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IZA DP No. 11647

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ABSTRACT

What Accounts for the US Ascendancy to Economic Superpower by the Early 20th Century: The Morrill Act – Human Capital Hypothesis*

Maddison's international panel data show that technically it was the faster growth rate of the US economy that led to its overtaking the UK as economic superpower. We explore the contributing factors. Identifying the land-grant colleges system triggered by the 1862/1890 Morrill Acts (MAs) as a major contributor, we develop this hypothesis theoretically and test it via difference-in-differences regression analyses viewing the MAs as the experiment, the US or US states as treatment groups, and the UK as chief control group in the country-level comparisons. Using national and state-level data, we estimate that the MAs produced sizeable educational and economic returns which catapulted the US into its leading status.

JEL Classification: N3, E24, H42, I2, O1, O57, O4

Keywords: aggregate human capital, public investment, education and research institutions, economic development, endogenous growth; institutions and growth, comparative studies of countries, economic history-education

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* This paper builds on earlier works by Ehrlich (2007, 2008). An earlier short version (Cook and Ehrlich, 2017) was presented at the ADBI-AGI conference "Public and Private Investment in Human Capital and Intergenerational Transfers in Asia," 14-15 November 2017. We are indebted to Jinyoung Kim, Zhiqiang Liu, Jong-Wha Lee, Naoyuki Yoshino, and participants at the ADBI-AGI conference for valuable comments and to Robert Tamura for sharing his data on historical enrollments in tertiary institutions and compiled state-level GDP series.

1. INTRODUCTION

The question motivating this study is whether the ascendancy of the US to a global economic power in the 20th century, for which numerous explanations have been offered by economic historians, can be explained, at least in part, by the Land-Grant colleges and universities system triggered by the Morrill Act of 1862 and subsequent expansions.[1] We pursue this question through a quasi-experimental design (QED) methodology in which the Morrill Acts are viewed as the experiment triggers, the US is the treatment group, and the UK – the world's economic superpower in much of the 19th century – serves as the main control group. While the specific aim of the study is to test the degree to which the US higher education movement, for which no parallel existed in the UK commonwealth or elsewhere in Europe at the time, has been a major factor explaining the US overtaking the UK as an economic superpower, the more general objective of the paper is to test the power of the "human capital hypothesis" to explain observed differences in long-term growth dynamics across specific countries and states.

Despite the wide acceptance in the endogenous growth literature of the idea that "human capital", incorporating embodied ability and skill as well as disembodied knowledge, is a major engine or driver of persistent and self-sustaining long-term growth (see, e.g., Lucas, 1988; Becker, Murphy and Tamura, 1990; Ehrlich and Lui, 1991), studies that have attempted to confirm this idea empirically have met with mixed success when using general schooling measures as proxies for human capital. These studies do confirm that general education, or formal schooling, raises the level of per-capita GDP, but the results concerning schooling's impact on the rate of economic growth have been more mixed (see e.g., Bils and Klenow, 2000; Barro, 1991; Ehrlich, 2007). In this context, our study deviates from much of the previous literature in two important ways: we select higher education as the education channel through which knowledge capital can drive technological innovations and factor productivity, or per-capita income growth.[2] Also, perhaps more import, the quasi-experimental methodology, which we pursue in this paper to link causally the Morrill Act with the subsequent spurt of higher education formation and the long-term rate of per-capita GDP growth in the US, may provide a more direct support for the validity of the "human capital hypothesis" as the underlying engine, or facilitator of endogenous economic growth.

Compared to the UK, where the Industrial Revolution began in the last part of the 18th century and spread to other major European countries, the US was a poor country. In the last few decades of the 19th century, and especially during the early part of the twentieth century, however, the U.S. *overtook* the U.K. and the other European economies, and went on to develop a considerable advantage over these countries in terms of not just GDP level, but per-capita GDP as well. The comparison of the US, especially with the UK, is made not just because the UK had reigned as the world's economic superpower in the 19th century, but also because the US had inherited its basic institutional and cultural setting from the UK since its inception as a UK colony and a destination country for largely English-speaking immigrants.

What may be less known is that over the same period the U.S. begun developing a considerable edge over Europe in the schooling attainments of its labor force, especially at the higher education level. That gap remained significant through the twentieth century, although it narrowed in the latter part of it, and is continuing to narrow. Largely accounting for the gap was the massive high school movement of 1915–1940, but an independent gap emerged as early as the 1860s with the U.S. foray into tertiary education beginning with the Morrill Acts of 1862 and 1890, and continuing especially with the GI Bill and the massive higher education movement following World War II. A basic argument of this paper is that the U.S. lead in knowledge formation, imperfectly measured by even higher educational attainments, has been a major, if not decisive instrument through which the U.S. overtook Europe as the economic superpower by the early twentieth century.

To illustrate the case empirically, it is worth noting that by popular measures of real income used for international comparisons (GDP, adjusted by Purchasing Power Parity) the US still maintains a considerably larger level of per-capita income relative to practically all major economies in the world. In the early 1800s, however, the US levels of GDP and GDP per capita were considerably below those of the UK, and it was not until 1872 for GDP and 1905 for GDP per capita when the U.S. first surpassed the United Kingdom in economic prowess.

Figures 1 and 2 illustrate the comparison poignantly. Abstracting from year-to-year and cyclical fluctuations, both the U.S. and U.K. graphs relating the logarithm of GDP or GDP per capita to chronological time appear over the long haul to resemble the shape of an upward-sloping straight line. The slope of each line represents the long-term annual growth rate of GDP or GDP per capita. The fundamental difference is that the slopes are higher for the U.S. relative to the U.K. In other words, the U.S. has overtaken the U.K. because its long-term growth rates have been higher: Over the 142-year period 1871–2012 (starting at the point of overtaking) the U.S. versus U.K. GDP growth rates have been 3.31% versus 1.88% per annum while the corresponding per-capita GDP growth rates were 1.8% versus 1.4%.[3] In recent decades, these gaps have narrowed. For example, over the period 1961–2012, the comparative growth rates of GDP in the U.S. versus the U.K. were 2.99% versus 2.13%, while those for per-capita GDP were 1.95% versus 1.89%, respectively.[4]

Our basic thesis is that observed differences in long-term per-capita income growth rates stem primarily from differences in the rates of growth of innovative human capital. In this context, the higher pace of growth in the US relative to the UK which began in last part of the 19th century, may have occurred in large measure through the channel of higher education, which the US pioneered decades before the UK and other major economic powers. Both human capital formation and its impact on growth, however, are ultimately conditional on supportive institutional factors which *reward* knowledge formation and innovative entrepreneurship.

In this context, our paper is largely a follow-up on earlier papers by one of us (Ehrlich 2007, and 2008) which have advanced this thesis. These papers sought to determine whether the thesis has legs to stand on through a survey of stylized facts and related literature indicating that the US led other major developed countries in higher educational attainments and basic research support per adult population, beginning in the latter part of the 19th Century and continuing over the 20th century, especially at the secondary and tertiary levels.

The point of departure in this paper is our attempt to examine the thesis by articulating a prototype endogenous growth model in which institutional factors coalesce to produce a significant favorable shift in individual opportunities to invest in higher education and thereby precipitate a more intensive rate of human capital formation and per-capita income growth. More important, we implement the model's specific propositions through a comprehensive empirical investigation of the long term impact of the Morrill Land Grant Acts of 1862 and 1890, to explain the comparative growth dynamics of the US vs. the UK as well as the diversity in the intermediate growth dynamics across the US states that asynchronously established land grant universities.

Our empirical implementation involves three related econometric tests based on (QED) methods and related difference-in-differences (DID) regression models. The first focuses on explaining the emerging differences between both higher educational attainments and per-capita income growth rates in the US vs. the UK and its colonies over the 10, 20, 30, and 50 years following the Congressional adoption of the Land-Grant-funded higher education system in 1862. The second is designed to bolster the findings of the country-level analysis by pursuing a similar (QED) method by which we seek to explain the differences in the growth rates of individual US states, based on the year the Land Grant Act was actually implemented in each state, as well as the intensity with which the act was implemented, based on the number of institutions of higher learning established across states. The third model similarly tests whether the effects we have ascribed to the Morrill Act following the adoption of the first act in 1862 also extend to the second Morrill Act of 1890, in which the method of financing was direct financial support rather than a land endowment. All such analyses are supported by robustness tests and corroborating, counter-factual regression analyses.

The paper proceeds as follows: In section 2 we include a short literature review. In section 3 we develop our prototype model. In section 4 we discuss the QED methodology and panel data we apply to test the impact of the Morrill Act of 1862 on the US economy, using the UK and other sets of countries as control groups, and in section 5 we report the results of the country-level regression analysis. Likewise in sections 6 we discuss the regression models we use to implement our QED method against US states' panel and cross-state data and report the results of the state-level analysis. In section 7 we repeat the state-level tests using the second Morrill Act of 1890 as an independent treatment. In section 8 we conclude.

2. LITERATURE REVIEW

We are not aware of studies that have attempted to link directly the Morrill Acts with the long term – or even the short term – growth dynamics in the US or in other countries. Most of the empirical literature has resorted to elementary and high-school attainments – length of schooling or qualitative measures of such schooling – in order to assess the link between human capital formation and the rate of growth of GDP per capita, using international data (e.g., Barro 1991, Barro and Lee 1994, Bills and Klenow 2000, and Hanushek et al 2017). As noted in the introduction, while these studies generally find a significant association between these measures of general schooling and the level and rate of economic growth, the impact of schooling on long-term growth is often weak and inconclusive, partly because of the difficulty in establishing a causal effect running from schooling to economic growth. Goldin and Katz (1999b) focus primarily on the "high school movement" between 1910 and 1940 as the driving force behind the US advent into world leadership in education in the 20th century, but this study does not link this development directly with economic growth.

More important in the context of this study, Goldin and Katz (1998, 1999a) and Katz (1983) do discuss the possible impact of institutions of higher education on economic development in the 19th century. Katz (1983) notes that the elite US colleges (particularly Harvard) became increasingly exclusive during the 19th century and contributed little to upward social mobility. They conclude that, by focusing on only wealthy families, the elite US colleges played a prominent role in the formation of a politically-empowered Northeastern elite, but did little to build a highly educated labor force. By contrast, in their 1998 study, Goldin and Katz find that exogenous technological shocks within the US' knowledge industry starting in the late 19th century resulted in a significant expansion of the scale and scope of the American university system between 1890 and 1940. They point out that the decades following the 1890s were a period of rapid expansion of American higher education which has raised the US to a position of prominence in terms of its higher education system worldwide.

Our study is quite compatible with Katz's and Goldin and Katz's view, and it offers indirect support for this view through our empirical analysis, which shows that the Morrill Acts and later expansions of that act have exerted lasting effects on measures of both human capital formation and per-capita income growth well into the 20th century. We find support for this view via our quasi-experimental study of the role of the first (1862) and second (1890) Morrill Acts using both international comparisons and US state-level data. Where we differ from these authors is in the identification of the trigger that is responsible for the relatively faster growth of the knowledge industry and economic output in the US relative to other major economies since the 19th century.

The simple logic behind this thesis is that without the First Morrill Act, there could be no 1887 Hatch Act, which established agricultural research stations that produced important innovation in agribusiness; there would be no 1890 Second Morrill Act that established the Historically Black Colleges and Universities (HBCUs), and no 1914 Smith-Lever Act, which broadened awareness of the agricultural and mechanical discoveries made at the land grant universities. In the following sections we test the two-part working hypothesis that: a. the Morrill Act was a largely exogenous policy change that exerted a pronounced effect on the growth of higher education in the US in the period 1862-2000; and b. that this growth of the "peoples", or "farmers", or "democracy" colleges, as they were termed in that period, has served as a driver of human capital, or knowledge capital formation, that explains both the faster growth of the US relative to the UK and other developed European countries, as well as the diversity in the long-term growth rates of US states that implemented the Morrill Act at varying time periods over the 19^{th} century.

3. THEORETICAL FRAMEWORK

We adopt a simple variant of the endogenous growth model offered in Lucas (1988), and Ehrlich et al. (2017) to explain how an exogenous shift in educational policy such as the Morrill Act nay have brought about a durable surge in the pace of economic growth within a regime of balanced growth. The case in point is the sharp rise in the US economy's growth rate illustrated in figures 1 and 2. Since the US has already been growing at a stable pace resembling that of the UK in the early part of the 19^{th} century, the model can help identify the channels through which the Morrill Act brought about the US surge and guide our empirical estimation of its causal effects.

a. Model specification:

Formally, we consider a closed and competitive economy with an implicit regulatory role for government. The implicit role is illustrated by allowing for government policies, such as the Morrill Act, to extend the opportunities for agents to invest in their human capital beyond the level of skill represented by elementary and high school education. For simplicity we assume that the latter are self-produced, and that the Morrill Act made it possible and affordable for agents to extend their education by enrolling in Land Grant public institutions of higher learning.

We assume that the economy includes a mass, N, of identical, infinitely-lived individual workers (agents) who optimally allocate their productive capacity – an endowment of productive time (normalized to unity), augmented by the agents' human capital – to either current production of final goods and services (effective labor), or investment in human capital (education or schooling). Specifically, we conceive of human capital as a store of productive knowledge incorporating both an embodied skill component, and a disembodied (transferrable) knowledge component, each of which augments the fractions of production capacity (working time), devoted to the production of current goods, denoted by $1-s(t)$, and the creation of new productive knowledge, denoted by $s(t)$, respectively. Each agent also accumulates physical capital through savings, and serves the triple role of producer, investor, and consumer. Note that by assuming that agents are infinitely lived, we cannot model fertility or other intergenerational transfers as additional choice variables, and treat population or labor force size (N) as a given constant. The basic propositions we derive in this section, however, can be shown to hold under more general human-capital-based endogenous growth models, where life is finite and fertility and savings are choice variables as well. [5]

Abstracting from leisure as a choice variable, the effective labor supplied by each agent, adjusted for the agents' production-oriented skill component associated with human capital, is thus given by $H(t)(1-s(t))$, and the effective labor force or total labor supply in the economy becomes $L(t) = NH(t)(1-s(t))$. Denoting the accumulated stock of physical capital by $K(t)$, we specify the economy's production of final goods via a Cobb-Douglas function

$$\text{(1) } Q(t) = AF[K(t), L(t)] \equiv AK(t)^{\beta}\{NH(t)(1-s(t))\}^{1-\beta},$$

where $\beta \in (0,1)$. A represents a factor-neutral non-rival production technology which is assumed to be exogenous and static.

Defining output in per-capita terms, $q(t) \equiv Q(t)/N$, equation (1) becomes a function of the average skill level in the economy $H(t)$ and the capital to labor ratio per worker, $k(t) = K(t)/L(t)$ as follows:

$$\text{(1a) } q(t) = Ak(t)^{\beta}\{H(t)(1-s(t))\}^{1-\beta}.$$

The representative agent's human capital production, or accumulation function, in turn, is given by the following process:

$$\text{(2) } \dot{H}(t) = IH(t)s(t),$$

where I stands for an institutional factor that can favorably affect the quality or effectiveness of individual investment in knowledge. In equation (2) $H(t)$ reflects the agents' transferrable knowledge-component associated with human capital – the level of accumulated knowledge that can be transferred or transformed into new or additions to knowledge, $\dot{H}(t)$. Equation (2) implies that human capital (both skill in production and innovative knowledge) can accumulate over time as long as there is positive net investment in education, or schooling, $s(t)$ by individual agents. This investment is augmented by the previously accumulated stock of productive knowledge, $H(t)$ starting from a positive endowed level (H(t=0)=H(0)).

