Appendix for "Institutions, Technology, and Trade"

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1 Robustness analysis

This section presents results that shed additional light on the robustness of our findings. We first turn to the time horizon that is considered in our main specification. Recall that the impact of institutional and technological change on price gaps is estimated from a window of twenty-five years before and after the 'change' (i.e., customs liberalization, currency agreements, or train connection). We report results from varying the size of this window. In general, a larger window will have two consequences. First, it amounts to employing data over a longer time horizon. If there are important adjustment costs that limit the market size in the short-term, then one expects to estimate a larger impact on price gaps with a relatively large window. At the same time, a larger window makes identification more difficult, since in the long-run many other processes may be important. This reduces the signal-to-noise ratio and will typically lead to lower estimates. Which of these effects dominates is not immediately clear.

In Table A1, we see that the customs and currency estimates are similar to the baseline results for the shorter window of fifteen years, and this is the case also for the train estimate. For a longer time horizon of thirty-five years, the estimates for institutional change move towards zero, especially for currency agreements. Using all available data, neither the customs nor the currency effect is significantly different from zero. In contrast, the trains estimate rises, from about -0.14 to -0.21. This however might reflect not only the long-run trains effect but also the weakening of the institution variables (the correlation of TR with CU is about 6%, and the correlation of TR with LT is about 7%). Overall, the results are consistent with the relatively large impact we estimate for trains, and moreover, the twenty-five year window in our baseline appears to be a good compromise between identifying power and time horizon of the estimate.

Next, we present results for market-pair instead of state-pair fixed effects in Table A2. Recall that the analysis is at the level of the market-pair, so using deterministic market-pair fixed amounts to the usual within estimator panel specification. This has the advantage that time-invariant heterogeneity (including bilateral distance) at the market-pair level is controlled for. It may, however, exacerbate measurement error problems. As seen from Table A2, the impact of the institution variables is somewhat lower, around -0.045 instead of -0.065, while the average trains effect is -0.14, as before, but it varies more across specifications (compare (4) and (1)). Overall, our main findings hold up both qualitative as well as quantitatively.

Table A2 also shows results for the estimation with limited-information maximum likelihood (LIML), which have in some settings, in particular when the instruments are weak, better properties than two-stage least squares (TSLS) estimators. In this case, both the currency and the customs point estimates are similar to those with TSLS, although the LIML estimates are less precise (and for currency agreements not significant at standard levels). The LIML trains estimates are somewhat larger than the TSLS estimates, although the difference is not statistically significant. We also show in specifications (3) and (6) of Table A2 that unweighted regressions yield very similar results to the baseline reported in Table 4 (recall that in the baseline we perform weighted regressions, where the number of

bilateral observations for a given market-pair serves as the weight).

We take another step by examining the sensitivity of our results with respect to features specific to particular countries. For example, Austria-Hungary around the year 1820 still had internal customs duties (Bairoch 1989, 6). This would be in contrast to our assumption that there are no customs borders between any two markets of a given state or country with contiguous areas ($CU_{ijt} = 1$, all t). The following analysis is also useful to examine whether our analysis holds even if we restrict the analysis to subsets of states. The six specifications in Table A3 show trains and currency results for restricted samples; "No Austria-H", for example, means that all observations involving a market in Austria-Hungary are dropped from the sample. On the right, we report the average of the six restricted-sample estimates, as well as the baseline estimate from Table 4.

It is clear that individual country- observations have a substantial effect on the currency estimate; the average is only about half the size of the baseline estimate. At the same time, it is reassuring that currency agreements in the fifteen German states, labeled "Only Germany" in Table A3, have a similar effect as in the sample as a whole. This allays concerns that our results are primarily identifying the difference between German and non-German states. Turning to the analysis of the customs and trains effects in Table A4, we find these results to be very robust. Also in the case of customs we estimate a significant price-gap reducing impact from liberalizations if the sample is limited to the German states. Moreover, the "Only Germany" customs impact is somewhat smaller than in the full sample, while the "Only Germany" currency point estimate is slightly larger than in the full sample, so there is no clear pattern of how sample composition affects the results.

