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## RESEARCH ARTICLE OPEN ACCESS

# The Effect of Compulsory Schooling on Vaccination Against COVID

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## ABSTRACT

We study the effect of education on vaccination against COVID in Germany in a sample of individuals above the age of 60. In ordinary least squares regressions, we find that, in this age group, one more year of education goes along with a 0.7 percentage point increase in the likelihood to get a COVID vaccination. In two stage least squares regressions where changes in compulsory schooling laws are used as exogenous variation for education, the effect of an additional year of education is estimated to be zero. The results hold for the compliers to the policy change which are older individuals at the lowest margin of education.

**JEL Classification:** I12, I26, J14

## 1 | Introduction

Vaccination is considered an important—maybe even the most important—strategy to overcome the COVID crisis, with the WHO aiming for a 70% vaccination coverage of the general population (WHO 2022). The first vaccines were developed immediately after the outbreak of COVID and large vaccination campaigns started around the end of 2020 in the high income countries. While, at that time, vaccination was mainly seen as a way to stop infections and to achieve herd immunity, several mutations of the virus made this goal hardly achievable. However, the fundamental benefit of vaccination seems to be a strong reduction of severe illness and mortality after a COVID infection (Nasreen et al. 2022; Nordström, Ballin, and Nordström 2022). High vaccination rates and mutations to less lethal variants of the virus are considered the two dominating reasons that most societies largely turned back to normal life throughout the year 2022 even though COVID infections remained on a high level (Robert Koch-Institut 2022b).

While in the first half of 2021 undersupply of vaccines was the most important problem, this was solved—for high-income countries—around mid of 2021. Then, however, a second problem came up: too low vaccination rates in order to achieve herd immunity. Researchers around the world and across fields of study try to understand the determinants of vaccination willingness and hesitancy, which is necessary to improve the success and acceptance of vaccination campaigns. While this is not necessarily important anymore for the current version of the Corona virus, this knowledge seems to be of high value for future pandemics but also for endemic viruses such as influenza and potential mutations of the Corona virus.

An important determinant of vaccination many scholars can agree on is education. Many studies report positive associations between COVID vaccination willingness and educational status around the world. See, for example Cascini et al. (2021) for a general overview and Bergen et al. (2023), Borgia et al. (2022), Graeber, Schmidt-Petri, and Schröder (2020), Mondal, Sinharoy,

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and Su (2021), Bergmann et al. (2021), Walkowiak and Walkowiak (2021), Huebener and Wagner (2021), Humer et al. (2023), for a non-exhaustive list of studies. Some of these studies use actual vaccination as outcomes, others use stated willingness. Moreover, these studies differ in important aspects such as countries, how education is measured, when the data are collected (e.g. before/after vaccination was available, during/after local peaks of infection rates). Yet, a typical result is the positive—though not necessarily economically large—association of education and vaccination willingness. This is an important finding as this education gradient can be interpreted as a problem of distributional justice that, in some way, would need to be addressed in societies that aim at preventing unjust inequalities in health. It adds to the general finding that health care and prevention are inequitably distributed (Carrieri and Wuebker 2013; Van Doorslaer, Koolman, and Jones 2004).

Another question is whether the relationship between education and vaccination is causal. While the aforementioned studies—especially in their sum—provide compelling evidence of a correlation between educational attainment and COVID vaccination, to the best of our knowledge, no study investigated whether schooling is a causal factor of vaccination willingness. Certain character traits, like time or risk preferences, might drive both schooling and vaccination willingness (Nuscheler and Roeder 2016). This would be important to understand when it comes to how this education gradient in vaccination could be tackled. There exists some evidence of the impact of education on general health behaviors. Brunello et al. (2016) show that schooling has a negative impact on smoking, drinking, exercise and the BMI, Fletcher and Frisvold (2009) show that college education increases the use of preventive health care services, such as physical examinations, dental examinations, cholesterol tests, and flu shots. In their review, Galama, Lleras-Muney, and van Kippersluis (2018) conclude that OLS estimates indicate a strong positive association between schooling and health behavior—in terms of smoking and obesity. Quasi-experimental methods usually result in larger confidence bands, they still suggest positive effects. While our study mainly speaks to the effects of education on health behavior, a potential link between education and health behavior is health status. For example, individuals with chronic conditions were prioritized in the COVID vaccination campaign in Germany (Vygen-Bonnet et al. 2021a). The literature on causal effects of education on health is quite large. For example, Schmitz and Tawiah (2023), list 22 studies that exploit exogenous variation in education. Nevertheless, the findings are not conclusive. According to Schmitz and Tawiah (2023) more than half of the studies do not find effects of education on health while there are also studies that do find positive effects. All in all, to the best of our knowledge, there is no study that claims to identify the causal effect of education on vaccination, in particular vaccination against COVID. It is our main contribution to the literature to fill this gap and provide a first study in this direction.

