casual observation into their personal characteristics can be informative. Their tenures respectively exhibit higher and lower than usual innovation: Ruijs de Beerenbrouck was a highly educated lawyer, while Roca was a military leader

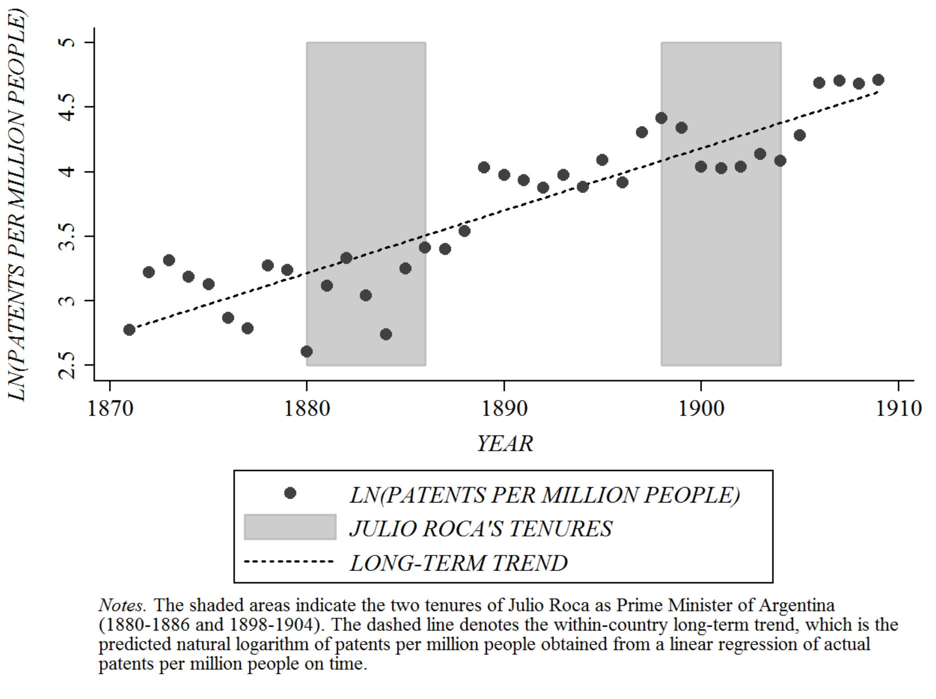


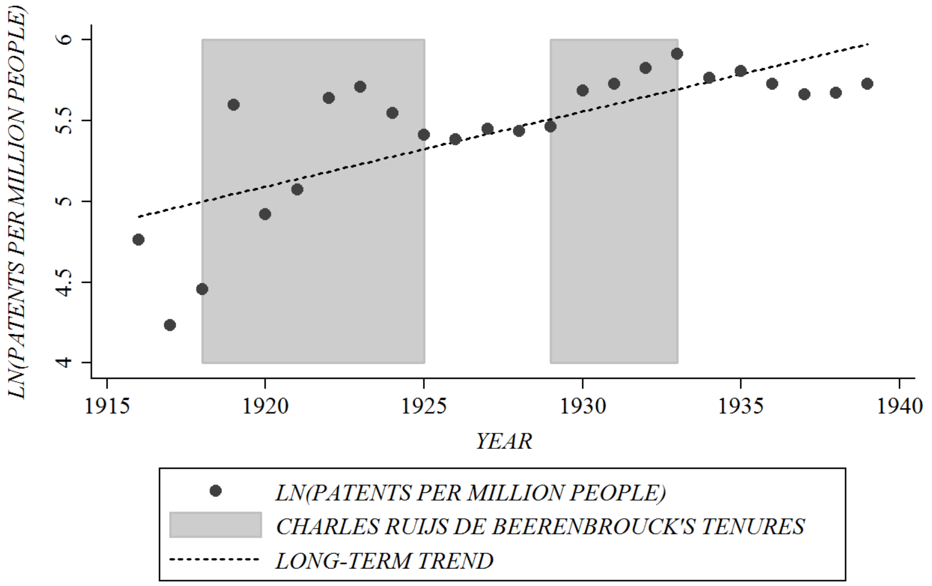
Figure 2: Argentina: Julio Roca's two terms.

previously involved in the slaughter of native Patagonian populations (Meri Clark 2009). Still, it is possible that leaders like Roca, Ruijs de Beerenbrouck and Palme coincidentally found themselves in power during particular phases of technological development. Careful empirical analysis is therefore needed; I introduce the econometric methodology in the following section.

