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Abstract

This paper investigates Becker, Hornung and Woessmann's recent claim that education had an important causal effect on Prussian industrialization and finds it unwarranted. The econometric analysis on which this claim is based suffers from severe problems, notably the omission of relevant variables which leads to serious bias in the estimated effect of education. When these problems are corrected, the conclusions of Becker, Hornung and Woessmann no longer hold. Education did not play an important role in enabling Prussia to catch up with Britain during the nineteenth century.

JEL-Code: I250, J240, N130, N330.

Keywords: education, industrialization, omitted variables, Prussian economic history.

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1. Introduction

Becker, Hornung and Woessman (BHW henceforth) argue that formal education made an important contribution to the industrialization of Prussia in the nineteenth century.[1] This claim about education and Prussian industrialization stands in sharp contrast to the role of education in Britain, where formal education is thought to have made only a minor contribution to the Industrial Revolution.[2] BHW conclude that Prussian experience is consistent with models of technological diffusion in which education contributes to growth by facilitating adoption of new technologies.[3] In BHW's view, Prussia's high standard of education was an important reason that it, as an industrial follower, caught up with Britain, the technological leader, during the nineteenth century.

This paper argues that BHW's estimates of the effect of education on Prussian industrialization greatly overstate its importance. As the paper shows, BHW's econometric analysis suffers from serious problems, the most important of which is the omission of relevant variables leading to severe bias in the estimated effect of education. When these problems are corrected, BHW's conclusions no longer hold. There is no evidence that education had any effect in the first phase of Prussian industrialization (up to about 1850), contrary to BHW. There is evidence that in the second phase (the latter half of the nineteenth century) education had a positive effect on some sectors, but not on overall industrialization. Education did not play an important role in enabling Prussia to catch up with Britain during the nineteenth century.

2. The BHW analysis

BHW's analysis of the contribution of education to Prussian industrialization is based on a detailed dataset which contains information about education and industrialization in 1816, 1849 and 1882 for all Prussian counties, together with measures

[1] BHW (2011)
[2] Mitch (2004)
[3] This view of the role of education in economic growth originates with Nelson and Phelps (1966).

of a number of other characteristics of these counties. The dataset is organised in terms of the 334 counties that existed in 1849. Although the administrative boundaries changed between 1816 and 1882, BHW were able to convert the data for the actual counties that existed at the beginning and end of the period into a consistent dataset for the counties as they existed in 1849.

BHW address a number of issues that arise in attempting to identify the causal effect of education on Prussian industrialization. The major institutional reforms that took place in Prussia after its military defeat by France in 1806 meant that by about 1820 Prussia was able in principle to benefit from the technological advances that had occurred in Britain and begin industrializing. 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 industrialized to differing extents, and the relationship between these differences and differences in the counties' educational levels provides the basic mechanism by which BHW identify the causal effect of education.

BHW recognise that education may be influenced by industrialization as well as being an influence on it. 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. Furthermore, if industrialization raised living standards, these higher incomes might have increased the demand for education. Any attempt to identify the causal effect of education on industrialization must therefore take account of reverse causality between education and industrialization. BHW do so by using pre-industrial education levels, measured by school enrolment rates in 1816, as an instrument for later education in their instrumental variables (IV henceforth) estimation of the cross-section effect of education on industrialization in 1849 and 1882. Their argument is that pre-industrial education can be used to isolate that component of subsequent education which did not depend on subsequent industrialization and so identify the causal effect of education. Pre-industrial education levels in Prussian counties were, of course, unaffected by any possible influences that industrialization had on education. Furthermore, BHW argue that differences in education levels among Prussian counties in 1816 reflected historical idiosyncracies and so had no direct effect on subsequent industrialization. Thus there are good arguments for supposing that pre-industrial education is a valid instrument for subsequent education, and its relevance can of course be tested.

BHW argue that Prussian industrialization occurred in two phases: the first from approximately 1835 to 1850, the second during the latter half of the nineteenth century.[4] BHW therefore estimate separate cross-section regression models of Prussian industrialization in 1849 and 1882. The definitions of variables used for their main analysis differ somewhat between the two periods. In 1849, industrialization in each county is measured as the share of *factory* employment in total population. BHW measure industrialization in this way for three industrial sectors – metal, textile, and all other branches – as well as total industrialization (the sum of the three sectors). In 1882, they measure industrialization as the share of *manufacturing* employment in total county population, again distinguishing between the same three sectoral components. Education in 1849 is measured as 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. For the 1882 regression, education is measured as the share in 1871 of those aged 10 or more able to read and write in the total population aged 10 or over, which is available only for this date.

The instrument for 1849 and 1882 education in the cross-section regressions is pre-industrial education. This is measured as enrolment in elementary and middle schools as a share of the relevant population (those aged from six to 14) in each county in 1816.

Pre-industrial education is more likely to satisfy the requirement for a valid instrument (namely, that it has no direct effect on subsequent industrialization) if the cross-section regression models include other measures of the pre-industrial characteristics of each county. This minimises the possibility that pre-industrial education

[4] BHW (2011), 98.

is correlated with the error term in the industrialization models because of being correlated with other pre-industrial features of counties that 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 a county's population living in cities in 1816 and the number of looms per capita in 1819 in each county 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 county population in 1819 is included as an indicator of whether a county was less likely to industrialize because of its more agricultural orientation. Various measures of pre-industrial public infrastructure which might have influenced subsequent industrialization are also included as regressors: the number of public buildings per capita in 1821, a dummy variable showing whether paved inter-regional roads existed in a county in 1815, and a measure of the capacity of river transport ships in 1819.

In addition to these measures of the pre-industrial characteristics of Prussian counties, the cross-section models include measures of the counties' 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 corresponding shares in 1882 are used for the 1882 model. Both models use the size of each county (in square kilometres) as a regressor.

Since the first phase of Prussian industrialization occurred between roughly 1835 and 1850, the cross-section regression model for 1849 provides an estimate of the effect of education on industrialization in this first phase. The second phase of Prussian industrialization took place in the second half of the nineteenth century. BHW estimate two cross-section models for 1882. One excludes industrialization in 1849 as a regressor, and hence provides an estimate of the effect of education in both phases of industrialization taken together. The other includes industrialization in 1849 and thus, by controlling for the level of industrialization at the end of the first phase, gives an estimate of the effect of education on industrialization solely in the second phase.