The specification of equation (2) as linear with respect to accumulated past knowledge follows from the basic premise of the human-capital-based endogenous growth paradigm that knowledge is the only productive capital asset that is not subject to diminishing returns (Ehrlich and Murphy, 2007 paraphrasing John Maurice Clark, 1923). This specification makes human capital the engine of growth in the model.[6] Equation (2) also implies that human capital accumulation can further be augmented by an institutional factor, I. In the context of this paper, I captures the impact of the land grant on individual investments in human capital through the channel of higher education. Since the grant is in the form of unimproved federal land, we abstract from any direct financial consequences to the federal government. More generally, I can be interpreted as any institutional, structural, or policy measure that enhances the incentive to invest in innovative knowledge.

Closing the system, the accumulation of physical capital per capita is given simply by,

(3) $\dot{k} = Ak(t)^{\beta}\{H(t)(1 - s(t))\}^{1-\beta} - c(t)$,

where $c(t)$ is consumption and the right-hand side of equation (3) savings as a fraction of output. For simplicity we abstract from any depreciation in the physical capital stock.

b. Decision rule and optimal choice:

The agents derive utility from consumption, and the preference of each agent is defined as

(4) $\int_0^{\infty} e^{-\rho t} \frac{1}{1-\sigma}\left(c(t)^{(1-\sigma)} - 1\right)dt.$

Using (2), (3) and (4), we can write the current value Hamiltonian for our optimization problem as

(5) $v(k, H, \theta_1, \theta_2, c, s, t) = \frac{1}{1-\sigma}(c^{1-\sigma} - 1) + \theta_1\left(Ak^{\beta}\{H(1-s)\}^{1-\beta} - c\right) + \theta_2 IHs$,

where consumption per capita, c and schooling investments, s, are the control variables in this problem and θ_1 and θ_2 are the co-state variables, or shadow prices of physical and human capital, respectively.

Using the first-order conditions for the optimal values of the control variables {c, s} and co-state variables $\{\theta_1, \theta_2\}$ along with the production functions (1a)-(3), we can derive the conditions characterizing the economy's equilibrium balanced growth path as follows:

(6) $\frac{\dot{q}}{q} = \frac{\dot{c}}{c} = \frac{\dot{k}}{k} = \frac{\dot{H}}{H} = \frac{I-\rho}{\sigma} \equiv g^*.$

Equation (6) indicates that in the steady state, balanced growth equilibrium, the growth rate of real output per capita, and hence the equilibrium rates of growth of consumption, physical, and human capital share the common value g* = [(1-ρ)/σ]. Several propositions follow.

c. Propositions

Inserting the growth rate from equation (6) into the human capital accumulation function, we can derive an explicit solution for the balanced growth equilibrium rate of investment in human capital

$$(7)\quad s^* = \frac{1}{\sigma}\left(1 - \frac{\rho}{I}\right) .$$

Proposition 1: *The economy is in a regime of persistent growth if, and only if, the institutional factor supporting the effectiveness of investment in human capital exceeds individual time preference, or I > ρ.*

Proof: The proposition follows from equations (2) and (7). A sufficiently pro-knowledge institutional setup (*I*) can assure that optimal investment in human capital is positive, allowing the economy to reach a steady-state growth equilibrium.

Proposition 2: An exogenous upward shift in the magnitude of the institutional factor contribution to investment in human capital raises both the steady state level of investment in schooling and the per-capita income growth rate. The impact is subject to diminishing returns.

Proof: From the expression of g^* in (6) and the optimal solution for s^* in (7) we can see that

$$(8)\quad \frac{\partial s^*}{\partial I} > 0; \frac{\partial^2 s^*}{\partial I^2} < 0; \frac{\partial g^*}{\partial I} > 0$$

Proposition 3: *A rise in the optimal investment in schooling as a result of an exogenous shift in one of the parameters that affects its value in equation (7) has an ambiguous effect on the per-capita output level at an early phase of the transition to a new steady state. But it exerts an unambiguous increase in the steady state growth rate along the balanced growth equilibrium path.*

Proof: Using equations (1a) and (2), we can derive the per capita output and human capital stock paths over the transition as a result of the shift in optimal investment in schooling following the institutional shock as follows:

$$(9)\quad \frac{\partial q(t)}{\partial s(t)} = (1-\beta)q(t)\left(It - \frac{1}{1-s(t)}\right).$$

From equations (2) and (6) we can also infer that in the new steady state resulting from an exogenous increase in the rate of investment in human capital we have:

(10) $\frac{\partial \hat{q}}{\partial s(t)} = \frac{\partial \hat{H}}{\partial s(t)} = I > 0,$

where $\hat{q} = \frac{\dot{q}}{q}$ and $\hat{H} = \frac{\dot{H}}{H}$.

The effect of an exogenous rise in investment in education on the growth rate of both human capital and per-capita output is thus shown to be dictated by the magnitude of the supporting institutional factor. Moreover, equations (9) and (10) imply the following corollary:

Corollary: The larger the value of a shift in an *institutional* factor that improves the effectiveness of investment in human capital, *I*, the greater would be its impact on both the short-term increase in per-capita output level, and the long-term growth rate of human capital formation and per-capita output along the steady-state balanced growth path (BGP).

The preceding propositions following our simple model are nevertheless sufficiently general in terms of the implications they offer about the potential role major public policy initiatives can play in effecting a durable shift in the economy's balanced growth equilibrium path. They also identify knowledge capital formation as the channel through which these institutional factors can affect the economy's rate of growth of productivity and per-capita real income. In our following empirical implementation, we use the Morrill acts as exogenous policy shifts that may have contributed to the surge in the US' long-term rate of economic growth through the higher education channel, and estimate the latter's potential quantitative importance.

4. EMPIRICAL IMPLEMENTATION

Following the theoretical model, we test the hypothesis that the Morrill acts triggered a sharp increase in economic growth through higher education formation, using a quasi-experimental-design (QED) framework which also enables us to assess the quantitative effect of the acts on indicators of human capital formation and per-capita income growth.

Specifically, we formulate a set of reduced-form difference-in-differences (DID) regression specifications in which the Morrill Act is the regulatory event, or 'treatment', using the QED terminology; the US is the 'treatment (or treated) group'; and the UK is the 'control group'. We apply these specifications to test the Morrill Act effects on the pace of human capital formation (through the higher education channel) and the rate of per-capita income growth in the US.

A. The implementation strategy: a natural experiment

a. The Land-Grant College Act of 1862 (the 1st Morrill Act)

Prior to the 1860s, higher education was the privilege of the offspring of just a tiny fraction of the population in both the UK and the US. While university tuition itself was quite low in the US and the UK during this era, prospective students (only males) were required to provide their own living arrangements (including food and servants.) These expenses alone excluded all but the wealthiest families from sending their children to university.

Religious affiliations further restricted higher education access in the UK: In order to be granted a degree from Oxford or Cambridge, a student was required to be a member of the Anglican Church. For 600 years, opposition from Oxford (est. 1096) and Cambridge (est. 1209) prevented a third degree-granting university from being established. It was not until 1837 when Durham University was granted a third royal university charter.

Modeled after the British universities, the early American universities, notably Harvard (est.1636) and Yale (est.1701), e.g., also had strong ties to theological organizations. Like their British counterparts, the cost of room and board, books, clothing and other supplies fell entirely upon the scholar—effectively serving just students from the wealthiest families.

The push for establishing a secular public higher education system in the US started in the early 1850s. The catalysts were members of the Illinois' Congressional delegation who persuaded Representative Justin Smith Morrill of Vermont to introduce it in Congress in 1853. But the bill failed largely as a result of opposition by conservative legislators and President Buchanan who were concerned about using public money to finance higher education.[7] Morrill made a second, and this time successful, attempt to pass his bill by Congress in 1857, but that was vetoed by President Buchanan who was a strict states-rights, anti-intervention Southern Constitutionalist. Only after the Southern delegation's secession from Congress in 1862 was Rep. Morrill finally successful in passing his "Land Grant Act."

There seems to be a general consensus among historians about three coincidental factors that may have converged in 1862 to help pass the act. The first is the stated mission of the public university system (see fn. 1), which was the study of agricultural and mechanical arts (the precursor of engineering), including military arts. Since the US was already in the first phase of the Civil War, this mission offered potential support for the Union army's military needs. The second factor was the willingness of President Lincoln, who had an interest in cultivating education for the masses, to sign the bill.[8] The third, and a major motivating factor, was the ingenious financing plan devised by Justin Morrill. Since the War effort depleted Federal coffers, Morrill suggested granting US states "scrips" of unimproved federal lands, rather than money, in exchange for building universities. Each Congressman and Senator was given 30,000 acres of federal land that could be combined to fund educational institutions by selling the land and raising funds to establish and endow "land-grant" colleges.[9]

The number of higher education institutions in the US exploded as states rushed to cash in on federal land grants. Between 1862 and 1889, 45 institutions of higher education were established in the US under the provisions of the First Morrill Act. Moreover, within the course of two decades, higher education became vastly more accessible and enrollments soon rose as well (see Figure 3). To our knowledge there was no comparable effort at the UK to pursue the establishment of a public university system until 1900. For this reason, we have identified the Morrill Act of 1862 as the trigger (and "treatment") for the quasi-experimental study we focus on in this section using country-level data. This act was followed by a second Morrill Act of 1890, which we also examine in our empirical analysis in section 7, using US states' data.

b. Justifying the quasi-experimental design

We consider the 1862 Morrill Act to be a good candidate for a quasi-natural experiment that can be used to measure the educational and economic impact of the US Land-Grant-funded university system in comparison with UK's educational system because of its apparent consistency with the major critical conditions that need to be satisfied in a valid natural experiment:

1. ***Strong Separation***. The quasi-natural experiment is based on the assumption that assignment to treatment and control groups is "as good as random". One concern is that the large influx of immigrants from the control group could corrupt our identification strategy if those immigrants did so for the purpose of acquiring higher education. This behavior would violate the "as good as

random" assumption. Over the post-Morrill Act time period, there was, indeed, a massive immigration wave from around the world into the US, especially from Europe and Asia, but it was largely comprised of unskilled immigrants lacking English language proficiency, who went on to compose the bulk of the low-wage US industrial labor force (see Glynn, 2011). Owing to their skill deficit, however, the first generation of immigrants entering the US between 1840 and 1900 were consequently quite unlikely to pursue higher education studies following entry to the US.

2. ***Similar pre-treatment trends.*** For the United Kingdom to be a useful control group for the US over the 1820-1900 period, the growth rates of higher-education institutions and per-capita GDP must be reasonably similar *prior* to the start of the Morrill Act treatment. The rationale is that in controlled experiments, comparability of treatment and control groups is achieved simply by *a priori* randomizing of the subjects in both groups. When observational data are used, the operational assumption is that the two groups must be "comparable", and while the assumption is not explicitly testable due to unobserved counterfactuals, the assignments of treatment and control can be justified by the *"parallel trends assumption"*

Prior to 1850, universities were socially exclusive in the US and UK. In 1800, the US had 18 degree-granting universities and the UK had 8, but access to higher education was comparable across the two countries in terms of universities per population and tertiary enrollments (see Figures 4 and 5, respectively). The same holds for per-capita income growth rates in the UK and the US (see Figure 6). Figures 4, 5, 6 thus indicate that the economies of the US and UK shared similar trends in universities, tertiary enrollments and GDP growth per-capita prior to 1862.

3. ***Exogeneity.*** The policy shift must be an exogenous event to overcome potential simultaneity relations between the dependent variable (per capita GDP growth) and the policy shift. The history of the emergence of the Morrill Act indicates that the latter was the result of the random confluence of the institutional and political factors stated in the previous subsection: the start of the Civil War that generated support by the Union states for mechanical and military arts; the departure from Congress of fiscally conservative Southern states; the tacit support for the act by President Lincoln; and the ingenious financing plan of Rep. Morrill that coalesced to pass the act in 1862. In fact, had it not been for the convergence of the political, military needs, and personal factors mentioned in subsection a, the act would have probably failed again in 1862 and delayed to a much later period.

4. ***Confounding factors.*** It follows from the logic of our model that the first Morrill Act could serve as a causal factor triggering the surge in higher education institutions and student enrollments and thus in per capita GDP. But were there other confounding treatments taking place at the same time that could explain part or all of the observed changes in per-capita real incomes? In 1862 there were two other acts passed by Congress: the Homestead Act, which opened up the Midwestern and western United States to settlers, and the Pacific Rail Act, which increased access to the interior of the country by private railroad firms, were also signed into law. Moreover, the reconstruction period that followed the disruption caused by the Civil War could have also impacted the level rate of economic growth. The First Morrill Act, however, fundamentally differed from these acts.

Although the 1862 Morrill Act was less extensive than that of the other two acts in terms of total acreage of federal land disbursed,[10] the acts have differed greatly in terms of their impact on knowledge-creation and productivity growth. The Homestead and Pacific Rail Acts were investments in territorial and trade expansion – opening the American Midwest and West to settlers and immigrants, many of whom proved ultimately to be *transient.* But these acts may have exerted primarily a "level" effect on GDP, albeit not necessarily on GDP per capita (GDPPC), while the Morrill Act's land grants were investments in human capital formation and thus, by our theoretical considerations were more likely to promote a higher rate of *sustained* growth in GDPPC going forward. Indeed, economic historians have ascribed only a modest contribution of rail transport to GDPPC over the latter part of the 19th century.[11] The same can be said in connection with the influx of Western and Northern European immigrants during 1840-1900 who were generally low-skilled and provided the bulk of raw labor for the growing industrial sector (Glynn, 2011). Similar considerations apply also to the period of reconstruction activity in the South following the Civil War which may have had transitory level effects on economic activity but not necessary long-term growth effects.

Controlling for the potential bias in our estimation of the net impact of the Morrill Act on per-capita income growth due to the confounding effects of these other events, however, does remain a major challenge in our analysis of the first Morrill Act, which we try to overcome using state level data. An independent natural experiment we can exploit to control for potential confounding effects is the second Morrill Act of 1890, since this policy experiment is not affected by similar confounding factors. Maintaining the required separation between treatment and control groups (US vs. the UK) using our country-level data, however, is no longer possible beyond the 1890s – the UK began its own civic university expansion in 1900. As a result, we are constrained to measure the effect of the second Morrill Act and conduct related tests using state level data, which we purse in sections 6 and 7.

B. Data used in the empirical analysis

a. Universities founded and tertiary enrollment data

The annual data on the evolution of the stocks of colleges and universities founded between 1820 and 2000 across different countries, which we use in our country-level DID regression analysis, come from the **Clio-Infra** project (see www.clio-infra.eu) compiled by Peter Foldvari (2014). In the country-level analysis, we also use decennial data on higher education enrollments (1820-2000), which we have taken from Tamura et al. (2015).[12] Additional data on US tertiary enrollments per one-hundred 18-24 year old population are taken from the National Center of Education Statistics (NCES) publication entitled, "120 Years of American Education: A Statistical Portrait" (1993) and US Census data from 1840 through 1860.

b. Country-level Per-capita GDP data

The annual per capita real GDP data we use in our country-level DID regression analysis [13] (real 1990 Geary-Khamis dollars) are taken from the historical series initially compiled by Maddison (2010) on GDP and GDP per capita and later updated to span A.D. 1-2013 by Bolt and Zanden (2014). Note that the time span of our sample (1820 – 2013) is quite distinct historically – the US achieved its independence only 40 years prior to the beginning of Maddison's (2010) data series.

