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Labor market spillover effects of a compulsory schooling reform in Germany

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Abstract

Compulsory schooling reforms are often used to estimate monetary returns to education. Such reforms are unrelated to individual characteristics and preferences and thus arguably able to eliminate selection bias. However, as these reforms affect a large number of individuals in the relevant age groups, they might have spillover effects on individuals not directly affected by the reform. Such spillover effects constitute a problem for identification and estimation of returns to schooling. As they are difficult to address, they are mostly ignored in the empirical literature. I show that the introduction of the compulsory ninth grade in Germany led to a labor supply shock that might have increased wages and employment of individuals who were not directly subject to the reform and were assumed not to be affected in previous research. To investigate in this kinds of spillover effects, I exploit the staggered introduction of the compulsory ninth grade across German federal states in a difference-in-differences approach. Based on large scale register and survey data, I find no evidence for persistent spillover effects for men. For women, however, my results suggest that the labor supply shock resulting from the reform may have led to a persistent increase in employment and wages.

JEL Classification: I20, I26, J20, J31

Keywords: Compulsory schooling, Education, Spillover effects, Cohort size, Wages, Employment

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1 Introduction

Compulsory schooling reforms are often used to estimate returns to education as they are unrelated to individual characteristics and preferences and thus arguably eliminate selection bias.[1] However, since these reforms affect a large number of individuals and thus have a high share of compliers, they might have spillover effects. Such spillover effects constitute a problem for identification and consistent estimation of returns to schooling as they imply that individuals who are used to estimate the counterfactual outcome distribution for treated units can be affected by the policy, too. Because this problem is difficult to address, it is often ignored or assumed away in the returns to education literature. This can also be problematic, because the total (general equilibrium) effect of a policy intervention can differ largely from partial equilibrium estimates based on standard policy evaluation techniques that assume away any kind of policy externalities on individuals or the economy as a whole (Abbring and Heckman, 2007).[2]

In this paper I study potential long-run spillover effects of a large scale educational reform that raised length of compulsory schooling from eight to nine years in all West German federal states. I argue that the reform likely exhibited spillover effects on individuals not directly subject to the reform since the reform led to a negative shock in the supply of basic track graduates in the reform year. I suspect that this negative labor supply shock led to a persistent increase in employment and wages of individuals who graduated in close temporal distance to the reform. In a recent study Morin (2015) estimates the wage effects of a positive shock in cohort size that was due to the abolition of the 13^{th} grade in Ontario (Canada). He indeed finds substantial wage losses (between 5 and 9 %) in the short-run for graduates who left school in the double-cohort year. Previous literature has shown that the role of initial labor market conditions are decisive factors also for later life labor market outcomes. Unfavorable labor market conditions such as high unemployment at labor market entry of graduates are known to have large and persistent effects on wages and employment (see e.g. Schwandt and Von Wachter, 2019; Oreopoulos et al., 2012; Kahn, 2010, or Von Wachter, 2020 for an overview). These negative effects can be explained to a large extend by an increase in mismatch between skills provided by graduates and skills demanded by employers during recessions (Liu et al., 2016).

To estimate the long-run spillover effects of the reform on labor market outcomes of individuals who were not directly affected by the increase in years of compulsory education, I exploit the staggered introduction of the compulsory ninth grade during the 1940ies,

[1] For Germany see e.g. Pischke and von Wachter (2008); Kemptner et al. (2011); Kamhöfer et al. (2019); Cygan-Rehm (2021); Margaryan et al. (2021).

[2] For example, Heckman and Lochner (1998) simulate the effects of a tuition policy on college attendance and earning in a framework allowing for general equilibrium effects and compare the estimates to their partial equilibrium counterparts. Their results indicate that the partial equilibrium effects for such a policy differ largely from the general equilibrium effects of the reform.

50ies and 60ies across states in a difference-in-differences design. By doing so, I essentially compare wage and employment differentials of basic track graduates who left school close to the reform year and individuals who left school earlier to the wage/employment differentials of basic track graduates of the same cohorts who lived in other states and thus were not affected by the reform at that time. In order to rule out that the estimates are driven by wage or employment effects of schooling or wage and employment effects due to a changes in the skill composition of the workforce, I exclude individuals who were directly affected by the reform from the estimation sample. The data come from the *Sample of Integrated Labor Market Biographies (SIAB)* and the *Qualification and Course of Employment (QaC)* surveys. While the SIAB provides social security records on earnings on a daily basis for nearly 1.5 million workers and therefore even allows for identification of small effects, its measure of schooling is quite broad. I therefore check the robustness of the results using data from the QaC survey, which provides more detailed information on schooling but has a smaller sample size.

The contribution of the study is threefold. First, it contributes to our understanding of the role of labor market conditions at the time of graduation on subsequent labor market outcomes. While the existing literature mainly examines the effects of (unfavorable) labor market conditions due to a reduction in labor demand, I focus on potentially favorable conditions due to a reduction in labor supply. Second, the results may provide important insights into whether previous studies of the monetary returns to additional years of compulsory education also report effects close to zero because they use as control units individuals who are affected by the reform, contrary to the assumptions made there. Finally, the paper adds to a small but growing strand of the economics of education literature that uses microdata and methods from the program evaluation literature to examine the externalities of education interventions (Bianchi, 2020; Morin, 2015; Moretti, 2004; Duflo, 2004).

