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EHES WORKING PAPERS IN ECONOMIC HISTORY | NO. 132

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JUNE 2018

EHES Working Paper | No. 132|June 2018

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Abstract

The scholarly discourse about twentieth century forced labour has raised important questions. For example, how profitable and productive has the employment of forced labour been in different political and economic contexts? The dominant take-away from the literature is that forced labour comes with negative productivity, but positive production effects. Yet much evidence on productivity is anecdotal. To add a new quantitative take on this issue, this paper analyses the natural experiment conducted in World War I Ruhr coal mining, where, beginning with 1915, Prisoner-of-War (POW) labour was successively employed in many, but not all mines. The question to be answered is whether mines employing POW labour incurred significant labour productivity losses compared to non-POW employing mines that cannot be explained otherwise. To this end, we borrow from the treatment effects literature and implement two estimators – a baseline difference-in-difference fixed effects estimator and a doubly robust treatment effects estimator. Our study is the first to assess the productivity effects of POW employment using a full population of establishments of a particular industry. Our findings strongly support the view that the benefits from employing POW labour – i.e., the output-effect – came at the expense of a significant loss in productivity.

JEL classification: D24, J24, N44, N54

Keywords: Coal, Difference-in-differences, Doubly-robust estimation, Germany, Prisoners of War, Productivity, Treatment effects, WWI

[1] Acknowledgements: I would like to thank Mark Spoerer, Michael Buchner, the participants in the Research Seminar in Economic History at the University of Regensburg (winter term 2017/18), and the participants in the session on "Human Capital" as part of the 43rd Annual Economic and Business History Society Conference (2018) for invaluable suggestions on the first draft.

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On the economics of forced labour: Did the employment of Prisoners-of-War depress German coal mining productivity in World War I?

1. INTRODUCTION

Twentieth century civilian unfree and Prisoner-of-War (POW) labour has attracted a fair share of scholarly attention so far. Especially the World War II (WWII) forced labour regimes established by the Nazis[1] and the Allies[2] have been extensively studied. But this also goes for the Franco-regime in Spain[3], the Gulag system in the Soviet Union[4], and more recent examples.[5] Compared to WWII the increased interest in the forced labour regimes of World War I (WWI) seems to be a more recent phenomenon.[6] Special attention gained the question as to what extent the WWI forced labour regime in Germany already foreshadowed the Nazis' approach with its deep racist sentiments.[7] Besides, the question as to the productivity of POW employment has not attracted scholarship to the same degree WWII has.

Scholarly interest in twentieth-century forms of forced labour may be condensed into three main lines of work: First, into providing reliable reconstructions of the historical numbers of forced labourers as the basis for any kind of further analysis.[8] Second, into evaluating living conditions and treatment under the different regimes;[9] as for the Nazi regime in particular, the question has arisen to what extent racist ideology determined the treatment of the different groups of forced labourers and, consequently, their chances of survival.[10] Third, into

[1] E.g., Herbert (1997); Bischof and Overmans (1999); Spoerer (2001); Seidel (2003); Vourkoutiotis (2003); Buggeln (2009); Lemmes (2010), pp. 405-436; Seidel (2010); Custodis (2012, 2014, 2016).

[2] E.g., Krammer (1983); Fickle and Ellis (1990); Moore (1997, 2015); Bischof and Overmans (1999); Wylie and Crossland (2016).

[3] Mendiola Gonzalo (2013, 2014).

[4] E.g., Overmans (1999), pp. 387-412, 441-482; Gregory and Lazarev (2003); Balabkins (2012).

[5] E.g., Maul (2007); O'Connell Davidson (2015); Kara (2017).

[6] E.g., Overmans (1999), pp. 297-386; Yanikdag (1999); Wurzer (2000); Rachamimov (2002); Rawe (2005a, 2005b, 2005c); Thiel (2005); Hinz (2006); Oltmer (2006a, 2006b); Feltman (2010); Lemmes (2010), pp. 398-405.

[7] Herbert (1984); Speed (1990); Oltmer (1998); Hinz (2006); Spoerer (2006, 2007).

[8] Moore (1997, 2015); Fleischhacker and Spoerer (2002); Otto et al. (2008); Custodis (2014).

[9] Krammer (1983); MacKenzie (1994); Alexander (2006); Spoerer (2007).

[10] Spoerer (2007); Buggeln (2009).

answering the related question of how productive and profitable employing forced labour, micro- or macro-economically, was in relation to free labour.[11]

Besides occasional evidence for only a small, if any, productivity differential between free and forced labour[12], the dominant take-away from the literature is that forced labour must be associated with a productivity penalty, but at the same time with an output premium;[13] the latter follows from considering the counterfactual situation in which the loss in regular employment, due to conscription during a war for example, would neither have been compensated by extra forced labour nor by better technology or additional capital per employee. In fact, losses in aggregate productivity should not come as a surprise considering that a worker's individual productivity depends on his qualification for the particular job, his motivation, and also his physical condition.[14] Forced labourers of any sort doubtlessly were not kept as well fed as regular labourers and consequently were not as fit as them – although there is historical evidence telling of authorities trying to raise productivity by improving nourishment.[15] They as well should have lacked motivation given the circumstances; there are, of course, examples of authorities trying to find the right incentives, which, however, usually turned out to remain largely ineffective, such as physical punishment[16], monetary rewards[17], or better food (see above). Eventually they should rather by chance than by design have been skilled exactly for the job they had been allocated to. Custodis (2012), for example, has studied the productivity of Italian POWs employed in British agriculture over 1941-1947 along with their contribution to GDP. Despite him showing that they were relatively less productive (though caught up in productivity over time) their contribution to GDP was substantial. A similar finding is illustrated in Custodis (2014) with regard to the employment of Italian and German POWs in agriculture and industry in Britain and her dominions around 1945 as well as in Custodis (2016) with regard to the contribution of POW and other foreign labour to the German economy in WWII. Put pointedly, these studies make the case for POW employment

[11] Davis (1977); Fickle and Ellis (1990); Herbert (1997), pp. 299-300; Spoerer (1999); Rauh-Kühne (2002); Alexander (2006); Custodis (2012, 2014, 2016); Mendiola Gonzalo (2013), pp. 194-203.

[12] Herbert (1997), pp. 300-305.

[13] Davis (1977); Spoerer (1999), pp. 65-70; Custodis (2012, 2014, 2016).

[14] Davis (1977), p. 623; Mendiola Gonzalo (2013), p. 204.

[15] Eichholtz (1991); Alexander (2006).

[16] Seidel (2003), pp. 115-117; Borodkin and Ertz (2003), pp. 89-91.

[17] Borodkin and Ertz (2003), pp. 95-103; Vourkoutiotis (2003), pp. 127-130; Rawe (20005c), p. 225.

triggering extensive growth in aggregate production over a limited time period at the expense of intensive growth.[18]

The purpose of this paper is to assess the other side of the medal, namely to estimate the productivity penalty potentially associated with the employment of POW labour. What makes this paper new is the analysis of mass data on establishments, thus the perspective on establishment-level productivity. Specifically, our approach exploits information inherent in the *natural experiment*[19] conducted in WWI Ruhr coal mining, in a key industry at the time: Beginning with 1915, POW labour was successively employed in many, but not all mines. If POWs were used, their share in a mine's total employment amounted in some cases to not less than between a quarter and one-third, while in other cases to not more than one or two percent. We apply treatment effects methodology to a data set on Ruhr coal mines over 1913-1918 to answer the question of whether mines that employed POW labour generally saw their labour productivity grow (fall) significantly more slowly (faster) over the war compared to mines that never used POW labour at all. To the best of our knowledge, this is the first study to systematically assess whether a wide range of POW employing *establishments* incurred a comparatively large productivity penalty *from precisely that use*.

Our estimation results strongly support the view that the benefits from employing POW labour – that is, re-increasing output levels under the extreme condition of war-induced resource scarcity – came at the expense of a loss in productivity. Having been exposed to the same growth-retardant factors, non-POW employing coal mines did not incur comparable productivity losses. This finding is consistent over both estimators applied – a baseline difference-in-differences fixed effects estimator and a doubly robust estimator combining inverse-probability weighting and regression adjustment. We add new quantitative, *non-anecdotal* evidence of a negative relationship between POW employment and productivity to the extensive literature on forced labour as well as to the special literature on the German coal industry during WWI. The latter indeed emphasizes the performance-depressing effect of POW employment, but has not yet adduced systematic prove using the Ruhr coal data's peculiarities.

The analysis proceeds as follows: Section 2 introduces the mine-level data set hitherto used. Section 3 illustrates the development of Ruhr coal mining in the German war economy. Section 4 briefly describes the POW regime and highlights some peculiarities of the data. Section 5 discusses the empirics based on two treatment effects estimators. Section 6 concludes.

[18] Custodis (2012), pp. 256-264; Custodis (2014); Custodis (2016), pp. 79-90.

[19] On natural experiments in economics, see, for example, Meyer (1995).

2. DATA

We start from a data set that covers the universe of Ruhr coal mines in the period 1913 to 1920;[20] POWs were employed in the sub-period 1915 to 1918. Our main source is a publication called *Die Bergwerke und Salinen im niederrheinisch-westfälischen Bergbaubezirk* supplementing the yearbook for the Ruhr coal district, namely the *Jahrbuch für den Oberbergamtsbezirk Dortmund*.[21] We gathered most data on mine characteristics as well as on the characteristics of the firms to which mines were affiliated from this source. Table A.1 in the Appendix gives an overview of all variables and their respective sources. Most importantly, we gathered data on a mine's geographical location within the Ruhr area (one of 23 mining offices), annual coal output in tons, and total mineworkers separable into regular miners and POWs. From these data, we can compute our measure of interest, namely labour productivity per total worker per mine and its growth rate as well as the share of POWs in total employment as our continuous treatment variable. Besides these core variables we collected a mine's age and the mother firm's output and market share. We also created a couple of dummy variables indicating the mother's company form and whether it was state-owned, vertically integrated, and operated just one mine. We extended these mine- and firm-level characteristics by data on the coal district level, that is, by the average number of shifts per employee, the average capital intensity, and the average real wage. Unfortunately, this information can neither be traced on the mine-level nor consistently on the firm-level.[22] We will explain the logic behind this choice of variables in the empirical part.

