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Ferrocarriles y Competencia Política: Evidencia de los Estados Unidos de América en el siglo XIX

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Esta tesis explora los corolarios políticos de la modernización económica. En concreto, el efecto de la conexión a la red de ferrocarriles sobre la competencia y la participación electoral (componentes claves de la democracia liberal representativa), en distritos (counties) de los Estados Unidos durante el siglo XIX. Teniendo en cuenta que la conexión a la red ferroviaria puede ser endógena al desarrollo político, la tesis emplea el diseño de variable instrumental, explotando el hecho de que un distrito localizado sobre una recta entre ciudades importantes y puertos importantes es propenso a conectarse a la red ferroviaria. También utiliza una aproximación de redes, replicando el diseño de Donaldson y Hornbeck (2016). Empíricamente, la tesis encuentra que la conexión a la red ferroviaria genera mayor competencia y participación política. La llamada "teoría de la modernización" ha sido usualmente analizada de manera cualitativa, o estadísticamente en base a variación de nivel nacional. Por esta razón, la relaci_on causal entre desarrollo económico y modernización política permanece disputada. Esta tesis contribuye a esta literatura examinando un aspecto concreto e importante de la modernización: la expansión de la red ferroviaria.

Palabras clave: Ferrocarriles, democracia, desarrollo, modernización.

"Railroads and Political Competition: Evidence from Nineteenth Century United States"

This thesis explores the political effects of economic modernization. In particular, the effect of the connection to the rail network on political competition and participation (key components of liberal representative democracy). The units of observation are counties in nineteenth century United States. Since connection to the rail network may be endogenous, this paper uses an instrumental variable design. It exploits the fact that counties that lay on a straight path from major cities to ports are more likely to be connected than comparable counties out of the straight path. It also uses a network approach, replicating Donaldson and Hornbeck's (2016) design. The thesis finds that connection to the network results in more competition and participation. Up until now, "modernization theory" has only been tested with country level variation. Therefore, the causal link between economic development and democratization remains disputed. This thesis contributes to that literature by examining a concrete aspect of modernization: the expansion of the rail network.

Keywords: Railroads, democracy, development, modernization.

Códigos JEL: N41, N71, D72.

Railroads and Political Competition: Evidence from Nineteenth Century United States

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1 Introduction

Modernization theory asserts that economic development leads to political development (Lipset 1959). In this paper, I help to unpack this thesis by testing whether one aspect of modernization, railroads, foster more open and competitive elections. Even though railroads were arguably transformational, there is no previous study of the effect of railroads on political outcomes, though there are many papers that estimate their impact for economic and social variables (Donaldson forthcoming, Donaldson and Burgess 2017, Donaldson and Hornbeck 2015, Perez 2017, Atack et. al. 2012, Atack et. al. 2009).

To measure political development, I borrow from the political science literature that strives to measure democracy. In particular, I focus on two dimensions of democracy: competitiveness and participation. For the former, I follow Przeworski et. al. (1996). They define democracy as a "...regime in which some governmental offices are filled as a consequence of **contested** elections" (p. 4). To measure contestedness I use two variables. First, *margin of victory* captures whether the (locally) most favored candidate faces serious competition within the county. Second, *uncontested* is a dummy that takes the value of 1 if there was no serious opposition (=1 if $margin > 0.7$). A real danger to competitiveness is that incumbents may use government resources to sway elections

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in their favor. As a matter of fact, Przeworski et. al. also states that: "...whenever in doubt, we classify as democracies only those systems in which incumbent parties actually did lose elections" (p. 5). Therefore, my third dependent variable is the incumbent party vote share.

Even a very competitive election may be undemocratic if part of the population is denied the vote. Therefore, I use turnout as the fourth dependent variable. Higher participation probably means that minorities and the poorest individuals were more likely to have voted. The fifth and last variable measures the Democratic party vote share, which captures both repression of black voters and support for segregationist policies (Naidu, 2012).

I find that elections in counties connected to the railroad network are more open and competitive. In particular, incumbents get less votes, margins are tighter, uncontested elections are less frequent and turnout is higher. Also, in the South, Democratic party share of the vote is smaller in connected counties, suggesting less repression of black voters and more support for segregationist policies (Naidu 2012).

The railroad data is from Atack (2016) and encompasses all railroad tracks in operation in 1911, together with the year of inauguration. I used this data to create a dummy variable that equals one if railroad tracks were present in a county-year. I also use Donaldson and Hornbeck's (2015, henceforth DH) measure of market access as an alternative independent variable, which captures the number and size of markets available to the county at low trade costs.