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 industrialization. The cross-section regression models for both periods yield estimates of this effect for total industrialization and non-textile industrialization that are both economically and statistically significant. However, BHW find no evidence that education contributed to textile industrialization, 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 industrialization in both its first and second phases.

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 industrialization.

Does BHW's strategy for identifying the causal effect of education on Prussian industrialization succeed in establishing that education played an important role? The remainder of this paper argues that it does not. The first problem with their strategy concerns their use of an alternative instrument as a test of the robustness of the estimated effect of education in the cross-section models, to which I now turn.

3. Is distance to Wittenberg a valid instrument?

In order to check the estimation results obtained using pre-industrial education as an instrument for later education, BHW also report the results of estimating their regression models using distance to Wittenberg as an instrument instead of pre-industrial education.[5] The argument for using this variable as an instrument is that distance to

[5] BHW (2011), Web Appendix F.

Wittenberg is a source of exogenous variation in education across Prussia because the Reformation originated in Wittenberg three hundred years earlier and Protestantism promoted literacy in order to enable ordinary people to read the Bible.[6] Hence education levels in Prussian counties were, BHW claim, exogenously influenced by their distance from Wittenberg because this reflected the extent to which Protestantism had influenced literacy. BHW state that the estimated effects of education on industrialization obtained using this alternative instrument confirm the estimated effects of education obtained using pre-industrial education as an instrument.[7] However, this statement is incorrect. The sizes of the estimated effects obtained using the two different instruments are very different.

Table 1 shows the IV estimates of the effect of education on Prussian industrialization obtained by BHW when they use pre-industrial education and distance to Wittenberg separately as instruments. The standard errors in this table are clustered at the level of the 280 units of observation in the data for 1816.[8] The estimated effects of education on industrialization are very much larger when distance to Wittenberg is used as the instrument than when pre-industrial education is used as the instrument. For the total industrialization measures, the estimated coefficient of education in 1849 is more than six times larger when distance to Wittenberg is the instrument than it is when pre-industrial education is the instrument, while the estimated coefficient of education in 1882 is nearly three times larger. These differences imply very substantial differences in the estimated economic effects of education. At the sample mean values, the estimated elasticity of the share of factory workers in total population in 1849 with respect to years of schooling in 1849 is 3.489 when distance to Wittenberg is used as the instrument and 0.534 when pre-industrial education is used as the instrument. The corresponding

[6] Becker and Woessmann (2009).
[7] BHW (2011), 94, 113-4.
[8] BHW (2011), 105 n.12.

Table 1: Estimated effects of education corresponding to different instruments

Education measure	Instrument	Dependent variable					
		Share of factory workers in total population 1849			Share of manufacturing workers in total population 1882		
		All	All except metal and textile	Metal	All	All except metal and textile	Metal
Years of schooling 1849	School enrolment rate 1816	0.182**	0.124***	0.106*			
		(0.080)	(0.046)	(0.058)			
	Distance to Wittenberg	1.193***	0.790***	0.222			
		(0.377)	(0.235)	(0.165)			
Literacy rate 1871	School enrolment rate 1816				0.136***	0.069***	0.093***
					(0.036)	(0.013)	(0.025)
	Distance to Wittenberg				0.384***	0.151***	0.160***
					(0.045)	(0.016)	(0.029)

Notes: Number of observations for all equations is 334. Standard errors clustered by the 280 units of observation in 1816 in parentheses. All equations include the following regressors, the coefficients of which are not reported: share of population under 15 1849, share of population over 60 1849, county area, share of population living in cities 1816, looms per capita 1819, steam engines in mining per capita 1849, sheep per capita 1819, share of farm labourers in population 1819, public buildings per capita 1821, paved road 1815 (dummy), tonnage of ships per capita 1819. ***, ** and * denote significance at the 0.01, 0.05 and 0.10 levels respectively.

estimates of the elasticity of the share of manufacturing workers in total population in 1882 with respect to the literacy rate in 1871 are 2.786 and 0.988. The differences are less pronounced for the other measures of industrialization, but they still correspond to substantial differences in the estimated effects of education on industrialization, depending on which instrument is used. The estimated effects of education on industrialization obtained using distance to Wittenberg as the single instrument confirm those obtained using pre-industrial education only in the very limited sense that both sets of estimates show that education has a positive effect which is statistically significantly different from zero. In terms of their economic significance, however, the two instruments yield very different results, and the estimates obtained using distance to Wittenberg as the instrument cannot be said to be consistent with those obtained using pre-industrial education.

This is a serious problem for BHW's identification strategy and casts doubt on their estimates of the causal effect of education on industrialization. Large differences in the IV estimates of the coefficient of an endogenous variable depending on which variable is used as an instrument suggest that one or both instruments are invalid. When two (or more) instruments are available for a single endogenous variable, the standard way of testing the validity of the overidentifying instruments, conditional on one instrument being valid, is to use the Hansen *J* test of overidentifying restrictions. BHW do not, however, use both pre-industrial education and distance to Wittenberg together as instruments for the relevant education measures to perform *J* tests. If BHW's preferred regression models in Tables 3, 4 and 5 of their paper are estimated using both instruments together, the *J* test strongly rejects the null hypothesis in most cases. In the 12 models estimated by IV, the *J* test rejects the null hypothesis at the 0.01 level for eight of them, and at the 0.05 level for a further two. When the dependent variable is the share of textile factory workers in the population in 1849, the *p* value of the *J* statistic is 0.0687. It is only when the dependent variable is the share of metal factory workers in the population in 1849 that there is no clear rejection of the overidentifying restriction (the *p* value of the *J* test in this case is 0.4473).

Rejection of the null hypothesis by the *J* test implies that either the regression model is misspecified or at least one of distance to Wittenberg and pre-industrial education is an invalid instrument. Whichever of these two possibilities is the case, this rejection raises serious concerns about BHW's estimates of the effect of education on Prussian industrialization.

