Economics
The Open-Access, Open-Assessment E-Journal

Discussion Paper
No. 2017-30 | June 09, 2017 | http://www.economics-ejournal.org/economics/discussionpapers/2017-30

Please cite the corresponding Journal Article at
http://www.economics-ejournal.org/economics/journalarticles/2018-3

A replication of 'Education and catch-up in the Industrial Revolution' (American Economic Journal: Macroeconomics, 2011)

Jeremy Edwards

Abstract
Although European economic history provides essentially no support for the view that education of the general population has a positive causal effect on economic growth, a recent paper by Becker, Hornung and Woessmann (*Education and catch-up in the Industrial Revolution*, 2011) claims that such education had a significant impact on Prussian industrialisation. The author shows that the instrumental variable they use to identify the causal effect of education is correlated with variables that influenced industrialisation but were omitted from their regression models. Once this specification error is corrected, the evidence shows that education of the general population had, if anything, a negative causal impact on industrialisation in Prussia.

(Replication Study)

JEL I25 N13 N63 O14
Keywords Education; industrialization; Prussia; regional effects; invalid instrument

Authors
Jeremy Edwards, University of Cambridge, UK, and CESifo, jsse12@yahoo.co.uk

The author thanks Sara Horrell and Sheilagh Ogilvie for very helpful comments. He also thanks Sascha Becker, Erik Hornung and Ludger Woessmann for making their dataset available, at http://www.aeaweb.org/articles.php?doi=10.1257/mac.3.3.92, thereby enabling the author to write this paper.

Citation Jeremy Edwards (2017). A replication of 'Education and catch-up in the Industrial Revolution' (American Economic Journal: Macroeconomics, 2011). Economics Discussion Papers, No 2017-30, Kiel Institute for the World Economy. http://www.economics-ejournal.org/economics/discussionpapers/2017-30

Received April 9, 2017 **Accepted as Economics Discussion Paper** June 2, 2017 **Published** June 9, 2017

1. Introduction

Many theories of economic growth emphasise the role of education as a causal factor in the growth process.[1] In this context, education is often interpreted as meaning education of the general population, and this is the sense in which the term 'education' will be understood throughout the present paper. However, cross-country data for the second half of the twentieth century provide no clear evidence that such education leads to faster economic growth (Pritchett 2006). European economic history also provides little support for the view that education of the general population has an important causal influence on economic growth. Although both education levels and income per capita increased in most European economies between 1550 and 1900, no study has so far been able to show that this association reflects a causal effect of education on growth rather than rising incomes enabling people to consume more education. This lack of evidence of a role for education of the general population in historical economic growth has led the recent literature to focus on the contribution of the knowledge and skills of particular groups to pre-twentieth century economic growth (Kelly et al. 2014, Squicciarini and Voigtländer 2015, Dittmar and Meisenzahl 2016).

One study does, however, claim to provide evidence that education of the general population had an important causal influence on industrialisation in the nineteenth century. Becker, Hornung and Woessmann (henceforth BHW) argue that such education made an important contribution to the industrialisation of Prussia.[2] The Prussian experience, they contend, is consistent with models of technological diffusion in which education contributes to growth by facilitating adoption of new

[1] See, for example, Lucas (1988), Romer (1990) and Mankiw et al. (1992).
[2] BHW (2011)

technologies.[3] In BHW's view, education played an important role in enabling Prussia, as an industrial follower, to catch up with Britain, the technological leader, during the nineteenth century. If correct, BHW's analysis of education and industrialisation in Prussia would be very significant, as it would provide, for the first time, evidence that education of the general population had a causal effect on economic growth before 1900.

To deal with potential two-way causation and identify the causal effect of education on growth, BHW use pre-industrial education as an instrumental variable for education during the period of Prussian industrialisation. However, as this paper will show, a number of variables that influenced industrialisation and were correlated with pre-industrial education are omitted from the cross-section regression specifications on the basis of which BHW conclude that education contributed to industrialisation. Pre-industrial education is therefore not a valid instrument in these regressions, and hence the estimated effects of education in these regressions do not correspond to the causal influence of education on industrialisation. When these omitted variables are included in the regression models, the estimated effects of education change dramatically. In the first half of the nineteenth century, the causal effect of education on overall industrialisation turns out to be negative and both economically and statistically significant. In the second half of the century this causal effect is negative, of modest economic significance, and not statistically significant. When the regression models are specified in such a way that pre-industrial education is a valid instrument, therefore, there is no evidence that education had a positive causal influence on overall industrialisation in Prussia: if anything, the causal effect was negative. Prussian experience in the nineteenth century cannot be used to support

[3] This view of the role of education in economic growth originates with Nelson and Phelps (1966).

the view that education of the general population plays an important positive role in the growth process, whether by facilitating the adoption of new technologies or by any other causal mechanism.

2. The BHW analysis

BHW analyse the contribution of education to Prussian industrialisation using a dataset for the 334 Prussian counties that existed in 1849.[4] The major institutional reforms which took place in Prussia after military defeat by France in 1806 made it possible, by about 1820, for Prussia to benefit from the technological advances that had occurred in Britain. Prussia's own industrial revolution began in the mid-1830s. BHW argue that the change in Prussian institutions which made this possible can be treated as exogenous from the point of view of their econometric analysis. After this exogenous change, different Prussian counties industrialised to differing extents, and the causal effect of education can be identified, they argue, by analysing the relationship between these differences and differences in the counties' educational levels.

BHW recognise that any causal relationship between education and industrialisation may go in both directions. The growth of factory production could have created new occupations with lower educational requirements, decreasing the general level of education; or it could have increased the demand for human capital, increasing educational levels. Conversely, if industrialisation raised living standards, these higher incomes might have increased the demand for education. Any attempt to identify the causal effect of education on industrialisation must take account of this

[4] County is BHW's translation of the German word *Kreis*. A *Kreis* is an administrative unit which is closer to the American than to the British sense of county.

possible reverse causality. BHW do so by carrying out an instrumental variables (henceforth IV) analysis of the cross-section effect of education on industrialisation in 1849 and 1882. They use the level of education in 1816, measured by enrolment in elementary and middle schools as a share of the population aged from six to 14, as an instrument for later education. Their argument is that education in 1816 can be used to isolate the component of subsequent education which did not depend on subsequent industrialisation, and thus to identify the causal effect of education. BHW further argue that differences in education levels among Prussian counties in 1816 reflected exogenous historical idiosyncracies and therefore had no direct effect on subsequent industrialisation. Based on these considerations, they argue that education in 1816 is a valid instrument for subsequent education.

Pre-industrial education is more likely to satisfy the requirement for a valid instrument - that it has no direct effect on subsequent industrialisation - if the cross-section regression models include other measures of the pre-industrial characteristics of each county. This reduces the likelihood that pre-industrial education is correlated with the error term in the regression models because of being correlated with other pre-industrial features of counties which have been incorrectly omitted from these models. BHW therefore include several such measures in their preferred specifications. As indicators of pre-industrial development, the share of the population living in cities in 1816 and the number of looms per capita in 1819 are included as regressors. To proxy for mineral resource availability, the number of steam engines used in mining in 1849 is included. The number of sheep in 1816 is used as a proxy for the availability of wool for the textile industry. The share of farm labourers in the population in 1819 is included as an indicator of whether a county was less likely to industrialise because of its more agricultural orientation. Various measures of pre-industrial public infrastructure which might have influenced subsequent industrialisation are also included as regressors: the number of public buildings per capita in 1821, a dummy variable registering the presence of paved inter-regional roads in 1815, and a measure of the capacity of river transport ships in 1819.

Prussian industrialisation occurred, BHW argue, in two phases: the first from approximately 1835 to 1850, the second during the latter half of the nineteenth century.[5] Consequently they estimate separate cross-section regression models of Prussian industrialisation in 1849 and 1882. The definitions of the variables used for their main analysis differ somewhat between the two periods. Total industrialisation in each county in 1849 is measured as the share of *factory* employment in total population, and BHW also disaggregate this measure of total industrialisation into three industrial sectors – metal, textile, and all other branches. Total industrialisation in 1882 is measured as the share of *manufacturing* employment in total county population, again distinguishing between the same three sectoral components. Education in 1849 is measured by the average number of *years of primary schooling* in the 1849 working population in each county, which is constructed from school enrolment data available for 1816 and 1849 and population data for 1849.[6] For the 1882 regression, education is measured by the *literacy rate* in 1871, defined as the share of those able to read and write in the total population aged 10 or over at this date.

The regression models also include measures of basic demographic and geographical features. The shares of the population below 15 and above 60 in the total population in 1849 are used as regressors for the 1849 model, and the shares below 15

[5] BHW (2011), 98.
[6] BHW (2011), 104, n. 9.

and above 70 in 1882 are used for the 1882 model. Both models use the geographical area of each county as a regressor.

The cross-section regression model for industrialisation in 1849 provides an estimate of the effect of education on industrialisation in the first phase of Prussian industrialisation, from roughly 1835 to 1850. The second phase of industrialisation occurred in the second half of the nineteenth century, and BHW estimate two cross-section models for 1882. One excludes industrialisation in 1849 as a regressor, and hence provides an estimate of the effect of education on industrialisation over the entire period from the beginning of Prussian industrialisation until 1882. The other includes industrialisation in 1849 and thus, by controlling for the level of industrialisation at the end of the first phase, provides an estimate of the effect of education on industrialisation solely in the second phase, from the middle of the nineteenth century until 1882.