Since railroad placement is endogenous to local and national politics, I use two strategies to claim causality: an instrumental variable and a network approach. For the first one I take advantage of the fact that, when two important cities are connected, all places in the in the cost-minimizing route are likely to be connected too, even though they were probably not targeted. Specifically, my instrument equals one if the county lies on a straight line between a big city in 1830 and the closest major port. This variable is most similar to the one used by Atack (2008) to estimate the impact of railroads on industrialization. This strategy is often called *inconsequential unit analysis* and have been used before to estimate the impact of transportation infrastructure by Chandra and Thompson (2000), Michaels (2008), Banerjee, Duflo, and Qian (2012), Faber (2014), Morten and Oliveira (2015), Yamasaki (2016) and Perez (2017).

The second strategy uses DH's market access calculations. Their objective is to estimate the impact of railroads on agricultural land value through their effect on counties' market access. I mostly replicate their models changing their dependent value for my proxies for democracy. An advantage of their approach is that a county's market access also depends on rail tracks built elsewhere in the network, which are plausibly exogenous to local politics. I find that the effect is somewhat robust to using only variation in access to distant markets and controlling by counties' own railroad track.

I include a theoretical framework to make sense of the results. The argument has three steps: First, railroads foster a transition out of an exclusively agricultural economy (Atack, Haines and Margo 2008). Second, the resulting economies are more heterogeneous, and therefore preferences over economic policies are more diverse. Third, a modified citizen candidate model (in the fashion of Besley and Coate 1997) suggests that when there is more disagreement over policy, more people run and incumbents get reelected less often. This argument helps us make sense of the results on margin of victory, uncontested elections and incumbent vote share.

A transition out of an agricultural economy also explains the results on turnout and Democratic vote share. During the civil war, franchise was extended to all African-Americans. Scholars have argued that landowners have historically been hostile to franchise extension (Boix 2003), and therefore they were probably more likely to use their de facto power to deny poor (mostly black) people the vote.

The paper is structured as follows: Section 2 presents the instrumental variable strategy and results. Section 3 describes DH's data and empirical specification and how I adapt them to answer the question at hand. Section 4 provides a theoretical framework to make sense of the results. Also, I suggest ways to test between the mechanism proposed by the theory and alternative ones. These tests are not conducted but left for future work. Section 5 concludes.

2 Instrumental Variables Approach

2.1 Data and Empirical Specification

I use five dependent variables, all of which attempt to measure how open and competitive elections were on a given county-year. I use three sources for electoral outcomes. First, ICPRS study 1 includes all elections to the House of Representatives from 1826 to 1910. I use it to calculate *margin of victory* (and therefore *uncontested*). Second, ICPRS study 8611, which is more limited in time (1840 to 1910), but it is cleaner, variable definitions are clearer (study 1 does not have a codebook), and includes data on turnout. I use it to calculate *incumbent share*, *Democratic share* and *turnout*. To figure out which was the incumbent party, I use ICPSR study 7428 (Biographical Characteristics of Members of Congress). All datasets are available online.

The first dependent variable is *margin of victory*, and calculates the difference in votes between the most voted and second most voted candidates in the county, as a proportion of total votes casted. The second variable is a dummy variable, *uncontested*, which equals 1 if *margin of victory* > 0.7. The third variable, *incumbent share*, calculates the vote share of the party that holds the congressional seat in that election year. The fourth variable is *turnout*. It is calculated as total voted casted divided by the number of citizens eligible to vote (always male, with age and race requirements varying by election). The last variable is the share of votes for the Democratic party.

It should be noted that for all variables the unit of observation is county-year. This may be problematic since elections to the House of Representatives happen at the congressional district level (typically bigger than counties). I have three reasons to use counties instead of congressional districts. First, even though historical election returns data at the congressional district level surely exist, at the moment I don't know of a readily accessible database. Second, identification of the effect of the railroad depends on being able to add fixed effects, that is, to add dummies for geographical units. However, I cannot make fixed geographical units out of congressional districts, since they change so much over time. Counties also change, but since they change only gradually from census to census I can implement a solution to "fix" them (see below). Third, for three of the dependent variables the interpretation does not suffer. Arguably, turnout, and incumbent and

Democratic shares in a given county are of interest in themselves.

This is not to say that using counties is uncontroversial. In particular, the interpretation of dependent variables *margin* and *uncontested* may be problematic. For example, imagine a congressional district that contains two counties. County A is heavily democratic and county B is heavily Republican. In my dataset, this contested congressional district counts as two observations, both categorized as uncontested. Therefore, in a future version of this paper I would like to test the robustness of my results to using congressional district-year as the unit of observation. This could be achieved by conducting the analysis on shorter periods of time (therefore limiting the amount of change districts experience).