Finally, we have also further examined the findings on indirect effects of institutions presented in Table 6. These robustness checks are presented in Table A5. We see that market pairs that were served by state railways see a considerably smaller reduction of price gaps with the arrival of steam trains, and in three out of four cases this effect is statistically significant at standard levels. Also the results for the more fundamental institutional quality variable, late abolition of serfdom, are seen to be robust: steam trains brought price gaps down by less, on average, if they operated in states that abolished serfdom relatively late (Table A5, on the right). The robustness checks also confirm our findings above that the quantitative effect of the proximate variable, $StateRailways$, appears to be larger than that of the fundamental variable, $LateAbolition$.

To sum up, we find the estimates to be robust, more so for the trains and customs estimates than that for currency agreements. Given the generally smaller number of observed changes for currency agreements relative to customs liberalizations or train adoptions, it is difficult to know for sure whether the impact is smaller or less-precisely estimated. In general, however, the robustness analysis confirms the main findings emphasized in the text.

2 Data Sources and Construction

Price data The two most important sources for information on wheat prices are Shiue and Keller (2007) and Seuffert (1857). The former covers markets in Bavaria, Belgium, France, Mecklenburg, and the Netherlands, while the latter provides information on markets in

Austria-Hungary, Baden, Brunswick, Hesse-Darmstadt, Hesse-Cassel, Hesse-Nassau, Saxony, Switzerland, and Wurttemberg. The wheat prices for Prussian markets were provided by Michael Kopsidis, see Kopsidis (2002). Additional sources that improve the coverage are Fremdling and Hohorst (1979), Gerhard and Kaufhold (1990) for Prussia, Hanauer (1878) for the Alsatian cities Mulhouse and Strassbourg, Shiue and Keller's (2007) data for Vienna, and Vierteljahrshefte (1935) for Berlin, Cologne, Hamburg, Leipzig, and Munich.

Since neither quantity nor monetary units were standardized in Europe during the 19th century, conversion rates are required for our analysis of absolute price differences, and all prices are converted into Bavarian *Gulden* per Bavarian *Schaeffel*. The conversion factors are taken from the original sources, reported to some extent in Shiue and Keller (2007), as well as from Seuffert (1857). Specifically, from the latter we have (page 351):

State	Quantity unit	Conversion factor into Bav. *Schaeffel*	Monetary unit	Conversion factor into Bav. *Gulden*
Baden	Malter	0.67	Gulden	1.00
Brunswick	Himten	0.14	Thaler	1.75
Belgium	Hectoliter	0.45	Francs	0.47
Frankfurt	Malter	0.51	Gulden	1.00
France	Hectoliter	0.45	Francs	0.47
Hamburg	Fass	0.24	Mark Banco	0.88
Hanover	Himten	0.14	Thaler	1.75
Hesse-Darmstadt	Malter	0.57	Gulden	1.00
Hesse-Cassel	Schaeffel	0.36	Gulden	1.00
Hesse-Nassau	Malter	0.49	Gulden	1.00
Netherlands	Zacken	0.45	Gulden Courants	0.99
Austria-Hungary	Metzen	0.27	Gulden	1.22
Prussia	Schaeffel	0.24	Thaler	1.75
Saxony	Schaeffel	0.46	Thaler	1.75
Switzerland	Concordia Malter	0.67	Concordia Francs	0.47
Wurttemberg	Schaeffel	0.80	Gulden	1.00

Other data The main sources of information on railway building are the digital historical maps provided at IEG's website at the University of Mainz, http://www.ieg-maps.uni-mainz.de/ and Putzger (1997). We derive the state versus private railways indicator presented in Table A6 from information in Fremdling, Federspiel, and Kunz (1995). The customs liberalization variable is based on information on the history of *Zollverein* treaties in Henderson (1959) and Hahn (1984), as well as the historical maps at IEG (2008). Also the currency agreement variable is based on information in Henderson (1959) and Hahn (1984), as well as Willis (1896). City population data comes from Bairoch et al. (1988) and de Vries (1984), while state and country-level population figures come from Kunz (2008) and Mitchell (1980). Information on serfdom, as shown in Table A6, comes from Blum (1978).