BHW find that pre-industrial education is strongly correlated with education in both 1849 and 1871 and thus satisfies the requirement of being a relevant instrument for IV estimation of the causal effect of education on industrialisation. The cross-section regression models for both periods yield estimates of this effect for total industrialisation and non-textile industrialisation that are both economically and statistically significant. However, BHW find no evidence that education contributed to textile industrialisation, which, they suggest, may be because in the textile sector technological developments were more incremental and child labour was more important. BHW conclude from their cross-section results that, except in textiles, education was an important causal influence on Prussian industrialisation in both its phases.

The BHW results for their preferred cross-section regression models of total industrialisation in the first and second phases of Prussian industrialisation are shown in Table 1, together with the results that I obtained by re-estimating their models. As Table 1 shows, I was able to reproduce the BHW results exactly. The estimated effects of education on total industrialisation correspond to elasticities of 0.53 in the first phase of industrialisation and 0.73 in the second phase.[7]

As an alternative to cross-section regression models, BHW also combine their observations for 1816, 1849 and 1882 into a panel dataset which they use to estimate fixed-effect models. These models control for any time-invariant unobserved heterogeneity which might be present in the cross-section models. BHW conclude that the results from their fixed-effect panel regressions confirm those from their cross-section regressions: education had an important causal effect on Prussian industrialisation.

BHW's conclusion that education played an important role in Prussian industrialisation is based on a particular econometric strategy. But is this strategy justified? The remainder of this paper argues that it is not. BHW's preferred regression models omit a number of variables that measure regional effects, which the historical literature has found were important influences on Prussian industrialisation. BHW's instrumental variable – education in 1816 – is correlated with variables that measure regional effects, and thus it is likely that, in their preferred models, BHW's instrument is not valid. Furthermore, BHW do not use a systematic model selection procedure to choose, from the large number of variables that might have influenced Prussian industrialisation, a regression model with which to conduct inference about the effects of education on industrialisation. When these problems are addressed, it

[7] Here and throughout the paper all reported elasticities are calculated at sample mean values.

Table 1: Reproduction of key results from BHW.

	Dependent variable			
	Share of all factory workers in total population 1849		Share of all manufacturing workers in total population 1882	
Regressors	(1.1)	(1.2)	(1.3)	(1.4)
Years of schooling 1849	0.182**	0.182**	-	-
	(0.080)	(0.080)	-	-
Share of population	0.050	0.050	-	-
< 15 years 1849	(0.050)	(0.050)	-	-
Share of population	0.085	0.085	-	-
> 60 years 1849	(0.074)	(0.074)	-	-
Literacy rate 1871	-	-	0.101***	0.101***
	-	-	(0.036)	(0.036)
Share of population	-	-	Not reported	-0.102
< 15 years 1882	-	-	Not reported	(0.093)
Share of population	-	-	Not reported	-0.560
> 70 years 1882	-	-	Not reported	(0.402)
County area	-0.005**	-0.005**	Not reported	-0.016***
	(0.002)	(0.002)	Not reported	(0.006)
Share of population living	0.020***	0.020***	0.024**	0.024**
in cities 1816	(0.007)	(0.007)	(0.012)	(0.012)
Looms per capita 1819	0.154***	0.154***	0.774**	0.774**
	(0.046)	(0.046)	(0.302)	(0.302)
Steam engines in mining	0.043***	0.043***	0.125***	0.125***
per capita 1849	(0.005)	(0.005)	(0.017)	(0.017)
Sheep per capita 1816	-0.0004	-0.0004	-0.019***	-0.019***
	(0.002)	(0.002)	(0.004)	(0.004)
Share of farm labourers in	-0.057***	-0.057***	-0.046	-0.046
total population 1819	(0.017)	(0.017)	(0.050)	(0.050)
Public buildings per	-0.290	-0.290	-0.575	-0.575
capita 1821	(0.283)	(0.283)	(0.604)	(0.604)
Paved streets 1815	0.003	0.003	0.009*	0.009*
	(0.002)	(0.002)	(0.005)	(0.005)
Tonnage of ships per	-0.032**	-0.032**	0.011	0.011
capita 1819	(0.015)	(0.015)	(0.031)	(0.031)
Share of all factory workers in	-	-	0.923***	0.923***
total population 1849	-	-	(0.168)	(0.168)
Constant	-0.010	-0.010	Not reported	0.078
	(0.020)	(0.020)	Not reported	(0.053)
R^2	0.266	0.266	0.702	0.702
First-stage *F* statistic	5507.59	5507.59	65.29	65.29

Notes: Number of observations for all equations is 334. Figures in parentheses are standard errors clustered at the level of the 280 independent units of observation in 1816. *, ** and *** denote significance at the 0.10, 0.05 and 0.01 levels respectively. Equation (1.1) is taken from Table 3 of BHW (where it is equation 6) and equation (1.3) is taken from Table 5 of BHW (where it is equation 2). Table 5 of BHW does not report estimated coefficients and standard errors for all variables in the regression model. Equations (1.2) and (1.4) report the results I obtained by re-estimating the BHW regression models. The school enrolment rate in 1816 is used as an instrumental variable for years of schooling in 1849 in equations (1.1) and (1.2) and for the literacy rate in 1871 in equations (1.3) and (1.4).

turns out that the data available for nineteenth century Prussia fail to yield empirical support for the view that education played a positive causal role – rather the contrary.

3. Regional effects and regression models of Prussian industrialisation

Any satisfactory analysis of the relationship between education and industrialisation in Prussia must take into account regional effects (Tipton 1976). Nineteenth-century Prussia consisted of territories that had been part of the Prussian state for very different lengths of time. The Duchy of Prussia was created in 1525 and was unified with Brandenburg in 1618 to become the state of Brandenburg-Prussia, which also included some small territories in the Rhineland. In 1701 this state became the Kingdom of Prussia, and during the eighteenth century it expanded by acquiring, *inter alia*, Pomerania, Silesia, and parts of Poland. In 1815, as part of the peace settlement at the end of the Napoleonic wars, Prussia acquired the remainder of the Rhineland, Westphalia, and various other territories.[8] Of the 334 counties in BHW's dataset, 112 became part of Prussia only after 1815, and a further 51 had only become Prussian between 1772 and 1815.

The industrialisation of Germany in general, and Prussia in particular, was characterised by two main features: a late start by European standards, and enormous regional variation.[9] Hardach (1991) dates the beginning of the German industrial revolution to 1784, when a mechanised spinning plant was opened in the Rhineland town of Ratingen. Kaufhold (1986) identifies 39 industrial regions, defined as having an above-average density of industrial employment and a large proportion of output

[8] From 1822 Prussia consisted of nine provinces: Brandenburg, East Prussia, West Prussia, Pomerania, Posen, the Rhineland, Saxony, Silesia and Westphalia. In 1829 East and West Prussia were merged to form the single province of Prussia. Prussia acquired three further provinces in 1866, but these are not included in the BHW dataset.
[9] Ogilvie (1996), 121.

sold beyond the region, in Germany around 1800. Of these, 14 were in territories that were part of Prussia by 1816: nine in the Rhineland, five in Westphalia, and two in Silesia. However, even in these Prussian regions there was only limited factory industrialisation by 1816. In other parts of Prussia – the provinces of Prussia, Posen and Pomerania – there was essentially no factory industrialisation in 1816.

This regional variation in Prussian industrialisation reflected different legacies of social institutions and thus different frameworks for economic activity.[10] The backwardness of the provinces of Prussia, Posen and Pomerania was due to the fact that the institutional powers of feudal landlords over the rural population had hardly declined since the sixteenth century, and in some regions had even been increased by the second serfdom. As a result, industry in these provinces hardly existed in the countryside and was largely restricted to the towns. But strong feudal landlords did not inevitably inhibit the growth of industry. Where, as in Silesia, conditions were less suited to agriculture, landlords saw more opportunities for extorting profit from rural industrial work. A dense linen proto-industry dominated by feudal landlords developed in Silesia in the seventeenth century at the same time as the second serfdom and lasted well into the nineteenth century.[11]

By contrast, in most western parts of Prussia the institutional powers of feudal landlords over the rural population had weakened by the sixteenth century, enabling proto-industry to develop in the countryside. The Rhineland was the most economically advanced part of Prussia in 1816, because the decline in landlord power combined with extensive political fragmentation to enable proto-industries to cross

[10] Ogilvie (1996), 122.
[11] Ogilvie (1996), 122-3.

territorial boundaries easily in order to locate where political and institutional conditions were least oppressive.[12]

The differing social and institutional framework of Prussian regions continued to influence industrialisation throughout the nineteenth century. The powers of feudal landlords remained strong in the backward eastern parts of Prussia even after the formal abolition of Prussian serfdom in 1806, and factory industrialisation here was delayed until the later nineteenth century. Despite its relatively advanced state in the early nineteenth century, Silesian factory industrialisation was hampered by the desire of feudal landlords to protect their proto-industrial feudal revenues through resistance to technological improvements in linen production, which they achieved with assistance from the Prussian state. In the west of Prussia, by contrast, the institutional framework remained more favourable to economic development throughout the nineteenth century.