Using these data, we construct time series for forward looking 10, 20, 30, and 50 year average per capita income growth associated with each year, following the method used in Barro et al. (1992).[14]

c. Additional US state data used in our corroborating state-level analysis

In the US-state natural experiment which we explore in section 6, data on state per capita real income are obtained from Turner et al. (2007), where state income is computed as the value added for each industry in the state, starting in 1840.[15] We also use data about the evolution of land grant universities across US states by year of establishment over the period 1862-1968, following the ratification of the first Morrill Act. Data about the year of establishment of each land-grant university as well as the number of land grant universities established in the same state (accounting for the act's intensive margin) is collected from individual university websites and Wikipedia.[16]

5. THE 1862 MORRILL ACT IMPACT AT THE NATIONAL LEVEL

A. The DID Regression Analysis

a. Specifying the regression models

The specification of the relevant DID regression model follows Proposition 2 of our model, which predicts the effect of an exogenous policy shift, here identified as the Morrill Act, on the equilibrium level of investment in higher education, as well as on the resulting long-term equilibrium of growth rate of real GDP per-capita (GDPPC). [17] We estimate the policy's educational effects through two indicators of investment in higher education: the number of universities founded per capita, and the enrollment rate in tertiary institutions. Since the impact of the policy shift on the balanced growth path is expected to evolve through a transition period, we proxy the growth rate (g^* in equation 6) by computing average forward looking growth rates of GDPPC from annual data over alternative horizons of alternative length, *T*. In all regressions, we limit the sample to include observations over 1820-1900 to guard against potential confounding effects due to the "redbrick" university expansion in the UK (see n.12).

We first test the impact of the 1862 Morrill Act (*MA*) on the stock of universities per capita ($UNIVPC_{it}$), where *i* denotes the country and *t* the time period, via the following reduced-form regression equation:

$$(11)\ UNIVPC_{it} = \alpha + \beta_t Y_t + \gamma_i country_i + \delta(MA_t \times country_{USA}) + X'_{it}\xi_{it} + \varepsilon_{it}$$

In equation (11), $country_i$ represents a country dummy taking the value $country_{USA} = 1$ for the US and 0 for the UK (or related control groups), β_t denotes year (Y_t) fixed effects, and γ_i denotes country fixed effects. The land grant dummy variable, MA_t, takes a value of 1 after the enactment of the first Morrill act in 1862 and zero before that time, accounting for the treatment's extensive margin. In this setup, the difference-in-differences estimator δ measures the impact of the Morrill Act policy on universities founded per population unit – its standard role in DID specification. A vector of covariates, denoted by X_{it}', is added to include the log **per-capita** income as an added control variable to account for country-level differences in the per-capita income levels, but lagged 10 years to avoid a potential simultaneity bias.

Second, we examine the impact of the Morrill Act on tertiary enrollment rates per 100 population aged 18-24., *Tertrate*, through a similar regression model:

$$(12)\ Tertrate_{it} = \alpha + \beta_t Y_t + \gamma_i country_i + \delta(MA_t \times country_{USA}) + X_{it}'\xi_{it} + \varepsilon_{it}$$

Due to data limitations, we can only use *decennial* fixed effects in this estimation.**[18]** The DID estimator δ in equation (12) here measures the impact of the Morrill Act on tertiary enrollment rates. The term X_{it}' represents the same vector of correlates used to estimate equation (11).

Finally, we estimate the Morrill Act's impact on GDP per-capita rate of growth by using a similar reduced-form regression model:

$$(13)\ \log(1 + g_{T,y_{it}}) = \alpha + \beta_t Y_t + \gamma_i country_i + \delta(MA_t \times country_{USA}) + X_{it}'\xi_{it} + \varepsilon_{it}$$

In equation (13), the dependent variable is the natural log of the forward looking T-year average gross growth rate of real per capita income in country *i* at year *t.*[19] The treatment effect estimator δ, measures the difference in the natural log of the growth rate of real per capita income in the United States versus the chosen control group, compared across the pre- and post-Morrill Act periods. In this application, the vector X_{it}' includes initial log per capita income instead of lagged log per capita income, to control for the initial level of per-capita real GDP in estimating its evolving growth rate over the transition phase following the act, as indicated by proposition 3.

As mentioned earlier, we allow for alternative time horizons (T) when estimating the impact of the Morrill Act on the equilibrium long-term per-capita GDP, but limit them to 4 such measures: T=10, 20, 30, and 50 years.[20] Averaging per capita income growth over relatively long forward looking time horizons is done also for another objective: to allows the DID estimator to capture any *lagged growth effects* from the passing of the Morrill Act to the subsequent establishment of the Morrill land grant universities. The potential time lags between the passage of the Morrill Act and time of adoption of a land grant, or the time in which the higher education investment becomes effective in production should therefore not affect to a significant degree the estimated treatment effect we ascribe to the Morrill Act at the country (or the state) levels.

In each of the three equations (11)-(13) the coefficient δ estimates the treatment effects coming from a shock in the institutional factor, *I*, in equation (8). Together, these coefficients thus offer a direct test of proposition 2, and indirectly of propositions 1 and 3 as well, that the Morrill Act exerted a positive effect on the long-term growth rates of per-capita real income through enhanced investment in human capital formation.

b. Forward looking growth-rate bias due to post-treatment estimation bias

A bias can arise in our estimate of the impact of the Morrill Act on growth, however, because of the way we need to calculate the average per capita GDP growth rates in the treatment group. For example, when we consider the 50-year average growth rate in 1850 in any US state, 38 of the 50 years between 1850 and 1900 take place *after* the enactment of the first Morrill land grant act. As a result, some growth observations in the pre-event period will be "post-treatment-contaminated" by the introduction of the Morrill Act, and become inflated upwards. This contamination will thus *diminish* the estimated value of the DID estimator concerning the treatment effects on the forward looking growth rate.

To overcome this bias, the offending observations (e.g., the 1860 observation in estimating the treatment effect on the 10-year growth rate**;** the 1850 and 1860 in estimating the effect on the 20-year growth rate; and the 1840, 1850 and 1860 observations in estimating the effect on the 30-year growth rates) are omitted to assure that the treatment effects are estimated without the downward bias. The treatment effects related to the 50 year growth rates, however, are estimated using growth rates calculated with all observations, including biased ones, due to the limited sample we have in the pre-Morrill Act period.

c. Choice of control groups

The most relevant control group in this analysis is the United Kingdom because it implicitly controls for the role of similar political, economic, legal and cultural institutions, which the 2 countries have shared throughout the pre- and post- Morrill Act event.

As robustness tests, however, we test the effects of the Morrill Act against data on additional major economies. These include a "Western British Offshoots" control group composed of the United Kingdom, Australia, New Zealand and Canada. These countries were at one point British colonies and had similar British-influenced institutions during the pre and post-treatment periods. We also use as a control group the 5 largest economies of Western Europe (UK, France, Germany, Spain, and Italy) which share a similar economic system. None of these countries had established a public higher education system similar to the Morrill Acts.

B. Regression results at the national level

a. The Morrill Act effect on university formation per capita

Using the data collected by Foldvari (2014) and the difference-in-differences regression specified by equation (11), we first examine the impact of the 1862 Morrill land grant act on indicators of higher education formation, starting with the number of universities per capita in the US compared to the UK and other relevant control groups. The separate controls used in this analysis are comprised of the UK alone, the block of the Western British Offshoot (WBO) including the UK, Australia, New Zealand and Canada; and the block of the 5 major European economies in the 19th century that includes the UK, France, Spain, Germany and Italy, which we dub as EU5.

The estimated effects of the 1862 Morrill Act on the growth in the stock of universities per capita in the US relative to these control groups are reported in Table 1. Robust standard errors are reported in parentheses and degrees of freedom are reported in brackets.

The results reported in Table 1 indicate that the United States experienced rapid expansion of its university system relative to the trend of university founding in the control groups, following the ratification of the 1862 Morrill Act. The results show that the Morrill Act was responsible for between 0.68 to 0.81 additional universities per 1000 US population, when compared to the UK, the WBO, and the EU5-- and an added 0.68 universities per 1000 population when compared specifically to the UK.

b. The Morrill Act effect on university enrollments per capita

We likewise measure the effect of the Morrill Act on the flow of higher education (tertiary) enrollments in the US relative to the control groups, as specified in equation (12). The results reported in Table 2 indicate the effect of the first Morrill Act on the tertiary enrollment rate (per 100 18-24 year old population) in the United States. In each case, the effect of the act over the period between 1862 and 1900 is found to be positive, significant, and broadly corroborating across the different control groups. The results indicate that the first Morrill Act was associated with the US adding approximately 0.642 additional enrollees per 100 when compared to the UK, or 0.975 and 0.982 additional enrollees per 100 relative to the other control countries.

c. The Morrill Act effect on US GDP per capita growth

As previously indicated, in measuring the impact of the Morrill Act on the long-term growth of per-capita GDP in the US relative to the British and European control countries, we use 10, 20, 30, and 50-year forward looking GDP per capita growth rates as dependent variables. We also use a "bias-censored" data set which omits growth rate observations that may be contaminated by post-Morrill-treatment GDP per capita levels (see section 4.C.b).[21]

The treatment effects are estimated via the DID regression model specified in equation (13). The results are reported in Table 3. To account for other determinants of growth over the transition to balanced growth paths, initial per capita real income is included as well. This variable is generally found to exhibit a negative effect on the growth rate, as can be inferred from proposition 3. Year and country fixed effects are included to control for unobserved differences between countries and secular time trends. The full results are illustrated in Table 3A in the appendix.

Table 3's results reveal how the Morrill land grant act of 1862 affected per capita GDP growth rates in the United States during the post-Morrill time period. The estimated treatment effects are positive and significant at the 1% level and are consistent across alternative forward looking time horizons accounting for 10- 20- and 30-year average growth rate effects. They are also quite sizeable. Compared to the UK, they suggest that the Morrill Act, by itself, could account for a growth rate differential of 0.98% in the US relative to the UK over the 30-year period following the act. Compared to the WBO, the estimated corresponding differential is 1.18%, and compared to the EU5 the corresponding differential is 1.2% over the same period. We also note that the estimated quantitative effects on the per capita GDP growth rate in the US vs. the controls diminish when estimated over periods with longer time horizons.

Three main factors are likely responsible for this pattern. The first is that forward looking growth rates computed over increasingly longer time horizons following a policy shock ultimately converge asymptotically to a steady state equilibrium level, as implied by proposition 3 of our theoretical section. Second, average growth estimates based on longer time periods include per capita GDP growth observations in the control groups after 1900. These observations may reflect the impact of higher-education initiatives that started taking place within the control groups, which would thus attenuate the relative average treatment effects estimated for the US. This potential bias is most relevant when considering the drop-off in the estimated 50-year forward looking growth rate in the US relative to the UK because of the launch of its "redbrick movement."

The third factor is the drop-off in the growth rate effects owing to the forward-bias associated with comparing the long-term growth rates associated with observations in the pre-Morrill Act treatment. This bias may be largely responsible for the drop-off in the estimated treatment effect over the 50-year forward looking growth rate, since in this DID regression we could not censor the observations to eliminate the bias, as explained in section 4.C.b above.

In summary, the three positive and statistically significant treatment effects reported in Tables 1-3 lend support to the basic propositions of our theoretical model concerning the effects of the Morrill Act on the economy's balanced growth path and the higher education channel through which the latter may have occurred. A word of caution concerning these reported effects, however, is that they are linked to the 1862 Morrill Act during a period in which confounding factors identified in section 4.A.b may have also impacted significantly the level or growth rate of per-capita GDP in the US. In the next section we employ US' state-level data to conduct a complementary DID regression analysis by which we test key assumptions in our analysis, provide corroborating estimates of the Morrill Act, isolate the role of confounding factors, and offer additional evidence of the impact of the land grant university system by testing the effects of the second Morrill Act as well.

6. THE 1862 MORRILL ACT IMPACT AT THE US STATES' LEVEL

The availability of US states' data on real per-capita income allows us to test the robustness of our findings concerning the impact of the 1862 Morrill Act at the country level by exploring the impact of its implementation across the US states over the period 1840-2000.[22] Furthermore, the data allow us to control for the potentially confounding effects of other events taking place in the 1860s that could bias our estimated effects of the Morrill Act at both the cross-country and cross-states DID regression analyses, and to test the assumed exogeneity of the act in both studies. As important, the state data allow us to explore the robustness of our findings in both applications, by estimating the independent treatment effects of the second Morrill Act of 1890 on the growth experience of affected US states, which we take up in section 7.

A. State-level DID analysis and results

a. The difference-in-differences models

Although the Morrill Act was supposed to apply uniformly to all US states, confederate states were effectively excluded from its provision over much of the 1860s. More generally, different states were not uniformly ready to establish a land grant college. The heterogeneity in the date of application of the Morrill Act across different states allows us to explore a related effect of the Morrill Act, where the relevant event, or treatment, is the founding year of the land grant college and the subsequent change in the growth rate specific to that state. By proposition 2 of our model, we expect any state that establishes a land grant university (LGU) in a given year to experience a positive land grant treatment effect on its forward looking average growth rates over any given time horizon, T, relative to all states that do not yet have an LGU (including itself in prior years), which thus form a relevant control group. Moreover, states differ not just in the founding date of the Morrill Act, but also in the number of colleges and universities founded. Consequently, we can explore the impact of the act at *both* the extensive and intensive margins.

We first examine the impact of the Morrill act at the *extensive margin* using models of the form:

$$(14)\ \log(1 + g_{T,y_{it}}) = \alpha + \beta_t Y_t + \gamma_i S_i + \delta MA_{it} + X'_{it}\xi_{it} + \varepsilon_{it}$$

This regression specification is similar to the ones specified in connection with our DID analysis at the country level, except that the dummy variable MA_{it} takes a value 0 in the years prior to implementing the Morrill Act, and 1 thereafter. The assumption underlying equation (14) is that students acquiring higher education in their home state by and large remain working in the same state. This assumption seem to be justified over the period of this study since most of the internal migration was from the more industrialized Eastern states toward the largely agrarian West Coast during the mid-19th century, and by African Americans migrating from the agricultural South to the industrialized Northeast and Midwest, largely in the early 20th century (see https://en.wikipedia.org/wiki/Internal_migration).

In equation (14), the dummy variables Y_t captures the year fixed effects.[23] The variable S_i captures the state fixed effects, and X'_{it} is again a vector of covariates including the log of real income per capita and additional dummy variables indicating whether a state had been admitted to the USA at time t, and whether a state seceded from the union during the US Civil War. These factors are necessary to include in the basic DID specification to make sure all states in the sample are under the jurisdiction of the US federal government administering the land grant university system. The treatment estimator δ represents, as in equation (13), the reduced-form impact of a Morrill land grant award on the State's forward looking growth rate of real per capita income.

As in the country-level regressions, the growth rate measures in equation (14) are computed over 10- to 50-year forward looking time spans to capture their evolution toward an equilibrium growth rate (g* in equation 6). Note that in the state-level analysis, the per-capita income observations are based on decennial data, rather than annual data. Thus, the potential time lags between the time of adoption of a land grant university and the time in which the higher education acquired at the university becomes effective in production should not significantly affect our estimated treatment effects on the 10- to 50-year forward looking growth rates, as we argued in the context of the country-level regression analysis.

Note, however, that the "forward looking" bias problem discussed in the context of our country-level regressions is again an issue in the state-level analysis and may exert a downward-bias on our estimated treatment effects. We therefore correct all 10, 20 and 30 year forward looking growth rates in the same way we did in country-level data, by removing the contaminated observations so as to calculate the treatment effects without any downward bias. The treatment effects for the 50-

year growth rate observations, however, include biased observations due to our limited pre-treatment sample**.**

To measure the impact of the Morrill act at the intensive margin, we estimate models of the form:

$$(15)\ \log(1+g_{T,y_{it}}) = \alpha + \beta_t Y_t + \gamma_i S_i + \delta MAunivs_{it} + X_{it}'\xi_{it} + \varepsilon_{it}$$

This model differs from equation (14) in that the variable $MAunivs_{it}$ here denotes the *number* of land-grant universities implemented by state i at year t, not just the year in which the Morrill Act was first implemented. The coefficient of interest δ thus measures the impact of one additional land grant university on the "T-year" forward looking per capita income growth rate in state i, following year t. The remainder of equation (15) remains the same as in equation (14).