To deal with changes in county borders, I transform the data to fixed geographical units (counties in 1870) using the method outlined in Hornbeck (2010). The original variables are population and number of votes per party. Using historical U.S. county boundary files (Manson et. al. 2017), county borders are intersected with county borders in 1870 using ArcMap. When counties overlap with more than one 1870 county, the county area is divided into those pieces using the ArcMap function *intersect*. Data for each piece is then calculated by multiplying the value by the share of its area in the 1870 county. Each 1870 county is then assigned the sum of all pieces falling within its area. This procedure assumes that data are evenly distributed across the county, and implies that the further from 1870, the larger the measurement error. Historical county files are available for census years (1820, 1830, 1840, ...). For elections in years between censuses, I assume that county boundaries are as in the closest census year. Results are robust to using 1890 county boundaries instead of 1870 borders.

Railroad data is from Atack (2016). The original shapefile includes all railroad tracts in operation in 1911, together with the year of inauguration. I used this data to create a dummy variable, $Train$ that equals one if railroad tracks were present in a county-year. Figures 1, 2 and 3 show different stages of the railroad network.

The instrumental variable, *Line*, equals one if the county lies in a straight line from a major city in 1830 (with a population bigger than 2500) and a major port. City population comes from the 1830 census and the complete list is available aupon request. The nine major ports (from