3 Empirical Strategy and Main Results

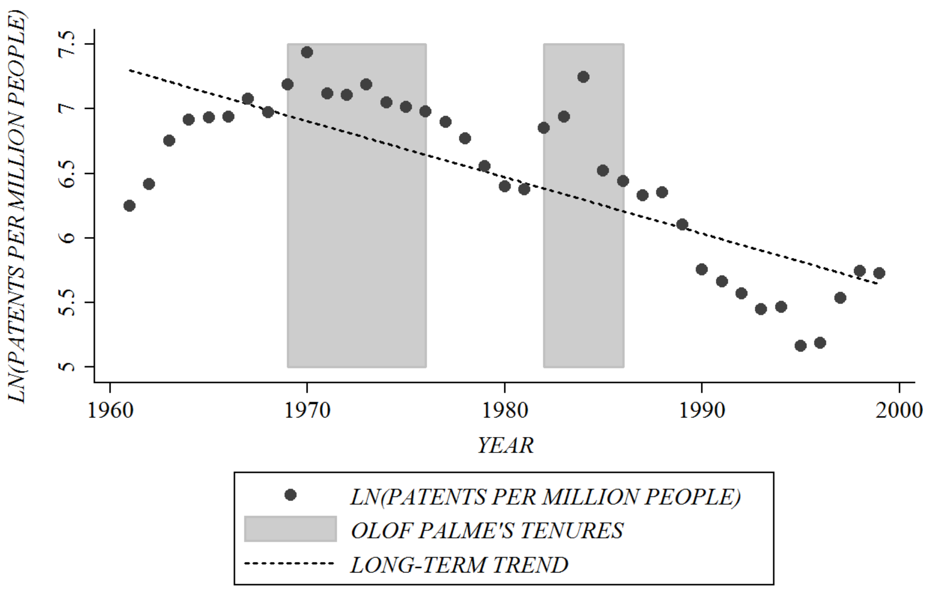
To measure the effects of political leadership on technical change, we would ideally like to randomly assign different leaders to identical countries and compare outcomes. By including country fixed effects in the regressions, we can control for time-invariant country characteristics, including geography and colonial history. Accounting for key confounding variables, which may affect both political leadership and technical change, mitigates possible biases in the estimators, and using heteroskedasticity and autocorrelation-robust standard errors facilitates inference. Jointly, these methods allow us to approximate a ceteris paribus interpretation of leader effects. Specifically, consider the significance of π in:

$$y_{\mathsf{it}} = \alpha_{\mathsf{i}} + \pi l_{\mathsf{it}} + u_{\mathsf{it}} \tag{1}$$



Notes. The shaded areas indicate the two tenures of Charles Ruijs de Beerenbrouck as Prime Minister of the Netherlands (1918-1925 and 1929-1933). The dashed line denotes the within-country long-term trend, which is the predicted natural logarithm of patents per million people obtained from a linear regression of actual patents per million people on time.

Figure 3: Netherlands: de Beerenbrouck's two terms.



Notes. The shaded areas indicate the two tenures of Olof Palme as Prime Minister of Sweden (1969-1976 and 1982-1986). The dashed line denotes the within-country long-term trend, which is the predicted natural logarithm of patents per million people obtained from a linear regression of actual patents per million people on time.