Finally, the information on cities' latitude and longitude that we employ to compute several variables ($DistCoast$, $ZollPop$, $Market\_Potential$, and $CurrPop$) comes from maporama, http://world.maporama.com/ .

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Table A1: The expansion of market size over different time horizons

	Customs liberalization and steam trains				Currency agreements and steam trains			
	25 year window	15 year window	35 year window	All Data	25 year window	15 year window	35 year window	All Data
Panel A: Second-Stage Results								
Train Connection	-0.129#	-0.114#	-0.156#	-0.229#	-0.162#	-0.126#	-0.155#	-0.202#
	(0.010)	(0.009)	(0.017)	(0.026)	(0.025)	(0.017)	(0.017)	(0.036)
Customs Liberalization	-0.072#	-0.072#	-0.011	-0.008				
	(0.010)	(0.008)	(0.013)	(0.012)				
Currency Agreement					-0.059#	-0.062#	-0.034#	0.003
					(0.022)	(0.015)	(0.011)	(0.013)
Panel B: First-Stage Statistics								
Trains First-Stage								
Shea Partial R-squared (percent)	5.6	6.6	4.2	3.3	4.8	7.4	3.9	3.5
Insitutions First-Stage								
Shea Partial R-squared (percent)	18.9	12.8	17.8	15.4	9.8	13.3	10.9	11.1
Number of excluded instruments	74	74	74	74	62	61	64	64
Hansen OverID test p-value	0.14	0.32	0.08	0.29	0.28	0.24	0.44	0.62
Number of observations	6,990	4,800	8,403	10,434	6,990	4,800	8,403	10,434

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-values of F-statistics of all regressions < 0.001
Sample observations are within a certain window before and after the establishment of a train connection, customs liberalization, or a currency agreement
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population (specificaitions (2) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state; p-value of F-test of excluded instruments <0.001 in all cases

Table A2: Additional controls and alternative estimators

	Customs liberalization and steam trains			Currency agreements and steam trains		
	Market-Pair FE	LIML	Alt. weights	Market-Pair FE	LIML	Alt. weights
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second-Stage Results						
Train connection	-0.067^{+}	-0.185$^{\#}$	-0.152$^{\#}$	-0.203$^{\#}$	-0.257$^{\#}$	-0.217$^{\#}$
	(0.039)	(0.056)	(0.015)	(0.060)	(0.084)	(0.026)
Customs liberalization	-0.039$^{\#}$	-0.066*	-0.067$^{\#}$			
	(0.012)	(0.028)	(0.011)			
Currency agreement				-0.054^{+}	-0.059	-0.066$^{\#}$
				(0.031)	(0.057)	(0.015)
Panel B: First-Stage Summary						
Shea R-squared (%)						
Trains	3.1	5.6	4.7	2.9	3.8	4.2
Institutions	22.9	18.9	19.1	15.4	13.9	17.6
Number of excluded instruments	38	74	74	38	60	63
Hansen OverID test p-value	0.51	0.16	0.13	0.22	0.43	0.22
Number of observations	6,593	6,990	6,990	6,593	6,990	6,990

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level; in parentheses.
#/*/+ Estimate is significant at the 1%/5%/10% level; all regressions include year fixed effects; p-values of F-statistics of all regressions < 0.001
Sample observations are within a twenty-five year span before and after the establishment of a train connection, customs liberalization, or a currency agreement
Specifications (1) and (4) include market-pair fixed effects instead of state-pair fixed effects; (2) and (5) employ limited-information maximum likelihood (LIML) instead of two-stage least squares. Specifications (3) and (6) do not weight the regressions by the number of observations for each market-pair