Figure 7 describes the graphical relationship between the presence of a land grant institution in a state and that state's rate of 30-year real income growth. The violin plots in that figure represent the rotated kernel densities of the distributions of the 30-year state per capita income growth rates for both the extensive (top panel) and intensive margins (bottom panel). The time period is constrained to the post-Morrill Act era after 1862. In this way, the violin plots describe the differences in 30-year forward looking state per capita income growth rates between early and later adopters of the Morrill Act.

b. **Extensive margin estimated effects**

The results pertaining to the extensive margin are presented in Table 4. In Specification (1) we test the univariate impact of heterogeneously assigned Morrill land grant awards across states, using the forward looking growth rate as the dependent variable and the Morrill land grant dummy variable as the independent variable.

Specification (2) reports treatment effects using regression equation (14) and includes year and state fixed effects, the natural logarithm of real per capita income, and the dummy variables for statehood and the confederate states. The latter correlates are added, since the 1862 Morrill Act excluded the Confederate ("rebelling") states during the Civil War when the Morrill Act was approved, but made the act applicable to all US states after the War ended.

The estimated treatment effects of the Morrill act at the extensive margin corroborate the effects derived from the national level experiment, as reported in Table 3. Specification (2) indicates that the Morrill Act resulted in a 0.83% to 1.2% growth advantage to states that implemented the act, when averaged over the forward looking 10, 20 and 30 years, compared to states which did not adopt the act. The treatment effect estimate over a 50-year growth period, 0.33%, while being subject to a downward forward-growth-rate bias relative to the **10, 20 and 30-year** forward-growth rates, is nevertheless statistically significantly as well. As for the estimated effects of other correlates used in the regressions implementing both equations (14) and (15), initial real GDP per capita has a negative and significant effect on all the corresponding forward looking growth rates, in line with this variable's estimated effect in the country-level regressions implementing equation 13. The Confederate and statehood dummy variables are found to be insignificant statistically. This may be because their role is subsumed by the state fixed effects dummy variables.[24]

c. **Intensive margin estimated effects**

The DID regression results concerning the intensive margin are reported in Table 5 using the same specifications as in Table 4. The results appear to be highly consistent across both tables.

Table 5's results indicate that the Morrill Act's marginal intensive effects brought about a 0.89% to 1.79% rise in average per capita real income growth over the subsequent 10 to 30 year period. The extensive and intensive margins effects have similar lower bounds (0.83% and 0.89%, respectively), but the intensive margin upper bound effect is apparently higher (1.79% vs. 1.2%), perhaps because of complementarities or positive spillover effects across institutions.

More interesting, perhaps, the range of treatment effects found in the state-level analysis is comparable in magnitude to the treatment effects we estimated using the US-UK DID regressions, ranging between 0.98% and 1.54%. Moreover, the pattern concerning the drop-off in the magnitude of the treatment effects that we observed at the country level repeats at the state-level regressions as well: the impact on the forward looking growth rate is highest over the 10-years growth rate and declines over the 20-, 30-, and especially 50-year growth rates. The magnitudes of the declines in the time periods with longer horizons may be larger at the state level because internal migration may have become more prevalent over time in the various US states.

These consistent results at the state and national levels lend support to the hypothesis that the Morrill Act may have been responsible for at least part of the observed surge in the per capita income growth rate in the US v. the UK in the 19th century and beyond. They leave open, however, the question of whether the estimated treatment effects also reflect the impact of the confounding factors discussed in section 4, and whether they are affected by a simultaneity bias. We take up these issues in sections B and C.

B. Controlling for potentially confounding factors

As we explained in section 4.A.b, confounding factors are a concern in any experimental design, but especially in natural experiments where randomization of treatment and control group assignments is impossible to implement. In our inquiry, these confounding factors include any event which occurred around the same time as the Morrill Act and could contribute to the US growth experience, thus biasing our estimated treatment effects of the act.

We have already mentioned four such potentially confounding factors: the US civil war and its aftermath, the 1862 Homestead Act, the 1862 Pacific Railway Act and the generally rapid pace of industrialization in the US during this time. Our choice of the UK as the primary comparison group (and the Western British Offshoot colonies as a secondary control) at the country-level analysis implicitly controls for the era of industrialization in the US, since the Industrial Revolution, which started in the UK in the latter part of the 18th century and spread to other European countries as well, preceded that in the US. In addition, both the UK and the US shared similar institutions prior to the American Revolution. Also, the higher pace of industrialization in the US in the latter part of the 19th century was itself an endogenous outcome of the higher education revolution we identify as the channel through which a higher pace of growth had occurred in the post-1862 period.

We are left, however, with the potentially confounding effects of the Homestead and Pacific Railway Acts enacted in the same year as the Morrill Act, as well as the aftermath of the US Civil War, which ended in 1865, on the pace of growth in the US at both the national and state levels after 1862. By the latter effects, we refer to the "Reconstruction period", typically assumed to span from 1865 to 1877, during which the ex-Confederate states experienced a surge in economic activity that could significantly raise measured levels of aggregate growth. The observed heterogeneity in the per-capita income growth rates across the various US states during that period provides us an opportunity to test for the distinct homogeneous effects of the Morrill Act relative to those of the alternative three factors, by implementing variations of equations (14) and (15).

In Tables 4 and 5, we attempt to control for the effects of all three potentially confounding factors by stratifying our sample into 3 subsamples a. observations unaffected by the Homestead Act; b. observations unaffected by the Pacific Railway Act; and c. observations unaffected by either the Homestead or the Pacific Rail Act (the intersection of the first two subsets).

These 3 stratifications aim to indirectly test the hypothesis that it was the 1862 Morrill land grant act, not the Homestead or Pacific Railway acts of 1862, which was responsible for the accelerated economic growth observed across US states (and, by implication, across the US vs. the UK) over the post Morrill Act period. The regression results, derived by implementing equations (14) and (15) for each of the specified strata are reported in columns 3, 4, and 5 of Tables 4 and 5, respectively. In column 3 we exclude from the total sample used in columns 1 and 2 those states affected by the Homestead Act of 1862.[25] In Specification (4), we exclude the states that were affected by the Pacific Railway act of 1862.[26] Specification (5) combines these restrictions and excludes all states exposed to either of the other two 1862 land grants.

The estimation results reported in Tables 4 and 5 indicate that the Morrill Act treatment remains positive and significant in all stratified subsamples, at both margins. The estimated quantitative effects using these subsamples are seen to exceed those in the total sample, possibly because the stratified groups, consisting of northern and eastern states, were more industrial, and thus in a better position to benefit from the higher education opportunities afforded by the new land grant institutions. The Morrill Act treatment effects may thus have been larger in these states.

As a robustness test of these findings, we use an alternative method to account for the potential effects of the three confounding factors by introducing dummy variables that distinguish the unaffected US states from the states that were affected by each of these factors and estimate the distinct “net” Morrill Act, conditional on the added controls. To do so, we apply the DID analysis using modified versions of equations (14) and (15) to investigate the possibility that each of these confounding factors can account for a surge in the states' long-term economic growth rate, aside from the Morrill Act.

More specifically, we omit all state fixed effects introduced as one of the correlates in equations (14) and (15) latter and replace them with the Homestead, Pacific Rail, and the Confederate states' regional effects in the DID equations accounting for the extensive and intensive margins of the Morrill Act effect in equations (16), and (17) below:

$$(16)\ \log(1+g_{T,y_{it}}) = \alpha + \beta_t Y_t + \gamma Homestead + \theta PacificRail + \delta MA_{it} + X'_{it}\xi_{it} + \varepsilon_{it}$$

$$(17)\ \log(1+g_{T,y_{it}}) = \alpha + \beta_t Y_t + \gamma Homestead + \theta PacificRail + \delta MAunivs_{it} + X'_{it}\xi_{it} + \varepsilon_{it}$$

Note that in these equations we already account for a Confederate dummy variables, which were introduced in our basic DID models (14) and (15), and subsumed in the covariate vector $\boldsymbol{X'_{it}}$.

The estimated effects of the three key potential explanatory variables in this model: the Morrill Act, the Homestead Act, and the Pacific Railway Act, along with the Confederate states (due to reconstruction) are reported in Table 6. It is clear from Table 6's results that the Homestead Act and the Pacific Railway Act had little or no significant impact on per capita state income growth rates in the US states, when the Morrill Act effect is controlled for as well. Even in the instances where the Confederate states do appear to experience statistically significant positive income growth effects, as shown by the estimated extensive margin effects of the Confederate dummy variable for the 20 - and 30-year forward looking growth rate regressions, the statistical significance of these effects varies by the time horizons of the growth rates, and it disappears at the intensive margin. The observed effects support our conjecture that the Civil War and Reconstruction had a transient *level* effect but no sustained, long-term *growth* effects on per capita real incomes in the former-Confederate South.[27]

By contrast, the estimated positive effects of the Morrill Act reported in Table 6 corroborate the results obtained in Tables 4 and 5, which have been derived using stratified subsample estimates. These regressions may also be viewed as counterfactual tests of the hypothesis that our estimated Morrill Act effects on the long-run per-capita income growth effects are to be ascribed to the ~~tested~~

confounding factors operating in the US over the period 1862-2000. The only deviation we have in Table 6 is that the Morrill Act's extensive margin effect loses statistical significance in the 50-year forward-looking per-capita GDP growth rate regression. The effect remains significant, however, in corresponding regression specification where the act's effect is estimated at its intensive margin. It is possible, therefore that the treatment's extensive margin growth effects subsume the intensive margin effects on human capital formation and per-capita income growth over a 50-year period in which all states have adopted additional land-grant institutions.

Overall, the DID regression results in tables 4-6 thus lend strong support to the hypothesis that the 1862 Morrill Act, unadulterated by other contemporaneous treatments, helped propel the United States to its position of economic leadership during the post-Morrill Act (coinciding with the "gilded age") period. This conclusion is based, however, on our assumption that the timing of adoption of the Morrill Act by the various US states was an exogenous event. In the following section we attempt to test this assumption directly.

C. Testing the assumed exogeneity of our treatment effects

Our discussion of the historical process leading to the enactment of the Morrill Act strongly suggests that the passage of the act can be viewed as an exogenous event, justifying studying it as a quasi-natural experiment. There may still exist some skepticism, however, whether its *adoption timing* by the US states is a strictly random assignment if the latter may be endogenously driven by states' independent anticipation of their future economic growth, as this would violate the assumed exogeneity of the Morrill Act in equations (14) and (15). This concern cannot be tested directly via our DID methodology since we lack a readily available method by which to test for endogenous treatment effects. To settle this concern, we use an instrumental-variable estimation method, which we apply using a cross-sectional regression analysis. Specifically, we estimate the following cross-sectional regression model using both OLS and IV estimation procedures:

$$\textbf{(18)}\ \log\left(1+g_{T,y_i}\right)=\zeta+\phi(land\ grant\ yr_i)+\varphi\log(realpcincome_i)+\xi_i\ .$$

In applying equation (18), we fix the time span over which we compute the dependent variable – the average growth rate of the states' real per-capita income – to 1850-1890. We choose 1890 as the ending year because by that year all 42 of the existing US states had established a land grant institution.[28] We pick 1850 as the starting year in order to allow for variability in the growth rates across states even prior to the introduction of the 1862 Morrill Act. The initial conditions are controlled for by adding the log of real per capita income at 1850 in equation (18). Testing the impact of the heterogeneous adoption of the Morrill Act across states would then allow for the estimation of homogeneous average treatment effects across states, which is the implicit methodology we have used in the DID specification of equations (14) and (15) for any one of the 4 fixed periods over which we computed the forward looking growth rate in each state.

In this sense, equation (18) can be viewed as the cross-sectional analog of the time series average representation of equation (14). The coefficient of the land grant adoption year is expected to have a negative sign by our theoretical model, because earlier adopters over the same given time periods would be subject to more treatment from the 1862 Morrill Act and thus experience a higher average growth rate, while the later adopters, having been exposed to the Morrill Act over a shorter time span, would be expected to experience lower average growth rates during the same given time period (here 1850-1890).

The OLS estimates of equation (18) are reported in Table 7 and indicate that earlier adoption and implementation of the Morrill Act across states has a positive and significant effect on the corresponding forward looking growth rates of real income computed over a 10- 20- and 30- year time horizons, consistent with our hypothesized treatment effects of the year of adoption. Not surprisingly, the magnitude of the estimated coefficients falls as the time horizon increases, since the relative differences in accumulated effects of the homogeneous treatment effects of earlier vs. later adoptions of the Morrill Act decrease over time when all states entering the sample establish a land grant university over the same 40 year period.

To address the potential endogenous selection problem in the OLS regressions, we choose the number of state's representatives and senators, i.e., the state's Congressional representation in log-form as a plausible instrument.[29] The main rationale for this selection is that the states with more Congressional representatives were more politically influential, and thus able to overcome administrative federal and local hurdles (which lands to release for the new universities, for example), and thus in a better position to join the Land Grant program sooner.

As we show below, this IV is also found to be significantly and monotonically associated with our treatment variable – the year at which the Morrill Act his actually implemented across states. While the standard exclusion restriction cannot be tested in this application since we use just a single IV, there is no reason to believe that the state's initial Congressional representation is causally associated with the state's per-capita income growth potential. We thus proceed to estimate equation (18) via the IV estimation procedure.

In the first stage of the required TSLS procedure we regress the year state i received a land grant on the state's Congressional representation in log form as well as the log initial real per capita income level in that state, as follows:

(19) $\boldsymbol{Landgrant\ yr_i = \alpha + \beta \mathbf{log}(statereps + senators)_i + \mathbf{log}(realpcincome)_i + \varepsilon_i}$

The first stage regression results are reported in Table A1 of the Appendix. The downward sloping relation between the land grant award year and the state log (*statereps+senators*) exhibited in Figure A1 in of the Appendix, supports the monotonicity required assumption. The strength of the first-stage results supports our choice of the state Congressional representation as a strong predictor of land grant timing. The F-statistics reported in Table 8, along with the significance of the IV in the first stage regression results reported in Table A1, indicate the validity of our choice of the state's Congressional representation (in log form) as IV.

In the second stage estimation, we regress the log of the forward looking T-year growth rates averaged over the relevant first Morrill Act period (1850-1890) on the *predicted* land grant year and the log real per capita income in state i in the reference year (1850) as follows:

(20) $\boldsymbol{\mathbf{log}\left(1 + g_{T,y_i}\right) = \zeta + \phi(fitted\ land\ grant\ yr_i) + \varphi\, \mathbf{log}(realpcincome_i) + \xi_i}$ **.**

The results of the IV estimation are reported in Table 8. The fitted land grant year estimated average effect on the state's 10, 20 and 30 year per capita income growth rates during the 40-year period between 1850 and 1890 is found to be statistically significant.

The critical message in Table 8, however, is the Wu-Hausman specification test. The null hypothesis of this specification test is that the land grant adoption years should be treated as exogenous variables. This is because in all the forward looking growth rate regressions with the different time horizons reported in this table, the null hypothesis cannot be rejected at the 5% level as all three p-values are larger than 0.05. This consistent result eliminates the concern that our earlier state-level DID regressions in section 6 (and thereby in the regressions at the country-level as well) might be affected by an endogenous selection bias associated with the adoption of the 1862 Morrill Act in the various US states.

Put differently, although the potential instrument we have selected for an IV estimation is found to be valid, our specification test indicates that there is no endogeneity problem to overcome in the results reported in Tables 4-6 since the test cannot reject the null hypothesis that the adoption date of the Morrill Act in the various states is an *exogenous* variable.