The baseline data set for 1913-1920, from which we will generate aggregate figures for Ruhr coal mining in Section 3, consists of 1,540 mine-year observations and covers 219 mines. However, for the purpose of the treatment effects analysis itself, a couple of observations need be dropped. First, the full 414 observations for the years 1919 and 1920; second, 59 observations of zero output;[23] and, third, another 68 observations of exceptionally high growth rates (cut-off at +/–100 percent) of labour productivity, output, and regular employment.[24] Thus, we will be analysing a (modestly) unbalanced panel of 999 mine-year observa-

[20] See Jopp (2017) for an earlier paper on this mine-level data set.

[21] Verein für die bergbaulichen Interessen (1913-1921).

[22] Jopp (2016) has estimated the capital intensity for a sample of firms – joint stock companies mostly.

[23] Output could have been zero because the mine was at the beginning of operation or at its end; or it temporarily stopped production.

[24] In most cases, these exceptionally high growth rates occurred in the first years of operation, when a mine was virtually new in the market. We felt it necessary to drop these outliers because they heavily influence average figures. The cut-off point at +/– 100 percent was arbitrarily set.

tions for the period 1913-18 still representing 184 mines. Table 1 presents descriptive statistics on this adjusted data set regardless of the treatment status. Below we will discuss differences in characteristics across groups in more depth.

3. WAR AND COAL: THE MACRO STORY

3.1. Stylized facts on production, employment, and productivity

Coal played a crucial role in the industrialisation process of many now developed countries including Germany.[25] Just before WWI German coal production had risen to 190 and that of Britain to 292 million tons. Globally both countries were outperformed only by the US.[26] For Germany, Table 1 gives an indication on the sources of domestic (peacetime) demand for coal. The iron and steel industry with its related industries unsurprisingly was the single most important consumer followed by the transport sector at some distance. The military's share – fuel for the navy most likely – was still small, but would certainly increase during the war.

With 114 million tons in 1913, representing around 60 percent of German coal production, the Ruhr mines produced the majority share of coal in the German Empire; and its share remained fairly constant over 1914-1918.[27] Figure 1 illustrates in three panels the macro story of Ruhr coal mining over WWI. After a steady increase from the middle of the nineteenth century on towards 1913, coal producers faced a first notable crisis in the years 1914 and 1915, when production declined by more or less a quarter, falling back to the level of 1910. Production then recovered to levels between 95 and 99 million tons over the remaining war years due to the ad-hoc reorientation towards a war economy (see panel (a)). As is evident from panel (b), the direct source of the decline in production was the net loss in regular employment of, at first, six percent from 1913 to 1914 and then another 23 percent from 1914 to 1915 (or 28 percent from 1913 to 1915). Although this net loss was caused by miners being drafted for military service in the first place, it also reflects the migration of foreign mineworkers (e.g., Dutch or Austrian workers) back into their countries of origin when the war broke out, the migration of miners into other industrial sectors (due to higher wages) and the increased mortality among those still working in the mines due to increased work pressure,

[25] E.g., Kander et al. (2013), pp. 131-144. In the following, we refer to hard coal exclusively.

[26] Verein für die bergbaulichen Interessen (1938), p. 509.

[27] Calculated from Fischer (1989), p. 1, Fischer (1995), pp. 1, 35, and *Die Bergwerke und Salinen im niederrheinisch-westfälischen Bergbaubezirk* (1919), p. 8. The figures include the mines west of the Rhine River.

which resulted in more fatal accidents.[28] From 1916 on regular employment rose once more and reached a plateau of 348,000 miners in the last two years of war. The net loss in regular, qualified mineworkers was partly offset by allocating POW labour to the mines; about 17,400 POWs in 1915, 47,400 in 1916, 55,100 in 1917, and 56,400 in 1918 were employed representing 5.6, 13.1, 13.7, and, respectively, 13.9 percent of the total workforce in Ruhr coal mining.[29] Figure 2 details this employment pattern by breaking down aggregate POW employment to monthly figures; in the second half of 1918, monthly POW employment even exceeded 70,000 several times. Finally, panel (c) shows the development of average labour productivity per miner estimated from the mine-level with employment shares as weights. With the beginning of war labour productivity declined by roughly nine percent, but recovered towards 1915 – contrary to the trend in production and employment. Then, likewise contrary to that trend, labour productivity decreased straight towards 1918, by 31 tons per miner overall (or by 15 percent).

3.2. Growth-retardant factors

Notwithstanding the arms race before 1914, there is broad consensus in the literature that none of the initial war parties were economically prepared to sustain a long and resource-consumptive conflict. Hence, when political and military authorities in the German Empire and elsewhere realized in the fall of 1914 that the expected short war would last much longer, ad-hoc efforts were made to install the war economies needed, leading to immediate frictions in, for example, the supply with raw materials, intermediate goods, or manpower.[30]

The literature has pointed to several factors besides the reduction in the workforce that jointly might explain why coal production and also productivity at the Ruhr could not be maintained over the war. First, disturbances in the nationwide division of labour, as reflected in the over-allocated transport system, let to backlogs in the removal of coal from the mines to

[28] Burghardt (1988), p. 96; Rawe (2005a), p. 37; Jopp (2013), pp. 121, 124. Mortality had more than tripled (from 5 per 1,000 miners at risk during 1908-13 to 17 during 1914-18). Since mortality has been calculated from figures pertaining to the miners' own insurance system, the higher mortality should also reflect battle deaths of conscripted miners because they remained in the insurance records, even though they dropped from the employment statistics.

[29] In German mining as a whole, roughly 63,500 POWs were employed in 1915, 127,600 in 1916, 148,000 in 1917, and 154,000 in 1918. Thereof 62.7 (29.4), 68.7 (35.4), 68.5 (35.5), and 66.4 (35.5) percent fell upon hard coal mining in general (Ruhr coal mining); see Zeitschrift für das Berg-, Hütten- und Salinenwesen in dem preussischen Staate 70 (1922), p. 152 (statistical part).

[30] Broadberry and Harrison (2005); Rawe (2005c), pp. 221-223; Ziegler (2013), pp. 30-48.

the consumers; this incentivized firms to produce less in order to keep the stockpiles low.[31] Second, due to the distortions in international trade important foreign markets for Ruhr coal (e.g., Italy, Russia, Northern France) were closed all of a sudden.[32] Third, with marginal two percent of coal produced with mechanical support, and not just by use of manual labour in combination with explosives as was the standard, the pre-war degree of mechanization at the coalface was negligible and remained so over the following years as mining companies were reluctant to invest on a level comparable to the immediate pre-war period;[33] this even holds for after the launch of the Hindenburg program.[34] The average capital intensity in Ruhr coal mining admittedly grew over the war, but because of the reduction in the denominator; it almost fell back to its pre-war level over 1920-1922, when Ruhr coal mining saw a massive inflow of homecoming soldiers.[35] Along with technological stagnation came the so-called *Raubbau*, the over-exploitation of (already developed) high-quality deposits, which jointly caused severe 'asset erosion' and even predetermined the difficulties in maintaining (Ruhr) coal production after the war.[36] Recently provided total factor productivity estimates vividly tell of the massive challenges of the Ruhr coal industry.[37]

In the longer term, measures of *Konjunktursteigerung* (e.g., increased work time) could only incompletely reverse the trend of decreasing production and productivity.[38] The initial loss of qualified underground personnel probably weighed most heavily. It was counteracted by successively employing POWs, foreign civil contract labour and also miners already retired on invalidity underground, while females and juveniles below age 16 were allocated to a hitherto unprecedented amount to surface activities.[39] According to Burghardt (1988), the share of females had doubled from 1913 (3.7 percent) to 1916 (7.5 percent), and the share of juveniles jumped from zero towards 3.0 percent from 1915 to 1916 (1918: 5.2 percent).[40] Apart from females and juveniles, the share of the group of foreign labourers grew,

[31] Burghardt (1988), p. 93; Shearer (1989), p. 105; Ziegler (2013), p. 37.

[32] Burghardt (1988), p. 97.

[33] Jopp (2017), p. 949.

[34] Burghardt (1988), pp. 99-103. The next available figure for the degree of mechanization is for 1925, when almost 50 percent of coal was won by mechanical support (Jopp 2017, p. 6).

[35] Jopp (2016), p. 1127-1129. Average capital intensity in Ruhr coal mining amounted to about 6,700 marks per mineworker in 1914 and rose to about 10,200 marks in 1915 and further to about 12,000 marks in 1918.

[36] Burghardt (1988), pp. 99-104; Shearer (1989), p. 106.

[37] Jopp (2016), p. 1134.

[38] Shearer (1989), p. 105.

[39] Rawe (2005a), pp. 38-40.

[40] Burghardt (1988), p. 94.

too. They consisted of civilian labour migrants who voluntarily kept working in the Ruhr mines or were hindered from migrating back to their country of origin, civilian deportees, and POWs.[41] The number of POWs employed in coal mining was only a fraction of the number employed in the entire German economy including the occupied areas. The majority of POWs was in fact allocated to agriculture.[42] Following Spoerer (2006), approximately 2.5 million POWs were employed over the war.[43] To this figure, some 800,000 foreign civilian forced labourers have to be added; together they amounted to around 10.5 percent of the German 1913 labour force and thus were of substantial importance to reduce labour scarcity.

The allocation of unqualified labourers to the mines together with distortions in the workforce's age structure, visible in Figure 3, is said to have notably lowered the average skill level in the industry causing the higher accident rates during the war.[44] As Burghardt (1988) summarizes on the employment of foreign labourers:

> "Language problems and lacking motivation, inferior nourishment and absent familiarity with the working conditions in coal mining were the root causes for an utterly low productivity among foreign workers. Their employment was out of all proportion to the additional amount of coal won."[45]

Yet it has not been tested quantitatively whether it was the employment of POW labour that *significantly* depressed productivity growth in Ruhr coal mining, or whether other factors were more important. This paper attempts to provide the first such test using mine-level data.

4. HOW POW ALLOCATION (MIGHT HAVE) WORKED

4.1. Institutional background

Basically, firms were not legally forced to employ POWs, even though the authorities increasingly threatened them with the use of force. Initial reluctance to draw on the unexpectedly rapidly growing POW reservoir up until January 1915 may be explained by safety concerns; and by the regular workforce's as well as the authorities' concerns about employers capitaliz-

[41] Rawe (2005b), pp. 69-248, and Rawe (2005c), p. 221. According to Rawe (2005b), p. 182, the first two groups accounted for roughly 0.5 (0.5, 6.3, 8.4, 6.3) percent of the total workforce in 1914 (1915, 1916, 1917, 1918); note that the total workforce here is what we call regular workforce in the context of Figure 1.