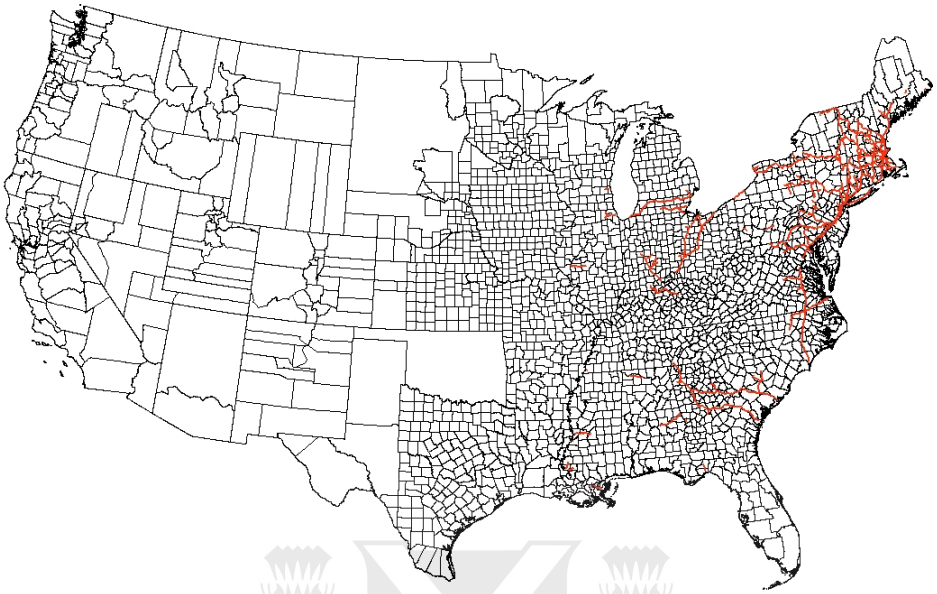

Figure 1: Railroad network in 1850

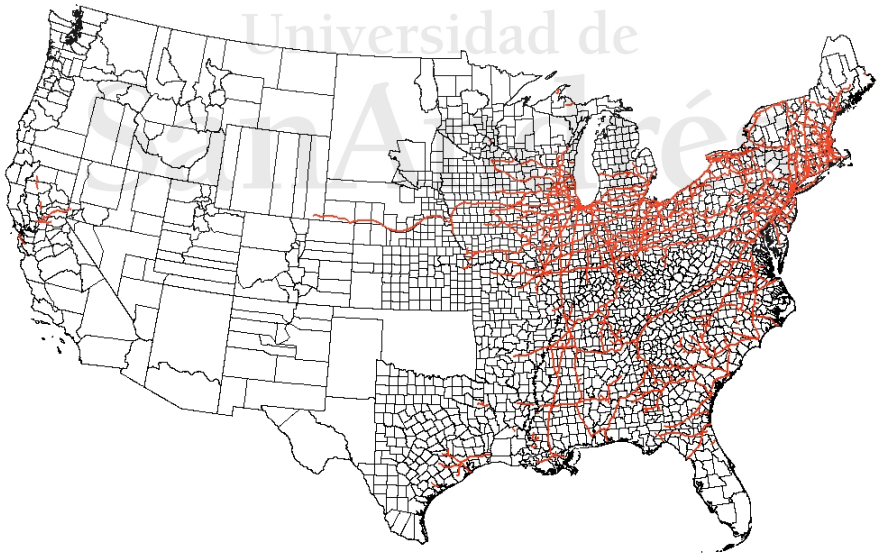

Figure 2: Railroad network in 1870

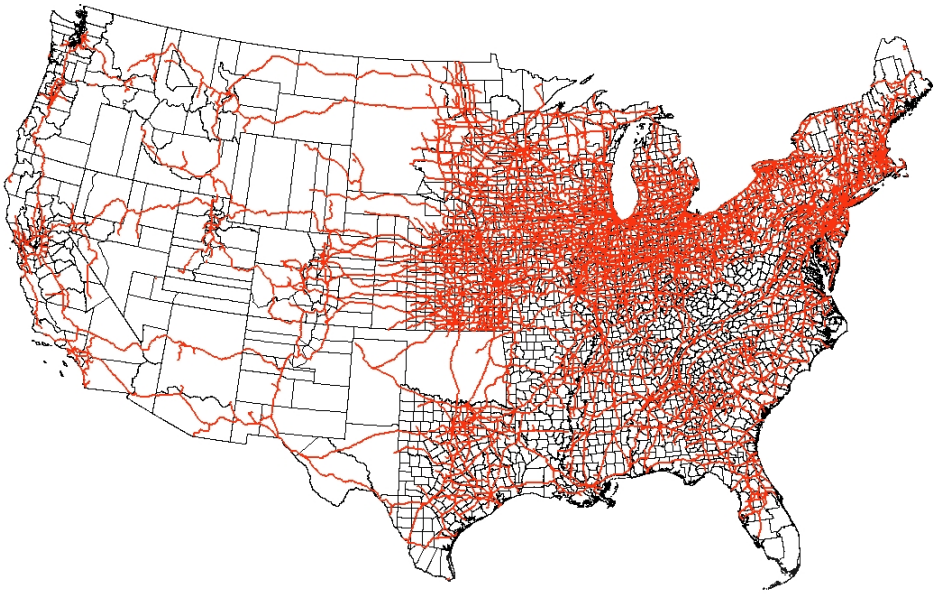

Figure 3: Railroad network in 1890

customs data, see Atack 2008) are Baltimore, Boston, Charleston, New Orleans, New York, Norfolk, Philadelphia, Portland (Maine) and Savannah. I calculated the minimum distances, drew the lines and intersected them with 1870 county borders using ArcMap. Figure 4 shows the lines in red.

The identification assumption is that, around the time of the introduction of the railroad (say 1840 to 1900), trains were the only shock to local politics that affected counties on the way between big cities and ports differently than to counties not in the way. Of course, the straight line is also likely to be a cost minimizing route for cart roads, highways and the telegraph, which may confound the results. Fortunately, at the time of the introduction of the railroad, cart roads where already there and highways were introduced much later, so any non-railroad trade effect is likely to be captured by county fixed effects. On the other hand, telegraphs and railroads are so complementary that their effects are unlikely to be disentangled. Even directly controlling for it would result in bad control bias, since telegraph wires are often installed because of the railroad.

In the first stage, I interact *Line* with year dummies to take advantage of the fact that being on the way does not have a constant effect on railroads. Figure 5 shows that in the first few years of the sample there was basically no difference between the two groups (because there were almost no trains). Also, the effect starts fading away around 1870, which is to be expected when the map