My results show that, although there was a substantial drop in the number of basic track graduates in the reform year, there were no long-term spillover effects of the reform on wages and employment for male graduates who left school shortly before the reform. For women, I find some evidence that graduating just before the reform increased employment and hourly wages in the long run, suggesting that the relative shortage of (male) entrants may have been compensated by female high school graduates who otherwise would not have entered the labor market at that time.

The remainder of the paper is structured as follows. In Section 2 I provide a short description of the educational system and the compulsory schooling reform. Section 3 describes the data and explains the empirical strategy. The results are presented in Section 5 and Section 6 concludes.

2 Institutional Background

In Germany students are tracked after they have finished primary school, i.e. after finishing fourth grade. Basically, there are three secondary school tracks available:[3] *Hauptschule*, the basic track that leads to a school leaving certificate after grade 8 or 9; *Realschule*, the intermediate track that leads to a school leaving certificate after grade 10; and *Gymnasium* the academic track that leads to a higher education entrance qualification after grade 13. While students from the basic and intermediate track typically start an apprenticeship after school completion, students from the academic track typically attend a university.

After Word War II all West German states stuck to a three-tiered school system with the aforementioned tracks. However, the length of schooling in the basic track varied over time and states. This is because education policies lay at the responsibility of individual states and not of the federal government. Some, mainly northern states introduced a compulsory ninth grade in the 1940ies, 50ies and early 60ies while others, foremost southern states decided to maintain eight years of compulsory schooling at that time. However, in 1964 all West German states agreed in the *Hamburg Accord* to harmonize length of compulsory schooling and the start of the school year across states. Until 1966 the school year started in April and ended in March of the following year in all states but Bayern, where students finished their grades in July and were promoted to the next grade in August. From 1967 onwards the school year started in August and ended in July all over West Germany. Thus, all states but Bayern had to move the start of the school year from April to August. This was done either by extending the length of the 1966/1967 school year by four months (i.e. having one long school year) or by splitting the transition period April 1966 - July 1967 into two short school years, the first lasting from April 1966 until November 1966 and the second lasting from December 1966 until July 1967 (see e.g. Pischke, 2007; Pischke and von Wachter, 2008; Helbig and Nikolai, 2015).

When the accord was signed, only five states had laws requiring students to stay in school for only eight years (see Table 1). Four of these states, namely Baden-Württemberg, Hessen, Nordrhein-Westfalen, and Rheinland-Pfalz, decided to use the school year transition period to introduce the compulsory grade 9. Thus, the compulsory schooling reform coincided with the short school years in these states. Although this coincidence might affect treatment intensity[4], it should not bias my estimates, as I exclude individuals from

[3] Comprehensive schools that do not track students before the tenth grade were introduced only in the early 1970ies and played a negligible role (as measured by enrollment) in the education system at that time.

[4] While the first cohort with 9 years of compulsory schooling left school 24 months after the last cohort with 8 years of compulsory schooling in all other states, this gap was only 16 months in the four states where the short school years coincided with the compulsory schooling reform (Table 1).

cohorts that were directly affected by the reforms.

Table 1: Years of Schooling for Basic Track Students by State

	BW	BY	HB	HE	HH	NI	NW	RP	SH	SL
First year when all students left school after grade 9	'67	'70	'59	'67	'50	'63	'67	'67	'57	'65
'66/'67 school year type	ssy	–	ssy	ssy	lsy	lsy	ssy	ssy	ssy	ssy
Last year when all students left school after grade 8	'66	'68	'57	'66	'48	'61	'66	'66	'55	'63
Last birth cohort with 8 years of comp. schooling	'51	'53	'42	'51	'33	'46	'51	'51	'40	'48
Months between graduation of last cohort with 8 and first cohort with 9 years of schooling	16	24	24	16	24	24	16	16	24	24

Notes: Largely based on Pischke and von Wachter (2005), Pischke and von Wachter (2008) and Pischke (2007), for details see Footnote 5; ssy: short school year, lsy: long school year; BW: Baden-Württemberg; BY: Bayern; HB: Bremen; HE: Hessen; HH: Hamburg; NI: Niedersachsen; NW: Nordrhein-Westfalen; RP: Rheinland-Pfalz; SH: Schleswig-Holstein; SL: Saarland.

If all basic track students who finished eighth grade in the reform year had to stay one year longer in school due to the introduction of the compulsory ninth grade, then there should be a drop in the number of basic track graduates in the reform year. Figure 1 shows the number of basic track graduates sampled in the *Qualification and Course of Employment (QaC)* surveys by year of graduation (centered around the year of the reform in the respective home state). There is indeed a sizable drop in the number of graduates from basic track schools in the reform year (almost -20% compared to the pre-reform year) which is, however, smaller than expected.