[42] Oltmer (2006b), p. 71.

[43] Spoerer (2006), p. 124. Figures by country of origin are provided by Spoerer (2006), p. 127. Russian and French POWs alone accounted for 57.3 and, respectively, 21.1 percent of all captured POWs. Figures on Ruhr coal mining for February 1918 reflect that nationality mix pretty well. According to Rawe (2005a), p. 45, in that month, about 46 (37, 5, 5, 5, 2) percent of all POWs were Russian (French, English, Belgian, Italian, other).

[44] Burghardt (1988), p. 93; Shearer (1989), p. 105; Ziegler (2013), pp. 33-34.

[45] Burghardt (1988), p. 97.

ing on the availability of POWs on the extended labour market by lowering wages and social standards, thereby risking ill humour at the home front. However, such concerns were soon abandoned as a reaction to the changing demands of the war economy. The firms' strategy of choice became filling part of the gap in the regular workforce with POWs.[46] For the sake of illustration, Table 3 gives the five largest employers of POW labour per year in Ruhr coal mining. In absolute terms, and at some distance to its followers, the *Deutsch-Luxemburgische Bergwerks- und Hüttenverein AG* was the largest employer of POW labour in each year; its peak employment in 1917 represents 22.4 percent of its total mine employment in that year.

The fact that mining firms voluntarily began to apply to the army[47] for POW assignments in early 1915 might have been to some extent to neutralize the threat of otherwise being forced. But to an even greater extent, it likely was an expression of simple economic calculus; the firms' key interest was to maintain or, respectively, raise production again.[48] Firms reckoned with lower individual POW productivity and profitability – not the least, because they had to bear the costs of food, shelter, and wages, for the POWs as well as their guards.[49] Despite such concerns, and as the scattered data allow to say, the majority of POWs apparently was allocated to critical underground activities nonetheless, to hewing and hauling coal; and only a fraction of the POWs actually were trained miners.[50] This statement in the literature is of utmost importance as it is the fundament on which our baseline hypothesis on the negative relationship between the number of POWs employed and productivity rests.

Figures on *individual* POW productivity come as anecdotal evidence. It is possible that mining entrepreneurs had their very own agenda in mind when circulating particular figures and when constantly complaining about the inferior productivity of POWs before the authorities.[51] A claimed relative productivity of 45-50 percent or less compared to experienced miners might be an honest testimony as likely as a calculated underprediction as a response to some incentive.[52] In fact, the wage regime arguably provided such an incentive as firms were supposed to pay POWs a wage according to regular miners' wages. Though, three quarters of it had to be paid directly to the army for later pay-out; one quarter had to be paid

[46] Laufer (1993), p. 208; Rawe (2005a), pp. 42-43, 46; Rawe (2005b), pp. 81-86, Klank (2008), pp. 137-139.

[47] The respective addressee was the *Stellvertretendes Generalkommando des VII. Armeekorps* located in the city of Münster; see Rawe (2005a), p. 45.

[48] Rawe (2005b), p. 252.

[49] Rawe (2005a), pp. 45-46.

[50] Laufer (1993), p. 209; Rawe (2005a), pp. 46-47.

[51] Rawe (2005a), p. 45.

[52] Rawe (2005a), p. 48.

directly to the POWs, with the possibility for an additional incentive pay to raise motivation or honour good performance. However, if firms could convince the army that the POWs' productivity was too low, the army could grant a wage reduction.[53] Basically, this guideline appears to be an invitation for falsely reporting the productivity of POW labour to economise on costs. Authorities would likely not have had the knowledge to independently cross-validate a firm's statement, so would rather have granted the reduction than not.[54]

4.2. Stylized facts on POW assignment in practice

How many mines exactly had used POW labour during the war and to what extent? Tables 4 and 5 arrange our data such that we can assess the distribution of POW employment among mines. First, Table 4 shows the number of mines by the relative number of POWs in a mine's total employment; consistent with the technical literature we may speak of the treatment intensity (with seven treatment levels here, from 0 to 6). It becomes clear that a fraction of mines – not the majority share – indeed never used POW labour. To be precise, annual data tell that once a mine began to employ POWs, it continued to do so up until the end of the war. Besides that, the Table indicates considerable cross-sectional variation in treatment intensity with between slightly over zero and up to 35 percent of total mine employment; and, generally, treatment intensity rose over time. In addition, Table 5 highlights another two stylized facts: Firstly, the POW share also varied in the longitudinal dimension, that is, within one and the same mine; and, secondly, mines did not all begin to employ POWs in 1915. Most mines did, indeed, namely 116. But a couple of mines only began in 1916, 1917, or even as late as 1918; in the remainder of this article we will simply refer to the treatment cohort of 1915, 1916, and so forth.

Furthermore, we can ask how the POW – or, generally speaking, the treatment – assignment process concretely worked. According to which practical principles did firms select mines to be receiving POWs? From Rawe's historical account we get that pure economical calculus should have driven the firms' decisions, but we do not get to know how exactly. However, having at least an idea is crucial for the treatment effects approach as treatment assignment, if it were not *purely* random (the ideal), should at least be *conditionally* random, that is, random after controlling for each and every factor influencing selection. As is standard

[53] Rawe (2005a), p. 45; Rawe (2005c), p. 225.

[54] Rawe (2005a), pp. 48-49, mentions that the army actually did get increasingly sceptical about the firms' frequent complains and their applications for wage reductions so that she wanted to explore this issue in more depth. Whether the army had really traced this, however, remains unclear.

in the relevant literature, we performed simple mean comparison tests regarding group characteristics in the pre-treatment period to get a hold of factors potentially raising the probability of a subject to be selected into treatment. The test depicted in Table 6 compares the treatment cohort of 1915 with the remaining mines as of 1914 for all mine- and firm-level characteristics. Note that the control group here includes the subgroup of mines that would use POW labour only since 1916. The test implies that mines using POWs from 1915 on were significantly different pre-treatment in two respects: First, they were larger, both in terms of output and workforce; and, second, they often belonged to vertically integrated firms. These were the so-called large foundry mines (*Hüttenzechen*) in the first place (e.g, the *DeutschLux* or *Gutehoffnungshütte*). That these firms, on average, significantly earlier applied for and received POWs is plausible as these firms represented a significant share of German metal processing and steel production capacities. Interestingly, pre-treatment labour productivity does not stand out as a potential criterion for selection. We did the same exercise for the treatment cohort 1916 (results not displayed). A comparison of these mines with the control group in 1915 points to significant differences exclusively in regular employment growth and output growth prior to selection into treatment; mines that would use POW labour from 1916 on had lost more workers and more output from 1914 to 1915 than all mines still not using POWs in 1916.

Table 7 shows the mean comparison between the control group of mines that never used POW labour at all and the rest, regardless of when POWs were first used. Globally speaking, mines using POW labour were much larger pre-treatment, in 1914; had lost fewer (!) regular miners from 1913 to 1914; had a higher labour productivity in 1914; belonged to multiple-mine firms in the first place; and belonged with greater likelihood to vertically integrated firms. So the assertion seems not to farfetched that in the firms' economical calculus mine size and the securing of the value chain played the most critical role in allocating POW labour. The evidence of labour productivity as a relevant determinant is mixed at best.

Considering the aforementioned institutional background, especially the built-in moral hazard regarding wage costs, our treatment effects strategy comes in handy as it has the advantage of being based on quantitative outcomes of many production units, and not on potentially doctored anecdotal evidence just relating to the one or other mine or firm. If the assumption of a quite low individual productivity of the average POW holds and if POWs were massively employed in functions critical to the mining process as the literature says, then we should find, conditional on controlling for other potential determinants, that POW employment significantly drove down labour productivity (growth). In this respect, a look at Figure 4

reveals that, broadly speaking, a negative relationship between labour productivity (growth) and the treatment level prevails in the data set, again globally speaking.

5. TREATMENT EFFECTS ANALYSIS: POWS AND PRODUCTIVITY

5.1. The potential-outcomes framework for binary treatment

The empirical analysis starts with a brief introduction into the formal foundation of treatment effects analysis, namely the potential-outcomes framework. Suppose we wish to explore the causal effect of a treatment on a continuous outcome variable Y_{jd} observed for a range of subjects $j = 1, ..., J$. Let $d = 0$ denote the case of no-treatment (the control case) and $d = 1$ the case of being treated. Hence, Y_{j0} is the potential outcome for subject j when being the control and Y_{j1} is the potential outcome when being treated. The baseline problem of observational data is given by the fact that we either observe Y_{j0} or Y_{j1} for subject j, but never both. Formally the general potential-outcome model can be described by the following three equations:

(1) $Y_{j0} = X_j\beta_0 + u_{j0}$,

(2) $Y_{j1} = X_j\beta_1 + u_{j1}$,

(3) $d_j = \begin{cases} 1 \text{ if } W_j\delta + v_j > 0 \\ 0, \text{ otherwise} \end{cases}$

Equations (1) and (2) model the outcome process as dependent on a set of covariates X and error terms u unrelated to either X or W. Equation (3) models the treatment assignment process where selection into treatment is dependent on a vector of covariates W and an error term v likewise unrelated to either X or W. If v is independent of the u the assumption of exogenously assigned treatment holds. We can alternatively say that W includes *all* variables that affect treatment assignment, such that there are no unobserved confounders left.

The aim is to make statements on two measures, namely the *average treatment effect in the populatio*n (ATE) and the *average treatment effect on the treated* (ATET), or:

(4) $\text{ATE} = \text{E}[Y_{1j} - Y_{0j}]$, and

(5) $\text{ATET} = \text{E}[Y_{1j} - Y_{0j} \mid d_j = 1]$.

In order to derive (4) and (5) two other assumptions, besides unconfoundedness (or conditional mean independence), have to be met, namely that the distribution of outcomes given con-

trols X is the same in both states d_j and that both groups of subjects overlap, i.e., each subject has the same probability to be selected for treatment.[55]

Starting from this framework, the various treatment models available to the empiricist can be distinguished along two lines. Most basically, there are models that keep the assumption of exogenous treatment outlined in the previous subsection; and there are models that allow for endogenous treatment due to unobserved confounders. Within the two strands, in turn, models either focus exclusively on the outcome-process or exclusively on the treatment assignment process, or on both simultaneously.