Reported in Tables 7 and 8, the regression results indicate that a single year of earlier adoption and establishment of a Morrill land grant university by a state would result in the state's per capita income growth rate becoming larger by 0.03% to 0.08% by our OLS estimates, and by 0.06% to 0.14%, by our IV estimates. This close similarity, both in terms of sign and magnitude, of the OLS results reported in Table 7 and the IV estimation results reported in Table 8, provides further evidence that the Morrill Act adoption years can be taken as exogenous variables. These results, obtained via our cross-state regression models specified in equation (18) and (20) help validate not just the OLS estimates of the treatment effects in Table 7 but also the qualitative results in Tables 4-6, obtained via the estimation of DID regression models specified in equations (14)-(17), which treat the adoption years of the Morrill Act, MA_{it}, as exogenous.

D. Using structural breakpoints analysis as corroborating time series evidence for the state-level regression results.

To support our hypothesis that the first Morrill Act was indeed responsible for the accelerated growth rates in the US we apply the following statistical test as corroborating time series evidence.

The rationale is simple. We observe an increase in the long-term growth rates of per capita real income at both the national and individual state levels following the passage of the 1862 Morrill Act. From a statistical point of view, the breakpoints concerning the time (year) in which the forward looking growth rate started accelerating in each state can be detected by performing a statistical test that detects the year of significant change in the time series of real income growth rates in each state (see Bai and Perron, 2003, for a description of one of these structural breakpoint detection algorithms). Thus, if the first Morrill Act was the cause for the accelerated growth rates over any long-term forward looking time horizon, the individual state breakpoints by which the acceleration is detected are not expected to deviate significantly from the actual years in which these states implemented the first Morrill Act. We note that the detection algorithm is applied to one series at a time; it does not view all forward looking growth rates of various horizons as a cross-section in detecting the breakpoints.

We apply the detection algorithm to each of the 50 states' forward looking 10-, 20-, 30-, and 50-year growth rates. We then treat the detected breakpoints years and the actual Morrill Act implementation years for each time series of forward looking growth rates of different horizons time in each of the various states as two US state-matched time series. We then apply the standard t-test to determine if the matched series are different statistically.

The results are reported in Table 9. It is evident that both the t-statistics and 95% confidence intervals indicate that the detected breakpoints are not different from the actual implementation years for each of the all 4 series of growth rates we constructed.

For robustness, we treat these two series, detected breakpoints and actual granting years for each state, as two independent samples and test whether these two series have the same mean. The resulting two-sample t-tests are reported in Table 10. The results show no statistical evidence indicating that the average grant year differs significantly from the average detected breakpoints.

As a further robustness test, we can formally test whether the detected breakpoint years are uniformly lagging by 4 or by 5 years after the implementation time of the Morrill Act. The hypothesis is that there should be such a time lag between the year in which the land grant institution is established and the year in which the learning outcomes of matriculating students can be manifested in the labor market and lift up workers' productivity and real wages. The results are summarized in Tables 9 and 10, which show that a common lag of 4 or 5 years cannot be rejected as valid, because the hypothesized lags are included in all 95% confidence intervals constructed around the predicted year of implementation of the Morrill Act in the adopting states.

This analysis thus provides corroborating time series evidence consistent with our hypothesis that the first Morrill Act is, at least to some degree, responsible for the accelerated long-term per-capita income growth rates observed in the various US states following their adoption of the Act.

7. CORROBORATION: THE 1890 MORRILL ACT GROWTH EFFECTS

As we pointed out in the introduction to this paper, the 1862 Morrill Act was supplemented by a second act 38 years later. Substantively, the second act allowed, under certain restrictions, the formation of additional land grant institutions in US states that have already subscribed to the Morrill Act by establishing at least one land grant institution. But the actual targets of this expansion were mainly ex-confederate states and some bordering states.

The motivation of the second act was to overcome racial segregation in higher education. One provision of the first Morrill Act stipulated that land grant institutions should not discriminate by gender or race. Segregation by race, however, was legal in the American South from the close of the Civil War until the mid-20th century. The second Morrill Act thus targeted those states that maintained a policy of racial segregation in higher education.

The Second Morrill Act of 1890 was then a compromise between the federal and state governments. The act required each state to show that race was not an admissions criterion, or else to designate a separate land-grant institution for persons of color.[30] Within 7 years of the passing of the Second Morrill Act, 17 states established a land grant institution under the provisions of the second Morrill Act. [31] Although the second Act granted cash instead of land to the eligible states, it gave colleges established under that act the same legal standing as the 1862 Act colleges; hence, the term "land-grant college" properly applies to both groups.

In the context of this study, we treat the second Morrill Act as a successive federal policy shift favoring higher education that is in essence equivalent to the first Morrill Act. This is because the establishment of land grant institution under the provision of the second Morrill Act amounts to adding another land grant institution in eligible states that already had been awarded one such institution under the provision of the first Morrill Act. We would then expect a DID regression analysis with the 1890 Morrill Act serving as the treatment to yield treatment effects that are qualitatively and perhaps quantitatively comparable to those obtained following 1864 Morrill Act as the treatment (see equation (10) in our theoretical section).

Formally, we implement the model specified in equation (14), which we have used to test the extensive margin of the 1862 Morrill Act, as the relevant model by which we test the 1890 Morrill Act. The two applications are different, however, in terms of the assignment of treatment and control groups in each. In the implementation of the 1862 Morrill act the treatment group is comprised of states that have adopted the act and thus have at least 1 land grant institution, while the control group is comprised of states with zero such institution. In the implementation of the 1890 Act, the control group is comprised of *eligible* state that had at least 1 land grant institution, while the control group is comprised of *ineligible* states that had at least 1 land grant institution. In both applications the numerical distinction between the treatment and control group is 1 vs. 0.

Two characteristics of the second Morrill Act are especially meaningful for our study. First, the reconstruction period following the Civil War was already well over by 1890, so the aftermath of the Civil War was no longer a potential confounding factor. Second, the assignments of eligible and ineligible states for receiving a Morrill land grant award was purely a derivative of the provision of the law and was thus independent of endogenous selection by the various states, making the treatment – the impact of the second Morrill Act - strictly exogenous. These distinctions add credibility to the estimated treatment effects.

The results are presented in Table 11. These results indicate that the Second Morrill Act had an effect quite similar to that of the First Morrill Act. Under Specification (2), states that received grants under the second Morrill Act grew between 1.18% and 2.61% faster than those with only first Morrill Act land grant schools, after controlling for state and year fixed effects, log initial real income per capita, Confederacy and statehood dummies. The second Morrill Act's quantitative effects are thus even larger, presumably because the eligible states had a larger share of the population with lower educational attainments, so the impact of the act on the forward looking per capita income growth rates could be larger.[32]

8. Summary and Concluding Remarks

As Maddison's data show, the US replaced the UK as the major global economic power because of its higher rate of output growth. This is clearly demonstrated in Figures 1 and 2 depicting the time paths of the US log GDP and log GDP per capita first overtaking the corresponding UK paths in 1872 and 1905, respectively. These trends are underscored by the fact that the trends in the 10-year rates of growth of GDP per capita, for example, which were quite similar in the US and the UK between 1820 and 1860, rose significantly in favor of the US in the post 1860 era. These overtaking paths have motivated our exploration of the idea that at least one important factor that may have contributed to the US advantage may be the public higher education movement in the US launched by the 1862 Morrill Act. We call this conjecture “the human capital hypothesis.”

This conjecture has already been pointed out in a previous essay (Ehrlich 2007). In this paper we attempt to put the conjecture into a testable hypothesis by first formulating a simple version of the standard human-capital-based endogenous growth model that links major shifts in policy such as the Morrill Act with corresponding changes in indicators of human capital formation and income growth. We then attempt to test the significance of the link and estimate its quantitative impact empirically through a battery of quasi-experimental-design (QED) estimation methods and corresponding DID regressions, which we apply to independent bodies of data. The main advantage of this technique is that it requires less data than would typically be required for estimating complete structural econometric models.

In this context, however, we are left with the arduous task of justifying systematically the key econometric assumptions needed to justify a valid DID analysis: mainly, an exogenous treatment, valid assignments of treatment and control groups, and adequate controls of alternative confounding treatments that would interfere with our ability to isolate the true impact of the Morrill Act or the land grant higher education university system created thereby, and thus bias the estimated treatment effects we ascribe to them.

The list of such confounding factors is challenging. They include the Homestead and Pacific Rail Acts that took place in 1862 the same year in which the Morrill Act was passed; the aftermath of the Civil War, or the period of reconstruction that took place over the period 1865-1877; the rapid period of industrialization following major innovations in agriculture, transportation, iron and steel making, telephone and telegraph communications, and electricity; and waves of external migration to the US, all of which could have contributed to growth in the level of real output, if not in durable productivity and per-capita income growth.

The strategy we pick for dealing with these challenges is to justify and apply the QED methodology against three distinct bodies of data bearing on the impact of the Morrill Acts: country level data where the treatment is the first Morrill Act and the US and mainly the UK are the principal treatment and control groups; state level data where the relevant treatment groups are states that adopt the act and the control groups are all the states that have not yet adopted the act; and finally an independent body of state level data where the second Morrill Act is the trigger, the treatment group is the states that were eligible to benefit from the act and the control group are the ineligible states. The state-level data allow us to also test the role of confounding factors and the exogeneity of the treatment in addition to the usual significance tests.

This strategy is reflected by the structure of our empirical analysis: We start with the country-level DID analysis, using data over the period 1840-1900 where we explore the average treatment effects of Morrill Act on two major indicators of higher education formation as well as forward looking rates of economic growth in the US relative to the UK – the latter measured over alternative time horizons, ranging from 10-, 20-, 30- and 50-years, as robustness measures. While we could not test the exogeneity of the 1862 Act as the relevant treatment, the historical evolution of the act supports this assumption, and the selection of the UK and related countries as relevant controls are justified by the similarity of institutional, cultural, and political-economic systems between these groups. The estimated treatment effects of the Morrill Act on the growth of higher education institutions, student enrollments, and growth in per-capita real income are significant statistically and in line with our theoretical expectations.

We next try to corroborate our findings at the country level by applying the QED methodology using state-level data, where the treatment variable is the actual year when the Morrill Act was implemented in each state that was eligible for the benefits of the first Morrill Act between 1862 and 1968. Our proposition 2 implies that states that implement the Morrill Act and establish a land-grant university (LGU) would show a positive effect relative to states that have not yet done so, and would thus exhibit larger forward looking growth rates of per-capita state income, especially over periods of shorter horizons, since over longer horizons all effects should ultimately converge asymptotically.

This prediction is confirmed at both the extensive margin, based on the timing of establishing the first land grant institution, as well as on the intensive margin in which we test for the marginal treatment effect of establishing additional LGUs in the same state, as shown in tables 4 and 5.

While these results are all statistically significant at the 1% level, however, it is arguable that they may still be distorted by the confounding factors we discussed earlier. But when we control for these confounding factors using two alternative test procedures (see table 6), the estimated treatment effects remain essentially the same while the potential confounding factors (mainly the reconstruction period which affected the Confederate states) exhibit at best some positive level effects but no durable growth effects. Equally important, when we test for the validity of our exogeneity assumption using the conventional cross-states rather than panel regressions, a 2SLS IV method fails to reject the null hypothesis that the OLS and 2SLS estimates are both consistent, which validates the statistical consistency of all our state-level regressions.

Furthermore, all of the state level results just summarized are corroborated by our test of the treatment effect of the second Morrill Act. This result is important since in these DID regressions we expect no discernible confounding factors that may distort the estimated treatment effects, nor any simultaneity biases due to endogenous treatment effects, since the assignment of control and treatment groups was dictated by the provision of the act, rather than by its adoption by different states. The estimated effects of the second Morrill Act, including the repeated tests of the role of confounding factors, strongly confirm the treatment effects estimated using both the country-level and the state-level DID regressions.

Finally, it is worth noting that our estimated treatment effects of the first Morrill Act on forward looking growth rates are all statistically significant across all the DID regressions using both national and state-level data as well across the first and second Morrill Acts. The effects on forward-looking growth rate based on our panel data, as summarized in Tables 1-6 and 11, are also quantitatively sizeable and similar in magnitude when compared within equal time horizons. Moreover, as predicted, these become lower as the time horizon over which they are measured becomes longer. All the estimated effects indicate, however, that the Morrill Acts have yielded a significant and sizable social return in the US over a long period of time following their enactment.

This analysis does not purport to explain the entire history of US ascendance to the major global economic power in the 20th century. The High School revolution emphasized by Goldin and Katz (1998), and Katz (1983), e.g., is likely to be equally important. Nor does it rule out the role of exogenous technological innovations in the 19th century driving the more rapid pace of industrialization during that period. However, since these were largely continuous over the period we explore, they are internalized by our methodology which focuses on the impact of discrete exogenous events occurring at both the country and state level as relevant treatments which capture the impact of the events over and above that of the technological innovations. Furthermore, these technological factors and the pace of industrialization have been aided and augmented by the growth in skill and sophistication of the labor force in the US that were being empowered by the higher education movement in that period.

The takeaway from this study is that the establishment of the land grant university system triggered by the Morrill Acts appears to be an important factor explaining especially the US overtaking the UK and related countries as an economic superpower. Moreover, the DID regression analysis we pursue in this paper offers new evidence consistent with the role of human capital, largely through the channel of higher education, as an engine of endogenous economic growth and development – what we call in this paper the human capital hypothesis.

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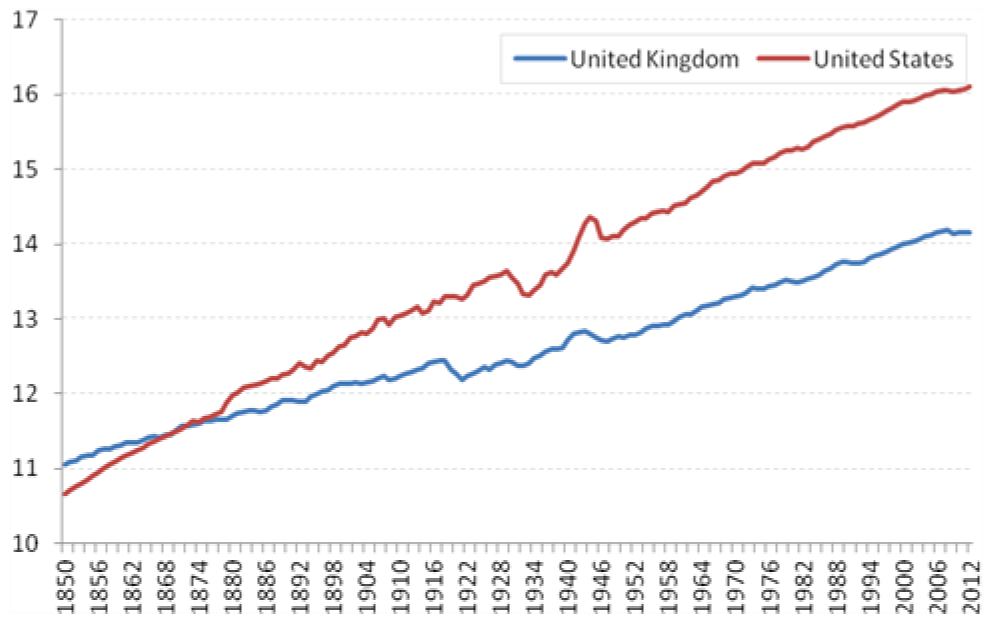
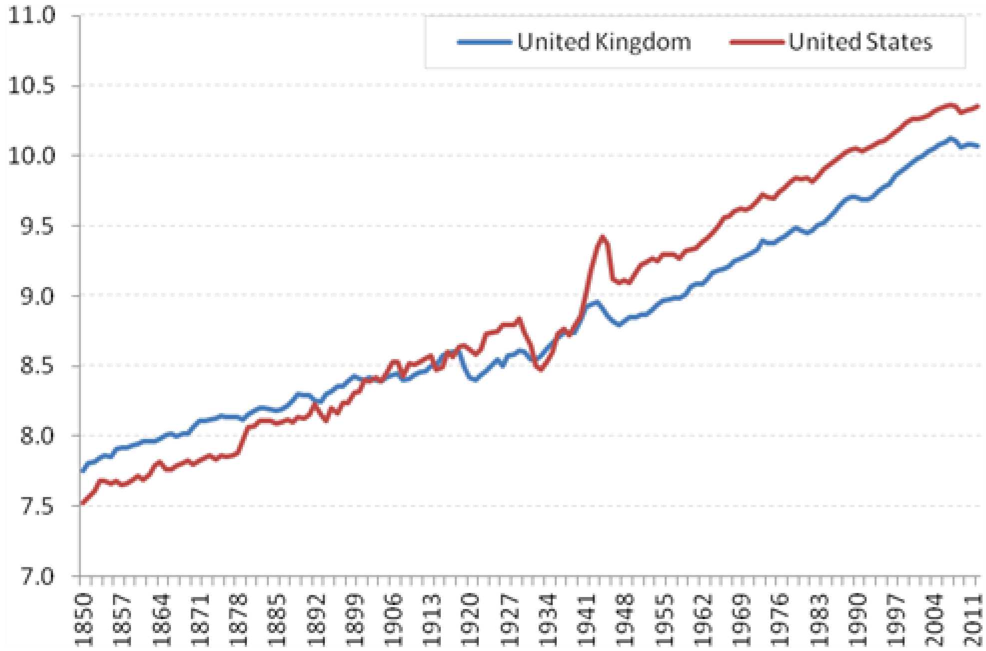
Figure 1: Comparison of U.S. and U.K. Real GDP in Log Terms (1850–2012)



Notes: GDP data are in real (PPP) 1990 Geary-Khamis million dollars. Data for 1851-1859 and 1861-1869 are imputed. For 2009-2012, GDP is computed using real GDP growth rate estimates from the IMF.