These features of Prussian industrialisation suggest that any regression model of such industrialisation should allow for the likelihood that the different social and institutional frameworks in different provinces influenced industrialisation in the various counties of Prussia. A natural way to do this is to suppose that there are province fixed effects on industrialisation, which can be captured by including provincial dummy variables as regressors in a regression model of Prussian industrialisation. The ifo Prussian Economic History Database allows the counties in BHW’s dataset to be categorised according to the provinces in which they fell in 1849.[13] There were 57 counties in the province of Prussia, 26 in Posen, 33 in Brandenburg, 26 in Pomerania, 41 in Saxony, 57 in Silesia, 35 in Westphalia and 59 in the Rhineland.

[12] Ogilvie (1996), 124-5.
[13] Becker et al. (2014).

Additional sources of institutional variation in the different regions of Prussia might also have influenced industrialisation. Different legal codes operated in different parts of nineteenth-century Prussia. Of the counties that became Prussian in 1816, 54 were in the Rhineland. Until 1900, when the German civil code (*Bürgerliches Gesetzbuch*) was introduced, these Rhineland counties operated under the French civil code, which had been imposed under French occupation in 1802. In contrast to the Prussian civil code of 1794 (*Allgemeines Landrecht*), which operated until 1900 in most other parts of Prussia and retained special landlord courts for peasants, the French civil code guaranteed equality before the law for all citizens.[14] Thus the legal code in the Rhineland was different in an important respect from that in the rest of Prussia. A further four of the counties that became Prussian after 1815 (those in Stralsund, part of the province of Pomerania) had previously been ruled by Sweden, and for much of the nineteenth century these counties had a special legal status in which Swedish laws continued to apply.

Other aspects of the institutional structure also varied between different parts of Prussia. Agrarian reform, the abolition of guilds, and the abolition of serfdom all occurred somewhat earlier in the Rhineland, as a consequence of French occupation, than in other parts of Prussia. Acemoglu et al. (2011) construct an index of institutional reform based on the civil code, agrarian reform, the abolition of guilds, and the abolition of serfdom. The value of this index for the Rhineland was considerably higher than for the provinces of Prussia, Brandenburg, Pomerania, Silesia and Westphalia in both 1850 and 1900, while the value for Saxony was modestly higher.[15] The variation in the institutional framework of nineteenth century

[14] Acemoglu et al. (2011), Online Appendix (https://assets.aeaweb.org/assets/production/articles-attachments/aer/data/dec2011/20100816_app.pdf), 11-13.
[15] Acemoglu et al. (2011), Table 1, 3292.

Prussia thus provides another reason why regional effects need to be taken into account in analysing Prussian industrialisation.

To some extent, BHW acknowledge these institutional and legal differences across Prussia in their tests of the robustness of their preferred regression specifications. One of these involves the addition of a dummy variable for the western parts of Prussia, which indicates whether a county was in the Rhineland or Westphalia. However, this only allows for limited provincial differences, whereas, as the discussion above shows, there are reasons to expect differences both between the Rhineland and Westphalia and between the various eastern provinces of Prussia. A second BHW robustness test involves the addition to their preferred specifications of the year in which a county was annexed by Prussia as a regressor, to investigate whether industrialisation depended on how long a county had been part of the Prussian institutional and legal framework.[16] But this is problematic. Using year of annexation on its own does not distinguish between counties that are likely to have had very different institutional and legal frameworks. For example, 112 counties became part of Prussia in 1816. Of these, 54 were in the Rhineland, but the other 58 were scattered among all the provinces of Prussia except Posen and the province of Prussia. The institutional framework of a county in the Rhineland which became Prussian in 1816 was very different from one in Pomerania that became Prussian at the same date. On its own, therefore, the year of annexation is likely to be an inaccurate measure of institutional and legal variation. Thus, in addition to the provincial dummy variables already mentioned, I use the year of annexation both on its own and interacted with province dummy variables as measures of institutional variation by region in the regression analysis of Prussian industrialisation.

[16] BHW (2011), 114.

It is therefore likely that BHW's preferred models omit some variables which influenced industrialisation in Prussia. However, BHW use the level of education in 1816 as an instrument for education in 1849 and 1882. Thus BHW's regression analysis will still give a consistent estimate of the causal effect of education on industrialisation if the level of education in 1816 was uncorrelated with these omitted variables and hence a valid instrument for subsequent education. This means it is important to ask whether education in 1816 is indeed uncorrelated with province dummies, the year of annexation, and their interactions.

Table 2 provides the answer. It shows OLS estimates of cross-section regressions using the 334 Prussian counties in BHW's sample. The dependent variable in all three equations is BHW's instrument: the school enrolment rate in 1816. The regressors are those in BHW's preferred regression models of industrialisation in 1849 (equation (2.1) in Table 2), 1882 (equation (2.2)), and 1849-1882 (equation (2.3)), together with year of annexation, province dummies, and interactions between them, as well as most of the other variables that BHW use as tests of robustness in their Tables 6 and 7. The only variables from BHW's Tables 6 and 7 not included as regressors are the latitude and longitude of counties, because these are very strongly correlated with the provincial dummy variables and the measures of distance to Berlin, London, and nearest provincial capital.

The results for year of annexation and province reported in Table 2 are the marginal effects for each variable. The marginal effect of year of annexation is evaluated at the mean values of the province dummies for the entire sample, while the marginal effects of the provinces are evaluated at the mean values of year of annexation for the province in question. The omitted province dummy is that for the province of Prussia, so the marginal effects of the provinces show the difference

Table 2: The relationship between county school enrolment in 1816 and other characteristics, Prussia, 1849, 1882, and 1849-82.

	Dependent variable: School enrolment rate 1816		
Regressors	(1.1)	(1.2)	(1.3)
Share of population < 15 years 1849	-0.631		
	(0.484)		
Share of population > 60 years 1849	0.149		
	(0.936)		
Share of population < 15 years 1882		-0.437**	-0.429*
		(0.219)	(0.220)
Share of population > 70 years 1882		0.136	0.047
		(1.456)	(1.492)
Share of factory workers in total population 1849			-0.219
			(0.349)
County area	0.028	0.033	0.032
	(0.021)	(0.021)	(0.021)
Share of population living in cities 1816	-0.110**	-0.086*	-0.082
	(0.055)	(0.051)	(0.053)
Looms per capita 1819	-0.229	-0.241	-0.226
	(0.274)	(0.264)	(0.257)
Steam engines in mining per capita 1849	-0.215***	-0.215***	-0.209***
	(0.059)	(0.062)	(0.062)
Sheep per capita 1816	0.027	0.024	0.024
	(0.022)	(0.022)	(0.022)
Share of farm labourers in total population 1819	-0.137	-0.129	-0.136
	(0.171)	(0.169)	(0.170)
Public buildings per capita 1821	8.777***	8.703***	8.552***
	(3.212)	(3.282)	(3.289)
Paved streets 1815	0.001	0.005	0.005
	(0.018)	(0.019)	(0.019)
Tonnage of ships per capita 1819	0.303	0.311	0.307
	(0.408)	(0.418)	(0.420)
Landownership inequality	-1.187**	-1.224**	-1.217**
	(0.522)	(0.530)	(0.530)
Distance to Berlin	-0.130	-0.091	-0.080
	(0.173)	(0.172)	(0.173)
Distance to London	0.005	-0.058	-0.076
	(0.134)	(0.127)	(0.130)
Distance to nearest provincial capital	-0.358*	-0.375*	-0.370*
	(0.195)	(0.194)	(0.195)
Polish parts	0.041	0.045	0.045
	(0.039)	(0.038)	(0.038)
Share of Protestants in total population 1816	0.115***	0.121***	0.123***
	(0.037)	(0.037)	(0.038)
Share of Jews in total population 1816	0.927	0.866	0.851
	(1.013)	(0.985)	(0.987)
Year in which annexed by Prussia	-1.100***	-1.046***	-1.048***
	(0.330)	(0.319)	(0.319)
Posen	-0.120	-0.125*	-0.125*
	(0.076)	(0.075)	(0.075)
Brandenburg	0.008	-0.008	-0.009
	(0.068)	(0.067)	(0.067)

Pomerania	0.050	0.034	0.031
	(0.048)	(0.049)	(0.049)
Saxony	0.299***	0.279***	0.277***
	(0.074)	(0.073)	(0.073)
Silesia	0.250***	0.249***	0.251***
	(0.043)	(0.044)	(0.044)
Westphalia	0.361***	0.323***	0.314***
	(0.103)	(0.096)	(0.097)
Rhineland	0.289**	0.238**	0.227*
	(0.126)	(0.121)	(0.122)
Constant	3.533***	3.558***	3.579***
	(0.796)	(0.795)	(0.796)
R^2	0.744	0.743	0.743

Notes: Number of observations for all equations is 334. Figures in parentheses are standard errors clustered at the level of the 280 independent units of observation in 1816. *, ** and *** denote significance at the 0.10, 0.05 and 0.01 levels respectively. See text for interpretation of estimated effects of year of annexation and province dummy variables.

compared to that province. I follow BHW by clustering the standard errors in these regressions at the level of the 280 independent units of observation in 1816. This is because BHW had to adjust the data reported in the 1816 census in order to construct a consistent dataset for the 334 Prussian counties as they existed in 1849, and hence the data for 1816 were not based on 334 independent observations.[17]

It is clear from Table 2 that, even controlling for the regressors included in BHW's preferred models, education in 1816 is correlated with several variables that were omitted from those models. In particular, landownership inequality, share of Protestants, year of annexation, and the dummy variables for Saxony, Silesia, Westphalia and the Rhineland all have estimated coefficients that are statistically significant at conventional levels in equations (2.1), (2.2) and (2.3). Some of these estimates correspond to quite small effects, but others are substantial, particularly the effects of year of annexation and the province dummies. The estimated elasticities for

[17] BHW (2011), 105 n.12, and online Appendix A1 (https://assets.aeaweb.org/assets/production/articles-attachments/aej/mac/app/2010-0021_app.pdf).

year of annexation, for instance, are all approximately -3. The marginal effects of several of the provincial dummies are roughly 50 per cent of the sample mean value of education in 1816, which was 0.577. Furthermore, as Section 5 will show, several of the variables which were omitted from BHW's preferred specifications but are correlated with education in 1816 do indeed influence Prussian industrialisation. The fact that these variables both have an effect on industrialisation and are correlated with the school enrolment rate in 1816 means that the latter variable is not a valid instrument for education in regression models that omit them, as BHW's preferred models do.