An obvious explanation for the weaker drop in basic track graduated is the coincidence of the short school years and the compulsory schooling reform in Nordrhein-Westfalen, Hessen, Rheinland-Pfalz and Baden-Württemberg. Students from the last cohort with eight years of compulsory schooling left school in March 1966 in these states, while the following cohort – the first cohort with nine years of compulsory schooling – left school in July of the following year after having finished grade 7 in March 1966, grade 8 in November 1966, and grade 9 in July 1967.[5] Other explanations include early introductions of the

[5]The coding in Pischke and von Wachter (2008) seems to differ slightly for these states. According to the working paper version of their article Pischke and von Wachter (2005), the first cohort with nine years of compulsory schooling in theses states was the 1953 birth cohort. However, using the same rule to calculate year of school enrollment and graduation as Pischke and von Wachter (2005), I end up with the 1952 birth cohort being the first cohort with nine years of compulsory schooling. This cohort, was enrolled into school in April 1959, finished grade 7 in March 1966 and then had to stay in school for two additional (school) years. As these two school years were the short school years, students from this birth

Figure 1: Basic Track Graduates by Year of Graduation

Notes: Own calculations based on QaC data. The graph shows the number of basic track graduates by year of graduation, where the year of graduation is centered around the year of introduction of the compulsory ninth grade.

compulsory ninth grade in some counties (see Pischke and von Wachter (2008) for details) and grade repetitions.

3 Empirical Approach

4 Identification and estimation

If basic school graduates achieve better job matches when there are less potential labor market entrants and thus less competitors for a job or apprenticeship, then raising compulsory schooling might increase wages in the long run for those basic track graduates who left school close to the reform. I define treatment as being part of the last cohort of basic track students with eight years of compulsory schooling. Since the timing of the introduction of the compulsory ninth grade varies across states, I use a generalized difference-in-differences (Diff-in-Diff) model with multiple groups as to investigate whether the compulsory schooling reform might have led to persistent wage gains for basic track students who left school in the pre-reform year:

$$y_{isct} = \alpha_s + \lambda_c + \delta_t + \alpha_s \times c_i + \beta last\_cohort_{sc} + \epsilon_{isct} \tag{1}$$

where y_{isct} is either the log wage of or an employment dummy for individual i as measured in year t, who lives in state s and is part of cohort c; α_s are state fixed effects, λ_c denote birth cohort and δ_t year fixed effects, $\alpha_s \times c_i$ are state specific linear birth cohort trends.

cohort should have graduated in July 1967, the first year when all basic track students were obliged to graduate after nine years of schooling.

Finally, *last_cohort* is a dummy for being part of the last cohort with 8 years of compulsory schooling, and ϵ_{isct} is an error term.

In this setting, β is the Diff-in-Diff parameter which gives the effect of graduating as part of the last cohort with 8 years of schooling. Note that other than in a classical Diff-in-Diff setting, the variable *last_cohort* is not determined by a combination of group membership and time in which the outcome is measured (i.e. before or after treatment), but by a combination of group membership (state) and cohort membership (year of birth). Still identification is based on the staggered introduction of the compulsory ninth grade across states. However, because this study focuses on the long-term effects of the reform, it takes advantage of the fact that the staggered implementation of the reform has led to differences in how birth cohorts are affected across states. Thus, by focusing on β_1, I compare the difference in wages between the last cohort of basic track graduates with 8 years of compulsory schooling and previous cohorts to the difference in wages between graduates from the same cohorts but different states, where a compulsory ninth grade was not yet introduced at that time. As it is possible that the labor market shock in the reform year did not only affect the last but also the second to last, third to last etc. cohort with 8 years of education I estimate alternative versions of equation 1. In these versions *last_cohorts* is either a dummy for being part of the last 2 or 3 cohorts with 8 years of compulsory schooling.

For several reasons individuals who left school after the introduction of the ninth grade are excluded from the analysis. First, these graduates might achieve higher wages due to the additional year of schooling. Second, if I would use these cohorts as control units, I had to assume that there were no changes in relative skill prices due to the compulsory schooling reform. Third, since students were enrolled into school in August from 1967 onwards, the cut-off date for enrollment was in the mid of the year for most students with 9 years of compulsory schooling, which makes it more difficult to impute cohort membership and year of graduation from year of birth. Thus, by excluding these cohorts, I can rule out that the estimates capture direct wage effects of the compulsory schooling reform and avoid that the results are driven by changes in skill prices due to the reform.

The main identifying assumption in this setting, the Common Trends (CT) assumption, implies that in absence of the reform, the wage differential between the last cohort with 8 years of compulsory schooling and earlier cohorts in treatment states would have been the same as the wage differential between these cohorts in states without a compulsory schooling reform (at that time). To assess whether this is a reasonable assumption, I estimate a more flexible version of Equation 1 that has the following form:

$$y_{isct} = \alpha_s + \lambda_c + \delta_t + \alpha_s \times c_i + \mu_a + \sum_{r=-9}^{r=-4} \mu_r + \epsilon_{isct} \tag{2}$$

where α_s, λ_c and δ_t are again state, birth cohort and year fixed effects and $\alpha_s \times c_i$ are state-specific trends in birth cohorts. Here, however I do not include one indicator for the last cohort with eight years of education, but instead include several so called event-time indicator variables μ_r for graduation cohorts based on their year of graduation measured relative to the reform year. Let k_s denote the reform year in state s and l_i the year of graduation of individual i, then r_{is} is defined as $r_{is} = l_i - k_s$. Thus, μ_{-9} is a dummy for having graduated nine years before the reform, μ_{-8} is a dummy for having graduated eight years before the reform, etc. Finally, μ_a is a dummy variable that takes on the value 1 if an individual has graduated more than 10 years before the reform, i.e. if $r_{is} < 10$, and $\mu{-}10$ is normalized to zero.