In the following, we build two models; one that focusses exclusively on the outcome process and assumes there are no flaws whatsoever in the data that could not be solved by inclusion of a set of fixed effects and further covariates; and a second that takes both processes into account thereby offering an alternative robust to some flaws such as the treatment assignment not being random.

5.2. A conventional difference-in differences benchmark

The first model we are going to apply is a conventional diff-in-diff estimator based on panel data which we implement as follows:

(6) $Y_{it} = \alpha_i + \tau_t + \beta * TREAT_i * AFTER_t + X_{it} * \delta + u_{it}$.

Here, i indexes mines and t indexes time. Y is the outcome of interest of which we consider two in the following, namely the growth of labour productivity and of output. The α's and τ's denote fixed mine- and year-effects accounting for unobserved time-invariant heterogeneity across mines – e.g., regarding capital use, fixed geology, or fixed characteristics of the mother company like company form – as well as for unobserved time-varying effects common to all mines in the cross-section – e.g., the general economic downturn, or distortions in the age structure. β is the coefficient of interest stating the differences between POW employing mines and non-POW employing mines in their differences post-treatment. The binary variable *TREAT* identifies the two groups by taking on the value 1 for treated and the value 0 for untreated mines. The dummy variable *AFTER* indicates the treatment period by taking on the value 1 for treated mines in the years of treatment (either 1915-18, 1916-18, 1917-18, or

[55] For the potential-outcome notation, see fundamentally Imbens (2004), pp. 5-11, Wooldridge (2010), pp. 903-907, Lechner (2011), and Greene (2012), pp. 928-933, for the model.

1918) and 0 otherwise. Finally, $X'\delta$ represents a vector of additional covariates that may explain labour productivity and, respectively, output growth; u is the error term.

Equation (6) covers the case of binary treatment. We measure the causal effect of just having used POW labour, or not, regardless of how many POWs were employed relative to regular miners. However, it has been shown in the previous section that there was considerable cross-sectional as well as longitudinal variation in the proportion of POWs. In order to use that information, too, we slightly modify equation (6) as follows:

(7) $Y_{it} = \alpha_i + \tau_t + \beta * POWSHARE_i * AFTER_t + X_{it} * \delta + u_{it}$.

Note that the only difference to equation (6) is in the substitution of the dummy variable TREAT by the variable POWSHARE. Here, β will allow for different difference-in-differences according to the extent of the proportion of POWs in total mine employment.

When applying a standard diff-in-diff model, it is common practice to assess the baseline assumption of parallel trends in outcomes in the control and in the treatment group if treatment were absent. Since there is no information on how the outcome of treated subjects would have developed if they had not been treated in what is the post-treatment period, it is checked for pre-treatment trends. Figure 5 therefore shows the development of average labour productivity in both groups and allows saying that the pre-treatment trends were broadly parallel. So there is no reason to believe that the parallel trends assumption is violated.

Table 8 presents the estimation results for both equations and both outcome variables of interest. In the upper part, results are shown for the binary treatment approach – highlighting the extensive margin; and in the lower part, results are shown for the treatment intensity approach – highlighting the intensive margin of POW employment. Besides fixed mine and year effects, all regressions include a set of variables capturing characteristics of the mother firm as well as district characteristics that cannot be measured on a more disaggregate level; see Section 2, Table 1, parts (B) and (C).[56] In fact, part of the dummies describing the mother firm drop out as they show no variation over time (and thus are captured by the mine fixed effects); the other part (single-mine firm; joint stock-company; vertically integrated), however, remains in the regression as they show a bit of variation due to mine turnover. For reasons of space, coefficients on firm and district characteristics are not shown in Table 8 and are

[56] We only included a firm's market share and not its output.

not discussed hereinafter.[57] In addition to the aforementioned effects, we included a mine's age and size (both also with a squared term) as well as lagged labour productivity into the regression concerning labour productivity growth;[58] the logic behind the inclusion of lagged labour productivity is that the higher the level of labour productivity had already been, the lower the growth potential. With the same logic, we included lagged mine size into the regression concerning output growth, along with a mine's age (and again its squared term). In all regressions, a mine's age and size as well the lagged terms are statistically significant and have the expected signs.

As to the causal effect of POW employment, we find mixed results. Note that the coefficients on the interaction term correspond to the *average treatment effect on the treated* according to equation (5) introduced in the previous subsection. On the extensive margin, having used POW labour per se is not related with an additional loss in productivity; see models (1) and (2). And only in model (3), the model without other covariates, is there a significant positive effect on the growth rate of production. Once controlled for further factors, the effect vanishes; see model (4). However, as already pointed out, modelling POW employment as a binary treatment is somewhat too restrictive. This is illustrated by models (5) to (8) which show significant treatment effects when it is controlled for treatment intensity. The significant negative coefficients in models (5) and (6) say that the growth penalty regarding labour productivity grew along the proportion of POWs; an increase in the POW share from one to eleven percent depressed labour productivity growth by 1.7 percent, which is a substantial loss in growth performance that exclusively came from the employment of POW labour. In so far, it severely mattered if a mine operated with only a small share of POWs in total employment or with a share of 20, 25, or even more percent of total employment. In particular, this effect remains intact after controlling for mine age, mine size, and other factors. In addition, fitting our expectations, the effect on the other side of the medal, namely output, was positive; the larger the POW share, the higher the growth rate of mine output, all else equal. So, based on the very restrictive assumptions of this baseline approach, we have *quantitative* evidence of a negative causal effect of POW labour on labour productivity while a positive causal effect on production prevails.

[57] A technical reason to keep dummy variables in the fixed-effects regression was that we tested for the suitability of the random-effects model in which the dummies would remain. However, in all cases a Hausman specification test rejected the suitability of the random-effects model.

[58] The inclusion of mine size as a measure of the scale of operations follows the example of Stoker et al. (2005), pp. 139-140, who explain labour productivity of U.S. coal mines over 1972-1995 with an information set not so different from ours.

5.3. A Doubly-robust treatment effects model

The fixed-effects estimator previously discussed treated selection into treatment as random. However, it has been shown that POW- and non-POW employing mines differed in the distribution of some characteristics which makes it more likely that post-treatment performance is correlated with pre-treatment characteristics. We therefore apply a second estimator that has the property of *double robustness*. Specifically, we apply inverse-probability weighted regression adjustment (IPWRA) as put forward, for example, by Wooldridge. IPWRA combines two models, namely a model for the outcome process and one for the treatment assignment process.[59] In a first step, the observational units' propensity score – i.e., the probability of being selected into treatment along pre-treatment characteristics – is estimated via logit or probit regression.[60] Then, the inverse of the propensity score is used to weight a unit's observation in the second step, the weighted regression model of the outcome process. This weighting procedure is intended to balance the distribution of covariates such that, ideally, it is equal for treated and non-treated units (or, more generally spoken, over groups of different treatment levels). The appeal of any doubly robust estimation procedure such as IPWRA is that the treatment effect can be consistently estimated even if one of the two models is not correctly specified; a likely problem occurring in empirical practice.

To retrieve estimates for the ATE and the ATET (see above), we basically model the outcome process Y – either the labour productivity (LP) or output growth (O) process – *linearly* and as dependent on X^{LP} = [mine age, mine size, labour productivity (-1), firm's market share, average capital intensity, shifts per employee, average real wage] and X^{O} = [mine age, output (-1), firm's market share, average capital intensity, shifts per employee, average real wage]; lags are indicated by (-1). The weights are retrieved beforehand from a *logit* regression explaining selection into treatment (yes = 1, no = 0) as dependent on W = [mine size (-1), labour productivity (-1), vertically integrated firm, single-mine firm, regular employment growth (-1), firm market share (-1)]. The covariates used to model treatment assignment correspond to those that popped up as significant in the simple mean comparison tests conducted in Subsection 4.2. Further note that we estimate both models for the binary treatment as well as the multivalued treatment case using four treatment levels (see below).[61]

[59] E.g., Wooldridge (2007); Wooldridge (2010), pp. 930-934; Uysal (2015).

[60] On the propensity score, see e.g. Rosenbaum and Rubin (1983).

[61] The reason for using just four treatment levels lies in technical matters to be explained in a few lines. For the multivalued treatment approach, see Cattaneo (2010) and Uysal (2015).

Before we turn to the treatment effects evidence, we briefly want to assess whether the specified models meet the basic assumptions of the potential-outcomes framework, namely unconfoundedness, proper balancing of covariates over groups, and sufficient overlap. First, there is no easy-to-apply statistical test on unconfoundedness. By all means, we cannot be sure that we provide a complete description of the treatment assignment process by including the six variables we chose, and just these. We chose them with an eye on the mean comparison tests, and insofar the choice is not arbitrary. However, we cannot exclude that there are additional relevant variables that we just did not gather.[62] Second, the balancing effect of the inverse-probability weighting can indeed be assessed by comparing the standardized differences or the variance ratios of the covariates' raw distributions with their weighted distributions.[63] In Table 9, we settle for standardized differences. These are shown for the baseline model (see previous paragraph) – model 1 – as well as for a second model – model 2 – which we extended for all interactions, namely six, between the four continuous covariates in *W*. The reason for including these additional interactions is that it improves the balancing.[64] After weighting with the inverse propensity score, the standardized differences should be close(r) to zero. Comparing the raw with the weighted standardized differences, this is for both models separately indeed the case. In some cases, adding additional interactions improved the balancing further, as the column on the far right indicates. Finally, third, we can graphically test whether the overlap assumption – namely that, after controlling, the *ex-ante* probability of being selected into treatment is broadly equal for every observational unit – is not violated. As Figure 6 illustrates for the model with interactions, the distributions of propensity scores by treatment level show sufficient overlap. In all, except for unconfoundedness, we are confident that our extended model 2 meets the basic assumptions of the potential-outcomes framework and thus provides consistent estimates of the treatment effect which we will turn to now.

Table 10 summarizes our doubly robust estimates of the ATE and the ATET in the binary treatment case as well as in a case of multivalued treatment. Given are the effects for the extended model. Originally, we wanted to settle for seven treatment levels, following the way of data presentation in Table 4. However, in order to improve the balancing further, we

[62] We performed a test for the baseline model *without* interactions on possible correlation between the unobservables in the potential-outcome models and the treatment assignment models – that is, on endogeneity – using STATA's *eteffects* command. We did not find evidence of endogeneity which supports the conclusion that treatment assignment is, as to be wished, *conditionally* random (test results available upon request). This, in turn, is important for the treatment assignment to be correctly specified.

[63] This is one tool STATA offers.

