A replication of 'Education and catch-up in the Industrial Revolution' (American Economic Journal: Macroeconomics, 2011-2018) Economy Compared with, ASEAN (10 Plus, Plus3 and Plus 6).

教育と産業革命におけるキャッチアップ' のレプリケーション (アメリカ経済ジャーナル。マクロ経済学、2011-2018) 経済、ASEAN との比較 (10 プラス Plus3 と 6 プラス)。

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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 BHW use to identify the causal effect of education is correlated with variables that influenced industrialisation but were omitted from their regression models. When this specification error is corrected, and a systematic model selection procedure is used, the evidence shows that education of the general population had, if anything, a negative causal impact on industrialisation in Prussia.

JEL I25 N13 N33 N63 O14
Keywords Education; industrialisation; Prussia; regional effects; invalid instrument

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1 Introduction

Many theories of economic growth emphasise the causal role of education.1 In this context, education is usually interpreted as meaning education of the general population, and this is how it will be understood in 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. No study has been able to show that the positive association between education levels and per capita income in most European economies between 1550 and 1900 reflects a causal effect of education on growth rather than rising incomes enabling people to consume more education. This lack of evidence has led recent literature to focus on the contribution of the knowledge and skills of particular groups (Kelly et al. 2014, Squicciarini and Voigtländer 2015, Dittmar and Meisenzahl 2016).

One study does 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 (BHW 2011). In BHW's view, education played an important role in enabling Prussia 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.

BHW use pre-industrial education as an instrumental variable (henceforth IV) to identify the causal effect of education. However, as this paper will show, a number of variables that influenced Prussian industrialisation and were correlated with pre-industrial education are omitted from BHW's cross-section regression specifications. Pre-industrial education is therefore not a valid IV 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, and a systematic model selection procedure is used, 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. There is no evidence that education had a positive causal influence on overall industrialisation in Prussia; if anything, the causal effect was negative.

1 See, for example, Lucas (1988), Romer (1990) and Mankiw et al. (1992).

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.[2] Prussia's industrialisation began in the mid-1830s. BHW argue that the change in Prussian institutions which made this possible can be treated as exogenous for the purposes 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. To the extent that industrialisation raised living standards, these higher incomes might have increased the demand for education. Identification of the causal effect of education on industrialisation must recognise this possible reverse causality. BHW do so by carrying out an 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 IV for later education. BHW argue that differences in education levels among Prussian counties in 1816 reflected exogenous historical idiosyncracies. Education in 1816 therefore had no direct effect on subsequent industrialisation and thus satisfies the exclusion restriction required to be a valid IV for subsequent education.

Pre-industrial education is more likely to satisfy the requirement for a valid IV if the crosssection 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. The share of the population living in cities in 1816 and the number of looms per capita in 1819 are included as indicators of preindustrial development. 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 greater orientation towards agriculture. 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 are included as measures of pre-industrial infrastructure which might have influenced subsequent industrialisation.

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 (BHW 2011: 98). They therefore estimate separate cross-section regression models of Prussian industrialisation in 1849 and in 1882. 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 – metals, textiles, 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 sectors. Education in 1849 is measured by the average number of *years of primary schooling* in the 1849 working population in each county. 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.

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

2 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.

used as regressors for the 1849 model, and the shares below 15 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 Prussian industrialisation in 1882 without taking account of earlier industrialisation. 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 second-phase industrialisation only, from the middle of the nineteenth century until 1882.

The BHW results for their preferred regression models of total industrialisation in both the first phase of Prussian industrialisation and the entire period until 1882 are shown in Table 1, together with the results I obtained by re-estimating their models. As Table 1 shows, I was able to reproduce the BHW results exactly. Panel A of Table 1 reports the OLS estimates corresponding to BHW's preferred IV estimates, Panel B the first-stage regressions for the IV estimation, and Panel C the IV estimates. BHW did not report OLS estimates of their preferred model for the first phase of industrialisation, which is why the first column in Panel A is empty. The standard errors in these regressions are clustered at the level of the 280 independent units of observation in 1816 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. Hence the data for 1816 were not based on 334 independent observations.[3]

The results in Panel B of Table 1 show 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 correlation in 1849 is so high as to raise some doubt about the validity of pre-industrial education as an IV, and this will be discussed shortly. BHW's regression models for both periods yield IV estimates (in Panel C) of the effect of education on total industrialisation that are both economically and statistically significant. The point estimates of the effect of education

3 BHW (2011: 105 n.12) and online Appendix A1 (https://assets.aeaweb.org/assets/production/articlesattachments/aej/mac/app/2010-0021_app.pdf).