Note that when estimating 2, I exclude cohorts that graduated in the 3 years prior to the introduction of the compulsory ninth grade, i.e. all cohorts that graduated in close temporal proximity to the reform and thus are arguably affected by the potential labor market shock of the reform. In doing so, I follow arguments of de Chaisemartin and D'Haultfoeuille (2020) who suggests to exclude treated individuals from the estimation when assessing the credibility of the common trends assumption in settings with potential effect heterogeneity, since otherwise the estimates for μ_r that are used to assess the common trends assumption can be contaminated by (the dynamics of) treatment effects.[6]

Another potential thread to the empirical strategy is that students might manipulate treatment status. This, however, requires them either to move to another state, to change the school track, or to delay graduation/push graduation forward. I argue, that it is rather unlikely that students or their parents move to another state due to a reform that does not affect them directly, especially since moving across state borders is quite seldom.[7]. Moreover, there seems to be little incentive to delay graduation by repeating a grade, as repeating a grade could be seen as a bad signal on the labor market. Skipping classes, on the other hand, is the absolute exception in the German education system and only possible under exceptional circumstances. Since students or their parents have to decide on a school track already after the fourth grade and switching from one school track to another is rare, I expect selection into treatment not to be a serious problem here.

[6] As shown by Goodman-Bacon (2021) the "contamination" results from using already treated units as control units and can occur in the presence of effect heterogeneity.

[7] Pischke (2003) reports that according to the ALLBUS survey more than 80% of respondents live in the same state as they have lived at the age of 6 (the usual school entrance age in Germany)

4.1 Data and Measurement

The main data source is the *Sample of Integrated Labour Market Biographies (SIAB)*,[8] a data set that provides social security records on a daily basis for about 2% of all individuals who i. have been in employment subject to social security, or ii. received unemployment benefits in the period 1975-2010.[9] For people sampled in the SIAB, the data provides information on all employment spells between 1975 and 2010, including the start and end date of each spell, daily wages up to the social security contribution ceiling (*Beitragsbemessungsgrenze*), occupation, a very broad measure of working time, the district region of the work place, and some basic demographic information such as year of birth, gender, nationality as well as schooling and vocational training (Vom Berge et al., 2013; Dorner et al., 2010). I use the spell data to generate a panel data set with yearly observations that includes the relevant information for the period 1975-1990 as present at the 1^{st} of July of each year.

While the great strength of SIAB is its high quality information about daily wages for a large number of employees (in total about 1.5 million), it also has two disadvantages. First, and more important, there is no distinct measure of school track and no information about year of graduation available in the SIAB. Instead, highest educational attainment and vocational training is combined in one variable, making it impossible to identify individuals' school tracks unambiguously. Second, SIAB does not contain the number of working hours. The only information related to working time included in the data set is a variable that allows to distinguish between people who work more than a certain number of hours a day and people working less than this amount (depending on the year the threshold varies between 15 and 20 hours). I thus cannot calculate hourly wages, but have to stick to daily wages when using SIAB.

To deal with these issues I also use the *Qualification and Course of Employment (QaC)*[10] surveys as a second data source. The QaC surveys have been conducted by the Federal Institute for Vocational Education and Training (BIBB) in cooperation with the Institute for Employment Research (IAB) in the years 1979, 1985/86, 1991/92 und 1998/99. They provide representative information about current and past employment (wages, occupations, vocational requirements, working conditions) and education among employees and self-employed persons. Although the sample size of these cross-sectional surveys are much smaller than the sample size of SIAB, they have some attractive features. First, they provide more detailed information about educational attainment than SIAB. The QaC data

[8]The data basis is the scientific use file of the Sample of Integrated Labor Market Biographies (SIAB-R 7510) provided by the Federal Institute for Employment Research (IAB).

[9]SIAB also covers information on individuals who have been employed in marginal part-time employment, participated in programs of active labor market policies, or have been registered as job-seekers, but only for some periods and not the entire period covered by SIAB.

[10]The data used in this paper were made available by the Zentralarchiv für Empirische Sozialforschung, Universität zu Köln.

does not only include information about highest educational attainment but also about year of graduation. Second, the QaC data contains the number of hours worked per week which can be used – in combination with weekly wages – to calculate hourly wages.

For both data sets applies that they do not include direct information about school track choice and state of residence at the age of school entry. I thus follow the standard procedure (see e.g. Pischke and von Wachter, 2008; Kemptner et al., 2011; Margaryan et al., 2021) and use highest educational attainment to impute school track and approximate state of residence during time at school by current state of workplace (SIAB) and state of residence (QaC), respectively. I do not expect this approach to have a sizable impact on my estimates, as mobility across state borders is quite low (Pischke, 2003) and changing the school track is quite complicated in Germany. Based on (imputed) school track, I then calculate the year of graduation for basic track graduates according to the following rule: year of birth plus 7 (the year after birth in which students enter school) plus 8 (number of years in school). Finally, comparing expected year of graduation and the year of the introduction of the compulsory ninth grade, I define the treatment status. Table A1 in the appendix gives an overview about the treatment status (*last_cohort*) by year of birth and state and cohorts included in the sample (see also Section 4).

When using the SIAB, I need an additional assumption to define treatment status. In the SIAB one can only distinguish between individuals who attended basic or intermediate track schools and individuals who attended academic track schools. I therefore assume that all students who do not hold a higher education entrance qualification, to have attended basic track schools when using the SIAB data. This, of course, means that some individuals are misclassified with respect to school track attendance.[11] Contrasting the results based on the SIAB with the results based on the QaC data helps to evaluate whether this assumption is crucial for my findings. It does not seem to be.