It seems likely that distance to Wittenberg is an invalid instrument for education in the models reported by BHW in Tables 3-5 of their paper, because the geographical robustness tests they report in Table 7 of their paper suggest that at least some geographical variables should be included as exogenous regressors in the regression models explaining industrialization. These geographical variables are not included in the models reported in Tables 3-5 of the BHW paper. In their absence it is possible that distance to Wittenberg will influence industrialization in these models, and hence not be a valid instrument for education. When distance to Wittenberg is added as an exogenous regressor to the 12 models discussed in the previous paragraph, and pre-industrial education alone is used as an instrument for later education, the estimated coefficient of distance to Wittenberg is negative in all cases and statistically significant at conventional levels in 10 of them. Conditional on pre-industrial education being a valid instrument, distance to Wittenberg is clearly not a valid instrument for education in regression models that include no geographical variables. However, even when several of the geographical variables investigated by BHW in Table 7 of their paper are included as exogenous regressors (specifically distance to Berlin, distance to next provincial capital, distance to London, latitude and longitude), distance to Wittenberg continues to have a statistically significant effect on industrialization in several cases. This suggests that distance to Wittenberg may have an influence on industrialization that is independent of its role as a proxy for omitted geographical variables, which would make its validity as an instrument for education even more questionable.[9]

[9] Full details of the estimation results summarised in this paragraph are available from the author on request.

However, it is also possible that pre-industrial education is not a valid instrument for later education, or that the regression models reported in Tables 3-5 of the BHW paper are misspecified. The only conclusions that can be drawn from the discussion in this section are that the estimated causal effects of education on Prussian industrialization obtained by BHW using pre-industrial education as an instrument are not confirmed by the use of distance to Wittenberg as an alternative instrument, and may reflect misspecification of the regression models used to analyse this question. At least one of BHW's instruments for education appears to be invalid. A resolution of these problems concerning instrument validity requires a investigation of the appropriate specification of the regression model used to test the effect of education on industrialization, which I now undertake.

4. Model specification issues

The preceding section showed that one reason distance to Wittenberg may not be a valid instrument for education in BHW's regression models is that it acts as a proxy for omitted geographical variables. This reflects a more general problem with BHW's empirical strategy for identifying the causal effect of education on industrialization. The preferred IV regression models they report in their Tables 3-5 always include as exogenous regressors basic demographic and geographical indicators as well as a number of indicators of pre-industrial development. However, in Tables 6 and 7 of their paper, BHW add some other variables to these basic specifications on a piecemeal basis in order to test whether the estimated effect of education on industrialization is subject to any omitted variable bias. The problem with piecemeal addition of possible explanatory variables as a method of testing the robustness of the estimated effect of education is that it provides no information about whether the education effect is robust to the inclusion of all these variables together. In Table 6 of their paper, BHW report the results of adding three variables separately as regressors to their preferred cross-section models for 1849 and 1882. In Table 7 they report the results of adding three more variables separately as regressors to the preferred cross-section models for these two years, as well as the results of adding three distance variables jointly and two locational variables jointly. Thus in total BHW add 11 variables to their preferred specifications. However they do so not in one single step but rather in eight different steps. The estimated effect of education on industrialization in 1849 is essentially unchanged in each of the eight different regressions that are estimated using this procedure, and this is also the case for the estimated effect in 1882. BHW conclude from the regressions reported in their Tables 6 and 7 that their estimates of the effect of education are robust to the addition of the these variables. However, their piecemeal procedure fails to test whether these estimates are robust to the simultaneous addition of all the variables together. The robustness of the BHW estimates to possible omitted variable bias remains an open question.

The only way to avoid these possible problems of omitted variable bias is to include, in general regression models which explain Prussian industrialization, all the variables which BHW add on a piecemeal basis to their basic specifications. The inclusion of all these variables jointly in such general models means that omitted variable bias cannot arise, at least as it applies to those variables for which BHW have data. The drawback of including all these variables in a general regression model explaining industrialization is that some of them may be irrelevant, in which case the coefficients of other variables will be estimated less precisely. But imprecision in the estimates of variables is a much less serious problem than bias in the estimates due to relevant variables having been omitted. To allow for possible omitted variable bias, I therefore estimate such general regression models of Prussian industrialization.

The regressors that BHW add on a piecemeal basis to their basic regression specifications as tests of robustness in Tables 6 and 7 of their paper are as follows. The proportion of Protestants in a county's total population is included to allow for a possible independent effect of a Protestant work ethic on industrialization, as hypothesised by Max Weber. The proportion of Jews in a county's total population is included because Jews were traditionally active as merchants rather than in industry. The year in which a county was annexed by Prussia is included to allow for the possibility that industrialization depended on how long a county had been part of the Prussian institutional and legal framework. A dummy variable for counties in western Prussia is included because of the possibility that counties located in the Rhineland and Westphalia had a different propensity to industrialize compared to those in eastern Prussia. A dummy variable for Polish-speaking counties is included to allow for a possible influence of language on industrialization. As was noted in the previous section, several geographical variables for each county are included: distance to Berlin (the Prussian capital), distance to the nearest provincial capital, distance to London (a proxy measure of distance from the source of the new industrial technologies), latitude and longitude. Finally, to allow for the possibility that the distribution of land ownership may influence both education and industrialization, the inequality of land ownership in each county in 1849 is included.

The general models of industrialization reported in what follows include as exogenous regressors all the 11 variables which BHW add on a piecemeal basis as regressors to their basic specifications as tests of robustness. They also include distance to Wittenberg as an exogenous regressor, since the results discussed in the previous section suggest that this variable may exercise an independent effect on industrialization, as well as the other regressors used by BHW in the preferred specifications in Tables 3-5 of their paper. Thus these general models have 23 regressors (24 in the models in Table 4, which analyse the progress of industrialization between 1849 and 1882), in addition to the potentially endogenous education variable. In contrast to the BHW approach, including all these regressors together means that there is no possibility of failure to detect omitted variable bias in the estimated effect of education on industrialization because of interactions between the variables which BHW add on a piecemeal basis. Since my focus is on the possible causal effect of education on Prussian industrialization, the estimated coefficients of these additional variables are not reported in Tables 2-4 below. Instead these tables report, for each general model estimated, the *p* value of the χ^2 test of the restrictions that would have to be imposed on the general model to obtain the corresponding model reported by BHW.[10]

[10] These restrictions are that the coefficients of distance to Wittenberg and the 11 variables added on a piecemeal basis by BHW are all zero.