Before investigating the implications of this finding for estimates of the causal effect of education on Prussian industrialisation, I consider some issues in the specification of appropriate regression models of Prussian industrialisation.

4. Specification of regression models of Prussian industrialisation

Adding the year of a county's annexation by Prussia, province dummies, and interactions between these variables as regressors to BHW's preferred models yields very strong evidence that these variables should be included. The null hypothesis that the coefficients of year of annexation, the province dummies, and their interactions are all zero is strongly rejected both for overall industrialisation and its three sectoral components. Furthermore, the addition of these variables to BHW's preferred models changes the estimated effect of education on industrialisation: there is no evidence at all that education had a positive effect in the first phase of Prussian industrialisation, and only limited evidence of a positive effect in the second phase.

However, selecting models by adding variables to a basic specification is not a satisfactory approach. The limitations of the specific-to-general procedure have been known at least since Anderson (1962) showed that the optimal procedure for the choice of degree of a polynomial regression was a general-to-specific approach. Correct inference about the effects of education on Prussian industrialisation requires an empirical model of industrialisation that can be justified by a convincing model selection procedure. The reason for this is that there is no clear theoretical model which specifies the explanatory variables that should be included in an empirical model of Prussian industrialisation: rather, there are a number of plausible candidate variables which might or might not be relevant determinants. In these circumstances, the conclusions drawn about the effect of education on industrialisation are likely to depend on which precise combination of the various plausible candidates is included in the regression together with education. A specification search of some form is unavoidable, but there are better and worse forms of specification search. I use a version of the general-to-specific model selection procedure advocated by Hendry and his co-authors (Hendry and Krolzig 2005, Campos et al. 2005) to choose an empirical model that provides a justifiable basis for inference about the variables that influence Prussian industrialisation.

In outline, the general-to-specific procedure involves starting with a very general regression model that is subjected to various tests of adequacy as a representation of the data-generating process. If these tests are passed, the very general model is simplified by removing variables on a series of different search paths, at each stage testing whether the removal of variables is justifiable and whether the simplified model continues to be an adequate representation of the data. Each search path terminates at the stage where these tests show that no further simplification is justifiable. If only one model remains after the different search paths have been explored, it is the selected model. If more than one remains, these models are tested against each other in an attempt to choose a single model, but there is no guarantee that just one model will be selected.

A criticism that is often levelled against this model selection procedure is that it involves data-mining – "the data-dependent process of selecting a statistical model" (Leamer 1978, 1). In much empirical economics, and certainly in the case of the determinants of Prussian industrialisation, some data-mining is unavoidable, because there are many plausible explanatory variables and no theoretical guidance as to which should be included in the regression model. The case for using the general-to-specific procedure is that it is a systematic method of model selection which has good properties, as Hendry and Krolzig (2005) and Campos et al. (2005) show. In particular, the two most serious concerns about this procedure – that selection of variables by significance tests will lead to biased coefficient estimates and that treating a selected model as if it were certain will result in under-estimates of coefficient standard errors – do not appear to be important in practice.

In order to provide more satisfactory empirical models of Prussian industrialisation with which to make inferences about the causal effect of education, I use the following version of the general-to-specific procedure. I begin with an over-parameterised general regression model with the following three groups of variables as regressors. The first group comprises most of the variables in BHW's preferred specifications. The variables not included despite having been used by BHW were those that might have been influenced by contemporaneous Prussian industrialisation and hence are potentially bad controls.[18] The general model for industrialisation in

[18] Angrist and Pischke (2009), 64-6.

1849 therefore excluded the shares of young and old in county population in 1849, the number of steam engines used in mining in 1849, and landownership inequality in 1849. For industrialisation in 1882, only the shares of young and old in the population in 1882 were excluded. The second group of variables consists of the year of annexation, province dummies, and interactions between them. The third group comprises most of the other variables that BHW use as tests of the robustness of the results of their preferred regressions. It includes religious indicators (the share of Protestants and the share of Jews in county population in 1816) to allow for possible effects of religion on industrialisation, and a dummy variable for counties located in present-day Poland to allow for possible Slavic-language effects. It also includes several geographical controls: the distance of each county from the nearest provincial capital, the distance from the Prussian capital Berlin, and the distance from London, the last of which allows for any effects on county-level industrialisation of distance from the country which was the industrial leader for most of the nineteenth century. It does not, however, include the latitude and longitude of the counties, despite these variables having been used by BHW in their robustness tests, for the same reason that these variables were not included in the regressions reported in Table 2.

The general regression model was estimated by IV with the instrument for the education variable being education in 1816. The test of adequacy applied to the general regression model was whether the null hypothesis that the estimated coefficients did not differ between two subsamples could be rejected at the 0.05 level. These subsamples were obtained by randomly dividing the full sample of 334 counties into two groups of equal size. This null hypothesis was always rejected – perhaps unsurprisingly, given that there were at least 30 variables whose estimated coefficients might differ between the two subsamples – and so the general model was expanded by allowing some variables to have different effects in the two subsamples. This expanded model was then simplified by removing variables, although removal of the education variable was never permitted throughout the simplification procedure because of the focus on whether education influenced industrialisation. Thus the following description of how variables were removed applies to all regressors in the expanded model except education.

First, several variables with the lowest absolute *t* statistic values in the expanded model were removed, and a *F* test was used to test whether this restriction was acceptable. A simplified regression model was then estimated, and its adequacy was tested using the two subsamples. If it was adequate, a first search path was begun by removing the variable with the lowest *t* statistic (in absolute value), estimating a further simplified model and subjecting it to a further test of adequacy. The restrictions required to obtain this further simplified model from the expanded model were also tested by a *F* test. Provided that both the test of adequacy and the test of restrictions were passed, the variable in the further simplified model with the lowest *t* statistic was removed and a still-further simplified model was estimated. These steps were repeated until either the test of adequacy or the test of restrictions were failed or no further variables could be removed because all had *t* statistics that were statistically significant with *p* values of 0.05 or lower. At this point the cases in which the estimated coefficients of a variable were statistically significantly different between the two subsamples were examined. In each case, if the difference was economically insignificant despite being statistically significant, the restriction that the coefficients were equal in the subsamples was imposed. Imposing this restriction did not have any effect on the inferences drawn about the variables that influenced industrialisation. The resulting model was the terminal model on the first search path. A second and a third search path from the initial simplified regression model were also explored, by removing the variable with, respectively, the second and the third lowest *t* statistic and repeating the steps described until a terminal model was reached. In all cases, the different search paths ended with the same terminal model, so there was no need to choose between terminal models.

The general-to-specific model selection procedure used here is very different from the approach used by BHW to investigate the robustness of their preferred regression models, even though it uses most of the variables considered by BHW in their robustness checks. In Tables 6 and 7 of their paper, BHW report the results of adding 11 variables to their preferred specifications, but they do so in eight separate steps. Hence BHW's conclusion that their estimates of the effect of education on industrialisation are robust to the addition of the these variables is not justifiable, because piecemeal addition of possible regressors provides no information about whether the estimates are robust to the *simultaneous* inclusion of all these variables.

5. Cross-section estimates of the effect of education on Prussian industrialisation

What are the estimated effects of education on Prussian industrialisation that emerge from the model selection procedure discussed in the previous section? Table 3 presents the results for the first phase of Prussian industrialisation, in which the dependent variable is a measure of industrialisation in 1849, while Table 5 presents the results for the second phase, in which the dependent variable is a measure of industrialisation in 1882 while industrialisation in 1849 is included as a regressor.[19]

[19] The results for industrialisation in the two phases combined are not reported, because they do not add anything to the overall analysis. Full details are available from the author on request.

5.1 The first phase of industrialisation

Table 3 shows IV and OLS estimates of the terminal regression model for Prussian industrialisation in 1849 obtained using the general-to-specific procedure. The first-stage *F* statistic for the IV estimates in Table 3 is extremely large. As a consequence, the (unreported) weak-instrument-robust 95 per cent confidence interval based on the Anderson-Rubin (1949) test statistic is almost identical to the standard 95 per cent confidence interval based on the asymptotic distribution of the IV estimator.[20] BHW point out that the very high first-stage *F* statistic results from the fact that the measure of years of schooling in 1849 is partly based on the school enrolment rate in 1816. The fact that BHW's measure of education in 1816 is an input into their measure of education in 1849 is reflected in the correlation of 0.981 between these two variables. Such a high correlation between the instrument and the potentially endogenous variable would usually be a positive feature, but when it results from the fact that the instrument has been used in the construction of the endogenous variable there are unavoidable concerns about the validity of the IV analysis. To allay such concerns, BHW report that their results for 1849 are qualitatively similar if education in 1849 is measured by enrolment in elementary and middle schools as a share of the population aged from six to 14 in 1849 rather than by years of schooling.[21] The same is true of the results in Table 3 of this paper: the conclusions I draw from this table are qualitatively unaffected if education in 1849 is instead measured by the school enrolment rate in 1849.[22]

[20] The weak-instrument-robust confidence interval was obtained using the Stata weakiv command of Finlay et al. (2013).
[21] BHW (2011), 104, n. 10.
[22] Full details of the results obtained when education in 1849 is measured by the school enrolment rate in 1849 are available from the author on request.