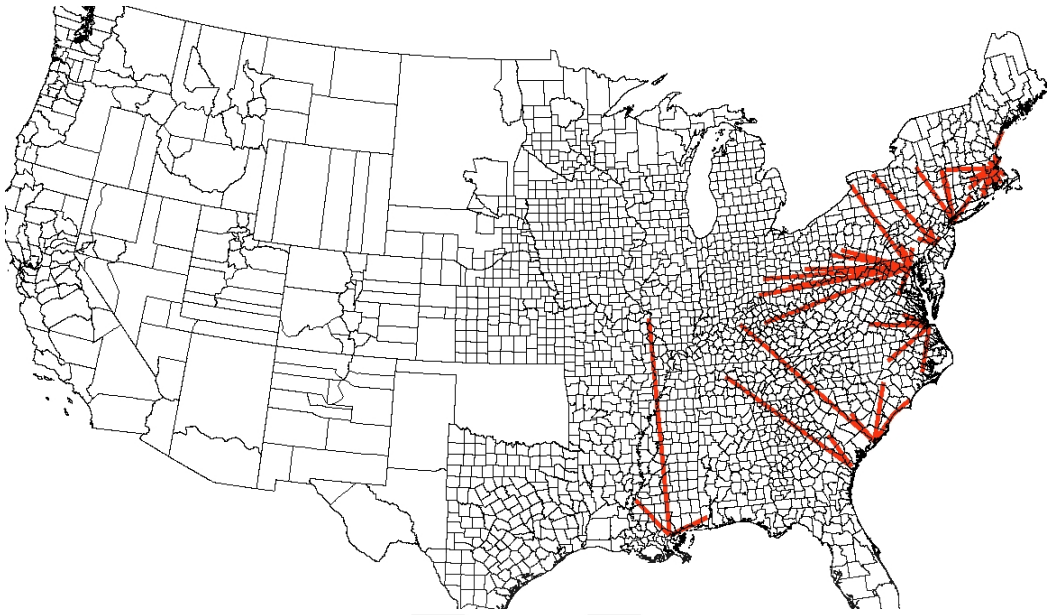

Figure 4: Straight lines between big cities in 1830 and major ports

starts "filling up". The counties containing the cities and ports (the endpoints of the line) are not included in any of the specifications.

The first stage equation for county i and year t is:

$$Train_{it} = \sum_{t=1830}^{1910} \alpha_t (Line_i * \mathbb{1}_t) + \gamma_i + \delta_t + \upsilon_{it}$$

Therefore, I am actually working with one endogenous regressor and eighty instruments. The second stage equation for county i and year t is:

$$Democracy_{it} = \beta \widehat{Train_{it}} + \gamma_i + \delta_t + \nu_{it}$$

Where *Democracy* is one of the four dependent variables listed above and $\widehat{Train_{it}}$ are the predicted values from the first stage. The coefficient β can be interpreted as a local average treatment effect (LATE) of the railroads on democracy if the exclusion restriction holds and if there are no defiers. In particular, the estimated effect is *local* to those county-years that got the railroad because they are on the line and would not have received it otherwise. Standard errors are clustered at the county level unless otherwise noted.

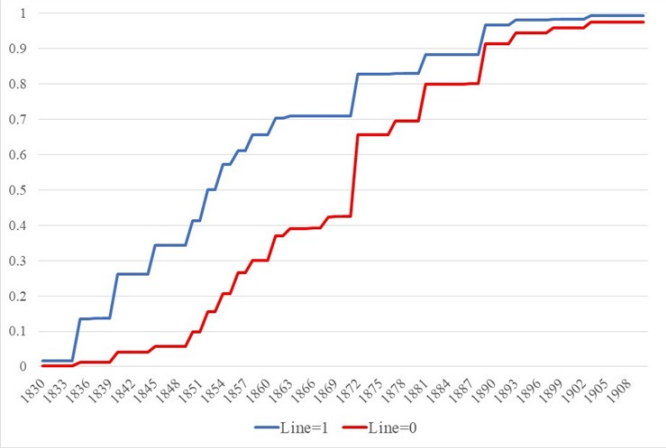

Figure 5: Proportion of counties with railroads, on and off the line

2.2 Results

Table 1 reports OLS models for each one of the five dependent variables. The last model is estimated only for Southern states (defined as those that formed the Confederacy). The number of observations is lower for turnout because I don't have population estimates for some county-years. Every specification includes county and year fixed effects. All variables are either proportions or dummies, so they range from zero to one. All results are significant and go in the right direction: margins, uncontested elections, incumbent and Democratic vote share go down, and turnout goes up.

In the OLS models, the effects are sizable. For counties with railroads: margins are 4.4 percentage points smaller, the probability of uncontested elections decreases in 4.5 pp, incumbent vote shares are 1.9 pp smaller and turnout is 3.8 pp higher in average. In the South, the Democratic party received 5.43 pp less.

As I have already argued, railroad placement may be endogenous to local or national politics. Therefore, table 2 reports the 2SLS estimates using the instrumental variables strategy outlined above. Results go in the expected direction (same signs as OLS). It loses significance for *margin* and *turnout* (though it is almost significant for margin). The coefficient for *margin* is twice the size of the OLS coefficient. For *uncontested*, it is around three times bigger. For turnout, it gives us a 28 pp increase, which is probably "too big to be true".

Table 1: OLS estimates

	(1) margin	(2) uncontested	(3) incumbent	(4) turnout	(5) dem share
train	-0.0445***	-0.0446***	-0.0192***	0.0385***	-0.0543***
	(0.0058)	(0.00647)	(0.00482)	(0.00522)	(0.00894)
ICPSR dataset #	1	1	8611	8611	8611
Observations	59,373	59,373	58,595	56,422	21,725
R-squared	0.062	0.046	0.089	0.160	0.192
N of counties	2,040	2,040	2,106	2,103	831

Standard errors clustered at the county level in parentheses.
Every regression includes year and county fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

However, we should not put a lot of weight in the 2SLS estimates. The first stage results reveal that the instrument is weaker than I expected. I report F statistics in table 2. The Kleingberg-Paap F statistic (which takes into account intra-cluster correlation in the first stage) does not reach the conventional value of 10 for any of models. In future versions of this work, I should find a better instrument. Two ideas come to mind. First, including slope information when calculating the cost minimizing route. The instrument may be weak because sometimes the straight line goes through mountainous areas where railroad placement is impossible. Second, using the cost minimizing network (as in Perez 2017) instead of lines between pairs of cities.