As a first outcome measure I use the log daily wages in the SIAB data and log hourly wages in the QaC data (both in 1995 Euro). Due to the relatively high level of collective bargaining coverage in Germany, which might prevent employers to pay higher wages to attract employees in times of scarce labor supply, especially in the apprenticeship occupations, I also consider possible effects of the reform on the employment probability of those not directly affected by the reform. I do this using a pseudo-panel that is constructed based on the SIAB panel. This pseudo-panel contains the same information as the panel for the wage regression plus a dummy variable for employment. This dummy variable indicates whether a person is in employment (subject to social security) or not. It takes the value 1 for persons who are in employment (subject to social security) according to the SIAB data on the reference date. Persons who do not appear in the raw data on the

[11]Note, however, that the intermediate track was not very popular until the 1970ies. In the period 1952-1969 only between one in ten and less than one in five students in the eighth grade attended a intermediate track school (Köhler and Lundgreen, 2014).

reference date of the respective year (i.e. who were not employed subject to social security on the reference date), but were employed on any other reference date during the period 1975-1990, are assigned the value 0.

Table 2: Descriptive Statistics

	SIAB		QaC	
	mean	sd	mean	sd
Outcome variables				
Log wage^{+}	4.112	0.462	2.221	0.439
Employed (d)$^{-}$	0.739	0.439		
Treatment variables				
Last cohort (d)	0.036	0.185	0.043	0.202
Last 2 cohorts (d)	0.070	0.255	0.088	0.284
Last 3 cohorts (d)	0.113	0.317	0.143	0.350
Control variables				
Female (d)	0.366	0.482	0.307	0.461
Age	43.281	7.669	44.540	7.536
Year of birth	1938.677	7.057	1940.163	6.911
Year	1981.958	4.498	1984.703	6.392
BW (d)	0.177	0.382	0.156	0.363
BY (d)	0.201	0.401	0.202	0.401
HB (d)	0.011	0.105	0.009	0.095
HH (d)	0.009	0.096	0.004	0.061
HE (d)	0.099	0.299	0.102	0.302
NI (d)	0.100	0.299	0.106	0.308
NW (d)	0.306	0.461	0.306	0.461
RP (d)	0.057	0.232	0.076	0.266
SH (d)	0.023	0.150	0.022	0.146
SL (d)	0.016	0.127	0.017	0.129
Observations	2,592,588		24,055	

Notes: (d) indicates dummy variables; $^{+}$ Wage refers to the daily wage in the SIAB and to the hourly wage in the QaC data; $^{-}$ The number of observations when using the pseudo-panel and employment as dependent variable is 3,512,494; BW: Baden-Württemberg; BY: Bayern; HB: Bremen; HE: Hessen; HH: Hamburg; NI: Niedersachsen; NW: Nordrhein-Westfalen; RP: Rheinland-Pfalz; SH: Schleswig-Holstein; SL: Saarland.

The data are restricted in all analysis to basic track graduates (as defined above) who are between 20 and 58 years old and who were born after 1922 and were not subject to the introduction of the compulsory ninth grade in all analyses. Table 2 shows sample statistics for both data sets. The average log wage is 4.1 in the SIAB and 2.2 in the QaC data, corresponding to a daily wage of 61.07 Euro (SIAB) and a hourly wage of 9.22 Euro (QaC). Individuals who are part of the sample are employed (subject to social security) on average on 74% of all 1^{st} of July during the period 1975 to 1990. About 4% of the observations belong to individuals who are part of the last cohort with 9 years of compulsory schooling. The average age is 43-45 years. In general, the descriptive statistics in SIAB and QaC are similar. The largest differences can be found for the proportion of females. This is about 30% in the SIAB data and 36% in the QaC data. The difference is not surprising, since women are relatively often in atypical employment and thus less often sampled in the SIAB compared to the QaC data.

5 Results

Table 3 gives the estimation results for the Diff-in-Diff model based on the SIAB for all basic track graduates and separately for men and women. Each row represents a separate regression. The first block shows the results for the wage regressions, the second block those for the regressions with the employment dummy as dependent variable. Within each block, the estimates reported in the first row are for the baseline treatment where the introduction of the compulsory ninth grade is assumed to affect only the last cohort with 8 years of compulsory schooling. The estimates in the second and third row account for the possibility that the negative shock in basic track graduates in the reform year might not only affect the employment prospects of the last cohort of graduates with 8 years of schooling but also of the second to last and third to last, respectively.

Table 3: Baseline Results

	All		Men		Women	
dep. var.	Coeff.	(S.e.)	Coeff.	(S.e.)	Coeff.	(S.e.)
log daily wages						
Last cohort	-0.001	(0.006)	0.002	(0.003)	-0.004	(0.017)
Last 2 cohorts	0.000	(0.007)	0.004	(0.003)	-0.000	(0.020)
Last 3 cohorts	0.009*	(0.004)	0.007**	(0.003)	0.018	(0.013)
Obs.	*2,617,600*		*1,644,659*		*972,941*	
employment						
Last cohort	0.003	(0.002)	0.000	(0.004)	0.008*	(0.004)
Last 2 cohorts	0.002	(0.002)	0.002	(0.005)	0.005	(0.006)
Last 3 cohorts	0.003	(0.003)	-0.003	(0.004)	0.013*	(0.007)
Obs.	*3,512,494*		*2,029,057*		*1,483,437*	

Notes: Coefficient β from estimations of Equation 2 based on data from the SIAB; Standard errors clustered at state level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Each estimate comes from a separate regression; Controls include state, year of birth and year fixed effects, a gender dummy and state-specific year of birth trends.