Table 1: Reproduction of BHW's main results

Panel A: OLS estimates of BHW preferred model			
	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)
Years of schooling 1849	0.216***	-	-

	(0.083)	-	-
Share of population	0.053	-	-
< 15 years 1849	(0.051)	-	-
Share of population	0.078	-	-
> 60 years 1849	(0.077)	-	-
Literacy rate 1871	-	0.215***	0.215***
	-	(0.021)	(0.021)
Share of population	-	0.034	0.034
< 15 years 1882	-	(0.080)	(0.080)
Share of population	-	-1.226***	-1.226***
> 70 years 1882	-	(0.396)	(0.396)
County area	-0.005**	-0.013***	-0.013***
	(0.002)	(0.005)	(0.005)
Share of population living	0.020***	0.041***	0.041***
in cities 1816	(0.007)	(0.013)	(0.013)
Looms per capita 1819	0.152***	0.827***	0.827***
	(0.048)	(0.306)	(0.306)
Steam engines in mining	0.043***	0.157***	0.157***
per capita 1849	(0.005)	(0.015)	(0.015)
Sheep per capita 1816	-0.0005	-0.024***	-0.024***
	(0.002)	(0.004)	(0.004)
Share of farm labourers in	-0.057***	-0.064	-0.064
total population 1819	(0.017)	(0.052)	(0.052)
Public buildings per	-0.347	-1.832***	-1.832***
capita 1821	(0.286)	(0.523)	(0.523)
Paved streets 1815	0.003	0.003	0.003
	(0.002)	(0.006)	(0.006)
Tonnage of ships per	-0.032**	-0.018	-0.018
capita 1819	(0.016)	(0.033)	(0.033)
Constant	-0.013	-0.010	-0.010
	(0.021)	(0.034)	(0.034)
R^2	0.267	0.684	0.684

Panel B: First-stage regressions for IV estimation of BHW preferred model

	Dependent variable			
	Years of schooling 1849		Literacy rate 1871	
Regressors	(1.4)	(1.5)	(1.6)	(1.7)
School enrolment rate 1816	0.060***	0.060***	0.315***	0.315***
	(0.001)	(0.001)	(0.038)	(0.038)
Share of population	0.020***	0.020***	-	-
< 15 years 1849	(0.008)	(0.008)	-	-

Share of population > 60 years 1849	0.083***	0.083***	-	-
	(0.016)	(0.016)	-	-
Share of population < 15 years 1882	-	-	-0.490***	-0.490***
	-	-	(0.180)	(0.180)
Share of population > 70 years 1882	-	-	5.300***	5.300***
	-	-	(1.089)	(1.089)
County area	-0.001**	-0.001**	-0.074***	-0.074***
	(0.000)	(0.000)	(0.016)	(0.016)
Share of population living in cities 1816	0.000	0.000	0.028	0.028
	(0.001)	(0.001)	(0.031)	(0.031)
Looms per capita 1819	0.006*	0.006*	0.629***	0.629***
	(0.003)	(0.003)	(0.215)	(0.215)
Steam engines in mining per capita 1849	0.002	0.002	0.092	0.092
	(0.001)	(0.001)	(0.057)	(0.057)
Sheep per capita 1816	0.000	0.000	0.038***	0.038***
	(0.000)	(0.000)	(0.012)	(0.012)
Share of farm labourers in total population 1819	-0.007**	-0.007**	-0.291**	-0.291**
	(0.004)	(0.004)	(0.119)	(0.119)
Public buildings per capita 1821	0.130***	0.130***	3.292*	3.292*
	(0.037)	(0.037)	(1.751)	(1.751)
Paved streets 1815	0.001*	0.001*	0.066***	0.066***
	(0.000)	(0.000)	(0.011)	(0.011)
Tonnage of ships per capita 1819	0.001	0.001	-0.023	-0.023
	(0.008)	(0.008)	(0.145)	(0.145)
Constant	0.006	0.006	0.718***	0.718***
	(0.004)	(0.004)	(0.085)	(0.085)
R^2	0.970	0.970	0.643	0.643

Panel C: IV estimates of BHW preferred model				
	Dependent variable			
	Share of all factory workers in total population 1849		Share of all manufacturing workers in total population 1882	
Regressors	(1.8)	(1.9)	(1.10)	(1.11)
Years of schooling 1849	0.182**	0.182**	-	-
	(0.080)	(0.080)	-	-
Share of population < 15 years 1849	0.050	0.050	-	-
	(0.050)	(0.050)	-	-
Share of population > 60 years 1849	0.085	0.085	-	-
	(0.074)	(0.074)	-	-
Literacy rate 1871	-	-	0.136***	0.136***
	-	-	(0.036)	(0.036)
Share of population < 15 years 1882	-	-	-0.051	-0.051
	-	-	(0.095)	(0.095)
Share of population > 70 years 1882	-	-	-0.802*	-0.802*
	-	-	(0.440)	(0.440)
County area	-0.005**	-0.005**	-0.019***	-0.019***
	(0.002)	(0.002)	(0.006)	(0.006)
Share of population living in cities 1816	0.020***	0.020***	0.038***	0.038***
	(0.007)	(0.007)	(0.013)	(0.013)
Looms per capita 1819	0.154***	0.154***	0.897***	0.897***
	(0.046)	(0.046)	(0.311)	(0.311)
Steam engines in mining per capita 1849	0.043***	0.043***	0.161***	0.161***
	(0.005)	(0.005)	(0.015)	(0.015)
Sheep per capita 1816	-0.0004	-0.0004	-0.021***	-0.021***
	(0.002)	(0.002)	(0.005)	(0.005)
Share of farm labourers in total population 1819	-0.057***	-0.057***	-0.087*	-0.087*
	(0.017)	(0.017)	(0.052)	(0.052)
Public buildings per capita 1821	-0.290	-0.290	-0.876	-0.876
	(0.283)	(0.283)	(0.632)	(0.632)
Paved streets 1815	0.003	0.003	0.010	0.010
	(0.002)	(0.002)	(0.006)	(0.006)
Tonnage of ships per capita 1819	-0.032**	-0.032**	-0.017	-0.017
	(0.015)	(0.015)	(0.033)	(0.033)
Constant	-0.010	-0.010	0.057	0.057
	(0.020)	(0.020)	(0.054)	(0.054)
R_2	0.266	0.266	0.666	0.666
First-stage *F* statistic	5507.59	5507.59	69.85	69.85