BHW's dataset includes a variable which measures, for each Prussian county, the average school enrolment rate in 1816 in both it and the counties with which it shares a border. It is reasonable to suppose that education levels in neighbouring counties in 1816 are correlated with each county's education levels in 1849 and 1871 because of movement of people between neighbouring counties. The difference between the school enrolment rate in each county and the average school enrolment rate in that county and its neighbours in 1816 therefore provides a second possible instrument for education in 1849 and 1871. The latter variable is called '1816 enrolment rate difference' in Tables 2 and 3 below. Such a second instrument is useful for a number of reasons. It increases the efficiency of the IV estimator. It also makes it possible to compare two just-identified IV estimators for the causal effect of education on industrialization using each instrument on its own. If each just-identified estimator is consistent, then the difference between them should be small relative to sampling variance. The *J* test of overidentification can be interpreted as a test of the null hypothesis that this difference is zero.[11] Failure to reject this null hypothesis is not evidence that both instruments are valid, but it does mean that the estimated coefficient of the education variable in the regression model for industrialization does not depend on which of the two instruments is used to construct a just-identified IV estimator, and hence that both instruments tell the same story. Tables 2-4 report the *p* value of the *J* statistic for each general model estimated. None of the results reported in these tables depend on the use of these two instruments: the same general conclusions are obtained if the IV estimates are based solely on BHW's preferred instrument, the school enrolment rate in 1816.

Tables 2-4 also report, for each general model, the *p* value of the *C* statistic for testing the null hypothesis that the education variable being treated as endogenous in IV estimation can actually be taken as an exogenous regressor (Hayashi (2000, 218-221), Baum et al. (2003)). This is the appropriate statistic for such a test when the errors are clustered, as is the case here: all standard errors reported in the tables below are clustered at the level of the 280 units of observation in the data for 1816. The *C* statistic amounts to a test of whether there is a statistically significant difference between the OLS and IV

[11] Angrist and Pischke (2009), 145.

estimates of the coefficient of the education variables in the various regressions. If the null hypothesis that education can be treated as an exogenous regressor is not rejected, then there is no need to resort to the IV estimator, and the OLS estimator, which has smaller asymptotic variance, is appropriate.

5. The causal effect of education on Prussian industrialization

This section reports the IV and OLS estimates of the effect of education on Prussian industrialization that are obtained from general cross-section regression models as described in the previous section. I follow BHW in reporting linear regression results despite the dependent variables being proportions lying between 0 and 1: the estimates obtained from models that allow for this boundedness of the dependent variable are hardly different. Table 2 shows the results obtained for the first phase of Prussian industrialization, in which alternative measures of the share of factory workers in total population in 1849 are used as the dependent variable in the regressions. Equations (1) – (4) in this table are the general models which correspond to equations (6) – (9) in Table 3 of BHW: they differ from the preferred specifications of BHW by also including as regressors the 12 variables discussed in section 4 above.

The first-stage equation for the IV estimates shows that both instruments are statistically significantly correlated with years of schooling in 1849. As BHW point out, the very large first-stage F statistic is due to the fact that the measure of years of schooling in 1849 is partly based on the school enrolment rate in 1816. The p values of the J statistics provide no reason to think that the two different instruments for education in 1849 yield significantly different estimates of the coefficient of this variable. The IV estimates of the causal effect of education on Prussian industrialization in the mid-nineteenth century reported in Table 2 tell a very different story from that told by BHW. BHW say that the "results of [our] preferred specification ... show strong evidence for a positive effect of education on industrial development outside the metal and textile industries during the first phase of the Industrial Revolution until 1849, and some evidence for a positive effect in the metal industry ... [but] there is no evidence that

Table 2: Cross-section estimates of the effect of education in the first phase of industrialization

	IV					OLS			
Dependent variable	First stage Schooling 1849	Second stage Share of factory workers in total population 1849				Share of factory workers in total population 1849			
		All	All except metal and textile	Metal	Textile	All	All except metal and textile	Metal	Textile
Education variable		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of schooling 1849		-0.017 (0.092)	-0.006 (0.046)	-0.004 (0.059)	-0.006 (0.050)	-0.018 (0.089)	0.003 (0.043)	-0.004 (0.057)	-0.017 (0.045)
School enrolment rate 1816	0.062*** (0.002)								
1816 enrolment rate difference	-0.006*** (0.002)								
R^2	0.978	0.351	0.341	0.209	0.283	0.351	0.342	0.209	0.283
First-stage *F* statistic	1547.39								
J test *p* value		0.719	0.130	0.849	0.299				
C test *p* value		0.986	0.069	0.936	0.118				
Test of restrictions *p* value		0.000	0.000	0.006	0.001				

Notes: Number of observations for all equations is 334. Standard errors clustered by the 280 units of observation in 1816 in parentheses. All equations include the following regressors, the coefficients of which are not reported: share of population under 15 1849, share of population over 60 1849, county area, share of population living in cities 1816, looms per capita 1819, steam engines in mining per capita 1849, sheep per capita 1819, share of farm labourers in population 1819, public buildings per capita 1821, paved road 1815 (dummy), tonnage of ships per capita 1819, share of Protestants 1816, share of Jews 1816, year annexed by Prussia, Western Prussia (dummy), present-day Poland (dummy), distance to Berlin, distance to nearest provincial capital, distance to London, distance to Wittenberg, latitude, longitude, inequality of land ownership 1849. *** denotes significance at the 0.01 level.

education positively affected industrialization in the textile industry".[12] The general models in Table 2, by contrast, show that when omitted variable bias is appropriately addressed there is no evidence whatsoever that education had a causal influence on industrialization in this period. The IV estimates of the coefficient of years of schooling in 1849 are consistently not significantly different from zero, and in fact are negative in sign. The *p* values of the tests of the restrictions that have to be imposed on the general models in Table 2 to obtain the models reported by BHW show that these restrictions are strongly rejected. Thus the positive causal effects of education found by BHW for the first phase of Prussian industrialization reflect omitted variable bias: when the 12 variables discussed in section 4 are added to BHW's preferred models the education effects disappear. The *p* values of the *C* statistics for the general models show that the null hypothesis that years of schooling is an exogenous regressor cannot be rejected at conventional levels for three of these models. Thus in three cases there is no need to use the IV estimator. The OLS estimates of the effect of education on industrialization for these three models shown in Table 2 produce the same conclusion as the IV estimates: there is no effect of education on industrialization in this period.