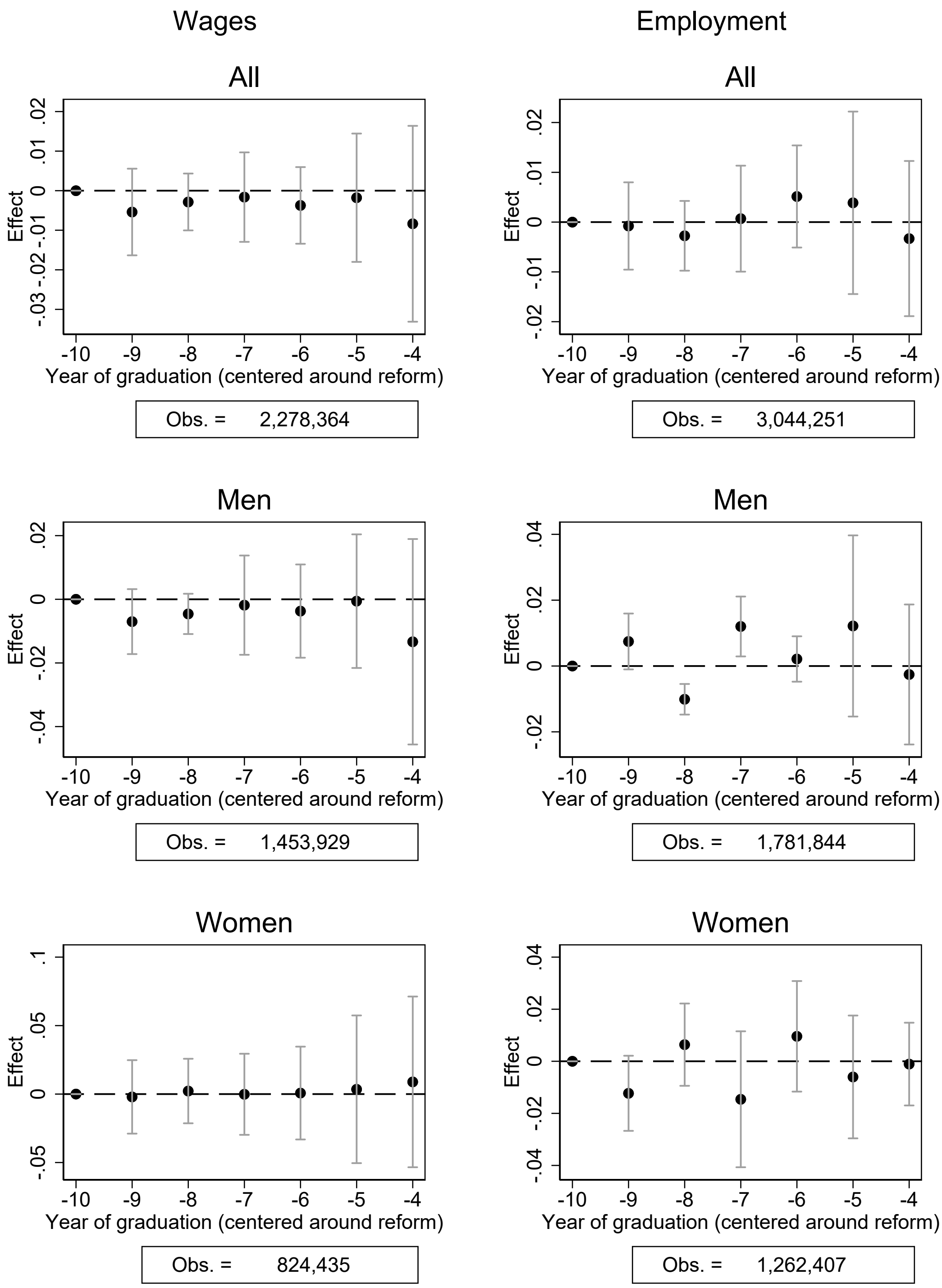
The estimates suggest that there was no persistent effect of graduating shortly before the reform for basic track graduates. According to the results, graduating in the pre-reform year (row 1) is associated with a wage loss by 0.1%. The estimated coefficients for graduating in one of the last two (row 2) are positive, but economically and statistically not significant. Only for those who graduated 3 years prior to the introduction of the compulsory ninth grade (row 3) I observe a slightly larger effect that is statistically also marginally significant. The effect size is, however, still rather small (less than 1%). Furthermore, if the shock in potential labor market entrants due to the introduction of the compulsory ninth grade led to better job (or apprenticeship) matches and thus persistent wage gains for labor market entrants one would expect a positive effect first and foremost for those graduating in the last pre-reform year and, to a smaller degree, for those graduating one or two years earlier. However, my estimates do neither suggest positive wage effects for either of these cohorts nor do they decrease with increasing distance to the reform year.