Figure 4: Sweden: Olof Palme's two terms.

where l is a set of dummy variables each denoting an individual leader (that is, l_{Blair} is equal to 1 for the United Kingdom for the years Tony Blair was in power, and 0 for all other years and countries). Leaders have no effect on the outcome variable ($\pi = 0$) if the leader dummies are not jointly different from zero. However, leadership transitions do not occur independently from economic outcomes. Instead, leader l is observed at time t then either replaced or retained at $t+1$. The structural equations are then:

$$y_{\mathrm{it}} = \alpha_{\mathrm{i}} + \pi l_{\mathrm{it}} + u_{\mathrm{it}} \tag{2}$$

$$p(l_{\mathrm{i,t+1}} = l_{\mathrm{i,t}}) = f(y_{\mathrm{it}}) \tag{3}$$

The probability that the leader remains in power is endogenously determined by economic conditions. This illustrates the idea that economic conditions influence political outcomes, such that y_{ilt} may have an effect on the choice of leader in country i at time t. Without some source of exogenous variation in political leadership, ordinary least square (OLS) estimates based on equation (1) cannot surmount this simultaneity and will therefore be biased and inconsistent.

When is there exogeneity in leadership transitions? When a political leader steps down due to ill health, or dies of natural or accidental causes while in power, the timing of the event is exogenous; that is, uncorrelated with u in (1). The unforeseen nature of these events generates quasi-experiments where leadership changes are not underpinned by economic conditions, circumventing the simultaneity issues that would arise from OLS estimates based on equation (1). This is the identification strategy introduced by Jones and Olken (2005) and employed by Besley, Montalvo and Reynal-Querol (2011) and Besley, Torsten Persson and Reynal-Querol (2016). This method relies on the comparison of average outcomes in the period before (PRE) and after (POST) randomly timed exits. I follow Jones and Olken (2005), Besley, Montalvo and Reynal-Querol (2011) and Besley, Persson and Reynal-Querol (2016) in setting the length of the PRE and POST windows equal to five years and excluding the year of exit from both periods. Average innovation before and after the randomly timed exit of leader z are respectively equal to:

$$\overline{PRE_{\mathrm{z}}} = \frac{1}{T} \sum_{\mathrm{i=1}}^{-T} patents\ per\ million\ inhabitants_{\mathrm{i,t}} \tag{4}$$

$$\overline{POST_{\mathrm{z}}} = \frac{1}{T} \sum_{\mathrm{i=1}}^{T} patents\ per\ million\ inhabitants_{\mathrm{i,t}} \tag{5}$$

Testing whether leaders have any effects on technical change amounts to testing whether innovation is unchanged in the period following a randomly timed exit. This null hypothesis is:

$$H_0 : \overline{POST_z} - \overline{PRE_z} = 0 \tag{6}$$

$\overline{POST_z}$ - $\overline{PRE_z}$ is distributed $\frac{2}{T}\sigma^2_{u_i} + 2\pi^2\sigma^2_l(1 - \rho_{m,n})$ (Jones and Olken, 2005), where σ^2_l is the variance in leader quality and $\rho_{m,n}$ is the correlation in leader quality between two consecutive leaders m and n. Under the null hypothesis that political leaders have no effect on technical change, π in equation (1) and in the variance of $\overline{POST_z}$ - $\overline{PRE_z}$ is equal to 0: $\overline{POST_z}$ - $\overline{PRE_z}$ is therefore distributed $N(0, \frac{2}{T}\sigma^2_{u_i})$. To test the null, I regress:

$$ln(PATENTSpmi_{i,t}) = \alpha_i + \beta_t + \delta_{1z}POST_{i,z,t} + \delta_{2z}PRE_{i,z,t} + u_{i,t} \tag{7}$$

where $POST_{i,z,t}$ and $PRE_{i,z,t}$ are dummy variables equal to 1 in the 5 years respectively after and before the randomly timed exit of leader z in country i, and zero in all other years and countries, and $PATENTSpmi_{i,t}$, the number of patents per million inhabitants in each country-year, measures technical change. Patents are a robust indicator of technology-driven economic growth (Jakob Madsen 2008, 2010) and are also the most broadly available measure of technological activity across time and space. For each country, $PATENTSpmi_{i,t}$ through all years is either the number of patents granted or applied for, depending on which measure is most widely available for each particular country. These data are mainly from the World Intellectual Property Organization (WIPO 2014); I also collect patent numbers from historical patent publications, country sources and communication with country patent offices. The final patent dataset covers 156 countries starting from the year 1617 for the United Kingdom. To test the null hypothesis, the Wald statistic is derived as:

$$W = \frac{1}{N_z}\sum_{i=1}^{z}\frac{(POST_{i,z} - PRE_{i,z})^2}{\frac{2\sigma^2_{u_i}}{T}} \tag{8}$$

where N_z is the total number of randomly timed exits and $\sigma^2_{u_i}$ is a country-specific variance corrected for autocorrelation and heteroskedasticity. Under the null, N*W is distributed χ^2 with N degrees of freedom. To identify randomly timed leadership transitions, I complement version 2.9 of Archigos (Hein Goemans, Kristian Skrede Gleditsch and Giacomo Chiozza 2009) with my own data collection effort. Archigos contains information on the dates and manners of exit for all world leaders starting from 1875. I supplement this information by using historical records to identify 311 leaders in the 1617-1874 period. The two datasets combined yield 288 cases of leaders dying in power of natural or accidental causes. I follow Jones and Olken (2005) in excluding those cases where a leader exits within 10 years of a previous exit, which would create an overlap between the first leader's $POST$ period and the second's PRE period. I also exclude those cases where no innovation data exists in either the PRE or $POST$ period. The final sample therefore includes 117 randomly timed exits starting from the year 1623 (see data appendix). For each of these leaders, I collect detailed educational and professional background information. For all degrees completed or non-degree studies, I collect information on the level, subject, country and length of study. I also collect data on each leader's most significant profession before entering politics. For leaders in power after 1900, I use Harris Lentz's (1994, 1999) two-volume *Encyclopedia of Heads of States and Governments* and Encyclopædia Britannica as the main sources. For data on pre-1900 leaders, I rely on a variety of other sources as listed in the data appendix, which also contains all other data definitions and sources.

I now turn to the empirical results. The baseline point estimate for Post – Pre (Table 1 Column 1) is equal to - 4.1%, meaning that innovation is on average about 4% smaller in the period immediately after the randomly timed exit of a political leader than it is before the exit. This suggests that leaders at the country level have sizeable effects on technical change. In this baseline result, the PRE and $POST$ periods start in the years respectively immediately before and immediately after the exit (that is, at *t +/- 1*). Four key potential concerns with the identification of leader effects may arise with this timing. First, there may be a strictly positive switch cost associated with having a new, inexperienced leader in the $POST$ period. If the new leader is initially less efficient than the incumbent, then a negative Post – Pre may reflect this switch cost rather than leader effects. Second, if there is some expectation of exit in the PRE period due to old age and deteriorating health, in actuality or in expectation, then the timing of the exit may become less uncertain. Third, there are unobserved time lags from research to patenting. While the exact distribution of the lags is unknown, it is possible that they are not properly addressed by the *t +/- 1* timing. Fourth, the quality of the replacement leader may not be normally distributed with zero mean. When the transition is randomly timed, there may not be a suitable replacement ready to step in immediately. It is however plausible that given a sub-par initial leader in the $POST$ period, the economy adjusts to select the best available leader relatively quickly. To address these concerns, I shift the timing of the PRE and $POST$ periods by 1 and 2 years in columns (2) and (3) of Table 1. This causes the $POST$ period to begin respectively 4 and 6 years after the PRE period ends. The comparison of $POST$ and PRE averages becomes less straightforward as we are now comparing non-contiguous periods of time: to attenuate this issue, I also include a country-specific time trend in columns (2) and (3). Generally, shifting the timing of PRE and $POST$ accounts for the possibility that the atypical realizations of technical change evidenced in the baseline result are entirely driven by short-term disturbances. Empirically, this turns out not to be a concern: at the *t +/- 3* timing, which finishes 8 years after the randomly timed exit, Post – Pre is still negative and significantly different from 0 (p = 0.00).

Table 1: Do Political Leaders Affect Technical Change?