Table 3: Estimates of the effect of education on Prussian industrialisation in 1849 using terminal model from general-to-specific procedure

	IV estimates				OLS estimates			
	Dependent variable: Share of factory workers in total population 1849				Dependent variable: Share of factory workers in total population 1849			
Regressors	All factories	All except metals and textiles	Metal factories	Textile factories	All factories	All except metals and textiles	Metal factories	Textile factories
	(3.1)	(3.2)	(3.3)	(3.4)	(3.5)	(3.6)	(3.7)	(3.8)
Years of schooling 1849	-0.204**	0.017	-0.182**	-0.039	-0.176*	0.025	-0.158**	-0.044
	(0.101)	(0.043)	(0.085)	(0.040)	(0.091)	(0.041)	(0.074)	(0.038)
Distance to Berlin	0.060***	0.008	0.029*	0.024**	0.059***	0.007	0.028*	0.024**
	(0.018)	(0.006)	(0.015)	(0.011)	(0.018)	(0.006)	(0.015)	(0.011)
Distance to London	-0.072***	-0.022***	-0.025**	-0.025***	-0.071***	-0.022***	-0.024**	-0.025***
	(0.014)	(0.006)	(0.011)	(0.009)	(0.014)	(0.006)	(0.011)	(0.009)
Share of Protestants in total population 1816	0.012***	0.007***	0.006**	-0.000	0.012***	0.007***	0.005**	-0.000
	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)
Share of population living in cities 1816	0.015***	0.009***	0.003	0.003	0.015***	0.009***	0.003	0.003
	(0.006)	(0.003)	(0.003)	(0.002)	(0.006)	(0.003)	(0.003)	(0.002)
Looms per capita 1819	0.086**	-0.010	0.034	0.063*	0.088**	-0.010	0.035	0.062*
	(0.042)	(0.019)	(0.042)	(0.037)	(0.042)	(0.019)	(0.042)	(0.037)
Share of farm labourers in total population 1819	-0.040**	-0.008	-0.020*	-0.012	-0.040**	-0.008	-0.020*	-0.012
	(0.018)	(0.013)	(0.011)	(0.009)	(0.018)	(0.013)	(0.011)	(0.009)
Public buildings per capita 1821	-0.879***	-0.416***	-0.206	-0.256**	-0.910***	-0.426***	-0.234*	-0.250**
	(0.258)	(0.149)	(0.134)	(0.106)	(0.258)	(0.150)	(0.134)	(0.104)
Brandenburg	0.003	-0.005***	0.004*	0.004***	0.002	-0.005***	0.004*	0.004***
	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)
Pomerania	-0.008***	-0.007***	-0.001	-0.001	-0.008***	-0.007***	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)

Silesia	0.010***	0.001	0.007***	0.002**	0.009***	0.001	0.006***	0.002**
	(0.003)	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)
Westphalia	-0.032***	-0.011**	-0.003	-0.017**	-0.032***	-0.011**	-0.003	-0.017**
	(0.008)	(0.005)	(0.004)	(0.007)	(0.008)	(0.005)	(0.004)	(0.007)
Rhineland	-0.047***	-0.013***	-0.015	-0.019**	-0.046***	-0.013***	-0.014	-0.019**
	(0.012)	(0.005)	(0.009)	(0.009)	(0.012)	(0.005)	(0.009)	(0.009)
Constant	0.083***	0.028***	0.029***	0.026***	0.081***	0.027***	0.027***	0.026***
	(0.014)	(0.006)	(0.011)	(0.010)	(0.014)	(0.006)	(0.010)	(0.010)
R^2	0.343	0.303	0.135	0.237	0.343	0.303	0.135	0.237
First-stage *F* statistic	3206.56	3206.56	3206.56	3206.56	-	-	-	-
C test *p* value	0.120	0.263	0.123	0.443	-	-	-	-

Notes: Number of observations for all equations is 334. Figures in parentheses are standard errors clustered at the level of the 280 independent units of observation in 1816. *, ** and *** denote significance at the 0.10, 0.05 and 0.01 levels respectively. The terminal model included province dummy variables for Brandenburg, Pomerania, Silesia and the Rhineland, together with province-year of annexation interaction terms for Brandenburg, Pomerania, Silesia and Westphalia.The figures reported for all provinces except the Rhineland are the marginal effects of the province evaluated at the corresponding province mean values of year of annexation. The figure reported for the Rhineland is the coefficient of the Rhineland dummy variable because the terminal model contains no interaction between year of annexation and the Rhineland dummy.

Table 3 also reports, for the IV estimates, the *p* value of the *C* statistic which tests the null hypothesis that the education variable being treated as endogenous in IVestimation is actually an exogenous regressor.[23] The *C* statistic amounts to a test of whether there is a statistically significant difference between the OLS and IV estimates of the coefficient of the education variable, and thus whether IV estimation is required. The *p* values show that this null hypothesis cannot be rejected at conventional levels for all four IV regressions. The *C* test does not necessarily have good power, but in Table 3 the respective IV and OLS estimates are similar, so it can be concluded that IV estimation is not required.

In contrast to the positive estimates of the effect of education on industrialisation reported by BHW in their Table 3, both the IV and OLS estimates of the effect of education on overall industrialisation in Table 3 are negative and statistically significant (the *p* value for years of schooling in equation (3.5) is 0.053). The estimates in equations (3.1) and (3.5) are also economically significant, corresponding to elasticities of -0.60 and -0.52 respectively. The IV estimate in equation (3.1) is similar to the IV estimate of the effect of education in the regression with which the model selection procedure began. Thus there is no reason to worry that the removal of variables in the general-to-specific procedure might have excluded some that were sufficiently correlated with education in 1816 to affect its validity as an instrument.

The negative effect of education on overall industrialisation is driven mainly by the negative effect of education on metal industrialisation: the estimates in equations (3.3) and (3.7) correspond to elasticities of -1.71 and -1.48 respectively.

[23] Hayashi (2000), pp. 218-21; Baum et al. (2003).

The point estimate of the effect of education in the textile sector is not statistically significant, but it is negative and fairly substantial, corresponding to elasticities of -0.71 (equation (3.4)) and -0.81 (equation (3.8)). Only for the non-metal non-textile sector is the estimated effect of education on industrialisation both economically and statistically insignificant.

The possibility of greater education actually lowering economic growth because of a perverse institutional environment in which education is used for socially harmful activities has been discussed in the literature (Pritchett 2001). However, it is not easy to see why such a negative effect of education should have been so much more pronounced in the metal sector than in other sectors of nineteenth-century Prussian industry. To check the robustness of this finding, I identified 11 observations which had a substantial influence on the estimated coefficient of education in equation (3.1) and re-estimated the regression model excluding these observations.[24] The results of this re-estimation are summarised in Table 4. Dropping these 11 observations dramatically changes the estimated effect of education on industrialisation in the metal sector: neither the IV nor the OLS estimate is statistically significant, and these point estimates correspond to elasticities that are only about one-quarter of the size of those obtained using the full sample. However, the estimated effect of education on overall industrialisation remains negative and statistically significant, though it corresponds to a smaller elasticity than that obtained from the full sample. When the influential observations are dropped, the negative effect of education on industrialisation is primarily driven by the negative effect in textile industrialisation.

[24] Following the approach of Belsley et al. (1980), an observation was identified as influential if the absolute value of the difference between the estimated regression coefficient for education with all observations included and with one observation excluded, scaled by its standard error in the latter case, was greater than 2/sqrt(334).

Table 4: Estimates of the effect of education on industrialisation in 1849 excluding 11 influential observations.

	Dependent variable: Share of factory workers in total population 1849			
	IV estimates			
	All factories	All except metals and textiles	Metal factories	Textile factories
Coefficient of years of schooling 1849	-0.115**	-0.007	-0.037	-0.071*
	(0.054)	(0.038)	(0.045)	(0.041)
Elasticity	-0.361	-0.041	-0.414	-1.340
R^2	0.475	0.314	0.218	0.243
First-stage *F* statistic	2814.50	2814.50	2814.50	2814.50
C test *p* value	0.524	0.163	0.904	0.713
	OLS estimates			
Coefficient of years of schooling 1849	-0.108**	0.004	-0.038	-0.074*
	(0.051)	(0.035)	(0.043)	(0.039)
Elasticity	-0.337	0.023	-0.425	-1.391
R^2	0.475	0.314	0.218	0.243

Notes: Number of observations in all cases is 323. Figures in parentheses are standard errors clustered at the level of the 280 independent units of observation in 1816. * and ** denote significance at the 0.10 and 0.05 levels respectively.