Data Source: Data from Angus Maddison, University of Groningen. *http://www.ggdc.net/maddison/maddison-project/home.htm*

Figure 2: Comparison of U.S. and U.K. Real GDP per capita in log terms (1850–2012)

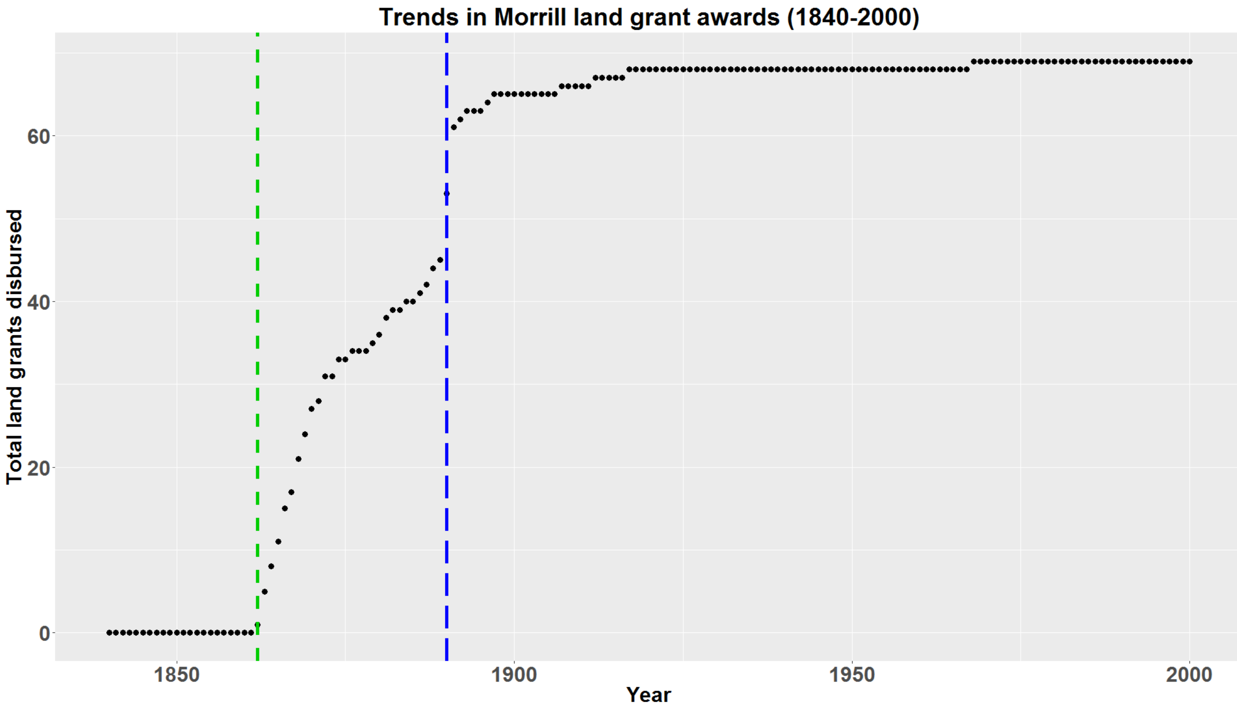


Notes: GDP data are in real (PPP) 1990 Geary-Khamis million dollars. For 2011-2012, GDP per capita is computed using GDP per capita growth rate estimates from the IMF.

Data Source: Angus Maddison, University of Groningen.

http://www.ggdc.net/maddison/maddison-project/home.htm.

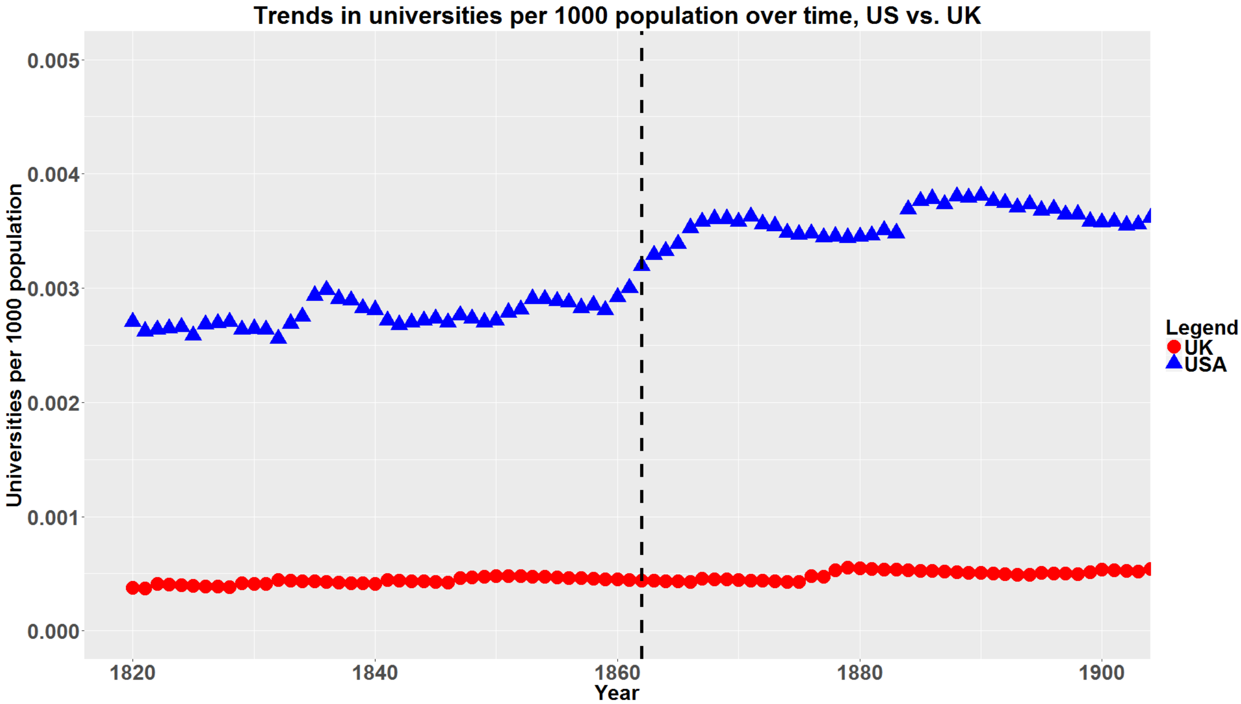
Figure 3: Cumulative Morrill land grant awards in the United States over time



Notes: First vertical line past 1862 denotes the passage of the 1862 Morrill Act. Second vertical line denotes passage of the 1890 Morrill Act extension.

Data Source: https://en.wikipedia.org/wiki/List_of_land-grant_universities

Figure 4: Comparative Trends in No. of Universities Per Capita: US vs. UK.

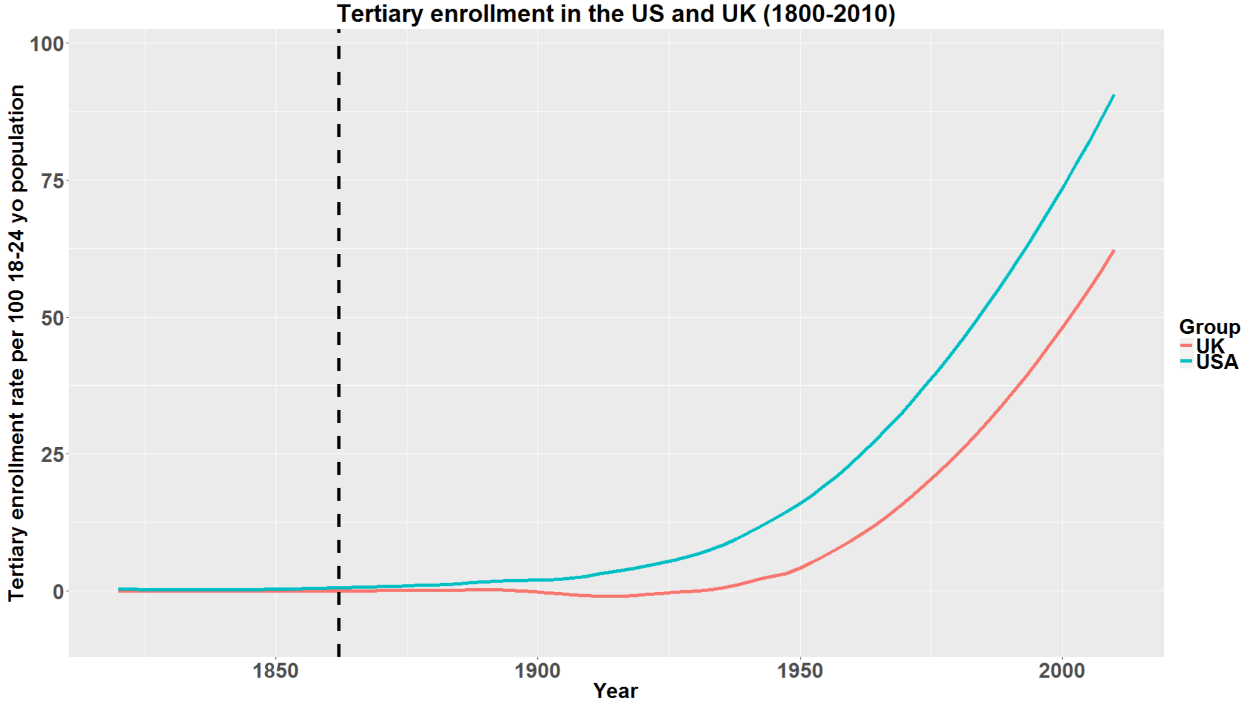


Notes: Population is in 1000s of citizens.

Data Sources: Peter Foldvari, Clio-Infra project, www.clio-infra.eu and Angus Maddison, University of Groningen.

http://www.ggdc.net/maddison/maddison-project/home.htm.

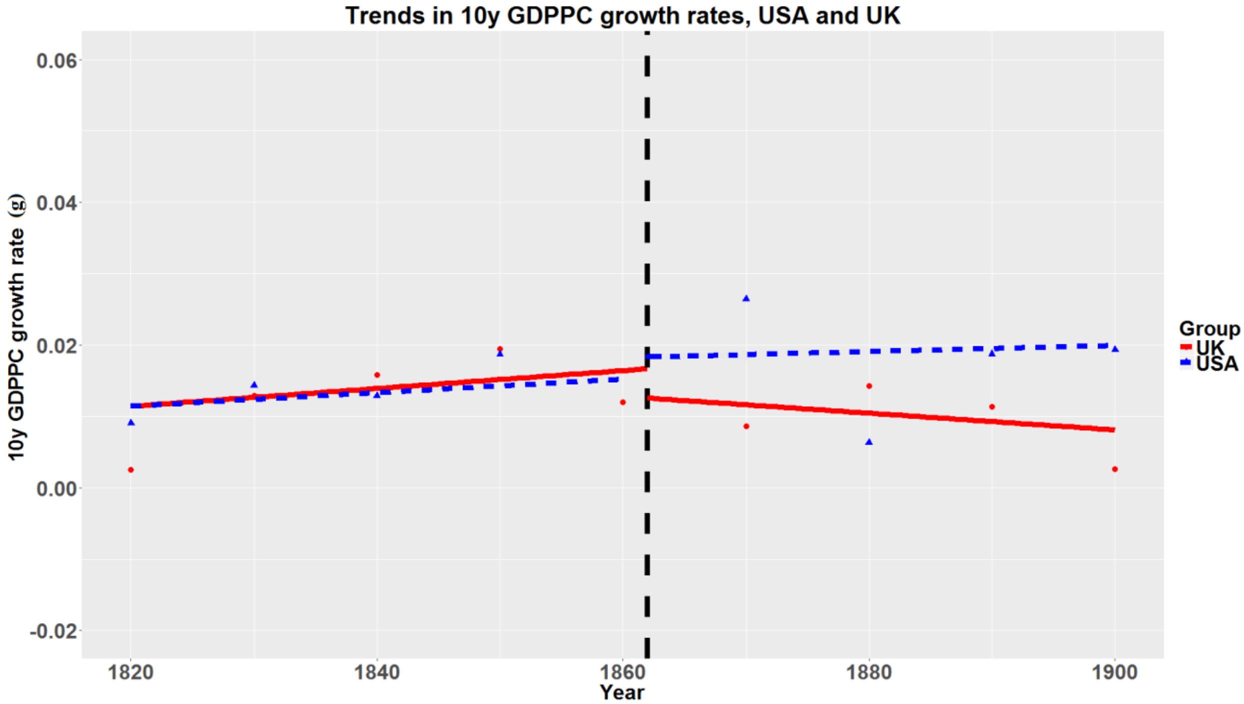
Figure 5: Comparative Trends in Tertiary Enrollment: US vs. UK.



Notes: Vertical line coincides with first Morrill Act ratification.