	Dep. Var.: $\ln(\text{PATENTSpmi}_{i,t})$		
	(1)	(2)	(3)
	t +/- 1	*t +/- 2*	*t +/- 3*
Post – Pre	-0.041***	-0.049***	-0.015***
Number of exits	117	112	104
Observations	8,029	8,029	8,029
Countries	156	156	156

Notes. PRE and POST windows start at t +/- 1, 2 and 3 in each column respectively and $t = 0$ is the year of random exit. For each random exit z, there is a dummy variable $POST_z$ equal to 1 in the 5 years after the random exit and 0 otherwise and a dummy variable PRE_z equal to 1 in the 5 years before the random exit and 0 otherwise. Post – Pre is the difference between the estimated coefficients for $POST_z$ and PRE_z averaged across the number of random exits. The null hypothesis that Post – Pre is equal to zero is tested using the Wald test shown in equation (8) where $\sigma^2_{u_i}$ accounts for heteroskedasticy and autocorrelation. All regressions include a constant term and country and time fixed effects. Columns (2) and (3) also include country-specific time trends. *** p<0.01, ** p<0.05, * p<0.1.

4 Shocks and Differences in Development

4.1 Income, Human Capital and Political Institutions

In the specification of Table 1, there may be omitted factors that simultaneously affect technical change and the extent to which leaders may affect technical change. To attend to these issues, I introduce GDP per capita, democracy and educational attainment into the regressions (respectively from Jutta Bolt and Jan Luiten van Zanden 2013; Monty G. Marshall and Keith Jaggers 2008; and Christian Morrisson and Fabrice Murtin 2009) in Table (2). It is important to control for the level of democracy because more democratic regimes have stronger checks and balances on the executive, which implies that the ruler has less discretion to set the economic agenda and is therefore less likely to affect economic outcomes. Indeed, Jones and Olken (2005) find that leaders affect economic growth in autocratic settings but not in democratic settings. Income per capita and educational attainment (measured by the average number of years of schooling in the population aged over 15) are two key correlates of development: it is important to control for both these factors because technical change in wealthier and/or more educated countries may respond to unexpected leadership transitions differently. This is because a wealthier or more highly educated population is likely to be less reliant on one individual's leadership. Failing to account for these measures of economic development may therefore result in overestimation of the true magnitude of Post – Pre. I estimate variations of:

$$ln(PATENTSpmi_{\mathrm{i,t}}) = \alpha_{\mathrm{i}} + \beta_{\mathrm{t}} + \delta_{1\mathrm{z}} POST_{\mathrm{i,z,t}} + \delta_{2\mathrm{z}} PRE_{\mathrm{i,z,t}} + X_{\mathrm{i,t-1}}\theta + u_{\mathrm{i,t}} \tag{9}$$

Table 2 displays the results: Post − Pre is significantly different from zero at the 1% level in all specifications. The finding that political leaders affect technical change is also robust to several country samples. For the larger country samples of columns (1) and (2), leader effects are in the neighborhood of - 4%, which is in the same order of magnitude as the baseline result of column (1) in Table 1.

4.2 Human Capital Shocks and Technical Shocks

Failing to explicitly account for shocks that affect technology indirectly through human capital may distort the estimated relationship between leaders and technical change. To see this, consider the impact of armed conflict. If a deadly war overlaps with the POST and PRE periods, the coefficient on Post – Pre will be misleading: the loss of innovation due to loss of human capital will be mistaken for leader effects. It is therefore important to control for armed conflicts casualties. This data is constructed from the Correlates of War database (Meredith Reid Sarkees and Frank Wayman 2010) and is equal to the number of war casualties per 100,000 inhabitants. Column (1) of Table 3 shows that, while armed conflicts casualties have significant effects on technical change, their inclusion does not render political leadership insignificant, as Post − Pre is still close to − 4%. To safeguard against the possibility that the results are driven by any idiosyncratic technical developments, I treat observations where the annual growth rate of innovation is outside one or two standard deviations from the country-specific mean as plausible technical shocks and purge those observations from the sample. I do so to avoid mistaking technological developments that are unrelated to political leadership for leader effects. Of course, this procedure may result in the exclusion of some cases where the true magnitude of leader effects is large. This is, however, a conservative approach, and may be prone to underestimating rather than overestimating the true size of leader effects. Columns (2) and (3) of Table 3 show the results, with Post – Pre remaining significantly different from zero and again fairly close to – 4% in magnitude.