3 Network Approach

3.1 Donaldson and Hornbeck's data and specification

DH examines the impact of railroads on the U.S economy with a focus on quantifying the aggregate impact on the agricultural sector in 1890. They argue that all economic effects of the railroad, direct and indirect (caveats in p. 801 ft. 2), can be summarized by changes in "market access", an expression derived from general equilibrium trade theory. In their empirical specifications, they define market access of county i in year t as $M_{it} \approx \sum_j \tau_{ijt}^{-\theta} N_j$, where j indexes counties other than i, M is market access, N is a measure of the purchasing power of a county (population in most

Table 2: 2SLS estimates

	(1) Margin	(2) Uncontested	(3) Incumbent	(4) Turnout
train	-0.0926	-0.148**	-0.0349	0.281***
	(0.0647)	(0.0720)	(0.0507)	(0.0881)
Returns dataset #	1	1	8611	8611
First stage				
Kleingberg-Paap F stat	7.86	7.86	2.82	3.48
Cragg-Donald F stat	7.78	7.78	14.46	13.48
Observations	59,356	59,356	58,577	56,407
R-squared	0.059	0.037	0.088	0.038
Number of id1870	2,023	2,023	2,088	2,088

Standard errors clustered at the county level in parentheses.
Every regression includes year and county fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

specifications), τ_{ijt} is a the trading cost between i and j at year t, and θ is a parameter known as trade elasticity (set in 8.22 for most specifications).

Variation in M_i comes from the dramatic decreases in trading costs generated by railroads. By decreasing trading costs, more markets become available at low costs. The advantage of their approach is that the market access of a county may be affected for tracks built far away down the network, an event which we can argue is exogenous to local politics. Therefore, by showing that the effects are somewhat robust to controlling for local railroads, I can strengthen the causality claim.

I replicate most models in DH's tables I and II, changing their dependent variable (log value of agricultural land) for four of the proxies for democracy (turnout will be added in the future). The baseline specification (Table 3, Model 1) is:

$$MA_{it} = \sum_{j \neq i} \tau_{ijt}^{-\theta} Pop_j$$

$$D_{it} = \beta ln(MA_{it}) + \delta_i + \delta_{st} + f(x_i, y_i)\delta_t + \epsilon_{it}$$

Where D_{it} is the value of one of the proxies for democracy for county i in year t, MA is market access as defined above, δ_i are county fixed effects δ_{st} are state year fixed effects and $f(x_i, y_i)\delta_t$ is a cubic polynomial interacted with year effects. Is the analogue to equation 13 in DH. Standard errors are clustered at the state level. Since DH calculate market access only for 1870 and 1890, there are only two time periods and we can the estimate equation above in differences.

3.2 Results

Table 3 shows the replication of DH's baseline (table I, column 1) on four of the democracy proxies. In model 1, I find that a 1% increase in market share is associated with a drop of 6.2 percentage points in the margin of victory. The result is robust to controlling for the margin of victory in 1890 (model 5). There is no effect on the share of the incumbent party (model 2). The Democratic party vote share in the South falls by 7.5 pp when MA increases in 1% (model 3). The probability of an uncontested election drops by 6.6 pp (model 4). It is encouraging to see that effects are significant and go in the expected direction. However, table 3 does not address endogeneity concerns.

Table 3: OLS estimates of Market Access on Democracy

	(1) margin	(2) incumbent	(3) dem share	(4) uncontested	(5) margin
diff log(MA)	-0.0620***	-0.000773	-0.0746***	-0.0661**	-0.0682***
	(0.0156)	(0.0132)	(0.0129)	(0.0279)	(0.0166)
margin					0.676***
					(0.0940)
Constant	0.991**	1.296*	3.338	1.186*	0.776**
	(0.488)	(0.642)	(3.569)	(0.685)	(0.364)
Observations	2,138	2,138	881	2,138	2,138
R-squared	0.341	0.527	0.435	0.245	0.500

Standard errors clustered at the county level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4 shows that the effects are largely robust to controlling for local railroads, though significantly smaller. Dependent variables are in the rows and different specifications in the columns.