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. Equations (1.4) and (1.8) are taken from Table 3 of BHW (where they are respectively equations 5 and 6) and equations (1.2), (1.6) and (1.10) are taken from Table 4 of BHW (where they are respectively equations 1, 5 and 6). Equations (1.1), (1.3), (1.5), (1.7), (1.9) and (1.11) are the results of my re-estimation of the BHW regression models. The school enrolment rate in 1816 is used as an instrument for years of schooling in 1849 in equations (1.8) and (1.9) and for the literacy rate in 1871 in equations (1.10) and (1.11).

imply elasticities of 0.53 in the first phase of industrialisation and 0.99 over the entire period.[4] Comparing the IV estimates in Panel C with the corresponding OLS estimates in Panel A shows that in both periods the former are smaller than the latter. The null hypothesis that education is an exogenous regressor can be tested using the *C* statistic.[5] The *p* values are 0.002 for the first period and 0.015 for the second, so the differences between the OLS and IV estimates are statistically significant, though the economic significance of the difference in the first period is modest. When BHW analyse the effect of education on industrialisation at the sectoral level, they find that education has a positive effect in the two non-textile sectors, but not in textiles.[6]

BHW point out that the reason for the extremely high first-stage *F* statistic for the IV estimates in the first phase of Prussian industrialisation is that their estimate of years of schooling in 1849 is constructed from the school enrolment rate in 1816, together with the school enrolment rate in 1849 and the age profile of each county's population in 1849 (BHW 2011: 104, n. 9). 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.

Although such a high correlation between the IV and the endogenous variable may raise concerns that the IV is essentially no different from the endogenous variable, an enormous firststage *F* statistic does not necessarily mean that the IV estimate of the effect of the endogenous variable is biased towards the OLS one. Even if there is an extremely high correlation between the IV and the endogenous variable, the IV estimate will still be consistent provided that the IV is uncorrelated with the error in the regression equation being estimated. However, this condition will not be satisfied in the present case unless there is no measurement error involved in the BHW procedure for constructing years of schooling in 1849. Any such measurement error will inevitably be correlated with the school enrolment rate in 1816 because this variable was used to estimate years of schooling in 1849. The problem here is not that the school enrolment rate in 1816 is intrinsically an invalid IV. It is rather that, when education in 1849 is measured by a variable that has been constructed using the school enrolment rate in 1816, the exclusion restriction required for the school enrolment rate in 1816 to be a valid IV will not be satisfied unless there is no measurement error. The extent of this problem depends on the magnitude of the error involved in BHW's procedure for calculating years of schooling in 1849.

To allay concerns about measuring education in 1849 by years of schooling in 1849, BHW note that their results for 1849 are qualitatively similar if education in 1849 is instead measured by enrolment in elementary and middle schools as a share of the population aged from six to 14 in 1849, although these results are not reported (BHW 2011: 104, n. 10). Table 2 shows the OLS and IV estimates of BHW's preferred model for total industrialisation in 1849 when education in 1849 is measured by the school enrolment rate in 1849, together with the first-stage

4 Here and throughout the paper all reported elasticities are calculated at sample mean values.

5 Hayashi (2000): 218-21); Baum et al. (2003). This test amounts to a test of whether the IV and OLS estimates are different.

6 These results are not reported in Table 1.

Table 2: BHW preferred model for 1849 with alternative education measure

	Dependent variable		
	Share of all factory workers in total population 1849 OLS	School enrolment rate 1849	Share of all factory workers in total population 1849 IV
Regressors	(2.1)	(2.2)	(2.3)
School enrolment rate 1849	0.027***	-	0.052**
	(0.007)	-	(0.023)
School enrolment rate 1816	-	0.209***	-
	-	(0.033)	-
Share of population < 15 years 1849	0.022	0.845***	0.010
	(0.047)	(0.308)	(0.046)
Share of population > 60 years 1849	0.005	3.987***	-0.107
	(0.084)	(0.604)	(0.128)
County area	-0.004**	-0.032***	-0.004**
	(0.002)	(0.012)	(0.002)
Share of population living in cities 1816	0.019***	-0.024	0.021***
	(0.007)	(0.044)	(0.008)
Looms per capita 1819	0.152***	0.264**	0.141***
	(0.046)	(0.133)	(0.047)
Steam engines in mining per capita 1849	0.041***	0.062	0.040***
	(0.005)	(0.050)	(0.006)
Sheep per capita 1816	-0.001	0.015	-0.001
	(0.002)	(0.014)	(0.002)
Share of farm labourers in total population 1819	-0.051***	-0.337**	-0.041**
	(0.017)	(0.144)	(0.018)
Public buildings per capita 1821	-0.274	5.462***	-0.550
	(0.249)	(1.471)	(0.359)
Paved streets 1815	0.003	0.018	0.002
	(0.002)	(0.011)	(0.003)
Tonnage of ships per capita 1819	-0.030**	-0.005	-0.031**
	(0.013)	(0.249)	(0.013)
Constant	-0.008	0.169	-0.018
	(0.019)	(0.149)	(0.023)
R_2	0.272	0.465	0.253
First-stage *F* statistic	-	-	41.29