The dramatic change in the estimated effect of education on industrialisation in the metal sector when 11 observations are dropped from the sample casts some doubt on the robustness of the estimates in Table 3. However, the only reason for dropping these observations is that they are identified as influential by a mechanical procedure, and it can be argued that this is not a compelling basis for so doing: the sample is what it is, and it should not be altered in the absence of clear evidence that particular observations are outliers because of mismeasurement or other anomalies. Thus it is unclear whether the negative effect of education on Prussian industrialisation in the first phase is driven by the effects of education in the metal or in the textile sector. However, it is clear that the effect of education on overall industrialisation was negative.

A possible explanation of the negative causal effect of education on industrialisation is that greater education reduced the supply of child labour to factories, thus increasing the cost of labour and lowering the profitability of industrial activity. Some support for this interpretation comes from the debates preceding the enactment of the Prussian child labour law in 1839: many opponents of this new legislation were concerned that removing children from their jobs in order to send them to school would be damaging to industry (Anderson 2013). The negative effects in Table 3 are thus consistent with contemporary evidence on the Prussian economy.

The general-to-specific selection procedure yields a terminal model which includes several regressors that did not appear in BHW's preferred specification: distance to Berlin, distance to London, share of Protestants, a number of province dummies, and a number of interactions between province dummies and year of annexation. The estimated effect of the share of Protestants in equation (3.1) corresponds to an elasticity of 0.42. The estimated effects of the two distance variables are more substantial: they correspond to elasticities of 1.12 (distance to Berlin) and -3.83 (distance to London). The elasticities of these three variables implied by the estimates in equation (3.5) are almost identical. Distance from London had a extremely large negative effect on Prussian industrialisation in all three sectors in 1849, suggesting that, in the first half of the nineteenth century, distance from the industrial leader (Britain) played a very important role in explaining the variation in industrialisation between different parts of Prussia. The estimated effect of provincial location on county industrialisation did not differ between Posen, Saxony and the province of Prussia, while the effect of being located in Brandenburg was only a little different from that in these three provinces. However, the estimated effects of being located in Pomerania, Silesia, Westphalia, and the Rhineland as compared to being in

Posen, Saxony or the province of Prussia are substantial, ranging from about 40 to about 260 per cent of the sample mean value of industrialisation in 1849. It is striking that in all equations in Table 3 the estimated effect of the Rhineland dummy is negative, in contrast to what might have been expected given the institutional advantages that the Rhineland is supposed to have had. The distance to London was smaller on average for counties in the Rhineland than for any other Prussian province: once this is taken into account, the effect on industrialisation in 1849 of being located in the Rhineland appears to be negative.[25]

The fact that a number of province dummies and the share of Protestants appear as regressors in the terminal model of first-phase Prussian industrialisation selected by the general-to-specific procedure suggests that inference about the causal effect of education on industrialisation based on models that do not include these variables is likely to be misleading. As Table 2 shows, these variables are correlated with education in 1816. Hence if those variables are omitted from regression models of industrialisation, education in 1816 is an invalid instrument. BHW's results are therefore misleading, and the causal effect of education on first-phase Prussian industrialisation is negative, not positive. Do the same conclusions apply to the second phase of Prussian industrialisation?

5.2 The second phase of industrialisation

Table 5 shows IV and OLS estimates of the terminal regression model for Prussian industrialisation in 1882 obtained using the general-to-specific procedure and with the 1849 share of factory workers in the relevant sector in total population

[25] If distance to London is dropped as a regressor from equation (3.1), the estimated coefficient of the Rhineland dummy becomes positive and is significant at the 0.001 level. This effect is equivalent to 71 per cent of sample mean industrialisation in 1849.

included as a regressor.[26] By controlling for the level of industrialisation in 1849, these estimates show the effect of education on industrialisation in Prussia specifically during the period 1849-82. In these regressions, education is measured by the literacy rate rather than by years of schooling.

The first-stage *F* statistics for the IV estimates are all about 20 and the weak-instrument-robust confidence intervals are somewhat different from the standard one based on the asymptotic distribution of the IV estimator. Table 5 therefore reports both forms of 95 per cent confidence interval for the estimate of the coefficient of literacy.

The *p* values of the *C* statistic reported in Table 5 show that the null hypothesis of no difference between the IV and OLS estimates of the effect of education is rejected at the 0.054 level for overall industrialisation. The IV and OLS point estimates are very different, corresponding to elasticities of -0.37 and 0.63 respectively, and there is little overlap in the confidence intervals, with the weak-instrument robust confidence interval for the IV estimate in particular being markedly different from the OLS one. Thus there is clear evidence that IV estimation is required to identify the causal effect of education on overall Prussian industrialisation in the period 1849-82. Equation (5.1) shows that this effect is negative but not statistically significantly different from zero. The estimate of the effect of education in equation (5.1) is similar to that in the general regression with which the model selection procedure began, which confirms that the removal of variables did not exclude any that were correlated with education in 1816 strongly enough to affect its validity as an instrument. The positive association between education and overall industrialisation shown by the OLS estimate in equation (5.5) is not causal, but rather reflects the

[26] As noted in Section 4, the number of steam engines used in mining in 1849, and landownership inequality in 1849 were also included in the general model for the second phase.

Table 5: Estimates of the effect of education on Prussian industrialisation in 1849-82 using terminal model from general-to-specific procedure

	IV estimates				OLS estimates			
	Dependent variable: Share of manufacturing workers in total population 1882				Dependent variable: Share of manufacturing workers in total population 1882			
Regressors	All factories	All except metals and textiles	Metal factories	Textile factories	All factories	All except metals and textiles	Metal factories	Textile factories
	(5.1)	(5.2)	(5.3)	(5.4)	(5.5)	(5.6)	(5.7)	(5.8)
Literacy rate 1871	-0.051	0.041	-0.064	-0.026	0.087***	0.037***	0.034***	0.022*
	(0.083)	(0.026)	(0.080)	(0.049)	(0.019)	(0.008)	(0.013)	(0.012)
Share of factory workers in sector in population 1849	0.654***	0.264**	0.938***	1.506***	0.672***	0.265**	0.962***	1.522***
	(0.117)	(0.134)	(0.208)	(0.404)	(0.114)	(0.135)	(0.215)	(0.409)
Distance to London	-0.131***	-0.036***	-0.031*	-0.050***	-0.102***	-0.036***	-0.011	-0.040***
	(0.027)	(0.008)	(0.019)	(0.019)	(0.019)	(0.007)	(0.016)	(0.014)
Share of Protestants in total population 1816	0.026**	0.002	0.026**	0.001	0.012	0.002	0.016***	-0.004
	(0.012)	(0.004)	(0.011)	(0.007)	(0.007)	(0.003)	(0.005)	(0.005)
County area	-0.012**	-0.004	-0.001	-0.009***	-0.007	-0.004*	0.002	-0.007**
	(0.006)	(0.002)	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)
Landownership inequality 1849	-0.278**	-0.021	-0.158*	-0.105*	-0.184**	-0.024	-0.091**	-0.073
	(0.110)	(0.037)	(0.080)	(0.060)	(0.075)	(0.031)	(0.043)	(0.047)
Steam engines in mining per capita 1849	0.126***	-0.002	0.156***	-0.041***	0.141***	-0.003	0.166***	-0.036***
	(0.021)	(0.006)	(0.032)	(0.015)	(0.020)	(0.005)	(0.031)	(0.014)
Share of population living in cities 1816	0.053***	0.029***	0.010	0.014	0.044***	0.029***	0.003	0.011
	(0.012)	(0.004)	(0.008)	(0.009)	(0.010)	(0.003)	(0.006)	(0.008)
Looms per capita 1819	0.609**	0.006	0.012	0.526**	0.640**	0.006	0.034	0.536**
	(0.251)	(0.032)	(0.063)	(0.247)	(0.251)	(0.032)	(0.055)	(0.247)
Sheep per capita 1816	-0.019***	-0.006***	-0.000	-0.010***	-0.021***	-0.006***	-0.002	-0.011***
	(0.005)	(0.002)	(0.003)	(0.003)	(0.005)	(0.002)	(0.003)	(0.003)

Public buildings per capita 1821	-2.501***	-0.525**	-0.905	-0.820*	-2.932***	-0.512**	-1.219**	-0.969**
	(0.683)	(0.235)	(0.632)	(0.436)	(0.673)	(0.216)	(0.545)	(0.439)
Posen	-0.028***	-0.006**	-0.005	-0.014**	-0.019***	-0.007***	0.002	-0.011***
	(0.009)	(0.003)	(0.006)	(0.006)	(0.006)	(0.002)	(0.004)	(0.004)
Brandenburg	0.008	-0.003	0.010	0.002	-0.007	-0.003	-0.001	-0.003
	(0.015)	(0.005)	(0.012)	(0.009)	(0.010)	(0.004)	(0.006)	(0.007)
Pomerania	-0.008	-0.009**	0.002	0.001	-0.020**	-0.008**	-0.007	-0.003
	(0.012)	(0.004)	(0.010)	(0.007)	(0.008)	(0.004)	(0.006)	(0.005)
Saxony	-0.011	-0.004	0.016	-0.017*	-0.022*	-0.004	0.008	-0.021**
	(0.017)	(0.006)	(0.014)	(0.011)	(0.013)	(0.005)	(0.011)	(0.009)
Silesia	0.026*	0.002	0.019	0.006	0.005	0.003	0.004	-0.001
	(0.015)	(0.005)	(0.014)	(0.009)	(0.007)	(0.003)	(0.005)	(0.005)
Westphalia	-0.046**	-0.021***	0.019	-0.034**	-0.055***	-0.020***	0.008	-0.039***
	(0.023)	(0.008)	(0.018)	(0.016)	(0.017)	(0.007)	(0.014)	(0.014)
Rhineland	-0.042*	-0.022***	0.018	-0.028*	-0.053***	-0.022***	0.010	-0.031**
	(0.024)	(0.008)	(0.020)	(0.016)	(0.020)	(0.007)	(0.016)	(0.014)
Constant	0.283***	0.051**	0.085	0.125**	0.157***	0.054***	-0.004	0.081***
	(0.081)	(0.025)	(0.068)	(0.052)	(0.033)	(0.011)	(0.026)	(0.025)
Standard 95 per cent confidence interval	[-0.215, 0.112]	[-0.011, 0.093]	[-0.221, 0.093]	[-0.123, 0.071]	[0.050, 0.124]	[0.022, 0.052]	[0.009, 0.059]	[-0.009, 0.045]
Weak-instrument-robust 95 per cent confidence interval	[-0.286, 0.097]	[-0.012, 0.105]	[-0.264 0.091]	[-0.161, 0.062]	-	-	-	-
R^2	0.730	0.719	0.563	0.572	0.751	0.719	0.596	0.581
First-stage *F* statistic	20.14	20.58	20.18	20.24	-	-	-	-
C test *p* value	0.054	0.871	0.199	0.238	-	-	-	-