Table A3: Country results - currency agreements

	No Austria-H (1)	No Belgium (2)	No France (3)	No Netherld. (4)	No Switzerld. (5)	Only Germany (6)	Average	Baseline Table 4
	Panel A: Second-Stage Results							
Train connection	-0.158#	-0.169#	-0.184#	-0.212#	-0.218#	-0.174#	-0.186	-0.162
	(0.026)	(0.024)	(0.025)	(0.028)	(0.027)	(0.019)		
Currency agreement	-0.006	0.030*	-0.090#	-0.008	-0.010	-0.068#	-0.025	-0.059
	(0.016)	(0.015)	(0.024)	(0.015)	(0.012)	(0.003)		
	Panel B: First-Stage Summary							
Shea R-squared (%)								
Trains	2.9	4.3	6.1	5.2	5.0	6.7		
Currency agreement	15.4	13.7	11.4	10.5	11.3	32.5		
Number of excluded instruments	60	59	59	59	61	46		
Hansen OverID test p-value	0.15	0.13	0.22	0.20	0.15	0.15		
Number of observations	6,544	6,371	5,656	6,711	6,720	4,348		

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses.
All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-values of F-statistics for all regressions < 0.001
Sample observations are within a twenty-five year span before and after the establishment of a train connection, customs liberalization, or a currency agreement
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population
(specificaitions (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state

Table A4: Country results - customs liberalization

	No Austria-H (1)	No Belgium (2)	No France (3)	No Netherlds (4)	No Switz.ld. (5)	Only Germany (6)	Average	Baseline Table 4
	Panel A: Second-Stage Results							
Train connection	-0.127#	-0.143#	-0.129#	-0.130#	-0.130#	-0.124#	-0.131	-0.129
	(0.012)	(0.008)	(0.008)	(0.010)	(0.010)	(0.005)		
Customs liberalization	-0.057#	-0.044#	-0.077#	-0.071#	-0.071#	-0.037#	-0.060	-0.072
	(0.012)	(0.008)	(0.009)	(0.010)	(0.010)	(0.006)		
	Panel B: First-Stage Summary							
Shea R-squared (%)								
Trains	5.8	4.7	6.7	5.8	5.6	6.8		
Customs liberalization	18.5	20.4	20.0	18.9	18.9	22.3		
Number of excluded instruments	69	67	69	69	69	53		
Hansen OverID test p-value	0.25	0.20	0.14	0.13	0.11	0.24		
Number of observations	6,544	6,371	5,656	6,711	6,720	4,348		

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses.
All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-value of F-statistics for all regressions < 0.001
Sample observations are within a twenty-five year span before and after the establishment of a train connection, customs liberalization, or a currency agreement
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population
(specificaitions (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state

Table A5: Robustness checks on direct versus indirect effects of institutions

	State Railways				Abolition of Serfdom			
	Years 1820 - 1880		Trimmed 95% sample		Years 1820 - 1880		Trimmed 95% sample	
Train Connection	-0.260#	-0.236#	-0.201#	-0.200#	-0.128#	-0.153#	-0.139#	-0.173#
	(0.076)	(0.056)	(0.039)	(0.036)	(0.010)	(0.021)	(0.009)	(0.019)
Train * State Railway	0.304*	0.150*	0.161*	0.094				
	(0.118)	(0.074)	(0.064)	(0.059)				
Train * Late Abolition of Serfdom					0.077#	0.044*	0.054#	0.007
					(0.015)	(0.019)	(0.008)	(0.011)
Customs liberalization	-0.159#		-0.062#		-0.076#		-0.052#	
	(0.059)		(0.029)		(0.010)		(0.008)	
Currency agreement		-0.024		-0.070#		-0.069*		-0.030+
		(0.018)		(0.014)		(0.031)		(0.016)
State Railways	-0.073#	-0.073#	-0.032#	-0.031#				
	(0.015)	(0.014)	(0.009)	(0.009)				
Late Abolition of Serfdom					-0.134#	-0.055	-0.129#	-0.001
					(0.007)	(0.042)	(0.005)	(0.023)
Shea R-squared (%)								
Trains	3.2	4.6	4.5	4.8	5.7	5.2	6	5.3
Trains * State Railways	1.8	3.3	2.5	3.7				
Trains * Late Abolition of Serfdom					7.3	12.7	18.8	16.5
Customs lib'n or currency agr't	2.2	6.2	5.7	5.4	17.7	8.1	11.5	11
Number of excluded instruments	37	26	37	27	74	63	74	63
Hansen OverID test p-value	0.08	0.36	0.10	0.27	0.22	0.22	0.22	0.26
Number of observations	6,072	6,072	6,642	6,642	6,072	6,072	6,642	6,642