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 (2.1) is estimated by OLS and equation (2.3) is estimated by IV, using the school enrolment rate in 1816 as an instrument. Equation (2.2) is the first-stage regression for the IV estimates.

regression for the IV estimates. Pre-industrial education is still a relevant IV for this measure of education in 1849, but the first-stage F statistic is much lower than in Table 1. The IV estimate of the effect of education is still positive and significant both statistically and economically: indeed, the point estimate in equation (2.3) implies an elasticity of 2.35, which is more than four times larger than the elasticity implied by the IV point estimate using years of schooling in 1849. In contrast to the corresponding estimates in Table 1, where the OLS point estimate of the effect of education is larger than the IV one, the OLS point estimate of this effect in equation (2.1) is smaller than the IV point estimate in equation (2.3), although the null hypothesis that the difference is zero cannot be rejected at conventional levels by the C test (the p value is 0.168). Thus there are some differences between the results for the effect of education on industrialisation in 1849 depending on which measure of education in 1849 is used in BHW's preferred model. Because the school enrolment rate in 1816 is likely to be an invalid IV when this measure is years of schooling in 1849, I use the school enrolment rate in 1849 as the measure of education in 1849 in the results that I report in the remainder of this paper. The main conclusions I draw do not depend on which measure is used.

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.

However, BHW's preferred regression models omit any variables that measure regional effects, which the historical literature has found were important influences on Prussian industrialisation. BHW's IV – education in 1816 – is correlated with variables that measure regional effects, and thus is likely to be an invalid instrument. 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 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.[7] 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.

There was a great deal of variation in the institutional frameworks of different parts of Prussia and this influenced economic development throughout the nineteenth century (Ogilvie 1996b: 265). 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 having very dense proto-industry in the early nineteenth century, Silesian industrialisation was hampered by the desire of feudal landlords to protect their proto-industrial feudal revenues through resistance to technological improvements in linen production, in which they were supported by the Prussian state. The Rhineland was the most economically advanced part of Prussia in 1816, because the early decline in landlord power combined with extensive political fragmentation to enable protoindustries easily to cross territorial boundaries in order to locate where political and institutional conditions were least oppressive. The institutional framework here remained favourable to economic development throughout the nineteenth century (Ogilvie 1996a: 124–125). Acemoglu et al. (2011) construct an index of German 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, and the value for Saxony modestly higher, than for the provinces of Prussia, Brandenburg, Pomerania, Silesia and Westphalia in both 1850 and 1900 (Acemoglu et al. 2011, Table 1, 3292).

These features of Prussian industrialisation suggest that any regression model 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 (Becker et al. 2014). 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.

To some extent, BHW acknowledge these institutional and legal differences across Prussia when testing the robustness of their preferred regression specifications. One such test involves adding a dummy variable indicating whether a county was in the Rhineland or Westphalia as a regressor. 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 other provinces of Prussia. A second BHW robustness test involves the addition 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 (BHW 2011: 114). However, the use of year of annexation on its own does not distinguish between counties that are likely to have had very

7 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.

different institutional and legal frameworks. For example, 54 of the 112 counties that became part of Prussia in 1816 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 also interacted with province dummy variables as measures of institutional variation by region in the regression analysis of Prussian industrialisation.

The omission of any variables measuring regional effects from BHW's regression models of industrialisation might not result in inconsistent estimates of the effect of education if BHW's IV – education in 1816 – could plausibly be argued to be uncorrelated with the omitted regional effects. Table 3 shows that this is not possible. It reports OLS estimates of regressions in which the dependent variable in all three equations is BHW's IV: the school enrolment rate in 1816. The regressors are the non-education variables in BHW's preferred regression models of industrialisation in 1849 (for equation (3.1) in Table 3), 1882 (equation (3.2)), and 1849–1882 (equation (3.3)), 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 both 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 3 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 compared to that province.

It is clear from Table 3 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. 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 (3.1), (3.2) and (3.3). 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 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 IV for education in regression models that omit them, as BHW's preferred models do.

Table 3: The relationship between county school enrolment in 1816 and other characteristics, Prussia, 1849, 1882, and 1849–82

Regressors	Dependent variable: School enrolment rate 1816 (3.1)	(3.2)	(3.3)
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)

	Dependent variable: School enrolment rate 1816		
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.

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, as Table 4 shows.[8] The IV estimates in Table 4, and throughout the paper, are just-identified so there are no tests of over-identification restrictions. The results for year of annexation and provinces reported in Table 4 are marginal effects, evaluated at the same values as in Table 3. The omitted province is Prussia. For all four regressions in Table 4, the null hypothesis that the coefficients of year of annexation, province dummies, and interactions between them were all zero was strongly rejected, as the *p* values in the table show. The first-stage *F* statistics in equations (4.1) and (4.3) are lower than those in BHW's preferred models (equations (2.3) and (1.11) above). Table 4 therefore reports, for the

8 Note that in equations (4.1) and (4.2) education is measured by the school enrolment rate in 1849 rather than by years of schooling in 1849, the measure used by BHW.