Each cell represents a different regression. Column 1 shows the baselines from table 3 for easy comparison. Column 2 models control for whether the county gained access to the railroad during that period (the endogenous variable in section 2). Column 3 models also control for railroad length (amount of track built in the county). Column 4 models control for railroads built within 20 miles. The buffer is increased to 40 miles in column 5.

As we move to the left, the coefficients on *margin* and *uncontested* become increasingly smaller, and lose significance in the last specification. For *democratic share*, it remains big and significant. The coefficient for *incumbent* remains zero throughout.

Table 4: Robustness to control for local railroads

	(1) diff log(MA)	(2) diff log(MA)	(3) diff log(MA)	(4) diff log(MA)	(5) diff log(MA)
margin	-0.0620***	-0.0450***	-0.0481***	-0.0375**	-0.0306
	(0.0156)	(0.0142)	(0.0147)	(0.0161)	(0.0212)
uncontested	-0.0661**	-0.0434	-0.0501*	-0.0386	-0.0201
	(0.0279)	(0.0267)	(0.0286)	(0.0285)	(0.0284)
dem share	-0.0746***	-0.0760***	-0.0790***	-0.0726***	-0.0696***
	(0.0129)	(0.0123)	(0.0146)	(0.0141)	(0.0196)
incumbent	-0.000773	0.00538	0.00731	0.00600	0.00381
	(0.0132)	(0.0174)	(0.0186)	(0.0210)	(0.0184)
Controls for					
Any railroad	No	Yes	Yes	Yes	Yes
Railroad length	No	No	Yes	Yes	Yes
Railroads within 20 miles	No	No	No	Yes	Yes
Railroads within 40 miles	No	No	No	No	Yes

County FE , year FE, state-year FE, and year specific cubic polynomials on magnitude and longitude included in every regression.
Standard errors clustered at the state level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

These results suggest that there is a positive and sizable effect of railroads on democracy. Also, they show how the network structure of the railroad can be exploited to make the causal claim more credible.

4 Theoretical Framework

4.1 Railroads and Contestedness

Why would railroads foster more open political competition? The short answer is that they produce an economic transformation that both makes preferences less homogeneous and that allows new players to dispute power from existent elites. In the following paragraphs I introduce a simple, verbal model to explain this mechanism. The main idea is not original to this paper but widespread in the modernization literature (for the seminal contribution, see Lipset 1959, for that most similar to this paper, see Fresh 2017). In a nutshell, I will argue that railroads foster economic development, which promotes preference heterogeneity, which in turn leads to a more open political competition.

Railroads cause economic development, which means not only a bigger economy but also a more complex and diverse one. There is evidence that railroads caused industrialization (Atack, Haines and Margo 2009). Also, they have an impact on innovation, suggesting that they are drivers of technological change (Yamasaki 2017). Last, they drove urbanization (Jedwab, Kerby and Moradi 2017). In sum, they foster the transition out of an exclusively agricultural economy.

In a pre-railroad, pre-industrial, exclusively agricultural economy, economic interests are homogeneous and there is not much of a point in two candidates running with different platforms. In a dynamic, changing and more diverse economy, new sectors (industry and services) may have interests that differ from traditional sectors. In the following paragraphs I use a citizen candidate model (like in Besley and Coate 1997) to verbally explain why an increase in policy preference heterogeneity may lead to the observed changes in the proxies for political development.

Every citizen can run for office by paying cost c. Only one citizen gets elected by plurality vote. Ties are decided randomly. Every citizen has policy preferences that are known to everyone. There is no commitment device, so whoever is elected will implement her ideal policy x_i^*. Also, the elected candidate gets an ego rent e. In addition, elected politicians can bring pork to the district, which takes the form of private goods equally distributed among constituents. Each candidate has a different ability to get pork from Washington a_i. If i is elected, everyone gets a private good $p = a_i$ This ability is known to everyone. Utility functions are strictly increasing in p and single

peaked in x.

In our setting, the policy is something that divides people in agriculture from people in industry. For example, it may be tariffs for industrial goods. While those working in industry want tariffs to protect a nascent manufacturing sector, people in agriculture want cheap goods from Europe. To make this illustration as stylized as possible assume there are only two states: pre-railroad and post-railroad. In the first state, everyone has the exact same preferences over policy $x_i^* = x^* \ \forall \ i$ (because everyone works in agriculture). In the second state, there is heterogeneity.

Assuming $e > c$, everyone wants to run if they can win. In the first state, however, the highest-ability candidate can beat everyone, because everyone that is not running will vote for her. Anticipating this, nobody else runs. In the only reasonable equilibrium, only the highest ability candidate runs and wins. [1].

In the post-railroad state, it is easy to show that equilibria with multiple candidates may exist (as in Besley and Coate 1997). Conditions and a numerical example are available upon request.