Notes: Number of observations for all equations is 334. Figures in parentheses are standard errors clustered at the level of the 280 independent units of observation in 1816. *, ** and *** denote significance at the 0.10, 0.05 and 0.01 levels respectively. The terminal model included dummy variables for Posen, Brandenburg, Pomerania, Saxony, Silesia, Westphalia and the Rhineland, together with province-year of annexation interaction terms for Posen, Brandenburg, Pomerania, Saxony, Silesia and the Rhineland.The figures reported for all provinces except Westphalia are the marginal effects of the province evaluated at the corresponding province mean values of year of annexation. The figure reported for Westphalia is the coefficient of the Westphalia dummy variable as the terminal model does not include an interaction between year of annexation and the Westphalia dummy.

effects of industrialisation in creating both a demand for better-educated workers and, by generating higher incomes, a demand for more education.

There is no unambiguous evidence of differences between the IV and OLS estimates of the effect of education in the three components of the overall industrial sector. In both the metal and textile sectors, the null hypothesis of no difference is rejected at only about the 0.2 level. However, the confidence intervals for the IV and OLS estimates are rather different, particularly when weak-instrument-robust ones are used. In addition, there are economically significant differences between the IV and OLS point estimates of the effect of education. The IV estimate for the metal sector corresponds to an elasticity of -1.75 in contrast to the OLS elasticity of 0.94, while in the textile sector the IV and OLS elasticities are -0.57 and 0.48 respectively. In such circumstances, there is a serious possibility of making type II errors by concluding that there are no differences between the IV and OLS estimates, and the *p* value of the *C* test is not informative about this possibility. It is only in the non-metal non-textile sector that there is clearly no need for IV estimation: the *p* value of the *C* test is 0.871 and the IV and OLS estimates are very similar. The OLS point estimate suggests that education did have a positive causal influence on industrialisation in this sector, corresponding to an elasticity of 0.673.

As in Table 3, the IV estimate of the effect of education on metal industrialisation in Table 5 is large and negative, though not statistically significant. Is this finding robust to the exclusion of influential observations? Table 6 shows the results of re-estimating the second-phase terminal model after dropping 18 observations that were identified as influential using the same procedure as described in footnote 24. Although the IV point estimate of the effect of education on industrialisation in the metal sector is still not statistically significant, it corresponds

Table 6: Estimates of the effect of education on industrialisation in 1849-82 excluding 18 influential observations.

	Dependent variable: Share of manufacturing workers in total population 1882			
	IV estimates			
	All factories	All except metals and textiles	Metal factories	Textile factories
Coefficient of literacy rate 1871	-0.009	0.005	0.005	-0.012
	(0.056)	(0.025)	(0.038)	(0.041)
Standard 95 per cent confidence interval	[-0.138, 0.100]	[-0.043, 0.054]	[-0.069, 0.080]	[-0.091, 0.068]
Weak-instrument-robust 95 per cent confidence interval	[-0.149, 0.099]	[-0.052, 0.057]	[-0.077 0.091]	[-0.116, 0.067]
Elasticity	-0.068	0.098	0.153	-0.259
R^2	0.771	0.734	0.627	0.626
First-stage F statistic	20.34	20.62	20.36	20.05
C test p value	0.128	0.292	0.538	0.394
	OLS estimates			
Coefficient of literacy rate 1871	0.065***	0.030***	0.026**	0.018
	(0.018)	(0.008)	(0.012)	(0.011)
Standard 95 per cent confidence interval	[0.029, 0.100]	[0.014, 0.045]	[0.002, 0.049]	[-0.003, 0.039]
Elasticity	0.477	0.540	0.727	0.389
R^2	0.777	0.740	0.628	0.629

Notes: Number of observations in all cases is 316. Figures in parentheses are standard errors clustered at the level of the 280 independent units of observation in 1816. ** and *** denote significance at the 0.05 and 0.01 levels respectively.

to an elasticity of 0.153 when these 18 observations are dropped as compared to one of -1.75 for the full sample. Consequently, the elasticity corresponding to the IV point estimate of the effect of education on overall industrialisation changes from -0.373 to -0.068, while remaining not statistically significantly different from zero. The exclusion of influential observations thus alters the results in Table 5 to some extent, but not enough to change the main finding.

The negative though statistically insignificant IV estimate of the effect of education on overall industrialisation in Prussia during the period 1849-82 shown in Table 5 is very different from the positive and both statistically and economically significant estimate reported by BHW in equation (2) of their Table 5. However, BHW's preferred regression specification for the second phase of Prussian industrialisation omits several variables which the general-to-specific procedure selects as relevant regressors in Table 5: the distance to London, the share of Protestants, landownership inequality, and a number of province dummies, both on their own and interacted with year of annexation. Some of these variables are correlated with the instrument for education in 1871, as Table 2 shows, and thus inference about the effect of education cannot be based on the BHW specification. The results in Tables 3 and 5 tell a consistent story: the causal effect of education on overall industrialisation in Prussia was negative in both the first and second phases, although in the second phase the negative effect was smaller and sufficiently poorly determined that it was not statistically significantly different from zero.

The other variables that influenced overall Prussian industrialisation in 1849-82 according to equation (5.1) include several that also exercised an effect in 1849 according to equations (3.1) and (3.5): distance to London, share of Protestants, share living in cities, looms, public buildings, and location in the provinces of Silesia, Westphalia, and the Rhineland. The size of their effects was substantially smaller in the second phase of industrialisation in almost all cases: thus, for example, the elasticity of the share of Protestants was 0.14 rather than 0.42 and that of the share living in cities was 0.11 rather than 0.21. The estimated effects on industrialisation of location in the Rhineland and Westphalia continued to be lower than in other provinces while the effect of location in Silesia continued to be larger. As in the first phase of industrialisation, the strong negative effect that distance to London is estimated to have had on second-phase industrialisation – its coefficient in equation (5.1) corresponds to an elasticity of -1.07 – explains why location in the Rhineland had a negative effect on industrialisation.[27] This negative effect was, however, less pronounced in the second phase, being 36 per cent of the sample mean value of industrialisation in 1882 rather than 260 per cent of sample mean industrialisation in 1849.

There are also some differences between the variables which are estimated to affect industrialisation in 1849 and 1882. Industrialisation in 1849, steam engines in mining in 1849, and landownership inequality in 1849 were not included as regressors in the 1849 analysis and therefore could not have any effect in the first phase. County area and sheep are included in the terminal model for 1849-82, but not in the terminal model for 1849. The distance to Berlin and the share of farm labourers are included in the 1849 terminal model but not in the 1882 one. In the first phase of Prussian industrialisation, location in Posen was estimated to have the same effect as location in Saxony and the province of Prussia, but in the second phase location in Posen had a markedly lower effect. These differences are not surprising: there is no reason to expect the influences on Prussian industrialisation to be identical in its first and second phases.

BHW also combine their three sets of cross-section observations for 1816, 1849 and 1882 into a panel dataset in order to estimate fixed-effect models of industrialisation. Before coming to a final conclusion about the causal effect of

[27] If distance to London is dropped as a regressor from equation (5.1), the estimated effect of the Rhineland is positive and significant at the 0.001 level. This effect is 73 per cent of sample mean industrialisation in 1882.

education on Prussian industrialisation, it is important to consider whether panel estimation can be informative about this question.

6. Panel data models of Prussian industrialisation

BHW argue that the results from their panel regression models confirm their cross-section estimates of the effect of education and show that these "cannot be driven by time-invariant omitted factors".[28] Is this claim correct?

Combining the observations for Prussian counties in 1816, 1849, and 1882 into a single panel creates some difficulties, because the definitions of the main variables – industrialisation and education – are not identical across the three periods. BHW's solutions to these problems are sensible, but these difficulties nonetheless mean that their panel analysis is based on a dataset in which the definitions of key variables are not consistent over time. Putting this concern to one side, there are other reasons to doubt the value of a panel analysis of the causal effect of education on Prussian industrialisation.