Table 4: Adding year of annexation and province dummies to BHW's preferred models

	Dependent variable			
	Share of all factory workers in total population 1849		Share of all manufacturing workers in total population 1882	
	IV	OLS	IV	OLS
Regressors	(4.1)	(4.2)	(4.3)	(4.4)
School enrolment rate 1849	-0.012	0.007	-	-
	(0.024)	(0.007)	-	-
Literacy rate 1871	-	-	0.046	0.163***
	-	-	(0.085)	(0.026)
Year in which annexed by Prussia	0.012	0.008	0.036	0.043
	(0.015)	(0.016)	(0.035)	(0.037)
Posen	-0.001	-0.001	-0.012**	-0.011*
	(0.002)	(0.001)	(0.006)	(0.006)
Brandenburg	0.009***	0.007***	0.063***	0.042***
	(0.003)	(0.002)	(0.018)	(0.009)
Pomerania	-0.000	-0.001	0.027	0.008
	(0.002)	(0.002)	(0.017)	(0.007)
Saxony	0.023***	0.019***	0.064***	0.035***
	(0.007)	(0.004)	(0.024)	(0.011)
Silesia	0.011**	0.008***	0.040**	0.018**
	(0.005)	(0.002)	(0.019)	(0.009)
Westphalia	0.016**	0.013**	0.043	0.010
	(0.008)	(0.006)	(0.027)	(0.012)
Rhineland	0.014**	0.010***	0.051*	0.020
	(0.006)	(0.003)	(0.027)	(0.014)
p value for test of significance of annexation, provinces, and interactions	0.000	0.000	0.000	0.000
Standard 95 per cent confidence interval	[-0.060, 0.035]	[-0.007, 0.018]	[-0.121, 0.214]	[0.112, 0.214]
Weak-instrument-robust 95 per cent confidence interval	[-0.075, 0.035]	-	[-0.187, 0.192]	-
R^2	0.349	0.356	0.710	0.726
First-stage *F* statistic	15.94	-	26.25	-
Endogeneity test *p* value	0.396	-	0.107	-

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. All equations include the following regressors, the coefficients of which are not reported: county area, share of population living in cities 1816, looms per capita 1819, steam engines in mining per capita 1849, sheep per capita 1816, share of farm labourers in population 1819, public buildings per capita 1821, paved streets 1815, tonnage of ships per capita 1819, and the share of young and old in the population in either 1849 or 1882 as appropriate.

coefficients of the education variables, weak-instrument-robust 95 per cent confidence intervals based on the Anderson–Rubin (1949) test statistic as well as the standard ones based on the asymptotic distribution of the IV estimator.[9] The former are somewhat larger than the latter, but not sufficiently so as to alter any inferences about the causal effects of education.

In the first phase of Prussian industrialisation, the IV point estimate of the effect of education is negative and corresponds to an elasticity of –0.57, but it is not statistically significantly different from zero. The OLS point estimate is positive and corresponds to an elasticity of 0.30, but it is also not statistically significantly different from zero. The *C* test of endogeneity does not reject the null hypothesis that the school enrolment rate in 1849 can be treated as an exogenous variable. Thus there is no evidence of any need for IV estimation and no evidence that education had a positive effect on total industrialisation in 1849 once the year of annexation, province dummies, and their interactions are added to BHW's preferred model.[10] In other words, once these variables are included, there is no evidence of reverse causation between education and industrialisation in the first phase of Prussian industrialisation.

For the entire period of Prussian industrialisation, the IV point estimate is positive but much smaller than the estimate reported by BHW. Furthermore, it is not statistically significantly different from zero. The positive OLS point estimate is, however, only about 25 per cent smaller than the estimate reported by BHW, and is statistically significant. Adding the year of annexation, province dummies, and their interactions to BHW's preferred model for the entire period does not make IV estimation unnecessary. The difference between the IV point estimate, which corresponds to an elasticity of 0.33, and the OLS one, which corresponds to an elasticity of 1.15, is economically significant. The *C* statistic suggests that this difference is not statistically significant at conventional levels, but care is required in the interpretation of this test. The wide confidence intervals for the IV estimate mean that there is a potentially large probability of making a type II error by concluding that there is no difference between the IV and OLS estimates. Although it is not conclusive, the evidence that there is reverse causation between education and industrialisation, and hence that IV estimation is required to identify the causal effect of education, is much stronger for the whole period of Prussian industrialisation than for the first phase alone. The IV point estimate of this causal effect is positive, but it is poorly determined. There is no compelling evidence that education had a positive causal influence once the year of annexation, province dummies, and their interactions are added to BHW's preferred model of the entire period of Prussian industrialisation. The positive OLS point estimate, reflecting an association rather than a causal effect, is economically and statistically significant, which is consistent with industrialisation having increased education.

Although the results in Table 4 cast considerable doubt on BHW's claims about the effect of education on industrialisation in Prussia, 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). In the next section, I address the question of how to select a satisfactory model of Prussian industrialisation.