5 Leaders and Foreign Education

Do foreign-educated leaders favor technical change? If foreign education does not matter, then the Post – Pre for foreign-educated leaders should not be significantly different from the Post – Pre for

Table 2: Determinants of Technical Change

	ln(PATENTSpmi$_{i,t}$)			
	(1)	(2)	(3)	(4)
Post – Pre	-0.041***	-0.043***	-0.070***	-0.099***
ln (per capita GDP)$_{i,t-1}$	0.363***			0.608***
	(0.000)			(0.058)
Polity2$_{i,t-1}$		0.140***		0.112***
		(0.041)		(0.050)
Years of schooling$_{i,t-1}$			0.114***	0.198***
			(0.024)	(0.029)
Number of exits	106	103	73	71
Observations	7,323	6,713	4,939	4,519
Countries	134	135	67	65

Notes. PRE and POST windows start at t +/- 1 and $t = 0$ is the year of random exit. For each random exit z, there is a dummy variable $POST_z$ equal to 1 in the 5 years after the random exit and 0 otherwise and a dummy variable PRE_z equal to 1 in the 5 years before the random exit and 0 otherwise. Post – Pre is the difference between the estimated coefficients for $POST_z$ and PRE_z averaged across the number of random exits. The null hypothesis that Post – Pre is equal to zero is tested using the Wald test shown in equation (8). All regressions include a constant term and country and time fixed effects. Standard errors are in parentheses and account for country-specific AR(1) and heteroskedasticity. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Technical Shocks and Human Capital Shocks

	$\ln(\text{PATENTSpmi}_{i,t})$		
	(1)	(2)	(3)
Post – Pre	-0.029***	-0.037***	-0.039***
War casualties	-0.022*		
	(0.011)		
Excluding:			
Atypical tech. change		Yes	
Most atypical tech. change			Yes
Number of exits	110	101	107
Observations	7,583	6,070	7,092
Countries	156	148	149

Notes. PRE and POST windows start at t +/- 1 and $t = 0$ is the year of random exit. For each random exit z, there is a dummy variable $POST_z$ equal to 1 in the 5 years after the random exit and 0 otherwise and a dummy variable PRE_z equal to 1 in the 5 years before the random exit and 0 otherwise. Post – Pre is the difference between the estimated coefficients for $POST_z$ and PRE_z averaged across the number of random exits. The null hypothesis that Post – Pre is equal to zero is tested using the Wald test shown in equation (8). All regressions include a constant term and country and time fixed effects. Standard errors are in parentheses and account for country-specific AR(1) and heteroskedasticity. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

home-educated leaders. However, a simple comparison of Post - Pre across the two groups may produce misleading results: the problem in this instance is methodological. Let F = {0;1} denote the foreign education status of each political leader. It might appear natural to conduct a test of the equality of:

$$E[POST - PRE|F = 1]$$

and

$$E[POST - PRE|F = 0]$$

However, such a comparison would be sensible only under the crucial assumption that the pool of domestic-educated leaders is an appropriate counterfactual for the pool of foreign-educated leaders, foreign education status notwithstanding. Of course, there are many (likely) ways this assumption could be violated: for example, it may be plausible that foreign-educated leaders have received more formal instruction than home-educated leaders, which could affect their attitudes vis-à-vis of technical change. Although this particular hypothesis is not borne out by the data (Pearson's Chi-square test comfortably rejects the null hypothesis that foreign-educated leaders are more highly educated), the general concern that foreign-educated leaders might have been favorable to technical change even if they had not been educated abroad remains. This is the nature of the selection problem encountered in Besley, Montalvo and Reynal-Querol (2011), who studied the effect of leader education on economic growth.