4.2 Railroads and participation

Scholars have argued that landowners have historically been hostile to franchise extension (Boix 2003). The main reason given is that their assets are immobile and thus more exposed to taxation and nationalization. In our context, landlords were the main beneficiaries of slavery. Even after the Civil War and emancipation, they fought to preserve a status quo favorable to whites. Therefore, it may not be surprising to see restrictions to participation lose steam as counties industrialize.

4.3 Empirical findings and the model

Uncontested races: The model suggests that interest heterogeneity rises the number of candidates, and results show that railroads decrease the number of uncontested races. Note that *uncontested* is a modified version of *margin of victory*, so the intuition applies for that variable too.

Turnover: (I am stretching the interpretation a bit, since the model does not have a dynamic component) In the pre-railroad scenario, the highest-ability citizen gets elected with probability

[1] A possible exception is a scenario where the whole population runs and vote for themselves, but for this we need the cost to be really small $c < \frac{e}{n}$

one. Three reasons suggest higher reelection rates: First, trivially, if ability is time-invariant, the same citizen will be reelected every time. Second, ability may grow with tenure, as they learn the ropes in Washington. Third, ability may not be known until after the election. In this case, voters only know the expectation of ability for challengers but the true value for the incumbent. Eventually, they find someone better than average, and they stick with her forever.

Turnout and Democratic vote share: As the railroad arrives, counties industrialize and urbanize and landed elites lose power. They had the more to lose from the franchise extension, and were probably using de facto power to block them. Therefore, we would expect restrictions to back off as they lose economic primacy.

4.4 Testing the mechanism

Even if we have an exogenous determinant of railroad access (as I argue we do in the previous section), distinguishing between different mechanisms is a challenge. The main problem is that railroads have an impact on many determinants of political competition, and they interact in complex ways. To make things worse, some of them may be unobservable.

However, even if it's impossible to show conclusive evidence on mechanisms, we may look for suggestive evidence. The first approach could be to use a measure of interest heterogeneity. For example, a Herfindahl index measuring sector concentration. And we want to know whether railroads have an effect on the index and whether the index is correlated with political competition.

A second approach to test the mechanism can be to follow Fresh (2017) strategy and to find out whether the new entrants to the political game come from non-traditional sectors. According to the argument above we would expect more candidates to come from industry or services in railroad counties. To find out whether this is true we need biographical information on candidates' background (where did they work before running, whether they own land, etc.).

4.5 Alternative mechanisms and how to test them

Third party: Railroads raised a new policy issue: monopoly power of railroad companies and the prices they charge farmers. Since this dimension of conflict is new to politics, established

parties may not have a stance in their platforms for it. New parties may seize the opportunity by including a solution to the issue in their platforms. In particular, the Populist Party advocated for nationalization of railroads. The emergence of a third party may be driving part of the observed effect. To test this mechanism, I can estimate the impact of railroad access on Populist vote share.

Education and democratic values: We know that railroads have an effect on schooling (Atack, Margo and Perlman 2012), and education has been linked to democratic values (Rueschemeyer et. al 1992). A negative relationship between schooling and the dependent variables would be suggestive evidence in favor of this mechanism.

Diffussion: Railroads allow the easy movement of people and therefore of ideas. Democracy is probably the normatively superior form of political organization. Increased connectivity may allow superior values to prevail over the rest. Previous literature in sociology suggest that this happens at the country level (Wejnert 2005, Brinks and Coppedge 2006, Torfason and Ingram 2010).

To test the mechanism, we can modify the network approach from section 3. The *market access* measure used there summarizes the main effect of the railroads through income. We can build a variable more suited to investigate whether railroads foster democracy through diffusion of values. For example, the *democratic access* of county i at year t may be calculated as:

$$DA_{it} = \sum_j \tau_{ijt} D_{jt}$$

where D_{jt} is an indicator variable that equals one if county j is above the mean value of some proxy of democracy and τ_{ijt} is a dummy variable that equals one if counties j and i are connected to the railroad network (it could also be equal to $1/TravelTime$, for example).

5 Conclusion

This paper has showed preliminary evidence on how railroads fostered political competition and participation in nineteenth century United States. By doing so, I pointed out transportation infrastructure as a channel through which economic development affects political development. I also argued that this effect may be coming from an increase in the heterogeneity of economic policy

preferences. Last, I listed alternative mechanisms.

Future versions of this paper would include: First, an implementation of the tests proposed in sections 4.4 and 4.5. We would then have suggestive evidence on the mechanisms behind the causal relation. Second, a better (more relevant) instrumental variable. Third, congressional district-year as an alternative unit of observation.

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