9 Here and throughout the paper weak-instrument-robust confidence intervals were obtained using the Stata *weakiv* command of Finlay et al. (2013).

10 Similar conclusions are reached if education in 1849 is measured by years of schooling in 1849. Full details are available from the author upon request.

4 Selection of regression models of Prussian industrialisation

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 a regression model of Prussian industrialisation. 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 explanatory variables is included in the regression together with education. A specification search of some form is unavoidable. 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 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 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, if possible, a single model.

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 is no theoretical guidance as to which of many plausible explanatory variables 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.

I use the following version of the general-to-specific procedure. I begin with a 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 bad controls (Angrist and Pischke 2009: 64–66). The general model for industrialisation in 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, since the variables measured in 1849 can be taken as predetermined for the 1882 regression. 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), and a dummy variable for counties located in present-day Poland. 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. This third group does not, however, include the latitude and longitude of the counties, for the same reason that these variables were not included in the regressions reported in Table 3.

The general regression model was estimated by IV with the instrument for the education variable being the school enrolment rate 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 – 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 for 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 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 5 presents the results for the first phase of Prussian industrialisation, in which the dependent variable is a measure of industrialisation in 1849. Table 7 presents the results for the second phase, in which the dependent variable is a measure of industrialisation in 1882 and industrialisation in 1849 is included as a regressor. As will become clear, it is more informative to analyse the two phases of Prussian industrialisation separately rather than to combine them.[11] Both tables show the effects of education on the three sectoral components of Prussian industrialisation – metals, textiles and the non-metal non-textile sector – so that BHW's conclusion that the positive causal effects of education on industrialisation operated only in the two non-textile sectors can be assessed.

5.1 The first phase of industrialisation

Table 5 shows IV and OLS estimates of the terminal regression model for Prussian industrialisation in 1849 obtained using the general-to-specific procedure described above. Education is measured by the school enrolment rate in 1849, but the results are broadly similar if years of schooling in 1849 is used as the education measure.[12] The first-stage *F* statistic for the IV estimates in Table 5 is large enough to suggest that weak-instrument problems do not arise, but to confirm this Table 5 also reports 95 per cent weak-instrument-robust confidence intervals for the coefficient of education. The weak-instrument-robust confidence intervals in Table 5 are in all cases only a little wider than the standard ones.

11 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.

12 The results using years of schooling in 1849 can be found in an earlier version of this paper, available at http://www.economics-ejournal.org/economics/discussionpapers/2017-30 .

Table 5: 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 (5.1)	All except metals and textiles (5.2)	Metal factories (5.3)	Textile factories (5.4)	All factories (5.5)	All except metals and textiles (5.6)	Metal factories (5.7)	Textile factories (5.8)
School enrolment rate 1849	-0.061**	0.000	-0.050**	-0.012	-0.011*	-0.002	-0.006	-0.003
	(0.030)	(0.012)	(0.025)	(0.011)	(0.007)	(0.003)	(0.004)	(0.003)
Distance to Berlin	0.048***	0.005	0.022	0.022**	0.048***	0.005	0.021*	0.021*
	(0.017)	(0.006)	(0.013)	(0.011)	(0.016)	(0.006)	(0.013)	(0.011)
Distance to London	-0.083***	-0.022***	-0.034**	-0.027***	-0.068***	-0.023***	-0.020**	-0.024**
	(0.018)	(0.007)	(0.015)	(0.010)	(0.013)	(0.006)	(0.010)	(0.010)
Share of Protestants in total population 1816	0.013***	0.007***	0.007**	0.000	0.010***	0.007***	0.003	-0.001
	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)
Looms per capita 1819	0.100**	0.006	0.029	0.065*	0.122***	0.005	0.048	0.069*
	(0.046)	(0.025)	(0.049)	(0.036)	(0.042)	(0.024)	(0.039)	(0.037)
Share of farm labourers in total population 1819	-0.044**	-0.015	-0.017	-0.013	-0.046**	-0.015	-0.019*	-0.013
	(0.019)	(0.013)	(0.012)	(0.009)	(0.018)	(0.013)	(0.010)	(0.009)
Public buildings per capita 1821	-0.648**	-0.375**	-0.060	-0.213*	-0.993***	-0.360**	-0.362**	-0.271***
	(0.280)	(0.161)	(0.161)	(0.122)	(0.270)	(0.148)	(0.164)	(0.092)
Brandenburg	0.001	-0.005***	0.003	0.003***	0.001	-0.005***	0.003	0.003***
	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)