[64] We follow here the STATA manual which discusses this possibility for improving the estimates.

reduced the treatment levels to just four – zero, low, medium, and high, so to say (POW share = 0; 0 < POW share ≤ 10 %; 10 < POW share ≤ 20 %; POW share > 20 %). Given in the upper part of Table 10 are the results for labour productivity growth as the outcome variable of interest, and in the lower part for output growth. Besides the ATE and the ATET we also provide the estimated potential-outcome mean, abbreviated POM (column 1) along with information on the treatment levels that are compared (column 2).[65]

Let us turn to the results on labour productivity growth first. The ATE refers to a *hypothetical* situation, as does the respective POM. For example, the POM of –0.5 percent in the binary treatment model is the average growth rate of labour productivity that would occur if *no* mine had employed POWs. Correspondingly, the ATE of +4.7 percent gives the average growth rate that would occur if *all* mines had employed POWs; as the coefficient is insignificant, there would in fact have occurred no difference in this hypothetical scenario. In a similar fashion, we have to interpret the coefficients in the multivalued treatment case. If all mines had operated with a POW share of between zero and ten (ten and twenty) percent, the average growth rate of labour productivity had been 8.4 (7.8) percent more than the average occurring if none had (–2.8 percent). Interestingly, if all mines had operated with a high POW share, counterfactual average growth would have been outright negative; all these effects are statistically significant.

Yet, it is the ATET that is of greater interest for us because it is the factual effect in the data. On the extensive margin, when treatment is just binary, the average growth rate of labour productivity was negative with –4.9 percent for all mines that did in fact employ POWs, which is a clear productivity penalty. Turning to the multivalued treatment model, we find that operating with a low POW share made no difference. But operating with a medium or high POW share in comparison to the control group did indeed; and this finding corroborates our somewhat simpler panel regression in that the growth penalty was higher for a higher POW share. In addition to these effects that compare each treatment level with the base-level, we also give the incremental change in the ATET – that is, the penalty in growth if a mine with a low (medium) POW share switched to a medium (high) share. So, switching to a medium (high) POW share from a low (medium) POW share cost 1.6 (1.1) percent in labour productivity growth which still is notable and also somewhat fits with our earlier estimate of a loss in productivity growth of 1.7 percent for every 10 percent increase in the POW share (see previous Subsection).

[65] For reasons of space, we neither display the coefficient estimates for the outcome models nor for the treatment assignment model (available upon request).

Turning to output growth, the evidence in Table 10 implies that on the extensive margin filling the workforce with POWs had no effect; it would have had though in a counterfactual world (see the binary treatment model). More importantly, we find that only for mines operating with a medium POW share the growth rate of output was significantly larger than for the control group; and, in particular, there are no significant incremental effects. In all, while the results on labour productivity growth are broadly unambiguous, our findings on the output effect, as the other side of the medal, is somewhat mixed once compared with the results on the baseline fixed-effects estimator. In other words, the usual take-away from the literature mentioned above that the use of forced labour generally has a significant output-enhancing effect (e.g., see Custodis' studies) is not met with our data. This observation certainly matches with the idea that the economic conditions for efficient mining during WWI were indeed quite unfavourable.

6. CONCLUSION

Answering the question as to the productivity and profitability is one of the major concerns of historians of forced labour. In particular, this question has popped up in the context of the world wars, when the employment of civilian unfree as well as of Prisoner-of-War labour was notably contributing to lowering labour scarcity in all major belligerents' war time economies. This paper set out to provide a fresh view on the issue by exploring the case of POW employment in World War I Ruhr coal mining, a key industry in the German economy at the time. A new mine-level data set has been used to trace the causal effect of POW employment on labour productivity and output growth on the highly disaggregated establishment-level. It has been claimed that the situation for Ruhr coal mining resembles a natural experiment in that, indeed, many mines had employed POW labour since 1915 or later, but a couple of mines never had. This makes this historical setting suitable for analysis with treatment effects methodology.

Consistent over two applied estimators – a baseline difference-in-differences fixed effects estimator and a doubly robust estimator combining inverse-probability weighting and regression adjustment – our statistical findings strongly support the view that the benefits from employing POW labour in the form of re-increasing output levels came at the expense of a loss in productivity. Controlling for growth-enhancing and -slowing factors, non-POW employing coal mines did not incur comparable productivity losses. We hereby add new quantitative, *non-anecdotal* evidence of a negative relationship between POW employment and productivity to the extensive literature on forced labour as well as to the special literature on the

German coal industry during WWI. The latter indeed emphasizes the performance-depressing effect of POW employment, but has not yet provided systematic prove using the peculiar structure of data on Ruhr coal mines.

Our approach based on establishment-level data is not suitable to make an explicit statement on the relative productivity of the average POW – that is, on the average POW's productivity as a percentage of the average regular miner's productivity. Nonetheless, the question remains as to how large the causal effect of POW labour on productivity was. For the ease of the argument, and to just get a feeling for the effect's extent, let us focus on the effect from the *binary* doubly robust model: A mine on average experienced a productivity decline by 4.9 percent (see Table 10) from the mere use of POW labour to an unspecified amount. Further consider that average labour productivity among the treated mines in the adjusted data set (and, formally, measured at the end of the year) amounted to 257.7 tons in 1914, 279.5 tons in 1915, and 241.2 tons in 1918. That gives a reduction in labour productivity over the war of between 16.5 and 38.3 tons. Note that the increase in labour productivity from 1914 to 1915 is a gross effect determined, among other things, by rising work time. Expressed in growth rates, these figures correspond to a net reduction in labour productivity of 6.8 or, respectively, 15.9 percent. Against these growth rates in turn, a 4.9 percent POW penalty is quite an extensive penalty. As a rule of thumb, and as a lower bound guesstimate, we may establish that at least about one-third of the average treated mine's labour productivity decline can be attributed to the use of POW labour. This suggests a notable productivity difference between the average POW and the average regular miner, which we can, however, not exactly specify.

Overall, anecdotal evidence on contemporary mining entrepreneurs' devastating assessment of relative POW productivity seems to be grounded in reality; if that anecdotal evidence was doctored in some way as a response to the incentives provided by the POW assignment process, it must have been only marginally doctored in light of our findings.

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TABLES

Table 1: Descriptive statistics (1913-1918, $N = 999$)

Variable	Mean	SD	Min	Max
(A) Mine characteristics				
Location	12.3	6.6	1	23
Mine age	42.7	20.8	2	89
Mine output (scale)	582,736	481,805	15	4,460,011
Mine output growth	–0.012	+0.178	–0.998	+0.986
Total employment	2,228	1,763	2	13,711
Regular employment	2,052	1,634	2	13,711
Regular employment growth	–0.015	+0.161	–0.916	+0.750
POWS	176	271	0	2,010
POW share	0.073	+0.082	0.000	0.330
Labour productivity	259	51	3	390
Labour productivity growth	–0.021	+0.127	–0.986	+0.801
(B) Firm characteristics				
Single-mine firm	0.29	0.45	0	1
Joint-stock company	0.57	0.49	0	1
Gewerkschaft	0.29	0.45	0	1
Limited-liability company	0.07	0.24	0	1
Other company type	0.07	0.26	0	1
State-owned	0.07	0.25	0	1
Vertically integrated	0.36	0.48	0	1
Firm output	2,937,716	2,580,618	15	10,400,000
Market share	0.030	0.026	0.000	0.091
(C) Mining district characteristics				
Capital intensity	9,544	2,441	5,500	11,987
Shifts	347	26	303	374
Real wage	1,372	237	1,077	1,755

Notes: Descriptive statistics on adjusted data set (= zero output mines deleted; mines below –100 % or above +100% labour productivity/output/regular employment growth deleted).

Sources: *Die Bergwerke und Salinen im niederrheinisch-westfälischen Bergbaubezirk* (1919, 1921). Also see Table A.1 in the Appendix.

Table 2: Domestic demand mix for hard coal as of 1912

Demand source	Share
Ore processing, iron and steel production, metal processing	46.1 %
Transport	16.8 %
Private households, agriculture, small business	13.2 %
Utilities	5.7 %
Stones and earths industry	4.7 %
Chemical industry	3.2 %
Textile industry	3.0 %
Food and beverage industry	2.7 %
Miscellaneous industries	1.6 %
Paper and pulp industry	1.4 %
Military	1.4 %

Notes: Figures refer to the domestic sales of the Rhenish-Westphalian Coal Syndicate.

Sources: Verein für die bergbaulichen Interessen (1935), p. 49.

Table 3: The five largest employers of POW labour in Ruhr coal mining

	1915		1916		1917		1918	
	rank	#POWs	rank	#POWs	rank	#POWs	rank	#POWs
(1) Deutsch-Luxemburgische Bergwerks- und Hüttenverein AG	1	2,478	1	4,733	1	5,004	1	4,894
(2) Gelsenkirchener Bergwerks-AG	2	1,492	2	3,175	3	3,298	3	3,347
(3) Bergwerksgesellschaft Hibernia AG	3	922	-	-	-	-	-	-
(4) Haniel	5	911	3	2,851	2	3,366	2	3,394
(5) Staatliche Bergwerksdirektion Preußen	4	722	-	-	4	2,906	4	2,826
(6) Harpener Bergbau-AG	-	-	4	2,436	5	2,665	5	2,694
(7) Gutehoffnungshütte Actienverein für Bergbau und Hütten betrieb	-	-	5	2,158	-	-	-	-

Sources: See Table 1.

Table 4: The distribution of mines over the proportion of POWs

	Share in a mine's total employment in % (≈ "treatment level")						
	Level 0	Level 1	Level 2	Level 3	Level 4	Level 5	Level 6
	0	>0 - ≤5	>5 - ≤10	>10 - ≤15	>15 - ≤20	>20 - ≤25	>25
1915	69 (37%)	43 (23%)	49 (26%)	18 (9%)	7 (4%)	1 (0%)	1 (1%)
1916	39 (21%)	16 (8%)	29 (15%)	47 (25%)	28 (15%)	18 (9%)	11 (6%)
1917	40 (21%)	11 (6%)	31 (16%)	39 (21%)	37 (19%)	20 (10%)	11 (6%)
1918	38 (20%)	11 (6%)	37 (19%)	32 (16%)	43 (22%)	21 (11%)	11 (5%)

Notes: Unadjusted data set for 1915-18 used here.

Sources: See Table 1.