Table 3 shows the results obtained when general regression models analagous to those in Table 2 are used to analyse the effect of education on industrialization in both phases of Prussian industrialization taken together. In this table alternative measures of the share of manufacturing workers in total population in 1882 are used as the dependent variable and the literacy rate in 1871 is the education variable. Equations (1) – (4) in this table are the general models which, by adding the 12 variables discussed in section 3, correspond to equations (6) – (9) in BHW's Table 4.

The first-stage equation for the IV estimates in Table 3 shows that both instruments are significantly related to literacy in 1871 and the *F* statistic for the two instruments is large enough for weak instrument problems not to be a concern. The *p* values of the *J* statistics provide no reason to think that the two different instruments for literacy in 1871 yield significantly different estimates of the coefficient of this variable.

[12] BHW (2011), 111.

Table 3: Cross-section estimates of the effect of education in both phases of industrialization taken together

	IV					OLS			
	First stage	Second stage							
Dependent variable	Literacy rate 1871	Share of manufacturing workers in total population 1882				Share of manufacturing workers in total population 1882			
		All	All except metal and textile	Metal	Textile	All	All except metal and textile	Metal	Textile
Education variable		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Literacy rate 1871		-0.030 (0.053)	0.028* (0.017)	0.019 (0.046)	-0.077* (0.041)	0.109*** (0.024)	0.033*** (0.009)	0.056*** (0.016)	0.019 (0.017)
School enrolment rate 1816	0.357*** (0.038)								
1816 enrolment rate difference	-0.300*** (0.053)								
R^2	0.868	0.707	0.732	0.540	0.482	0.726	0.732	0.545	0.513
First-stage *F* statistic	44.27								
J test *p* value		0.864	0.254	0.661	0.772				
C test *p* value		0.002	0.661	0.426	0.002				
Test of restrictions *p* value		0.000	0.000	0.001	0.003				

Notes: Number of observations for all equations is 334. Standard errors clustered by the 280 units of observation in 1816 in parentheses. All equations include the following regressors, the coefficients of which are not reported: share of population under 15 1882, share of population over 60 1882, county area, share of population living in cities 1816, looms per capita 1819, steam engines in mining per capita 1849, sheep per capita 1819, share of farm labourers in population 1819, public buildings per capita 1821, paved road 1815 (dummy), tonnage of ships per capita 1819, share of Protestants 1816, share of Jews 1816, year annexed by Prussia, Western Prussia (dummy), present-day Poland (dummy), distance to Berlin, distance to nearest provincial capital, distance to London, distance to Wittenberg, latitude, longitude, inequality of land ownership 1849. *** and * denote significance at the 0.01 and 0.10 levels respectively.

The *p* values of the tests of the restrictions that have to be imposed on the general models in Table 3 to obtain the models reported by BHW show that these restrictions are strongly rejected. This means that inference about the effect of education on industrialization in the two phases of Prussian industrialization taken together should be based on the models in Table 3 rather than the BHW specialisations of them.

The IV estimates of the coefficient of the literacy rate in 1871 reported in Table 3 show that the causal effect of education on industrialization in both phases of Prussian industrialization taken together differs depending on which measure of industrialization is used as dependent variable. When the dependent variable is the share in total population of manufacturing workers in all factories and in textile factories only, the education coefficient is actually negative and, in the case of textile factories, significantly different from zero at the 0.10 level. Furthermore, for these two equations – (1) and (4) in the table – the *C* statistic strongly rejects the null hypothesis that literacy in 1871 is an exogenous variable. However, when the dependent variable is the share in total population of manufacturing workers in all factories other than metal and textile factories and in metal factories only (equations (2) and (3)), the estimated coefficient of literacy in 1871 is positive and, in the case of all non-metal non-textile factories, significantly different from zero at the 0.10 level. For these two equations the *C* statistic does not reject the null hypothesis that the literacy rate in 1871 is an exogenous variable. Thus there is no evidence that use of the OLS estimator for these two equations is inappropriate. In other words, there is no evidence that the IV estimates of the coefficients in these equations are statistically significantly different from the OLS estimates. Equations (6) and (7) in Table 3 show that when these equations are estimated by OLS, the coefficient of literacy in 1871 is positive and significantly different from zero at the 0.01 level.

This might seem to indicate that, at least for industrialization in the Prussian metal and non-metal non-textile sectors, education had a positive causal effect. But the strength of the case for giving the OLS estimate of the coefficient of a potentially endogenous variable a causal interpretation when there is no evidence that it is statistically significantly different from the IV estimate depends on the precision of the IV estimate.

When the IV estimate is precise, the fact that it is not significantly different from the OLS one means that it is reasonable to give the OLS estimate a causal interpretation: it has been compared to a well-determined IV estimate that is not subject to reverse causation concerns and found not to be significantly different. But when the IV estimate is imprecise it is not so reasonable to give the OLS estimate a causal interpretation if there is no statistically significant difference between the two. The fact that a poorly-determined IV estimate is not significantly different from the OLS estimate is evidence that the instruments used are unable to identify the possible causal effect with any degree of precision, not that the OLS estimate can reasonably be given a causal interpretation. The coefficient of literacy in 1871 is quite precisely estimated by IV in equation (2) and hence the coefficient estimated by OLS in equation (6) – the one for the non-metal non-textile sector – can be given a causal interpretation. However, it is less clear that such an interpretation can be given to the coefficient estimated by OLS in equation (7) – the one for the metal sector – because the IV estimate of the coefficient of the 1871 literacy rate in equation (3) is extremely imprecise (the 95% confidence interval ranges from -0.071 to 0.108).