Pomerania	-0.012***	-0.007***	-0.003*	-0.002*	-0.009***	-0.007***	-0.001	-0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Silesia	0.012***	0.000	0.009**	0.003	0.006***	0.000	0.004***	0.002**
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Westphalia	-0.036***	-0.011**	-0.006	-0.018***	-0.032***	-0.011**	-0.003	-0.018**
	(0.009)	(0.005)	(0.005)	(0.007)	(0.008)	(0.005)	(0.004)	(0.007)
Rhineland	-0.047***	-0.014***	-0.015	-0.019**	-0.044***	-0.014***	-0.012	-0.018*
	(0.013)	(0.005)	(0.009)	(0.009)	(0.012)	(0.005)	(0.008)	(0.009)
Constant	0.139***	0.032**	0.070**	0.037***	0.087***	0.034***	0.025***	0.028***
	(0.036)	(0.013)	(0.031)	(0.014)	(0.014)	(0.007)	(0.009)	(0.010)
Standard 95 per cent	[-0.121,	[-0.025,	[-0.010,	[-0.034,	[-0.024,	[-0.008,	[-0.014,	[-0.009,
confidence interval	-0.002]	0.026]	-0.001]	0.011]	0.002]	0.004]	0.002]	0.002]
Weak-instrument-robust 95	[-0.134,	[-0.023,	[-0.111,	[-0.038,	-	-	-	-
per cent confidence interval	-0.008]	0.024]	-0.003	0.011]	-	-	-	-
R^2	0.252	0.265	0.018	0.221	0.313	0.266	0.122	0.231
First-stage *F* statistic	29.19	29.19	29.19	29.19	-	-	-	-
Endogeneity test *p* value	0.054	0.843	0.051	0.405	-	-	-	-

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 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.

The IV estimate of the effect of education on total industrialisation in 1849 is negative and statistically significantly different from zero. It is also (marginally) statistically significantly different from the OLS estimate in equation (5.5) according to the endogeneity test. Furthermore, it is economically highly significant: the point estimate in equation (5.1) corresponds to an elasticity at sample mean values of –2.78. The elasticity corresponding to the OLS point estimate in equation (5.5) is –0.50. The IV estimate in (5.1) 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 IV.

The estimates for the three sectoral components of total industrialisation show that this large negative effect of education is driven mainly by the metal sector. In the non-metal non-textile sector, there is evidence neither of an effect of education on industrialisation in that sector nor of a difference between the IV and OLS estimates. In the textile sector, there is no evidence of a statistically significant difference between the IV and OLS estimates, and in neither case is the effect of education statistically significant, but the point estimate is negative and economically significant, corresponding to elasticities of –3.29 (equation (5.4) and –0.90 (equation 5.8). In the metal sector, however, the endogeneity test shows that the difference between the IV and OLS estimates is (marginally) statistically significant, and the IV point estimate is extremely large, corresponding to an elasticity of –7.29.

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 13 observations which had a substantial influence on the estimated coefficient of education in equation (5.1) and re-estimated the regression model excluding these observations.[13] The results of this re-estimation are summarised in Table 6.

Dropping these 13 observations dramatically changes the estimated effect of education on industrialisation in the metal sector: the IV estimate is no longer statistically significant and the IV point estimate corresponds to an elasticity that is only about one-seventh of the size of that obtained using the full sample, though it is still economically significant. Furthermore, there is now no evidence of a statistically significant difference between the IV and OLS estimates for the metal sector. However, both the IV and OLS point estimates of the effect of education on industrialisation in all three sectors are all negative, as are the IV and OLS point estimates of the effect of education on overall industrialisation. The IV point estimate of the effect on overall industrialisation corresponds to an elasticity of –1.614, but the standard p value of this estimate is only 0.062 while the weak-instrument-robust value is 0.072, so it is not very well determined. The OLS point estimate corresponds to an elasticity of –0.458 and is statistically significant at

13 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 6: Estimates of the effect of education on industrialisation in 1849 excluding 13 influential observations

	Dependent variable: Share of factory workers in total population 1849			
	IV estimates			
	All factories	All except metals and textiles	tal factories	Textile factories
Coefficient of years of schooling 1849	-0.033*	-0.007	-0.006	-0.020
	(0.018)	(0.013)	(0.014)	(0.013)
Elasticity	-1.614	-0.621	-1.057	-5.938
Standard 95 per cent confidence interval	[-0.067, 0.002]	[-0.032, 0.018]	[-0.033, 0.021]	[-0.046, 0.006]
Weak-instrument-robust 95 per cent confidence interval	[-0.072, 0.003]	[-0.032, 0.019]	[-0.033, 0.027]	[-0.052, 0.005]
R^2	0.422	0.262	0.209	0.196
First-stage *F* statistic	23.78	23.78	23.78	23.78
Endogeneity test *p* value	0.154	0.772	0.758	0.172
	OLS estimates			
Coefficient of years of schooling 1849	-0.009**	-0.003	-0.002	-0.004
	(0.005)	(0.003)	(0.003)	(0.003)
Elasticity	-0.458	-0.305	-0.368	-1.133
Standard 95 per cent confidence interval	[-0.018, -0.0002]	[-0.009, 0.002]	[-0.008, 0.004]	[-0.009, 0.002]
R^2	0.445	0.263	0.212	0.232

Notes: Number of observations in all cases is 321. 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.

conventional levels. The difference between the IV and OLS estimates is economically but not statistically significant, though the possible low power of the endogeneity test must be borne in mind. The magnitude of the negative effect of education on overall industrialisation thus remains unclear, but there is no question that the effect is negative, even allowing for possible outliers. When the influential observations are dropped, the negative effect of education on industrialisation, though present in all three sectors, is primarily driven by the negative effect in textile industrialisation.