The omitted variables in BHW's cross-section regression analysis that have been shown to be correlated with their instrumental variable, and hence lead to inconsistent estimates of the effect of education, are all time-invariant ones – province dummies, the year of annexation, the share of Protestants in 1816, and landownership inequality in 1849. It might therefore be thought that BHW's fixed effect panel regressions, which allow for time-invariant unobserved influences on Prussian industrialisation, indeed show that these omitted variables cannot drive their cross-section results. However, although it is true that the omitted variables themselves do

[28] BHW (2011), 118.

not vary over time, their effects do vary over time, as the discussion at the end of Section 5 points out. BHW’s fixed-effect panel regressions do not allow for the possibility of time-varying effects of time-invariant variables, and hence fail to address the question of whether constraining time-invariant variables to have the same effects on industrialisation in different time periods might bias their panel estimates.

It is, of course, possible to allow the effects of time-invariant variables to vary over time in fixed-effect regression models, so this problem with the BHW panel estimates is not insurmountable. However, there is a much more fundamental problem with any attempt to use the BHW data for panel estimation of the causal effect of education on Prussian industrialisation; it does not contain a valid instrument for education in the panel context. The only plausible instrument for current education that is available to obtain panel IV estimates is education lagged one period, and this is what BHW use. The need for IV estimation arises because, as a consequence of reverse causation, current education is expected to be correlated with the error term in the equation explaining current industrialisation. But this implies that lagged education will be correlated with the lagged error term, and this lagged error term is a component of the time-demeaned error term that is used in fixed-effect estimation. If current education is an endogenous regressor, lagged education will inevitably be correlated with the error term in the fixed-effect regression model and hence cannot be a valid instrument for current education in such a panel model. Lagged education simply cannot be used as an instrument in order to test whether any association between education and industrialisation in panel regression models reflects a causal influence of the former on the latter. Unfortunately, BHW’s data do not contain any alternative instruments for education in panel regression models, and thus it is not possible to analyse the causal effect of education on industrialisation in such models.

This leads to an ineluctable conclusion: panel analysis of the BHW dataset cannot throw any light on whether education had a causal influence on Prussian industrialisation.

7. Conclusion

The conclusion of this paper is simple: there is no evidence that education had a positive causal effect on overall Prussian industrialisation. Rather, in the first phase of Prussian industrialisation, education had an unambiguous negative influence on overall industrialisation, while in the second phase it had an effect that was negative but poorly determined. To be sure, in the period 1849-82 there is evidence that education had a positive causal effect on industrialisation in the non-metal non-textile sector, but in terms of the influence on overall industrialisation this was outweighed by negative effects in other industrial sectors. An important question for future research is why education had a clear negative effect on Prussian industrialisation in the first phase. The conjecture that greater education lowered industrialisation by reducing the supply of child labour to factories needs more thorough investigation. If increased education did cause industrialisation to fall by reducing child labour, the overall assessment of this negative effect becomes a much more complicated matter. Another question that requires further research is whether the negative effect of education on industrialisation was more pronounced in the metal or the textile sector.

BHW reached very different conclusions about the causal effect of education on Prussian industrialisation because their preferred regression models excluded regressors that were correlated with pre-industrial education, the instrumental variable they used in an attempt to identify the causal influence of education. Thus pre-industrial education is an invalid instrument in BHW's preferred regression models and the estimates of the causal effects of education obtained from these models are inconsistent. This key point emerged from the use in this paper of a systematic procedure to select regression models of Prussian industrialisation.

By including a number of variables that were excluded from BHW's preferred specifications, the regression models that were selected by the procedure used in this paper not only increased the plausibility of the claim the pre-industrial education is a valid instrumental variable, but also yielded a number of new findings about the determinants of Prussian industrialisation. The most striking of these is the importance of the distance from London, which had a substantial negative effect in both phases of Prussian industrialisation, particularly the first one. Here, too, further research is needed to establish in what precise way proximity to the industrial leader in the nineteenth century contributed to Prussian industrialisation.

The more general conclusion to be drawn from this paper is that there is still no evidence that education of the population in general had an important causal effect on economic development before 1900. The absence of such evidence remains a major puzzle for economists and historians. Until it is possible either to find evidence of such a causal influence of education of the general population before the twentieth century, or to provide an explanation of why the causal role of such education became important only after 1900, the emphasis placed on the role of education of the general population in the growth process will remain unconvincing.

References

Acemoglu, D., Cantoni, D., Johnson, S.H. and Robinson, J.A. (2011). 'The Consequences of Radical Reform: The French Revolution', American Economic Review, 101, 3286-3307, doi: 10.1257/aer.101.7.3286

Anderson, E. (2013). 'Ideas in Action: The Politics of Prussian Child Labor Reform, 1817-1839', Theory and Society, 42, 81-119, doi: 10.1007/s11186-012-9186-4

Anderson, T.W. (1962). 'The Choice of the Degree of a Polynomial Regression as a Multiple Decision Problem', Annals of Mathematical Statistics, 33, 255-265, doi: 10.1214/aoms/1177704729

Anderson, T.W. and Rubin, H. (1949). 'Estimation of the Parameters of a Single Equation in a Complete System of Stochastic Equations', Annals of Mathematical Statistics, 20, 46-63, doi: 10.1214/aoms/1177730090

Angrist, J.D. and Pischke, J-S. (2009). Mostly Harmless Econometrics. Princeton: Princeton University Press.

Baum, C.F., Schaffer, M.E and Stillman, S. (2003). 'Instrumental Variables and GMM: Estimation and Testing', The Stata Journal, 3, 1-31.

Becker, S.O., Hornung, E. and Woessmann, L. (2011). 'Education and Catch-Up in the Industrial Revolution', American Economic Journal: Macroeconomics, 3, 92–126, doi: 10.1257/mac.3.3.92

Becker, S.O., Cinnerella, F., Hornung, E. and Woessmann, L. (2014). 'iPEHD - The ifo Prussian Economic History Database', Historical Methods: A Journal of Quantitative and Interdisciplinary History, 47, 57-66, doi: 10.1080/01615440.2013.852370

Belsley, D.A., Kuh, E. and Welsch, R.E. (1980). Regression Diagnostics: Identifying Influential Data and Sources of Collinearity, New York: John Wiley and Sons.

Campos, J., Ericsson, N.R. and Hendry, D.F. (2005). 'Introduction', in Campos, J., Ericsson, N.R. and Hendry, D.F. (eds.), General-to-Specific Modelling, Cheltenham: Edward Elgar.

Dittmar, J. and Meisenzahl, R.M. (2016). 'State Capacity and Public Goods: Institutional Change, Human Capital and Growth in Early Modern Germany', CEP Discussion Paper 1418, London School of Economics.

Finlay, K., Magnusson, L.M. and Schaffer, M.E. (2013). 'weakiv: Weak-instrument-robust tests and confidence intervals for instrumental-variable (IV) estimation of linear, probit and tobit models', https://ideas.repec.org/c/boc/bocode/s457684.html

Hardach, G. (1991). 'Aspekte der Industriellen Revolution', Geschichte und Gesellschaft, 17, 102-113.

Hayashi, F. (2000). Econometrics. Princeton: Princeton University Press.

Hendry, D.F. and Krolzig, H-M. (2005). 'The Properties of Automatic GETS Modelling', Economic Journal, 115, C32-C61, doi: 10.1111/j.0013-0133.2005.00979.x

Kaufhold, K.-H. (1986). 'Gewerbelandschaften in der frühen Neuzeit', in H. Pohl (ed.), Gewerbe- and Industrielandschaften vom Spätmittelalter bis ins 20. Jahrhundert. Stuttgart: Steiner.

Kelly, M., Mokyr, J. and O'Gráda, C. (2014). 'Precocious Albion: A New Interpretation of the British Industrial Revolution', Annual Review of Economics, 6, 363-389, doi: 10.1146/annurev-economics-080213-041042

Leamer, E.E. (1978). Specification Searches, New York: John Wiley and Sons.

Lucas, R.E., Jr. (1988). 'On the Mechanics of Economic Development', Journal of Monetary Economics, 22, 3-42, doi: 10.1016/0304-3932(88)90168-7

Mankiw, N.G., Romer, D. and Weil, D.N. (1992). 'A Contribution to the Empirics of Economic Growth', Quarterly Journal of Economics, 107, 407-437, doi: 10.2307/2118477

Nelson, R. and Phelps, E.S. (1966). 'Investment in Humans, Technological Diffusion, and Economic Growth', American Economic Review, 56, 69-75.

Ogilvie, S.C. (1996). 'Proto-Industrialization in Germany', in Ogilvie, S.C. and Cerman, M. (eds.), European Proto-Industrialization, Cambridge: Cambridge University Press.

Pritchett, L. (2001). 'Where Has All the Education Gone?', World Bank Economic Review, 15, 367-391, doi: 10.1093/wber/15.3.367

Pritchett, L. (2006). 'Does Learning to Add Up Add Up: The Returns to Schooling in Aggregate Data', in Hanushek, E.A. and Welch, F. (eds.), Handbook of the Economics of Education, Vol. 1, Amsterdam: North-Holland.

Romer, P.M. (1990). 'Endogenous Technical Change', Journal of Political Economy, 98, S71-S102.

Squicciarini, M. and Voigtländer, N. (2015). 'Human Capital and Industrialization: Evidence from the Age of Enlightenment', Quarterly Journal of Economics, 130, 1825-1883, doi: 10.1093/qje/qjv025

Tipton, F.B. (1976). Regional Variations in the Economic Development of Germany During the Nineteenth Century. Middletown: Wesleyan University Press.