Data Sources: Tamura (2015), National Center of Education Statistics (1993), US Census (1840, 1850, 1860)

Figure 6: Comparative Trends in GDPPC Growth: US vs. UK

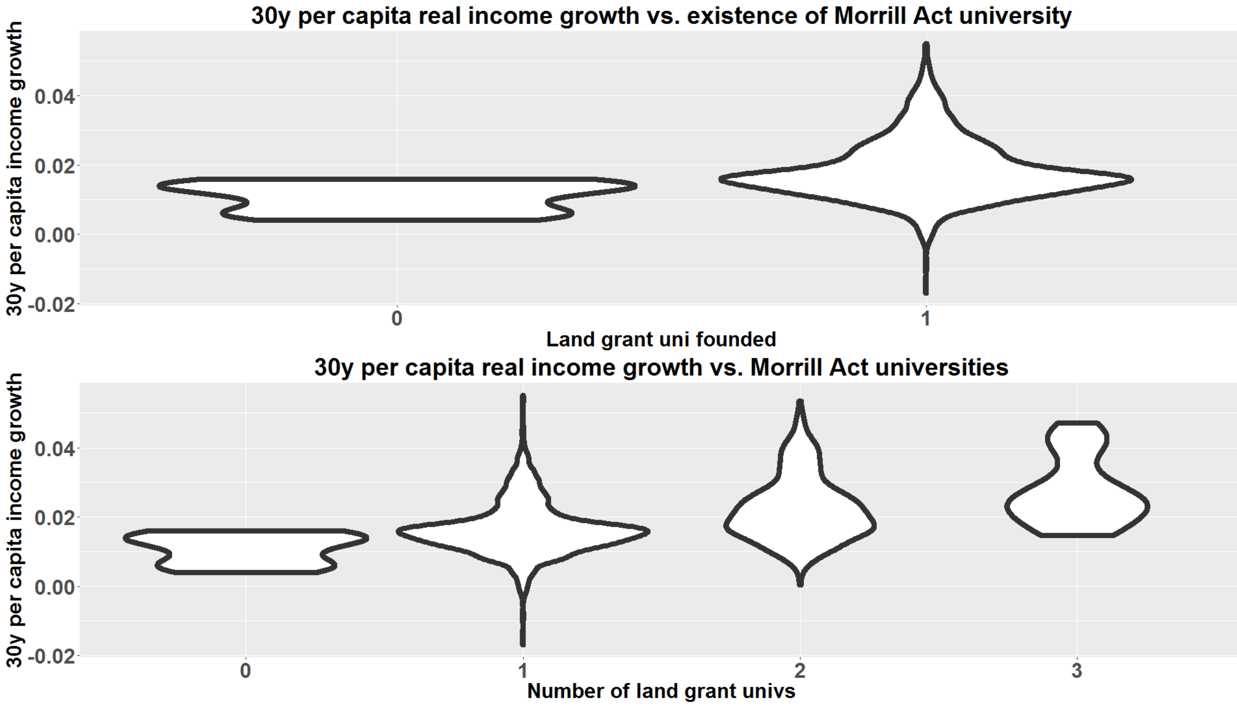


Notes: GDP data are in real (PPP) 1990 Geary-Khamis million dollars. Baseline regression, not adjusted for any correlates. The US growth rate is found to be a full percentage point above the UK.

*Data Source***:** Maddison-Project, http://www.ggdc.net/maddison/maddison-project/home.htm, 2013 version.

http://www.ggdc.net/maddison/maddison-project/home.htm.

Figure 7: 30-year per capita income growth rates vs. Morrill universities founded (1862-2000)



Notes: Violin plots reflect the rotated kernel densities of real per capita income growth rates for a given number of Morrill Act universities (0, 1, 2, 3) in the post-Morrill Act era. Top graphic represents extensive margin. Bottom graphic represents intensive margin.

Data Source: Turner et al. (2007), and https://en.wikipedia.org/wiki/List_of_land-grant_universities.

Table 1. 1862 Morrill Act Effect on US Universities Per Capita (1820-1900)

Control group	**UK**	**WBO**	**EU5**
Universities per capita	0.00068***	0.00081**	0.00072***
(SE)	(0.00006)	(0.00034)	(0.00007)
DF	[68]	[188]	[291]
Year dummies	Yes	Yes	Yes
Country dummies	Yes	Yes	Yes

Notes: *** – 99% significance, ** – 95% significance, * – 90% significance. HAC robust standard errors reported in parentheses. Degrees of freedom in brackets. Covariates include 10y-lagged log real per capita income level. The treatment group is the United States. The WBO control group is composed of (UK, CAN, AUS, NZL); the EU5 control is composed of (FRA, SPA, UK, ITA, GER). Treatment effects are estimated using equation (11).

Data Sources: number of universities founded across countries comes from Foldvari (2014.) Data on population, real per capita income is taken from Maddison (2010.)

Table 2. 1862 Morrill Act Effect on US Tertiary Enrollment Rate (1820-1900)

Control group	UK	WBO	EU5
Tertiary enrollment rate	0.642***	0.975***	0.982***
(SE)	(0.193)	(0.190)	(0.230)
DF	[5]	[23]	[28]
Decennial dummies	Yes	Yes	Yes
Country dummies	Yes	Yes	Yes

Notes: See Table 1. Treatment effects are estimated using equation (12).

Data Source: Tertiary enrollment rates are from Tamura et al. (2015.) Tertiary enrollment rate is a fraction of 100 population. Covariates include decennial and country level fixed effects and 10y-lagged log per capita income.

Table 3. 1862 Morrill Act Effects on US GDP per-capita Growth Rate (1820-1900)

Control Group	UK	WBO	EU5
10-year g-rate	0.0154***	0.0143***	0.0188***
(SE)	(0.0033)	(0.0034)	(0.0032)
DF	[95]	[243]	[315]
20 year g-rate	0.0101***	0.0117***	0.0164***
(SE)	(0.0015)	(0.0025)	(0.0026)
DF	[94]	[244]	[315]
30 year g-rate	0.0098***	0.0118***	0.0120***
(SE)	(0.0008)	(0.0027)	(0.0016)
DF	[93]	[243]	[317]
50 year g-rate	0.0058***	0.0059***	0.0051***
(SE)	(0.0011)	(0.0011)	(0.0018)
DF	[96]	[246]	[320]

Notes: *** – 99% significance, ** – 95% significance, * – 90% significance. HAC robust standard errors reported in parentheses. Degrees of freedom in brackets. Covariates include real per capita income level, country and decennial fixed effects. The treatment group is the United States. WBO control is composed of (UK, CAN, AUS, NZL) and the EU5 control is composed of (FRA, SPA, UK, ITA, GER). Treatment effects are estimated using equation (13).

Data source: per capita real income are taken from Maddison (2010.)

Table 4. 1862 Morrill Grant Effect on US State's Real Per-Capita Income Growth Rate (1840-2000) – extensive margin effects

	Specification				
Dependent Variable	(1)	(2)	(3)	(4)	(5)
10 year	0.0068**	0.0110**	0.0150***	0.0199***	0.0148***
GDPPC growth	(0.0028)	(0.0056)	(0.0050)	(0.0052)	(0.0050)
	[3376]	[3309]	[1381]	[2248]	[1313]
20 year growth	0.0078***	0.0083*	0.0147***	0.0170***	0.0146***
GDPPC growth	(0.0028)	(0.0050)	(0.0021)	(0.0018)	(0.0022)
	[2827]	[2770]	[1144]	[1869]	[1087]
30 year growth	0.0114***	0.0120***	0.0101***	0.0122***	0.0100***
GDPPC growth	(0.0024)	(0.0008)	(0.0011)	(0.0010)	(0.0011)
	[2282]	[2217]	[908]	[1493]	[862]
50 year growth	0.0082***	0.0033***	0.0039**	0.0056***	0.0042**
GDPPC growth	(0.0014)	(0.0009)	(0.0019)	(0.0015)	(0.0021)
	[1430]	[1367]	[574]	[931]	[545]
Covariates	No	Yes	Yes	Yes	Yes
Stratified by	NA	NA	Homestead	Pacrail	Both

Notes: *** – 99% significance, ** – 95% significance, * – 90% significance. HAC robust standard errors reported in parentheses. Degrees of freedom are reported in brackets. Covariates include log initial state income level, confederate state dummies, statehood indicator, year and state fixed effects. Specifications 1 and 2 use the full sample, including all states in the US and DC and all year 1840-2000. (n = 8212) Specification 3 are the results in US states which were not affected by Homestead Act land grants. Specification 4 are the results in US states which were not affected by the Pacific Railway Act land grants. Specification 5 are the results from states unaffected by either Homestead or Pacific Railway Acts. Treatment effects are estimated using equation (14).

Data source: per capita real income data come from Turner et al. (2007).

Table 5. Marginal Morrill Grant Effect on US State's Real GDP per-capita Growth Rate (1840-2000) – intensive margin effects

	Specification				
Dependent Variable	(1)	(2)	(3)	(4)	(5)
10 year	0.0051***	0.0179***	0.0198***	0.0186***	0.0200***
GDPPC growth	(0.0006)	(0.0029)	(0.0034)	(0.0027)	(0.0034)
	[3376]	[3309]	[1381]	[2247]	[1313]
20 year growth	0.0053***	0.0112***	0.0153***	0.0131***	0.0155***
GDPPC growth	(0.0011)	(0.0016)	(0.0019)	(0.0017)	(0.0019)
	[2827]	[2761]	[1144]	[1869]	[1087]
30 year growth	0.0058***	0.0089***	0.0105***	0.0096***	0.0108***
GDPPC growth	(0.0010)	(0.0014)	(0.0019)	(0.0015)	(0.0020)
	[2282]	[2217]	[908]	[1493]	[862]
50 year growth	0.0057***	0.0056***	0.0059***	0.0067***	0.0063***
GDPPC growth	(0.0007)	(0.0006)	(0.0011)	(0.0006)	(0.0011)
	[1430]	[1367]	[574]	[931]	[545]
Covariates	No	Yes	Yes	Yes	Yes
Stratified by	NA	NA	Homestead	Pacrail	Both

Notes: See Table 4. Treatment effects are estimated using equation (15).

Data source: see Table 4.

Table 6. Testing the Morrill Act Effects Controlling for Confounding Factors (1840-2000)

	Dependent Variable	Morrill Act effect	Confederate states effect	Homestead Act effect	Pacific Railway Act effect	DF
Extensive margin	10y growth rate	0.0110*	0.0009	0.0004	0.0006	[3366]
	(SE)	(0.0060)	(0.0009)	(0.0009)	(0.0009)	
	20y growth rate	0.0107**	0.0018**	0.0002	0.0001	[2817]
	(SE)	(0.0053)	(0.0008)	(0.0007)	(0.0006)	
	30y growth rate	0.0100***	0.0024***	0.0002	0.0001	[2272]
	(SE)	(0.0019)	(0.0007)	(0.0005)	(0.0005)	
	50y growth rate	0.0008	0.0007	-0.0005	-0.0002	[1420]
	(SE)	(0.0012)	(0.0006)	(0.0005)	(0.0004)	
Intensive margin	10y growth rate	0.0031***	-0.0015	0.0005	0.0013	[3366]
	(SE)	(0.0011)	(0.0013)	(0.0009)	(0.0010)	
	20y growth rate	0.0027***	-0.0003	0.0003	0.0007	[2817]
	(SE)	(0.0008)	(0.0011)	(0.0007)	(0.0007)	
	30y growth rate	0.0024***	0.0005	0.0003	0.0007	[2272]
	(SE)	(0.0007)	(0.0009)	(0.0005)	(0.0005)	
	50y growth rate	0.0023***	-0.0007	0.0005	0.0003	[1420]
	(SE)	(0.0006)	(0.0007)	(0.0005)	(0.0005)	

Notes: *** – 99% significance, ** – 95% significance, * – 90% significance. HAC robust standard errors reported in parentheses. DF= degrees of freedom. Treatment effects are estimated using modified versions of equations (16) and (17).

Data Source: See Table 4.

Table 7. Per capita state income growth regressions (1850-1890): OLS estimates

Variable	(1) OLS 10y growth rate	(2) OLS 20y growth rate	(3) OLS 30y growth rate
Land grant year	-0.0007*** (0.0001)	-0.0008*** (0.0003)	-0.0003* (0.00017)
Log(realincome)	-0.0103*** (0.0020)	-0.0077*** (0.0025)	-0.0076*** (0.0020)
Constant	1.434 (0.265)	1.577 (0.515)	0.601* (0.318)
Observations	33	33	33
Adjusted R-squared	0.562	0.545	0.467

Notes: Heteroskedasticity robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Data Sources: see Table 4. Land grant years were collected from university websites and https://en.wikipedia.org/wiki/List_of_land-grant_universities. Regression estimates are generated using equation (18)

Table 8. Per-Capita State Income Growth Regressions (1850-1890): IV Estimates with state Congressional representation as instrument

Variable	(1) 2SLS 10y growth rate	(2) 2SLS 20y growth rate	(3) 2SLS 30y growth rate
Land grant year	-0.0011*** (0.0003)	-0.0014*** (0.0004)	-0.0006** (0.0002)
Log(realincome)	-0.0114*** (0.0028)	-0.0093*** (0.0033)	-0.0083*** (0.0023)
Constant	2.175*** (0.503)	2.644*** (0.808)	1.137*** (0.458)
First stage F-statistic	9.757*** (p <0.01)	9.757*** (p <0.01)	9.757*** (p <0.01)
Wu-Hausman test statistic	3.163* (p = 0.085)	3.326* (p = 0.078)	1.715 (p = 0.200)
Observations	33	33	33
Adjusted R-squared	0.452	0.303	0.332

Notes: Heteroskedasticity robust standard errors reported in parentheses except where otherwise noted. *** p<0.01, ** p<0.05, * p<0.1. P-values are reported in parentheses for first stage F- and Wu-Hausman tests. First-stage F-statistics greater than 10 are a "rule of thumb" for a strong first stage. The Wu-Hausman statistic tests whether the IV is just as consistent as OLS. The null is OLS and IV are equally consistent, rejecting the null implies IV is consistent and OLS is not. Regression estimates are generated using equation (20).

Data Sources: see Table 4. Land grant years were collected from university websites as well as from https://en.wikipedia.org/wiki/List_of_land-grant_universities. Congressional representation was collected from https://en.wikipedia.org/wiki/United_States_congressional_apportionment.

Table 9: Paired differences t-test concerning State Per Capita Income Growth Rates Morrill grant award year vs. Structural break year

Comparison time series:	Mean difference	t-statistic	95% CI	df
10y growth rates	2.608	0.487	[-8.14, 13.36]	50
20y growth rates	-1.118	-0.199	[-12.41, 10.18]	50
30y growth rates	-4.102	-0.773	[-14.76, 6.57]	48
50y growth rates	-1.438	-0.368	[-9.29, 6.41]	47

Data source: see Table 4.

Table 10: Two-sample independent t-test Break years vs. Morrill grant award years

Comparison time series:	Mean land grant year	Mean break year	t-statistic	95% CI	df
10y growth rate	1877.118	1874.510	0.543	[-6.94,12.16]	82.7
20y growth rate	1877.118	1878.235	-0.231	[-10.73, 8.49]	82.3
30y growth rate	1877.118	1879.796	-0.579	[-11.87, 6.53]	82.3
50y growth rate	1877.118	1875.208	0.493	[-5.79, 9.61]	93.3

Data source: see Table 4.