The dramatic change in the estimated effect of education on industrialisation in the metal sector when 13 observations are dropped from the sample casts some doubt on the robustness of the estimates in Table 5. But 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 excluding observations: the sample is what it is, and it should not be altered unless there is clear evidence that particular observations are outliers because of mismeasurement or other anomalies. However, regardless of whether the negative effect of education on Prussian industrialisation in the first phase was mainly driven by the effects in the metal or in the textile sector, 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 5 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 (5.1) corresponds to an elasticity of 0.45. The estimated effects of the two distance variables are more substantial: they correspond to elasticities of 0.90 (distance to Berlin) and –4.43 (distance to London). Distance from London has 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 does not differ between Posen, Saxony and the province of Prussia, while the effect of being located in Brandenburg is only a little different from that in those 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 65 to about 265 per cent of the sample mean value of industrialisation in 1849. It is striking that in all equations in Table 5 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. This finding illustrates the advantage of using a systematic model selection procedure, which enabled the importance of the distance to London as a influence on Prussian industrialisation to become clear. 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 of location in the Rhineland on industrialisation in 1849 was negative.[14]

The estimated effect of education on total industrialisation in 1849 in Table 5 differs from that in Table 4, which emphasises the need to use a systematic model selection procedure rather than simply add regressors to BHW's specification. BHW's results are misleading both because their IV is correlated with some omitted variables and because their regression model did not result from a systematic model selection procedure. The effect of education on first-phase

14 If distance to London is dropped as a regressor from equation (5.1), the estimated coefficient of the Rhineland dummy becomes positive and is significant at the 0.05 level. This effect is equivalent to 68 per cent of sample mean industrialisation in 1849.

Prussian industrialisation was negative, not positive. Do the same conclusions apply to the second phase of Prussian industrialisation?

5.2 The second phase of industrialisation

Table 7 shows IV and OLS estimates of the terminal regression model for Prussian industrialisation in 1882 obtained using the general-to-specific procedure. The share of factory workers in the total population in 1849 is also included as a regressor.[15] 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 rather than the entire period up to 1882, and thus permit a clear comparison with the first phase of industrialisation. In these regressions, education is measured by the literacy rate rather than by the school enrolment rate.

The first-stage *F* statistics for the IV estimates are all about 20 and the weak-instrumentrobust confidence intervals are somewhat different from the standard ones. The *p* values of the endogeneity test reported in Table 7 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 (7.1) shows that this effect is negative but not statistically significantly different from zero. The estimate of the effect of education in equation (7.1) is similar to that in the general regression with which the model selection procedure began, confirming that the removal of variables did not exclude any that were correlated with education in 1816 strongly enough to affect its validity as an IV. The positive association between education and overall industrialisation shown by the OLS estimate in equation (7.5) is not causal, but rather reflects the 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 endogeneity

15 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 7: 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			
	All factories	All except metals and textiles	Metal factories	Textile factories	All factories	All except metals and textiles	Metal factories	Textile factories
Regressors	(7.1)	(7.2)	(7.3)	(7.4)	(7.5)	(7.6)	(7.7)	(7.8)
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 total 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)	(0.017)	(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	-	-	-	-
Endogeneity 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 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.

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 endogeneity 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 5, the IV point estimate of the effect of education on metal industrialisation in Table 7 is large and negative, though not statistically significant. Is this finding robust to the exclusion of influential observations? Table 8 shows the results of re-estimating the secondphase terminal model after dropping 18 observations that were identified as influential using the same procedure as described in footnote 13. The IV point estimate of the effect of education on industrialisation in the metal sector now corresponds to an elasticity of 0.153, as compared to an elasticity 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 7 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 7 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 7: 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 IV for education in 1871, as Table 3 shows, and thus inference about the effect of education cannot be based on the BHW specification. The results in Tables 5 and 7 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 (7.1) include several that also exercised an effect in 1849 according to equation (5.1): distance to London, share of Protestants, looms, public buildings, and location in the provinces of Silesia, Westphalia, and the Rhineland. The size of their effects was 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 0.45 and that of public buildings was –0.10 rather than –0.16. The estimated effects on industrialisation of location in the Rhineland and Westphalia continued to be lower than for location 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

Table 8: Estimates of the effect of education on industrialisation in 1849–82 e 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
Endogeneity 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.

had a negative effect on industrialisation.[16] 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 265 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. The share of population living in cities in 1816, county area, and sheep per capita in 1816 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 education on Prussian industrialisation, it is necessary to consider whether panel estimation can be informative about this question.

16 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.

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" (BHW 2011, 118). Is this claim correct?

The omitted variables in BHW's cross-section regression analysis that have been shown to be correlated with their IV, and hence lead to inconsistent estimates of the effect of education, are indeed ones that do not vary over time. However, 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 the panel estimates.

The effects of time-invariant variables can be allowed 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 IV 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 IV for current education in such a panel model. Lagged education simply cannot be used as an IV 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 using 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. There is evidence that, in the period 1849–82, 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 its 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 IV, pre-industrial education, was correlated with variables that were omitted from their preferred regression models. Thus pre-industrial education is an invalid

IV in these models and the estimates of the causal effects of education obtained from them are inconsistent.

By including a number of variables that were omitted from BHW's preferred specifications, the regression models selected by the procedure used in this paper not only increased the plausibility of the claim the pre-industrial education is a valid IV, 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.

Acknowledgements I thank Sara Horrell, Sheilagh Ogilvie, Bob Reed (the editor), and three anonymous referees for very helpful comments. I also thank 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 me to write this paper.

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