The results in Table 3 lead to a very different conclusion from that of BHW. BHW's IV estimates suggest that, in the first and second phases of Prussian industrialization taken together, education had a significant positive effect on both overall industrialization and industrialization in the metal and non-metal non-textile sectors, but no effect on industrialization in the textile sector.[13] However, the restrictions that have to be imposed on the models reported in Table 3 to obtain the BHW specifications are strongly rejected, which implies that their estimates of the effect of education suffer from omitted variable bias. There is no evidence from the results in Table 3 that literacy in 1871 had a positive effect on total manufacturing employment in 1882, contrary to BHW's conclusion. This is because there are marked differences between the effect of literacy on different components of manufacturing employment. There is a clear positive causal effect of literacy in 1871 on the share of manufacturing workers in non-metal non-textile factories in the total population in 1882 (for Prussia as a whole, this share was 4.6

[13] BHW (2011), 111.

per cent). At the sample mean values, the coefficient in equation (6) corresponds to an elasticity of 0.609. BHW also find such a positive effect, although of a rather larger size. The causal effect of 1871 literacy on the share of manufacturing workers in metal factories in 1882 (which was 3.1 per cent for Prussia as a whole) is less clear, for the reason discussed in the previous paragraph; but the coefficient in equation (7) implies a positive association that is large: at sample mean values the corresponding elasticity is 1.526. The corresponding finding of BHW is a positive coefficient, again of a rather larger size. However, the causal effect of literacy in 1871 on the share of manufacturing workers in textile factories in 1882 (3.9 per cent for Prussia as a whole) is negative and nearly statistically significantly different from zero at conventional levels.[14] The size of this effect is also large: at sample mean values the coefficient in equation (4) corresponds to an elasticity of -1.681. By contrast, BHW find that this coefficient is negative, not significantly different from zero, and much smaller in absolute value.

Table 4 shows the results obtained when a measure of industrialization in 1849 that corresponds to the dependent variable is added as an exogenous regressor to the regression models reported in Table 3. Thus, for example, when the dependent variable is the share of manufacturing workers in metal factories in total population in 1882, the share of metal factory workers in total population in 1849 is included as a regressor. These results show the effect of education in the second phase of Prussian industrialization, in other words on the progress of industrialization between 1849 and 1882. Equations (1) – (4) in Table 4 are the general models that correspond to equations (2) – (5) of BHW's Table 5. Since each of equations (1) – (4) has a different measure of industrialization in 1849, there are four different first-stage regressions; to economise on space, these first-stage results are not reported in Table 4, but the relevant F statistic is always greater than 43. In each of equations (1) – (4) the (unreported) estimated coefficient of the relevant measure of industrialization in 1849 is positive and statistically significant. The estimated coefficients of literacy in 1871 in Table 4 are almost identical to the corresponding estimates in Table 3, as are the J and C tests and the tests of restrictions. The conclusions to be drawn from Table 4 about the causal effect of

[14] The p value of the t statistic for the coefficient of literacy in 1871 in equation (4) of Table 3 is 0.059.

Table 4: Cross-section estimates of the effect of education in the second phase of industrialization

	IV				OLS			
Dependent variable	Share of manufacturing workers in total population 1882				Share of manufacturing workers in total population 1882			
	All	All except metal and textile	Metal	Textile	All	All except metal and textile	Metal	Textile
Education variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Literacy rate 1871	-0.030	0.026	0.021	-0.068*	0.101***	0.033***	0.052***	0.014
	(0.052)	(0.016)	(0.042)	(0.040)	(0.022)	(0.008)	(0.014)	(0.016)
R^2	0.735	0.740	0.626	0.569	0.753	0.740	0.629	0.592
J test *p* value	0.764	0.167	0.612	0.974				
C test *p* value	0.002	0.587	0.505	0.001				
Test of restrictions *p* value	0.000	0.000	0.011	0.059				

Notes: Number of observations for all equations is 334. Standard errors clustered by the 280 units of observation in 1816 in parentheses. All equations include the regressors in the notes to Table 3, the coefficients of which are not reported. In addition, equations (1) and (5) include the share of all factory workers in total population in 1849; equations (2) and (6) include the share of non-metal non-textile factory workers in total population in 1849; equations (3) and (7) include the share of metal factory workers in total population in 1849; and equations (4) and (8) include the share of textile factory workers in total population in 1849. The coefficients of these measures of 1849 industrialization are also not reported. *** and * denote significance at the 0.01 and 0.10 levels respectively.

education in the second phase of Prussian industrialization are so very similar to those drawn from Table 3 about its effect on both phases of industrialization taken together that a discussion of them would be otiose. The similarity between the estimated effects of education in Tables 3 and 4 is not surprising given the results in Table 2, which show that education had no effect in the first phase of Prussian industrialization.

The overall conclusion from Tables 2-4 is that the BHW estimates of the effect of education on Prussian industrialization are subject to serious omitted variable bias. The restrictions that have to be imposed on the general regression models reported in these tables to obtain the BHW specifications are always rejected. The results of estimating these general cross-section models show that education had no effect on industrialization in 1849. In 1882, there is no evidence of any effect of education on overall industrialization, but there is evidence of different effects in different sectors of the economy. There is a negative causal effect in the textile sector, a positive association which might reflect a causal effect in the metals sector, and a definite positive causal effect in the non-metal non-textile sector. Are these conclusions borne out by panel estimates?

6. Panel estimates of the effect of education on Prussian industrialization

The results reported in section 5 suggest that BHW's estimates greatly overstate the effect of education on Prussian industrialization because they suffer from omitted variable bias. However, the omitted variables that I have shown to be the source of this bias – the 11 variables added by BHW on a piecemeal basis and distance to Wittenberg – do not vary over time. Thus the estimated effect of education in BHW's fixed-effect panel regression models should not be subject to this omitted variable bias. BHW argue that the results from their panel regression models confirm their other estimates of the effect of education, showing that these "cannot be driven by time-invariant omitted factors".[15] Is this claim correct? Are panel estimates of the effect of education inconsistent with the results reported in Tables 2-4?

[15] BHW (2011), 118.

Combining the observations for Prussian counties in 1816, 1849 and 1882 into a single panel raises some problems, because the definitions of the main variables are not identical across the three periods.[16] I follow the BHW solutions to these problems in all respects except one. BHW use employment in brick-making plants, lime kilns, and glass kilns as a share of total county population as a measure of industrialization in 1816, but note that their results are robust if instead it is assumed that industrialization in 1816 was zero. For the panel results reported in this section, I use the BHW measure of total industrialization in 1816.[17] However, unlike BHW, I also use panel models to analyse industrialization in different sectors (metal, textile, non-metal non-textile), and for this I assume that, in each of these sectors, industrialization in 1816 was zero. Any errors introduced by this assumption will be trivial, since the mean value of employment in brick-making plants, lime kilns, and glass kilns as a share of total county population in 1816 was a mere 0.15 per cent.