The results stratified by gender in the following columns are similar with the pooled results. The estimates for graduating in the last pre-reform year are small and not significant. Not surprisingly, considering the results for both genders, I find the largest estimates for the broadest treatment definition. As these estimates are nevertheless rather small and since I cannot provide a reasonable explanation for these results, I prefer to interpret them as a statistical artifact at this stage of the analysis instead of evidence for spillover effects of the compulsory schooling reform.

The second part of Table 3 shows the results for the employment dummy as dependent variable. In line with the results for wages, I find no evidence that graduating prior but close to the reform year had an effect on employment prospects for men. All estimates for men are almost indistinguishable from zero. For women the results are somewhat less clear cut. All of them are positive and two out of three also statistically significant and thus seem to be compatible with the aforementioned hypothesis, at least to some extend. The largest estimate for women indicates that graduating in the 3 years prior to the introduction of the ninth grade increased the likelihood of being employed by 1.3 percentage points, corresponding to an increase of 2% as measured at the mean of the dependent variable.

The validity of the estimation approach used here hinges on the assumption of common trends. Figure 2 shows how wages (left panels) and employment (right panels) of graduates developed by year of graduation (centered around the reform year), conditional on fixed effects and relative to wages/employment of the cohort that graduated 10 years before the reform (reference group). For neither outcomes there is any evidence for a violation of the CT assumption. This impression holds when looking on men and women separately. In total, two out of the 36 (i.e. 5.56% of) estimates differ statistically from zero at the 5% significance level. This is what one would expect simply by chance. Even if ignoring statistical significance, I do not find any indication for a systematic deviation from the common trend assumption, instead the estimates fluctuate randomly around zero.

Figure 2: Assessment of CT Assumption



Notes: Coefficients μ_r from estimations of Equation 2 based on data from the SIAB. μ_{-10} is restricted to zero. 95% confidence intervals reported. Standard errors clustered on state level.

As the SIAB data does not include information on the year of graduation, the treatment assignment is based on year of birth in the results presented so far. Using calculated instead of actual year of graduation to assign treatment status might introduce measurement error and thus bias. To prove robustness of results with respect to the assignment to treatment and control group, Table 4 repeats the analysis but now using QaC data. The first three columns of the table repeat the baseline analysis and report the results for the treatment assignment based on year of birth (column header *calculated year of graduation*). Columns 4-6 give the results for the treatment assignment based on *Stated year of graduation*. Note that the dependent variable is now the log hourly wage instead of the log daily wage.

When looking at the results for the QaC data, there are two notable things. First, whether I assign treatment status on the basis of home state and year of birth or whether I use self-stated year of graduation does not seem to matter much, suggesting that bias due to measurement error or self-selection into treatment is negligible. Second, there are marked differences between the results for men and women when using the QaC data instead of SIAB. While the results for men are in line with the baseline results based on the SIAB data, pointing to zero effects of graduating shortly before the reform, the results for women strongly suggest positive and statistically significant effects. Specifically, I find that graduating in the last year before the reform took effect, increased hourly wages by 6.4-7.4%. For the broader treatment definitions which include also graduates who graduated two and three years, respectively, before the reform, the estimates are somewhat smaller but still sizable. Due the reduced sample size and thus low precision they are not statistically significant. The overall impression, however, fits to the hypothesis that the advantage of graduating close to the reform decreases with increasing distance between year of graduation and year of the reform.

Table 4: Results QaC

	Calculated year of graduation			Stated year of graduation		
	All	Men	Women	All	Men	Women
dep. var.	Coeff. (S.e.)	Coeff. (S.e.)	Coeff. (S.e.)	Coeff. (S.e.)	Coeff. (S.e.)	Coeff. (S.e.)
log hourly wages						
Last cohort	0.027	-0.000	0.074**	0.020	0.001	0.064**
	(0.024)	(0.021)	(0.028)	(0.019)	(0.020)	(0.021)
Last 2 cohorts	0.016	-0.007	0.056**	0.016	-0.010	0.061
	(0.022)	(0.014)	(0.036)	(0.023)	(0.014)	(0.040)
Last 3 cohorts	0.024*	0.012	0.045*	0.024	0.009	0.050
	(0.015)	(0.010)	(0.029)	(0.016)	(0.010)	(0.032)
Obs.	*24,055*	*16,682*	*7,373*	*23,915*	*16,584*	*7,331*

Notes: Coefficient β from estimations of Equation 2 based on data from the QaC; Standard errors clustered at state level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Each estimate comes from a separate regression; Controls include state, year of birth and year fixed effects, a gender dummy and state-specific year of birth trends

Assuming that the results for women in the QaC data really capture the effects of the reform, there are at least two potential explanations for the differences compared to the results for women in the SIAB.[12] The first explanation relates to the fact that SIAB and QaC do not sample the same population. While the SIAB sample includes only individuals subject to social security (e.g. no self-employed or marginally employed workers and no civil servants), the QaC sample includes individuals in any type of work. Thus, if graduating shortly before the reform has increased wages only for women and only in sectors/occupations not sampled in the SIAB, e.g. for women who are in marginal employment, this might explain the differences. The second, also speculative explanation relates to differences in the definition of the dependent variable, i.e. that I use daily wages in the SIAB but hourly wages in the QaC data: It could be that the shortage of (male) workers and potential (male) trainees led to better job offers and higher demand for female workers. This may have led to more women entering the labor/apprenticeship market immediately after graduation who otherwise would not have entered (at that time). In the long run, this might then reflect in higher hourly wages. If, however, these women respond to an increase in higher hourly wages with a reduction of working hours (e.g. because women "choose" their total income), this could explain why effects can be found for women in the QaC for hourly wages but not for daily wages in the SIAB data. Considering that the women in the estimation sample were born before 1953 and thus on average had a rather low labor market attachment compared to men and remembering that in the SIAB I indeed find weak evidence for positive employment effects of the labor supply shock, this seems to be a reasonable (though still somewhat speculative) explanation.