Table 5: Mean POW share by treatment cohort (in percent)

Cohort	1915 (*N*=116)		1916 (*N*=28)		1917 (*N*=1)		1918 (*N*=5)	
Year	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd
1915	7.2	5.1	-	-	-	-	-	-
1916	15.0	6.5	10.0	7.1	-	-	-	-
1917	15.2	6.2	11.4	6.8	4.9	-	-	-
1918	15.5	6.1	12.6	6.5	5.8	-	2.7	1.7

Notes: Unadjusted data set for 1915-18 used here.

Sources: See Table 1.

Table 6: Characteristics of mines across groups in 1914 – mines employing POWs in 1915 vs. mines not employing POWs in 1915 (mean and standard deviations)

Variable	All mines	No POWs employed in 1915	POWs employed in 1915	Difference
(A) Mine characteristics				
Location	12.3	13.2	12.0	1.2
	[6.6]	[6.2]	[6.8]	(1.1)
Mine age	42.3	43.3	41.9	1.4
	[19.8]	[21.4]	[19.1]	(3.4)
Mine output (scale)	586,490	330,740	707,515	376,776***
	[501,354]	[259,551]	[542,036]	(62,404)
Mine output growth	–0.14	–0.15	–0.14	–0.01
	[0.17]	[0.27]	[0.11]	(0.04)
Total employment	2,267	1,280	2,734	–1,453***
	[1,809]	[995]	[1,920]	(227)
Regular employment growth	–0.07	–0.09	–0.06	–0.03
	[0.14]	[0.22]	[0.06]	(0.03)
Labour productivity	252.7	244.7	256.5	–11.8
	[44.6]	[64.5]	[30.8]	(9.3)
Labour productivity growth	–0.08	–0.07	–0.08	–0.01
	[0.10]	[0.16]	[0.06]	(0.02)
Single-mine firm	0.3	0.3	0.3	0.1
	[0.5]	[0.5]	[0.4]	(0.1)
(B) Firm characteristics				
Joint-stock company	0.58	0.55	0.60	–0.05
	[0.49]	[0.50]	[0.49]	(0.08)
Gewerkschaft	0.29	0.32	0.28	0.04
	[0.45]	[0.47]	[0.45]	(0.08)
Limited-liability company	0.06	0.07	0.05	0.02
	[0.24]	[0.27]	[0.23]	(0.04)
Other company type	0.06	0.06	0.06	–0.00
	[0.24]	[0.23]	[0.24]	(0.04)
State-owned	0.07	0.09	0.06	0.03
	[0.26]	[0.29]	[0.24]	(0.05)
Vertically integrated	0.34	0.17	0.43	–0.26***
	[0.48]	[0.38]	[0.50]	(0.07)
Market share	0.03	0.02	0.03	–0.01*
	[0.03]	[0.02]	[0.03]	(0.00)
Number of mines	165	112	53	

Notes: t-test on equality of mean across groups performed on the adjusted data set (see Table 1). Unequal variance in the groups assumed. Standard deviations are in brackets and standard errors are in parentheses. Significance levels are as follows: * p<0.10, ** p<0.05, *** p<0.01.

Sources: Author's own computations.

Table 7: Characteristics of mines across groups in 1914 – mines employing POWs sometime over the war vs. mines never employing POWs over the war (mean and standard deviations)

Variable	All mines	No POWs employed over the war	POWs employed over the war	Difference
(A) Mine characteristics				
Location	12.3	13.7	12.2	1.5
	[6.6]	[5.8]	[6.7]	(1.4)
Mine age	42.3	40.7	42.6	-1.9
	[19.8]	[22.7]	[19.4]	(5.4)
Mine output (scale)	586,490	169,487	640,758	–471,271***
	[501,354]	[165,620]	[505,110]	(56,491)
Mine output growth	–0.14	–0.25	–0.13	–0.12
	[0.17]	[0.36]	[0.13]	(–0.08)
Total employment	2,267	713	2,469	–1,756***
	[1,809]	[649]	[1,815]	(211)
Regular employment growth	–0.07	–0.19	–0.06	–13.0*
	[0.14]	[0.33]	[0.08]	(0.07)
Labour productivity	252.7	214.3	257.7	–43.5**
	[44.6]	[80.9]	[35.0]	(18.8)
Labour productivity growth	–0.08	–0.09	–0.07	–0.02
	[0.10]	[0.25]	[0.07]	(0.06)
Single-mine firm	0.31	0.53	0.28	0.24*
	[0.46]	[0.51]	[0.45]	(0.12)
(B) Firm characteristics				
Joint-stock company	0.58	0.47	0.59	–0.12
	[0.49]	[0.51]	[0.49]	(0.12)
Gewerkschaft	0.29	0.42	0.27	0.15
	[0.45]	[0.51]	[0.45]	(0.12)
Limited-liability company	0.06	0.05	0.06	–0.01
	[0.24]	[0.23]	[0.24]	(0.06)
Other company type	0.06	0.05	0.06	–0.01
	[0.24]	[0.23]	[0.24]	(0.06)
State-owned	0.07	0.10	0.07	0.04
	[0.26]	[0.31]	[0.25]	(0.07)
Vertically integrated	0.34	0.16	0.37	–0.21**
	[0.04]	[0.04]	[0.08]	(0.09)
Market share	0.03	0.02	0.03	–0.01
	[0.03]	[0.03]	[0.02]	(0.01)
Number of mines	165	146	19	

Notes: t-test on equality of mean across groups performed on the adjusted data set (see Table 1). Unequal variance in the groups assumed. Standard deviations are in brackets and standard errors are in parentheses. Significance levels are as follows: * p<0.10, ** p<0.05, *** p<0.01.

Sources: Author's own computations.

Table 8: Baseline treatment effects analysis

Variables	Labour productivity growth	Labour productivity growth	Output growth	Output growth
	(1)	(2)	(3)	(4)
Treat*After	-0.0032	0.0036	0.0372*	0.0192
	(-0.1192)	(0.0129)	(0.0202)	(0.0180)
Mine age		-0.0084		-0.0054
		(0.0200)		(0.0282)
Mine age squared		0.0001***		0.0003***
		(0.0000)		(0.0000)
Mine size		3.74e-07***		
		(7.37e-08)		
Mine size squared		-5.04e-14***		
		(1.41e-14)		
Labour productivity (-1)		-0.0022***		
		(0.0001)		
Mine size (-1)				-5.58e-07***
				(4.18e-08)
Firm characteristics	No	Yes	No	Yes
District characteristics	No	Yes	No	Yes
Fixed mine effects	Yes	Yes	Yes	Yes
Fixed year effects	Yes	Yes	Yes	Yes
# observations	999	999	999	999
F-Statistic	74.36***	56.11***	78.65***	64.53***
R-squared (overall)	0.2437	0.1128	0.2328	0.0030
	(5)	(6)	(7)	(8)
POW share*After	-0.0017**	-0.0018***	0.0021*	0.0027***
	(0.0008)	(0.0007)	(0.0011)	(0.0010)
Mine age		-0.0038		-0.0123
		(0.0200)		(0.0280)
Mine age squared		0.0001**		0.0003***
		(0.0000)		(0.0000)
Mine size		3.78e-07***		
		(7.30e-08)		
Mine size squared		-5.13e-14***		
		(1.41e-14)		
Labour productivity(-1)		-0.0022***		
		(0.0001)		
Mine size (-1)				-5.57e-07***
				(4.15e-08)
Firm characteristics	No	Yes	No	Yes
District characteristics	No	Yes	No	Yes
Mine effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
# observations	999	999	999	999
F-Statistic	75.60***	57.07***	78.69***	65.53***
R-squared (within)	0.2371	0.0949	0.2344	0.0074

Notes: Constant term not displayed. Standard errors are in parentheses. Significance levels are as follows: * p<0.10, ** p<0.05, *** p<0.01.

Sources: Author's own computations.

Table 9: A nonparametric test for balancing of covariates – standardized differences

Variable	Model 1: Baseline		Model 2: Additional interactions		Improvement from model 1 towards model 2
	Raw	Weighted	Raw	Weighted	
(A) Binary treatment model					
Mine size (-1)	–0.141	+0.043	–0.141	+0.058	No
Labour productivity (-1)	–0.318	+0.060	–0.318	–0.011	Yes
Vertically integrated firm	+0.114	+0.038	+0.114	+0.014	Yes
Single-mine firm	+0.011	–0.048	+0.011	–0.006	Yes
Regular employment growth (-1)	–0.281	–0.049	–0.281	–0.058	No
Firm market share (-1)	–0.009	+0.054	–0.009	+0.029	Yes
(B) Multivalued treatment model					
(B1) Treatment Level 1					
Mine size (-1)	+0.154	–0.000	+0.154	–0.039	No
Labour productivity (-1)	–0.036	+0.073	–0.036	–0.001	Yes
Vertically integrated firm	+0.203	+0.008	+0.203	–0.050	No
Single-mine firm	–0.173	–0.035	–0.173	+0.003	Yes
Regular employment growth (-1)	–0.515	–0.185	–0.515	–0.155	Yes
Firm market share (-1)	+0.232	+0.030	+0.232	+0.002	Yes
(B2) Treatment level 2					
Mine size (-1)	+0.212	+0.001	+0.212	+0.070	No
Labour productivity (-1)	–0.227	+0.004	–0.227	–0.008	No
Vertically integrated firm	+0.250	+0.036	+0.250	+0.008	Yes
Single-mine firm	–0.176	–0.000	–0.176	+0.014	No
Regular employment growth (-1)	–0.277	+0.050	–0.277	–0.008	Yes
Firm market share (-1)	+0.051	+0.020	+0.051	+0.028	No
(B3) Treatment level 3					
Mine size (-1)	–0.068	–0.099	–0.068	–0.029	Yes
Labour productivity (-1)	–0.341	+0.149	–0.341	+0.282	No
Vertically integrated firm	+0.473	–0.044	+0.473	+0.002	Yes
Single-mine firm	–0.023	+0.068	–0.023	–0.078	No
Regular employment growth (-1)	–0.531	–0.127	–0.531	–0.382	No
Firm market share (-1)	–0.157	–0.078	–0.157	+0.183	No

Notes: Output on interactions omitted for reasons of space.

Sources: Author's own computations.