Bootstrap sampling is well suited to address this problem. By constructing treatment and control groups that vary only by the number of foreign-educated leaders they include, we are able to estimate the marginal effect of having foreign-educated leaders. I randomly allocate each leader to one of two groups of equal size: the treatment group receives a high share s of all foreign-educated leaders (28 of in total), and the control group receives the remainder, 1 - s. To alleviate heterogeneity concerns, I impose the additional restriction that the distribution of educational achievements must be identical in the treatment and control groups; that is, the two groups have the same number of leaders with no degrees, leaders with undergraduate degrees, and so forth. To construct the bootstrap samples, I perform this randomization 1000 times over while keeping s fixed. I then repeat the sampling for different values of s. By allowing the share of foreign-educated leaders to increase in small increments in the treatment group, we are able to measure the *ceteris paribus* effect of having foreign-educated leaders. The null hypothesis is then:

$$H_0 : E[POST - PRE|TR = 1] - E[POST - PRE|TR = 0] = 0 \tag{10}$$

where TR = {0;1} denotes treatment status and observations are randomly allocated to treatment or control subject to the constraints set out above.

The first row of Table 4 shows that when the two groups receive equal shares of foreign-educated leaders, Post − Pre is not significantly different across the treatment and control groups. When s is increased to 0.6 however, technical change drops after the exits of the leaders in treatment (relative to control), suggesting a positive effect of the leaders in treatment relative to control. Increasing s further widens the gap between the effects of the leaders in treatment and the effects of the leaders in control: this suggests, on aggregate, that foreign-educated leaders have a positive effect on technical change.[2]

6 Conclusion

The first goal of this paper was to improve our understanding of the relationship between political leaders and outcomes. Previous literature in this area indicates that leaders affect many economic outcomes, including economic growth (Jones and Olken 2005) and distributive politics (Hodler and Raschky 2014), dispelling Tolstoy and Marx's deterministic view of history, which contends that leaders are merely incidental to underlying socioeconomic forces. With the exception of monetary policy (Jones and Olken 2005), the channels through which leaders affect growth had remained mostly unidentified. In this paper, I identified technical change as one key economic outcome that is affected by political leaders. This finding is of significant importance because technical change is at the heart of economic growth, suggesting good leadership can drive growth via fostering innovation. This finding ties into key questions in the political selection literature, such as what exactly constitutes good leadership and how to identify good would-be leaders at time of selection. Further research into these questions is needed to strengthen our understanding of political leadership and ultimately design better institutions.

This article's second goal was to contribute to the large literature on the development and return migration literature, which generally finds positive effects of return migration on several facets of development. In this paper, the return migrant of interest is the political leader, and the novel finding is that foreign-educated leaders have beneficial effects on technical change. These leader-level results are consistent with previous research on return migration and technical change (Luo, Lovely and Popp 2013; Zucker and Darby 2007). The old adage that travel broadens the mind, a notion not unfamiliar to many an economist, finds empirical support in this paper. Further analysis of the channels through which foreign education affects technical change remains to be carried out. Are the differences between foreign and home-educated leaders due to policy preferences, leader ability, or network effects? I leave this question open for further research.

[2] In an alternate set of treatments (not reported), I relax the restriction that the distribution of the level of education must be identical across groups: this does not alter the results.

Table 4: Post - Pre in Treatment and Control Conditional on the Share of the Foreign-Educated in Treatment s

s	Normalized Post - Pre in Treatment	Normalized Post - Pre in Control	Mean Difference
0.5	0.021	0.020	0.001
	(0.016)	(0.015)	(0.022)
0.6	-0.014	0.054	-0.068***
	(0.015)	(0.016)	(0.022)
0.7	-0.041	0.081	-0.122***
	(0.015)	(0.016)	(0.022)
0.8	-0.072	0.112	-0.183***
	(0.015)	(0.016)	(0.022)
0.9	-0.106	0.146	-0.253***
	(0.015)	(0.016)	(0.022)
1	-0.141	0.181	-0.322***
	(0.015)	(0.016)	(0.022)

Notes. For each leader, the Post – Pre estimated from the baseline specification (Table 1 Column 1) is normalized by the distance to the technology frontier (see data appendix) to account for differences in development. s denotes the share of the foreign-educated in treatment; "s = 0.6" means that 60% of the foreign-educated leaders have been allocated to the treatment group. The treatment and control groups are of equal size and have identically distributed educational achievement. Under the null hypothesis that foreign education does not matter, Post – Pre is not significantly different across treatment and control. Significance at the 1, 5 and 10% levels is denoted by ***, ** and * respectively and reflects results from t-tests on the mean difference of Post – Pre. Standard errors in parentheses.

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