In their Table 8, BHW report estimates of the effect of education on total Prussian industrialization obtained using both a panel of all three periods and a panel of the last two periods (1849 and 1882). They do not, however, present any estimates of this effect from a panel comprising just the first two periods (1816 and 1849). The cross-section results in Tables 2 and 4 above show that the effect of education on industrialization differs between the first and second phases of industrialization, so it is important to investigate whether this is also the case for panel estimates of the effect of education. Equations (1) – (4) in Table 5 show the coefficients of education and the two population share variables in the first phase of industrialization obtained from OLS estimation of panel models that include both county and time fixed effects. The corresponding OLS estimates for the second phase of industrialization are shown in equations (5) – (8) in this table. Equation (5) in Table 5 is the same as equation (6) in BHW's Table 8.

The panel models shown in Table 5 estimate the effect of education on Prussian industrialization by relating changes in the level of education over the first or second

[16] BHW (2011), 117-8.
[17] The results are hardly altered if instead it is assumed that total industrialization in 1816 was zero.

Table 5: Panel fixed-effect estimates of the effect of education in the two phases of industrialization

	OLS							
	First two industrialization periods (1816, 1849)				Second two industrialization periods (1849, 1882)			
	All	All except metal and textile	Metal	Textile	All	All except metal and textile	Metal	Textile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regressors								
Education	-0.005	-0.018***	0.006	0.005	0.130***	0.027***	0.044***	0.059***
	(0.014)	(0.006)	(0.009)	(0.006)	(0.022)	(0.009)	(0.015)	(0.014)
Share of young population	0.030	-0.005	0.036	-0.001	0.155*	0.025	0.099*	0.031
	(0.053)	(0.022)	(0.042)	(0.015)	(0.093)	(0.030)	(0.056)	(0.044)
Share of old population	-0.286**	-0.081	-0.140**	-0.064*	-2.592***	-0.693***	-0.912***	-0.986***
	(0.112)	(0.058)	(0.064)	(0.036)	(0.278)	(0.086)	(0.225)	(0.178)
Observations	657	657	657	657	668	668	668	668
R^2 (within)	0.492	0.578	0.212	0.151	0.862	0.877	0.521	0.678
R^2 (overall)	0.205	0.187	0.076	0.032	0.560	0.626	0.279	0.314

(Continued on next page)

Table 5 (continued)

	IV using second two industrialization periods (1849, 1882)				
	First stage	Second stage			
Dependent variable	Education	All	All except metal and textile	Metal	Textile
	(9)	(10)	(11)	(12)	(13)
Regressors					
Education		0.060	-0.010	0.031	0.039*
		(0.040)	(0.015)	(0.023)	(0.022)
Share of young population	0.207	0.166*	0.031	0.101*	0.034
	(0.363)	(0.088)	(0.030)	(0.055)	(0.042)
Share of old population	-2.707***	-2.633***	-0.715***	-0.920***	-0.998***
	(0.666)	(0.274)	(0.086)	(0.226)	(0.174)
Lagged education	1.392***				
	(0.134)				
Observations	668	668	668	668	668
R^2 (within)	0.831	0.856	0.865	0.519	0.675
R^2 (overall)	0.734	0.492	0.474	0.257	0.298
First stage *F* statistic	107.16				
C test *p* value		0.010	0.001	0.352	0.115

Notes: Standard errors clustered by the 280 units of observation in 1816 in parentheses. All equations include time period fixed effects, the coefficients of which are not reported. ***, ** and * denote significance at the 0.01, 0.05 and 0.10 levels respectively.

phases of industrialization to the corresponding changes in the level of industrialization while controlling for population share and time period. Thus the estimated effects of education from Table 5 should be compared with the cross-section results from Tables 2 and 4, since these tables respectively give the cross-section estimates of the effect of education on Prussian industrialization in its first and second phases.

The estimated effects of education using OLS in Table 5 are similar to the OLS estimates reported in Tables 2 and 4. Just as in Table 2, there is no evidence of a positive effect of education in the first phase of industrialization from equations (1) – (4) in Table 5. In three of the equations the coefficient of education is not significantly different from zero, while for industrialization in the non-metal non-textile sector this coefficient is negative and statistically significant at the 0.01 level.[18] Although equations (2) – (4) are estimated on the assumption that industrialization in 1816 in the relevant sector was zero, equation (1) is estimated making the same assumption as BHW about the value of total industrialization in 1816, so these results are not an artefact of the assumption that industrialization was zero in particular industrial sectors. Just as for the OLS results in Table 4, there is evidence of a positive association between education and industrialization in the second phase of industrialization from equations (5) – (8) in Table 5. Indeed, the coefficient of education is positive and statistically significant at the 0.01 level in all four equations, in contrast to Table 4, where this coefficient is not statistically significant (though positive) in the textile sector. The sizes of the estimated coefficients of education in equations (6) and (7) of Table 5 are broadly comparable to those in equations (6) and (7) of Table 4, but for equations (5) and (8) there are more marked differences in the sizes of the respective education coefficients. Nevertheless, it is clear that the OLS results from the panel models reported in Table 5 confirm the main feature of the cross-section OLS results in Tables 2 and 4: there is a major difference between the coefficients of education estimated by OLS in the first and second phases of industrialization. Had BHW estimated their fixed-effect models using the panel based only on 1816 and 1849, this difference would have been apparent.

[18] The size of this effect is, however, not really economically significant: at sample mean values it corresponds to an elasticity of -0.098.

The estimates in equations (1) – (8) in Table 5 do not take account of possible reverse causality between education and industrialization. In order to identify the causal effect of education on industrialization, it is necessary to turn to IV estimation of panel regression models, but the difficulty here is the lack of instruments that can be used for panel IV estimation. BHW use education lagged one period as an instrument for current education to obtain a panel IV estimate of the effect of education in the second phase of industrialization, but such an estimate for the first phase cannot be obtained using lagged education as an instrument. In fact, the cross-section estimates in Table 2 provide only very limited evidence of reverse causality between education and industrialization in the first phase of Prussian industrialization, so that it is not unreasonable to give the estimated effects of education in equations (1) – (4) of Table 5 a causal interpretation. However, the cross-section estimates in Table 4 suggest that such reverse causality is a more serious concern in the second phase of Prussian industrialization, and thus that the causal influence of education on industrialization in this period is probably not given by the estimated coefficients of education in equations (5) – (8) of Table 5.