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. Sample as in Table 6.
All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-values of F-statistics of all regressions < 0.001
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population
(specifications (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state

Table A6: Measures of instutional quality

Number	City Name	State/Country	Abolition of Serfdom*	State Railway Indicator**
1	Aachen	Prussia	1807	0
2	Augsburg	Bavaria	1808	1
3	Karlsruhe	Baden	1783	1
4	Bamberg	Bavaria	1808	1
5	Bar le Duc	France	1789	1
6	Basle	Switzerland	1789	0
7	Bayreuth	Bavaria	1808	0
8	Berlin	Prussia	1807	0
9	Bingen	Hesse-Darmstadt	1820	0
10	Boizenburg	Mecklenburg	1820	1
11	Braunschweig	Brunswick	1832	1
12	Bremen	Free City	1783	1
13	Brugge	Belgium	1789	1
14	Brussels	Belgium	1789	1
15	Chalons-sur-Marne	France	1789	1
16	Dresden	Saxony	1832	0
17	Erding	Bavaria	1808	1
18	Frankfurt	Free City	1783	1
19	Giessen	Hesse-Darmstadt	1820	0
20	Goettingen	Hannover	1831	1
21	Grabow	Mecklenburg	1820	1
22	Hamburg	Free City	1783	1
23	Hamm	Prussia	1807	0
24	Hannover	Hannover	1831	1
25	Herdecke	Prussia	1807	0
26	Kassel	Hesse-Cassel	1831	0
27	Kempten	Bavaria	1808	1
28	Köln	Prussia	1807	0
29	Landshut	Bavaria	1808	0
30	Leipzig	Saxony	1832	0
31	Lindau	Bavaria	1808	1
32	Lucerne	Switzerland	1789	0
33	Luebeck	Free City	1783	0
34	Luneville	France	1789	1
35	Mainz	Hesse-Darmstadt	1820	0
36	Memmingen	Bavaria	1808	1
37	Minden	Prussia	1807	0
38	Mulhouse	France	1789	1
39	Munich	Bavaria	1808	1
40	Münster	Prussia	1807	1
41	Nijmegen	Netherlands	1789	0
42	Noerdlingen	Bavaria	1808	1
43	Nurnberg	Bavaria	1808	1
44	Parchim	Mecklenburg	1820	1
45	Prague	Austria-Hungary	1848	0
46	Regensburg	Bavaria	1808	0
47	Rorschach	Switzerland	1789	0
48	Rostock	Mecklenburg	1820	1
49	Saarlouis	Prussia	1807	1
50	Salzburg	Austria-Hungary	1848	0
51	Schwerin	Mecklenburg	1820	1
52	Soest	Prussia	1807	1
53	Strassbourg	France	1789	1
54	Straubing	Bavaria	1808	0
55	Stuttgart	Wuerttemberg	1817	1
56	Toulouse	France	1789	1
57	Ulm	Wuerttemberg	1817	1
58	Utrecht	Netherlands	1789	0
59	Venice	Austria-Hungary	1848	0
60	Vienna	Austria-Hungary	1848	0
61	Wetzlar	Prussia	1807	0
62	Wiesbaden	Hesse-Nassau	1812	0
63	Wismar	Mecklenburg	1820	1
64	Worms	Hesse-Darmstadt	1820	0
65	Wuerzburg	Bavaria	1808	1
66	Xanten	Prussia	1807	0
67	Zweibruecken	Bavaria	1808	0
68	Zwickau	Saxony	1832	0

* Year of initial decree of emancipation; source: Blum (1978) and own estimates for missing data: For Belgium (founded in 1830) and the Netherlands, the French date is employed; for the Free Cities, which were urban and relatively democratic, we pick the earliest year in the sample.