Table 11. The 1890 Morrill Act Effect on US state real per capita income growth (1862-2000)

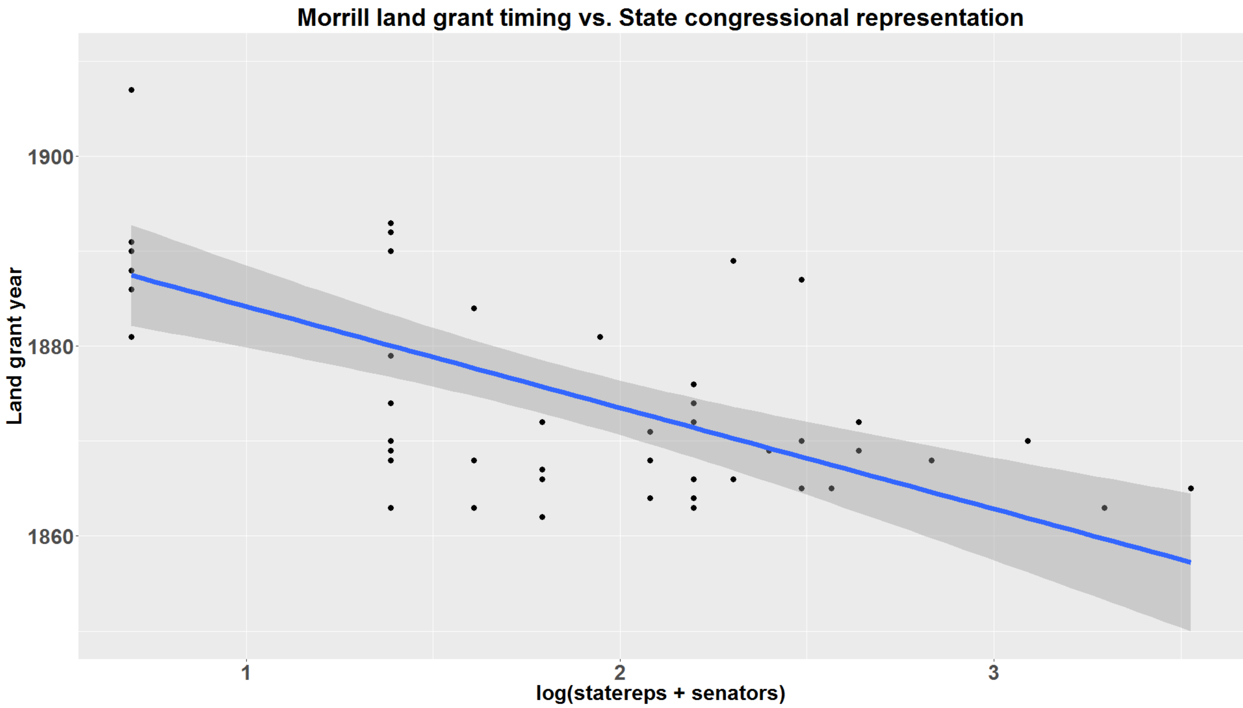
	Specification				
Dependent Variable	(1)	(2)	(3)	(4)	(5)
10 year	0.0064***	0.0261***	0.0214***	0.0227***	0.0214***
GDPPC growth	(0.0017)	(0.0041)	(0.0044)	(0.0039)	(0.0046)
	[3318]	[3199]	[1255]	[2092]	[1189]
20 year growth	0.0067***	0.0182***	0.0166***	0.0174***	0.0170***
GDPPC growth	(0.0012)	(0.0023)	(0.0023)	(0.0023)	(0.0024)
	[2808]	[2699]	[1055]	[1762]	[999]
30 year growth	0.0070***	0.0118***	0.0107***	0.0110***	0.0107***
GDPPC growth	(0.0010)	(0.0014)	(0.0015)	(0.0014)	(0.0015)
	[2298]	[2199]	[855]	[1432]	[809]
50 year growth	0.0071***	0.0062***	0.0057***	0.0061***	0.0057***
GDPPC growth	(0.0003)	(0.0012)	(0.0013)	(0.0012)	(0.0013)
	[1300]	[1222]	[476]	[795]	[450]
Covariates	No	Yes	Yes	Yes	Yes
Stratified by	NA	NA	Homestead	Pac-rail	Both

Notes: see Table 4. Treatment effects generated using regression equation (14).

Data source: see Table 4.

Appendix

Figure A1: Monotonicity of the first-stage regression – IV estimates



Notes: Shaded area represents the 95% confidence interval of the blue least-squares fitted line.

Data Sources: Land grant years were collected from university websites and https://en.wikipedia.org/wiki/List_of_land-grant_universities. Congressional representation was collected from https://en.wikipedia.org/wiki/United_States_congressional_apportionment.

Table 3A. 1862 Morrill Act Effects on US per capita GDP growth rate (UK as control group): full regression results

	Dependent variable			
Regressor	**10y g-rate**	**20y g-rate**	**30y g-rate**	**50y g-rate**
Morrill Act effect	0.0154***	0.0101***	0.0098***	0.0058***
(SE)	(0.0033)	(0.0008)	(0.0008)	(0.0011)
Log income per capita	-0.0473***	-0.0144***	-0.0142***	-0.0174***
(SE)	(0.0110)	(0.0049)	(0.0049)	(0.0034)
USA FE	-0.0135***	-0.0051**	-0.0036**	-0.0018
(SE)	(0.0039)	(0.0020)	(0.0015)	(0.0011)
1830 FE	0.0092***	0.0074***	0.0054***	0.0039***
(SE)	(0.0017)	(0.0019)	(0.0008)	(0.0012)
1840 FE	0.0258***	0.0103***	0.0081***	0.0058***
(SE)	(0.0031)	(0.0021)	(0.0012)	(0.0013)
1850 FE	0.0274***	0.0089***	0.0066***	0.0073***
(SE)	(0.0040)	(0.0027)	(0.0022)	(0.0019)
1860 FE	0.0338***	0.0088***	0.0081***	0.0092***
(SE)	(0.0052)	(0.0031)	(0.0029)	(0.0023)
1870 FE	0.0361***	0.0087***	0.0083**	0.0089***
(SE)	(0.0068)	(0.0033)	(0.0037)	(0.0027)
1880 FE	0.0377***	0.0113***	0.0089**	0.0092***
(SE)	(0.0080)	(0.0039)	(0.0043)	(0.0030)
1890 FE	0.0492***	0.0134***	0.0095*	0.0162***
(SE)	(0.0088)	(0.0043)	(0.0050)	(0.0033)
1900 FE	0.0483***	0.0087*	0.0091*	0.0157***
(SE)	(0.0091)	(0.0049)	(0.0054)	(0.0037)
Intercept	0.357***	0.1173***	0.1163***	0.1406***
(SE)	(0.0822)	(0.0314)	(0.0367)	(0.0249)
Adj. R-sq	0.570	0.687	0.791	0.734
DF	95	94	93	96

Notes: *** – 99% significance, ** – 95% significance, * – 90% significance. HAC robust standard errors reported in parentheses. Degrees of freedom in brackets. Covariates include real per capita income level, country and decennial fixed effects. Treatment effects are estimated using equation (13).

Data source: per capita real income are taken from Maddison (2010.)

Table 8A. First stage regression (1850-1890):
Impact of state representation on land grant award timing

Variable	**(1) OLS** **Land grant award year**
Log(state reps + senators)	-5.867*** (1.283)
Log(realincome)	-5292** (2.003)
Constant	1939.35*** (18.18)
Observations	33
Adjusted R-squared	0.258

Notes: Heteroskedasticity robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Estimates generated using regression equation (19).

Data Sources: Real income per capita data is from Turner et al (2007). Land grant years were collected from university websites and https://en.wikipedia.org/wiki/List_of_land-grant_universities. Data on Congressional representation within states over time was collected from https://en.wikipedia.org/wiki/United_States_congressional_apportionment

Endnotes

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[1] The first Morrill Act, enacted on July 2, 1862, donated public lands to the several states and territories for the benefit of establishing colleges focusing on agriculture and mechanic arts, but later expanded to include other topics (including economics) and agricultural experiment stations. The second Morrill Act, enacted on Aug.30, 1890 required each state to show that race was not an admissions criterion, or else to designate a separate land-grant institution for persons of color included. That act granted cash instead of land, but colleges under the act were given the same legal standing as the 1862 land-grant colleges. See https://en.wikipedia.org/wiki/Morrill_Land-Grant_Acts.

[2] For one recent exception see Ehrlich, Li, and Liu (2017).

[3] These statistics are taken from Maddison 2008. All figures are converted to 1990 U.S. dollars using the Geary-Khamis Purchasing-Power-Parity (PPP) method. For 2009-2012. GDP is computed using real GDP growth rate estimates from the IMF. Similar graphs apply to other major European countries as well. For example the percentage growth rates of GDP and GDP per capita (in parentheses) over the period 1850–2012—starting when the U.S. overtook other major European countries in per-capita GDP—were: 3.34 (1.74) for the U.S.; 1.9 (1.42) for the U.K.; 1.97 (1.6) for France; 2.21 (1.66) for Germany; 2.15 (1.53) for Italy; 2.36 (1.67) for Spain.

[4] The shorter-term trends have been uneven for other major European countries. Over the period 1961–2003, e.g., the per-capita GDP growth rate in France and Italy were 0.21% and 0.40% higher than in the U.S., respectively, while in Germany it was .14% lower. However, over the period 1976–2003, the U.S.'s per-capita GDP growth was 0.28% higher than France's, 0.47% higher than Germany's, and .06% higher than Italy's.

[5] See, e.g., Ehrlich and Lui (1991 section V) where parents, motivated by altruism, determine both fertility, investment in children's human capital, and savings in an overlapping-generation setting. Although fertility is a choice variable that can be affected by shifts in economic and educational policies, optimal investment in human capital and hence the rate of per-capita income growth can be shown to be independent of fertility and savings when the economy is in a growth equilibrium.

[6] This linearity assumption is not required for the impact of the rate of investment *s(t)*, which could be subject to diminishing returns, i.e., *s(t)* in equation (2) can be raised to a power $\gamma \in [0,1]$ with no loss of generality.

[7] A breakthrough was made in 1855, when Governor Kinsley Bingham signed a bill establishing an agricultural college in the state of Michigan, funded through a 14,000 acre state land grant. This small college would become Michigan State University and a model for a further federal expansion.

[8] Land-grant colleges were called people's colleges or farmers' colleges, and they were also known as democracy's colleges. In 1863, a year after signing the Morrill Act, President Lincoln approved an act by Congress that established the National Science Foundation.

[9] The formal act directs "That there be granted to the several States, for the purposes hereinafter mentioned in this subchapter, an amount of public land, to be apportioned to each State a quantity equal to thirty thousand acres for each Senator and Representative in Congress to which the States are respectively entitled by the apportionment under the census of 1860: *Provided*, that no mineral lands shall be selected or purchased under the provisions of said sections. Overall, the 1862 Morrill Act allocated 17,400,000 acres (70,000 km^2) of land, which when sold yielded a collective endowment of $7.55 million. See

https://www.ourdocuments.gov/print_friendly.php?flash=true&page=transcript&doc=33&title=Transcript+of+Morrill+Act+%281862%29, and https://en.wikipedia.org/wiki/Morrill_Land-Grant_Acts.

[10] 17.4 million acres were disbursed by the First Morrill Act, compared to 270 million acres under the Homestead Act and 170 million acres under the Pacific Rail Act.

[11] Fogel (1962) estimated that by 1890 the existence of railroads added at most 2-3% to US GDP per capita, and similar estimates were made by Fishlow (1965) over the period between the passage of the pacific Act and the turn of the 20th century.

[12] We limit the period of analysis to 2000 in the country-level analysis to avoid a possible confounding effect due to the "redbrick" movement in the UK, which was a civic expansion of British higher education, similar to the 1862 Morrill Act expansion. The data on US tertiary enrollment 1840-1990 are constructed from the National Center for Education Statistics enrollment rate data 1870-2000, the US census enrollment data 1840-1860, and the US census data on population aged 18-24, 1840-1860. The 1820 and 1830 observations are by Tamura et al. (2015.)

[13] For some countries in our control groups -- Canada and New Zealand before 1870 and Germany and Spain before 1850 available data are decennial. The DID regressions are based on all available data.

[14] Barro computes T-year average growth rates in GDPPC as $g = \frac{1}{T} log(y_{t+T}/y_t)$ where *y* = *GDP/N*.

[15] Turner et al (2007) compile a time series of decennial state real income per worker until 1929 and annual data through 2000 thereafter. All available data are used in our regression analysis unless otherwise noted.

[16] https://en.wikipedia.org/wiki/List_of_land-grant_universities

[17] We note that over the period we analyze in this paper, the US has also experienced a continuous reduction in fertility. This trend is expected by more comprehensive human-capital-based endogenous growth models to accompany the upward trend in per-capita human capital formation and income growth. In this analysis, we do not attempt to link the Morrill Act with possible consequent changes in fertility, however, since we focus on the effect of the act on per-capita human capital formation and income growth. As indicated by our analysis in section 3 and our discussion in n. 5, the latter effects are expected to hold independently of any related changes in fertility or population growth. In particular, the latter endogenous changes do not affect the specification of the reduced-form DID regression equations we implement in the following empirical analysis.

[18] Since the available yearly data on tertiary enrollments vary across the US and the UK and other control groups, all observation years are rounded to the nearest decennial year.

[19] The log transformation is useful for "drawing down" the long tail of the distribution (normalizing the distribution) of growth rates.

[20] We do not present results going beyond these time intervals because of the forward looking growth rate bias affecting these DID regressions discussed in the next subsection, as well as the natural dilution in the average treatment effects estimated over even a 50-year time horizon. We have run, however, a 100-year forward looking growth rate as a dependent variable using equation (13) and found statistically significant but low treatment effect of the 1862 Morrill Act (0.14%), relative to the impact associated with shorter time periods in Table 3. In the DID regressions using US *state-level* data, the 100-year time horizon

effects become low and insignificant as well. The effect of the Morrill Act in each state is likely to be even more diluted because US states become more similar in their higher education expansions and because of increased mobility across states of graduating students.

[21] The censored data set pertains only to the US time series. We delete the 1860 observation from the 10 year growth rate time series; the 1850 and 1860 observations from the 20 year growth rate series; and the 1840, 1850 and 1860 observations from the 30 year growth rate series. The 50 year time series cannot be corrected in this way, due to our shortened time series in the pre-Morrill period.

[22] The sources for the US States' data are discussed in section 4.B. The Morrill Act adoption data are taken from https://en.wikipedia.org/wiki/List_of_land-grant_universities.

[23] Only decennial data are available over 1840-1929. After 1929 annual data are available. All data are used to estimate the Morrill Act treatment effects at the state level.

[24] To test the sensitivity of the time period of our analysis that extends over to the 20th century, we constrain the data to 1840-1940 and re-estimate equations (14) and (15) using regression Specification 2. Despite this data truncation, we find that the treatment effect estimators at both margins remain significantly positive at the 95% level in all the estimated DID regressions.

[25] States affected by the Homestead Act include: Alabama, Alaska, Arizona, Arkansas, California, Colorado, Florida, Idaho, Illinois, Indiana, Iowa, Kansas, Louisiana, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Ohio, Oklahoma, Oregon, South Dakota, Utah, Washington, Wisconsin, Wyoming. The Homestead Act distributed 270 million acres across the American Midwest to farmers in these states during the period 1862 to 1934.

[26] States affected by the Pacific Railway Act include: Arizona, California, Colorado, Idaho, Kansas, Montana, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, Oregon, South Dakota, Texas, Utah, Washington, Wyoming. The Pacific Railway Act distributed 175 million acres of federal lands in the Midwest to railroad firms for the purpose of (and as compensation for) building the 1,912 mile-long transcontinental railroad.

[27] We have also tested for a potential confounding effects due to external immigration by introducing in regression model (16) a dummy variable that distinguishes the period of mass immigration (1860-1920) and some variations thereof. The effects were insignificant and had virtually no effect on the values of our estimated treatment effects.

[28] There were actually more land grant universities (LGUs) than states in 1890: New Mexico, Oklahoma and Utah had LGUs but did not become US states until 1912, 1907 and 1896, respectively.

[29] The data are taken from https://en.wikipedia.org/wiki/United_States_congressional_apportionment and https://en.wikipedia.org/wiki/List_of_land-grant_universities. The log transformation of the number or Congressional state representatives and the land grant establishment year appears to be log-linear.

[30] It thus led quickly to the formation of establishment of the Historically Black Colleges and Universities, some of which are among the 70 colleges and universities that evolved from the 2 Morrill Acts. See https://en.wikipedia.org/wiki/Historically_black_colleges_and_universities.

[31] Included in the treatment group are *Alabama*, *Arkansas*, Delaware, *Florida*, *Georgia*, Kentucky, *Louisiana*, Maryland, *Mississippi*, Missouri, *North Carolina*, Oklahoma, *South Carolina*, *Tennessee*,

Texas, *Virginia* and West Virginia. Former confederate states are italicized. Not included in this list is Ohio (Central State University) which did not receive its 1890 land grant status until 2014.

[32] We have also investigated the impact of adding the confounding factor dummy variables in all the specifications of Table 11, as we did in testing the impact of the 1862 first Morrill Act. However, neither the Homestead and Pacific Railway nor the Confederate state dummies are found to be significant statistically, while the 1890 2nd Morrill Act indicator remains positive and significant in all specification.