6 Conclusion

In this paper I investigate in potential long-run labor market spillover effects of the introduction of a compulsory ninth grade in West German basic track schools during the 1940ies, 1950ies and 1960ies. The reform led to a negative shock in basic track graduates in the reform year which might have improved job match quality, career opportunities and thus employment prospects and wages of individuals who graduated close to the reform year. To estimate the effects of the labor supply shock on later wages and employment, I make use of large administrative and survey data and exploit the staggered introduction of the compulsory ninth grade across West German federal states in a Diff-in-Diff design.

I do not find evidence that the shock in the supply of labor market entrants due to the compulsory schooling reform had a long-run effect on labor market outcomes of men who graduated shortly before the reform. However, I find some evidence that the reform has

[12] A violation of the CT assumption for the QaC sample does not appear to be one of them, as shown in Figure 3 in the appendix.

increased employment and hourly wages (but not earnings from employment) of women who graduated in close temporal distance. A potential explanation for these results is that employers tended to fill vacant (apprenticeship) positions with female graduates who otherwise would not have entered the labor market at that time and that the observed effect for women is a result of increased work experience among these women. Note that the finding of zero long-run effects for men does not necessarily imply zero effects in the short or medium run. It might also be, that positive wage or employment effects of favorable labor market conditions vanish after some years, just as the negative wage effects of disadvantageous labor market conditions (e.g. high unemployment) seems to fade after 10-15 years (Von Wachter, 2020), and that these dynamics are not symmetric for men and women. While I cannot provide estimates for short- and medium run effects due to data limitations and thus cannot provide evidence for one explanation or the other, I can at least show that such effects – if they exist – are not very persistent for men.

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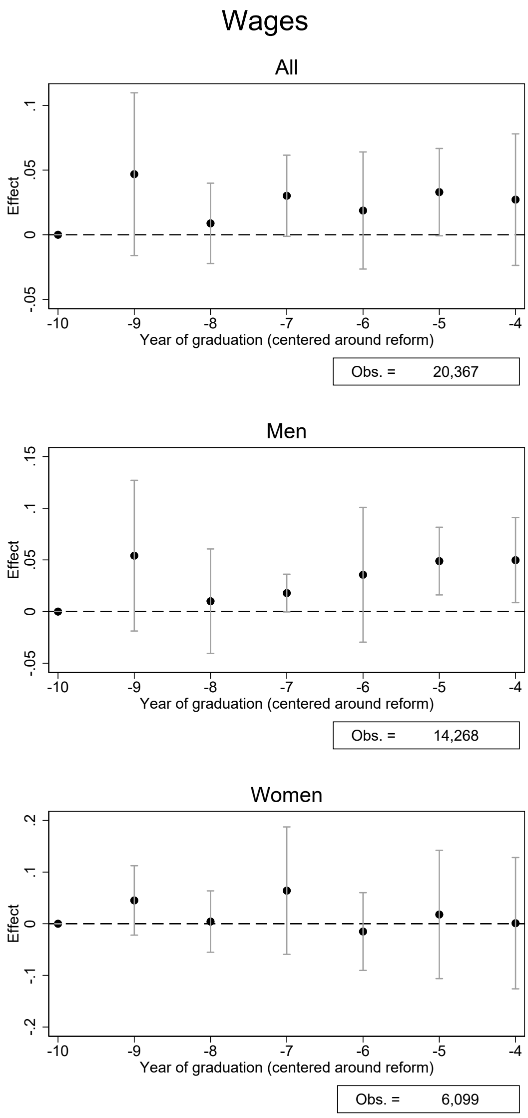
Appendix

Figure 3: Assessment of CT Assumption in the QaC Data



Notes: Coefficients μ_r from estimations of Equation 2 based on data from the QaC. μ_{-10} is restricted to zero. 95% confidence intervals reported. Standard errors clustered on state level.

Table A1: Sample and Baseline Treatment by Year of Birth (Basic Track)

	'21	'22	...	'32	'33	'34	...	'39	'40	'41	'42	'43	'44	'45	'46	'47	'48	'49	'50	'51	'52	'53	'54	'55
HH	0	0	0	0	**1**	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
SH	0	0	0	0	0	0	0	0	**1**	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
HB	0	0	0	0	0	0	0	0	0	0	**1**	–	–	–	–	–	–	–	–	–	–	–	–	–
NI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**1**	–	–	–	–	–	–	–	–	–
SL	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**1**	–	–	–	–	–	–	–
NW	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**1**	–	–	–
HE	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**1**	–	–	–
RP	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**1**	–	–	–
BW	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**1**	–	–	–
BY	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**1**	–

Notes: Cohorts highlighted in gray are included in the sample; 0 indicates graduation before reform year; 1 indicates graduation in reform year.