Table 10: Doubly robust estimates of treatment effects

Effect	Level comparison	Model 2: Additional interactions	
(1) Labour productivity growth			
(A) Binary treatment model			
ATE	1 vs. 0	+0.0466**	(0.0212)
POM	0	–0.0054	(0.0176)
ATET	1 vs. 0	–0.0491**	(0.0242)
POM	0	+0.0221	(0.0238)
(B) Multivalued treatment model			
ATE	1 vs. 0	+0.0840***	(0.0225)
	2 vs. 0	+0.0776**	(0.0307)
	3 vs. 0	–0.1050***	(0.0357)
POM	0	–0.0284**	(0.0141)
ATET	1 vs. 0	–0.0148	(0.0136)
	2 vs. 0	–0.0329**	(0.0148)
	3 vs. 0	–0.1399***	(0.0259)
POM	0	+0.0205	(0.0132)
ATET (incremental change)	2 vs. 1	–0.0160*	(0.0010)
	3 vs. 2	–0.0114	(0.0100)
(2) Output growth			
(A) Binary treatment model			
ATE	1 vs. 0	+0.0404*	(0.0210)
POM	0	–0.0447***	(0.0131)
ATET	1 vs. 0	+0.0183	(0.0194)
POM	0	–0.0141	(0.0185)
(B) Multivalued treatment model			
ATE	1 vs. 0	+0.0106	(0.0351)
	2 vs. 0	+0.1379***	(0.0434)
	3 vs. 0	+0.0557**	(0.0226)
POM	0	–0.0720***	(0.0145)
ATET	1 vs. 0	+0.0190	(0.0140)
	2 vs. 0	+0.0318**	(0.0153)
	3 vs. 0	+0.1399	(0.0039)
POM	0	–0.0494***	(0.0126)
ATET (incremental change)	2 vs. 1	–0.0095	(0.0184)
	3 vs. 2	+0.0082	(0.0121)

Notes: Robust standard errors in parentheses. ATE is “average treatment effect in the population”. ATET is “average treatment effect on the treated”. POM is “potential-outcome mean”.

Sources: Author’s own computations.

Table A.1: Variable description and sources

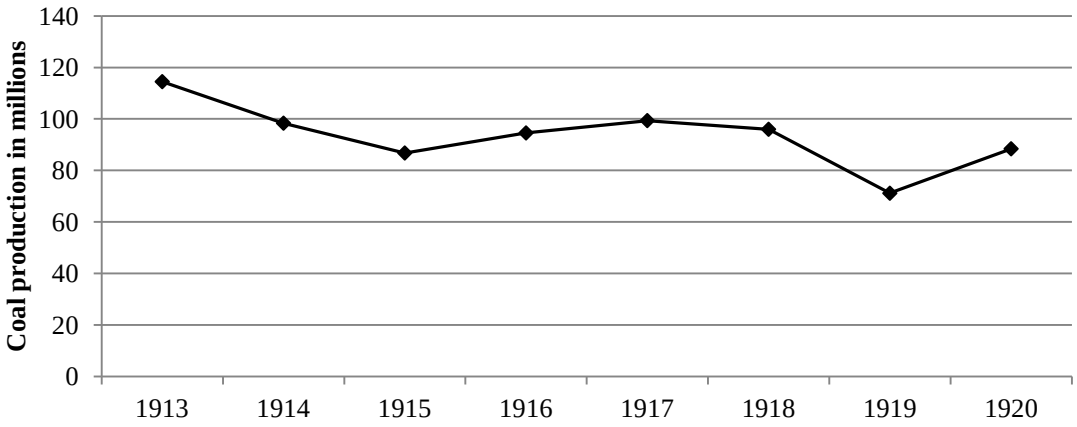
Variable	Description	Source
(A) Mine characteristics		
Location	1, 2, …, 23 for the mining offices (*Bergreviere*) the Ruhr coal district was subdivided into	Jahrbuch
Mine age	Years since mine i's first year of operation	Gebhardt (1957); Huske (1998)
Mine output (scale)	Annual hard coal production in tons	Jahrbuch
Mine output growth	[scale (t) – scale(t-1)] / scale(t-1)	Author's calculation
Total employment	# of overall employed mineworkers	Jahrbuch
Regular employment	# of employed regular mineworkers	Jahrbuch
Regular employment growth	[regular employment(t) – regular employment(t-1)] / regular employment(t-1)	Author's calculation
POWS	# of Prisoners-of-War employed	Jahrbuch
POW share	POWS divided by total employment	Author's calculation
Labour productivity	Mine output divided by total employment	Author's calculation
Labour productivity growth	[labour productivity(t) – labour productivity(t-1)] / labour productivity(t-1)	Author's calculation
(B) Firm characteristics		
Single-mine firm	1 if mine is a firm's only mine	Jahrbuch
Joint-stock company	1 if firm is joint-stock company	Jahrbuch
Gewerkschaft	1 if firm is Gewerkschaft	Jahrbuch
Limited-liability company	1 if firm is limited-liability company	Jahrbuch
Other company type	1 if firm has other company form	Jahrbuch
State-owned	1 if firm is state-owned	Gebhardt (1957)
Vertically integrated	1 if firm is vertically integrated (e.g., foundry)	Gebhardt (1957)
Firm output	Sum of output over mines belonging to firm j	Jahrbuch
Market share	Firm j's output in percent of total Ruhr output	Author's calculation
(C) Mining district characteristics		
Capital intensity	Capital per employee in marks	Burhop/Lübbers (2009), p. 510; Jopp (2016), p. 1129
Shifts	Average number of shifts per mineworker	Jopp (2016)
Real wage	Average real wage in Ruhr coal mining	Sommariva/Tullio (1986), p. 232, for CPI/ ZBHSW (1913-20)

Notes: All variables collected year-by-year. "Jahrbuch" is short for *Jahrbuch für den Oberbergamtsbezirk Dortmund* (and the respective supplement; see text). "ZBHSW" is short for *Zeitschrift für das Berg-, Hütten- und Salinenwesen in dem preussischen Staate*.

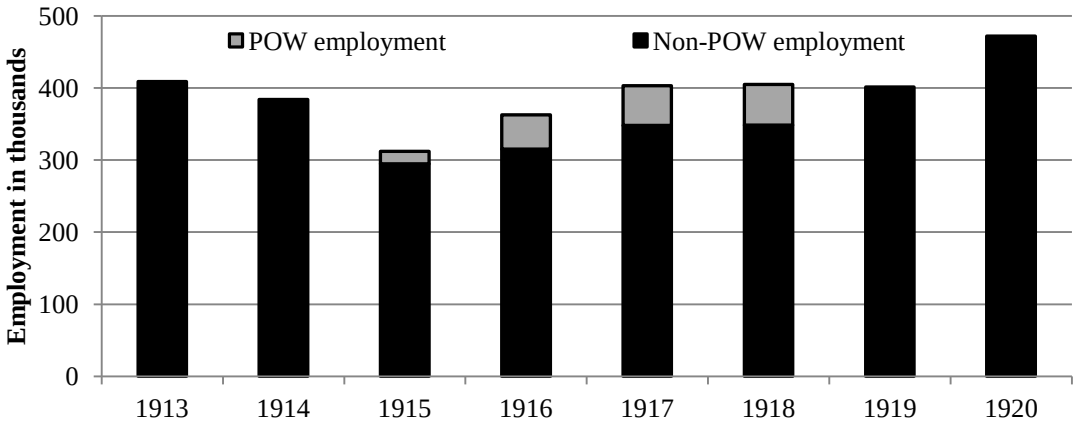
FIGURES

Figure 1: Ruhr coal mining around World War I (1913-1920)

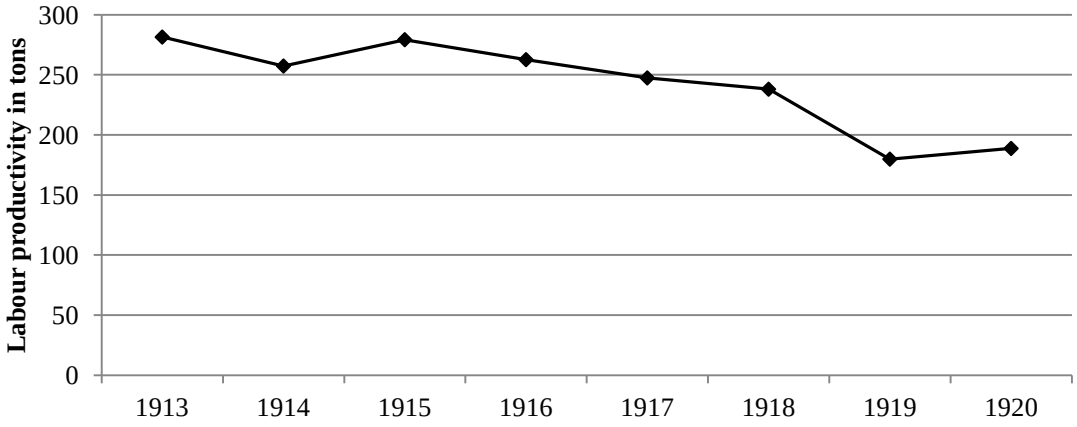
(a) Coal output



(b) Employment

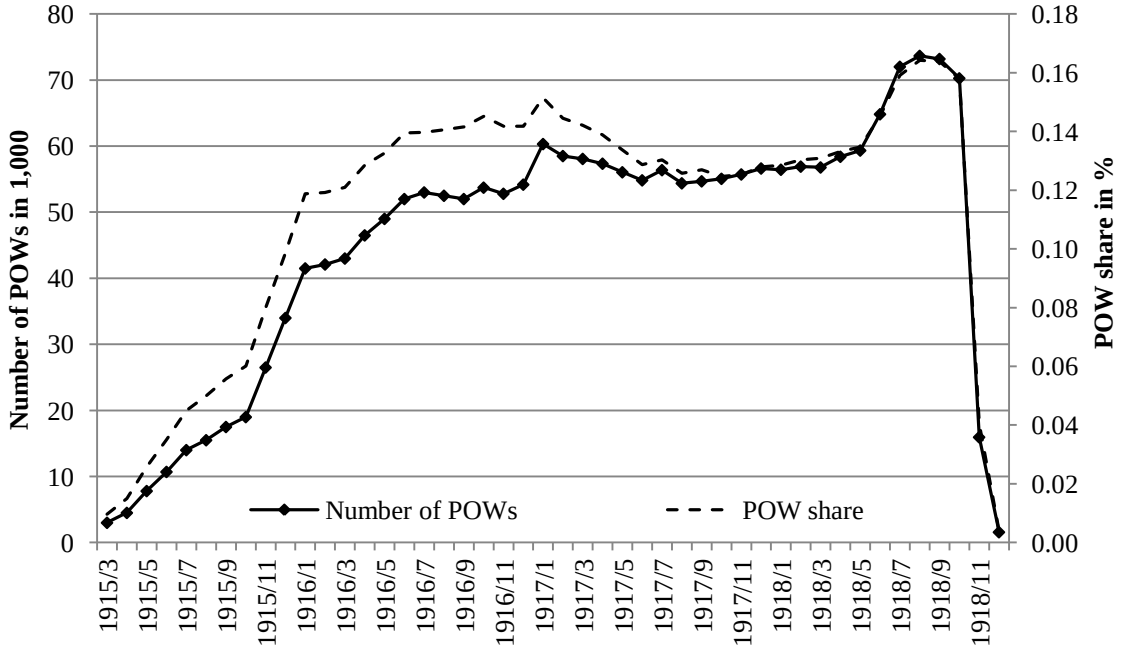


(c) Labour productivity



Sources: See Table 1.