Equation (9) in BHW's Table 8 gives their IV estimate of the effect of education on total industrialization in the second phase of Prussian industrialization. The estimated coefficient of education in this equation is 0.049, which is less than half the size of the corresponding cross-section estimate in their Table 5 (0.101). Furthermore, the estimated coefficient in equation (9) of BHW's Table 8 is not significantly different from zero even at the 0.10 level with clustered standard errors. BHW indicate in their Table 8 that this coefficient is significant at the 0.10 level, but the IV estimates they report do not in fact cluster the standard errors: the clustered standard error is actually 0.037. Hence this IV estimate does not confirm BHW's other estimates of the effect of education in the second phase of industrialization.

A problem with the specification of equation (9) in BHW's Table 8 is that it includes the dependent variable lagged one period as a regressor. With such a specification, the fixed-effect estimator yields inconsistent estimates even in the absence of endogeneity concerns, because the lagged dependent variable is correlated with the error in the fixed-effect regression equation.[19] For this reason, none of the panel regressions reported in Table 5 of this paper include lagged dependent variables.

The use of education lagged one period as an instrument for current education is also problematic. The need for IV estimation arises because current education is expected to be correlated with the error term in the equation explaining current industrialization as a consequence of reverse causation. 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 be correlated with the error term in the fixed-effect regression model and hence is an invalid instrument for current education. This problem is, unfortunately, easier to state than to solve. There are no other plausible instruments for education: even if there were no doubts about the validity of distance to Wittenberg as an instrument, it is time-invariant and so cannot be used in a fixed-effect model. For comparison with BHW, Table 5 therefore reports IV estimates of equations corresponding to equations (5) – (8) using lagged education as an instrument for current education. However, the likelihood that lagged education is not a valid instrument means that it would be unwise to place much weight on these IV results.

Equation (9) in Table 5 is the first-stage equation for the IV estimation and shows that lagged education is strongly correlated with current education. Equations (10) – (13) are the IV equivalents of the OLS estimates in equations (5) – (8). Equation (10) differs from equation (9) in BHW's Table 8 only by not including the lagged dependent variable. The estimated effect of education on total industrialization in equation (10) is, like that in BHW's equation (9), not significantly different from zero. The serious doubt that exists about the validity of lagged education as an instrument means that a detailed discussion of equations (10) – (13) in Table 5 is not worthwhile. The one point that should be made is that, even if lagged education were to be a valid instrument, the panel IV estimates in Table 5 would provide very little support for the claim that education had a causal role in the progress of Prussian industrialization between 1849 and 1882: in particular, they

[19] See, for example, Cameron and Trivedi (2005), p. 764.

would not confirm the magnitude of this effect claimed by BHW on the basis of their Table 5. In BHW's Table 5, the IV estimates of the education coefficient in the models for all manufacturing, non-metal non-textile manufacturing, and metal manufacturing are respectively 0.101, 0.060 and 0.076, and always significant at the 0.01 level. The corresponding panel IV estimates in Table 5 of this paper are very different, and certainly cannot be said to confirm the estimates reported by BHW in their Table 5. Rather, these panel estimates, being so different from BHW's cross-section results, suggest that the BHW cross-section results are indeed driven by time-invariant omitted variables.

The doubt about the validity of lagged education as an instrument for current education in these panel models means that it is best to focus on the OLS results in Table 5 when making comparisons with the cross-section models reported in Tables 2 and 4 of this paper. As has been seen, the OLS panel results confirm the cross-section ones in most respects. There is no evidence of a positive effect of education in the first phase of Prussian industrialization, which is consistent with both the IV and the OLS estimates in Table 2. The OLS panel results for the second phase of industrialization in Table 5 show a clear positive association between education and industrialization as do most of the corresponding OLS cross-section results in Table 4, so these two sets of OLS estimates are broadly consistent with each other. However, the OLS panel estimates of the effect of education on industrialization in the second phase should not be given a causal interpretation, because Table 4 shows that there are differences between the IV and OLS estimates of this effect in the cross-section models. In particular, there is evidence from Table 4 of differences between the IV and OLS estimates of the education effect for total industrialization and in the textile sector. Overall, the analysis of panel models in this section provides no reason to alter the general conclusion from the analysis of cross-section models in section 5: BHW's estimates suffer from omitted variable bias which renders untenable their conclusion that education made an important contribution to Prussian industrialization.

7. Conclusion

The conclusion from this reassessment of BHW's analysis is simple: formal education did not play an important causal role in Prussian industrialization, contrary to what BHW claim. There is no evidence at all that education had any positive effect during the first phase of Prussian industrialization. During the second phase there is evidence that education had a positive effect in some, though not all, industrial sectors, but it had a negative effect in the textile sector and no effect on overall industrialization. Even in the sectors where education does appear to have had a positive impact in the second phase, the size of this effect is smaller than BHW claim. The reason BHW greatly exaggerate the importance of education as a cause of Prussian industrialization is that their estimates of this effect suffer from serious omitted variable bias.

Since education did not play an important causal role in Prussian industrialization, the broader implications of Prussia's experience in the nineteenth century are quite different from those suggested by BHW. The absence of any evidence that education had a positive effect in the first phase of Prussian industrialization means that there is no obvious difference between Prussia and Britain in the contribution of education to the initial stages of industrialization. It also means that the Prussian evidence does not support the contention that education is an important influence on the ability of follower countries to catch up with a technological leader by developing new industrial sectors on the basis of imported technology. In the initial phase of Prussian industrialization, when Prussia was most obviously a follower, education played no such role. Education does appear to have made some limited positive contribution to the second phase of Prussian industrialization, but this was only in particular sectors, not for the industrial sector as a whole. Furthermore, it is questionable whether Prussia in 1880 was still a follower compared to other leading industrialized economies, so it is unclear that this finding of a limited effect supports the hypothesis that education is important in enabling followers to catch up with leaders, even if attention is restricted to particular industrial sectors rather than the entire economy.

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