** State had a major role in financing and operating the key railway serving this market early on; coding for the German markets based on the descriptions of the rail lines in Fremdling, Federspiel, and Kunz (1995), pp. 20-55. The markets outside Germany are coded based on O'Brien (1983) and sources therein We focus on the early period to make our distinction since most railways were nationalized in the late 19th century

Figure A1: Cities in states further away from the seaboard join the *Zollverein* earlier

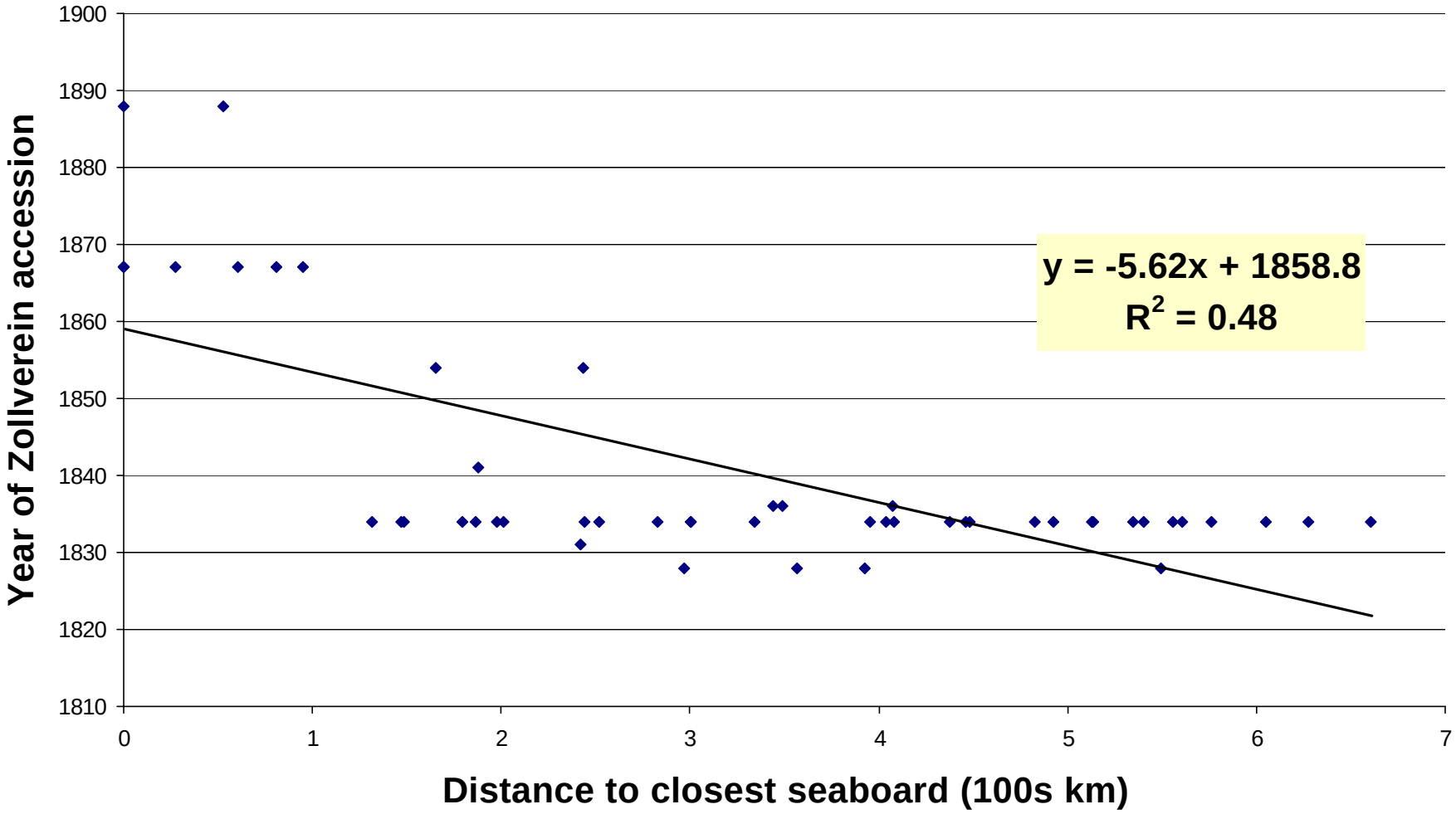

Figure A2: Larger Cities Have Railways Earlier