Figure 2: Monthly POW employment in Ruhr coal mining (1915-1918)



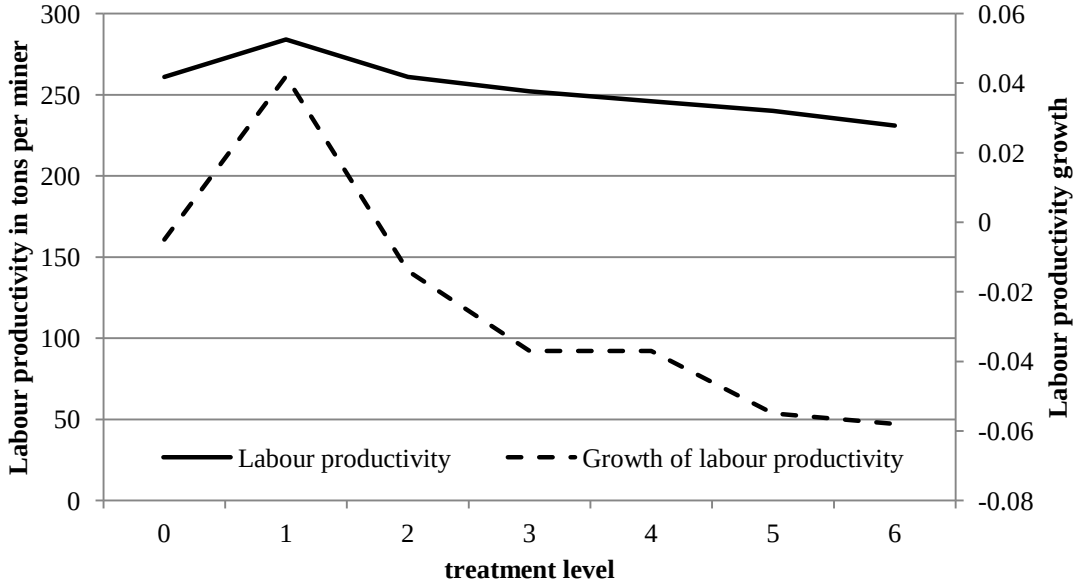
Sources: Data taken from Rawe (2005b: 75).

Figure 3: The distorted age structure among Ruhr miners

Notes: Age structure for 1917 and 1918 similar to that of 1916.

Sources: Data taken from Rawe (2005b: 64).

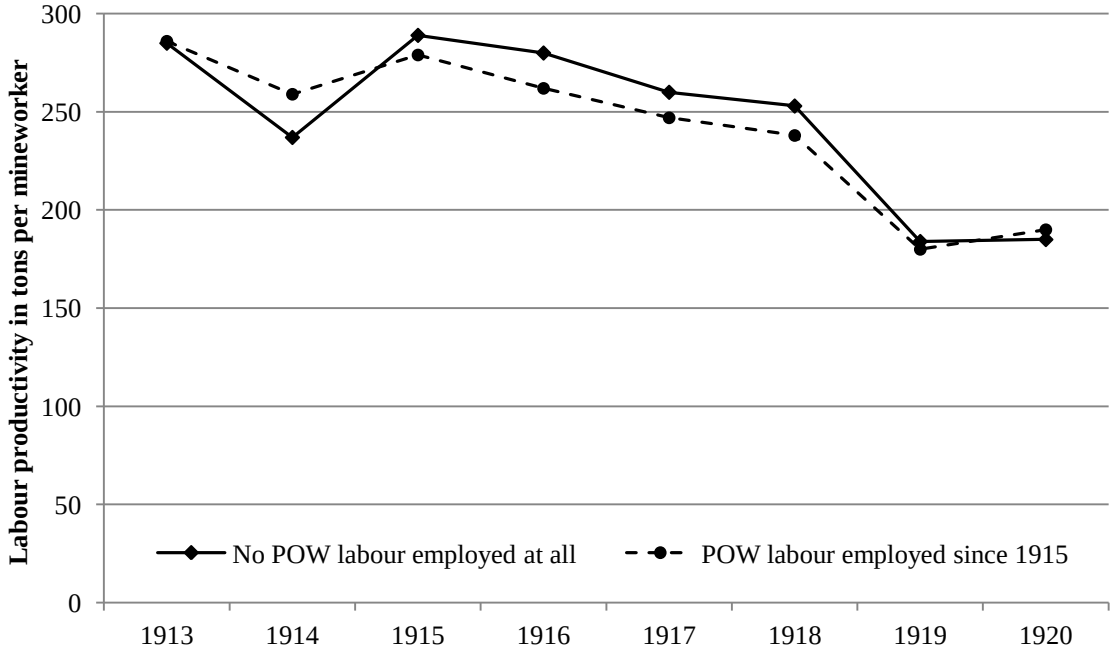
Figure 4: The relationship between treatment level and outcome (1915-18)



Notes: Adjusted data set used. For treatment level definition see Table 4.

Sources: See Table 1.

Figure 5: Labour productivity of treated vs. nontreated mines (1913-20)

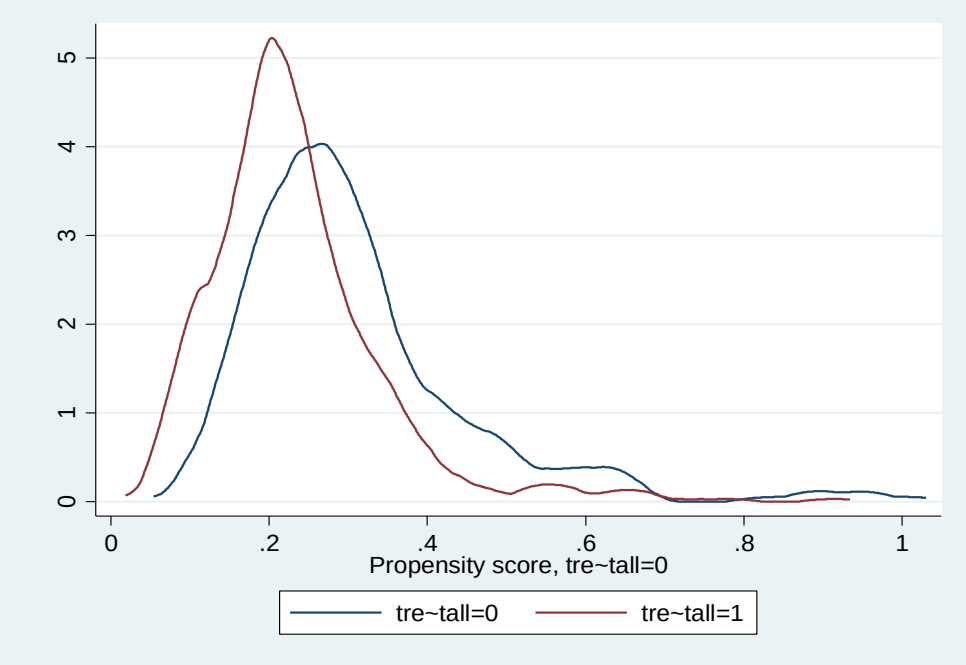


Notes: Labour productivity weighted with employment shares. Adjusted data set used.

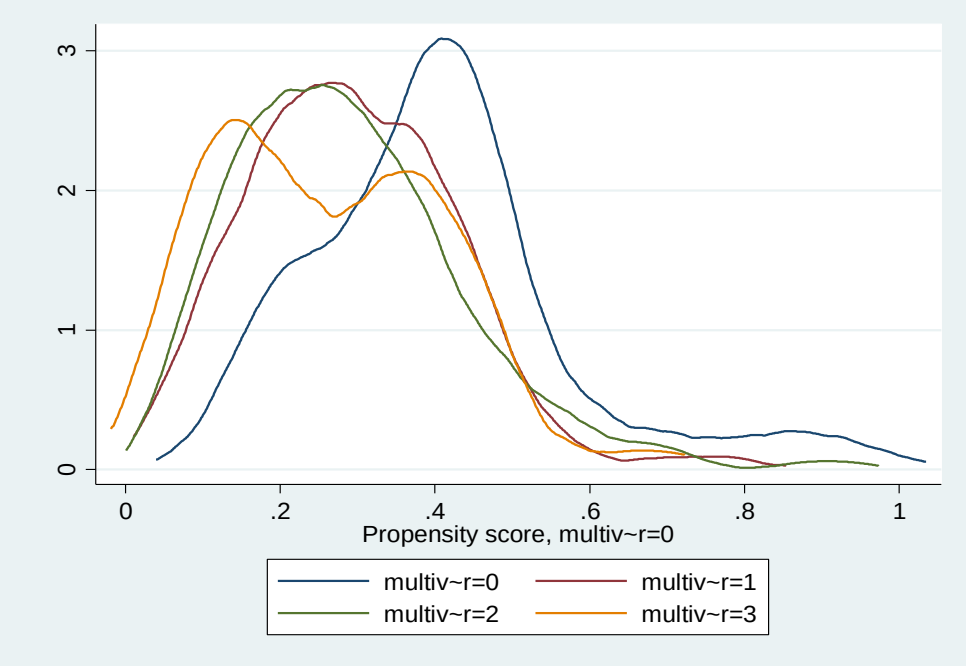
Sources: See Table 1.

Figure 6: A graphical test of the overlap assumption – propensity score distributions

(a) Binary treatment



(b) Multivalued treatment



Notes: Propensity scores given for model 2 (with interactions). Epanechnikov kernel is used.

Sources: Author's own computations.

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