In this paper, we study the effect of education on vaccination against COVID in West Germany using information from three different surveys administered between mid of 2021 and beginning of 2022. These surveys relate actual vaccination status to educational attainment of—in our estimation sample—more than 6000 individuals. Our identification strategy makes use of

changes in compulsory schooling laws and allows us to estimate local average treatment effects for individuals between 59 and 95 years of age. The reforms increased compulsory schooling from eight to 9 years in the 1940s–1960s in Germany with some variation in timing across federal states. Our results only hold for a specific subgroup of individuals but, arguably, a highly important one: older individuals—usually denoted the high risk group and thus benefiting most from vaccinations—at the lowest margin of education (those forced to increase their years of education from eight to 9 years). As a result, we do not find a significant effect of an additional year of compulsory schooling on vaccination against COVID. In several different specifications of instrumental variables estimations, the estimated effects fluctuate around zero.

This also holds for an event-study specification for the effect of the compulsory schooling reform on vaccination that accounts for potential problems of heterogeneous effects in staggered-entry settings by applying the estimator proposed by Sun and Abraham (2021). Finally, given that we have a small sample and imprecise estimates are an issue, we carry out an ex-post power analysis as suggested by Black et al. (2022). Doing so, we identify a minimum detectable effect size of the compulsory schooling reform of an 0.8% point increase in COVID vaccination.

We do not claim that our main result—no sizable local average treatment effect of education on vaccination against COVID—has high external validity and can be generalized to other age groups or other education levels. Still, we think that this is an important piece of evidence and a start to create a picture on the causal effect of education on COVID vaccination. Moreover, getting results that only hold for specific subgroups of compliers is inherent to reduced-form instrumental variables regressions of all kind, not just this study.

The remainder of this paper is structured as follows. In Section 2 we describe the institutional background on COVID vaccination and education in Germany. In Section 3 we present the data and the empirical approach. Results are reported in Section 4 followed by the conclusion in Section 5.

## 2 | Institutional Background and COVID Vaccination in Germany

In this section we present the institutional setting. This includes the German educational system, with a focus on the schooling reforms that we use as exogenous variation, as well as the course of the COVID pandemic in Germany and the vaccination campaign.

### 2.1 | Educational System and Schooling Reforms

Starting at the age of six, children in Germany spend their first four school years in primary school, before switching to one of the three secondary school tracks. These tracks can be differentiated into basic (*Hauptschule*), intermediate (*Realschule*) and high schools (*Gymnasium*). The basic track (up to 8th or 9th grade) prepares students for apprenticeship, the intermediate track (up to 10th grade) qualifies students for apprenticeship or

training in white collar jobs, and the high school certificate (up to 12th or 13th) gives access to academic education in colleges or universities. West German basic track schools used to cover grades five to eight, till compulsory schooling years were increased to nine years in the period between 1946 and 1969. This reform was implemented in different years by the various states, as decisions and policies regarding the educational system in Germany are made at the federal state level. Some states introduced a compulsory ninth grade earlier, while the majority of the states only introduced an additional year of schooling due to the Hamburg Accord (*Hamburger Abkommen*) in 1964 (Kamhöfer and Schmitz 2016). There exist different specifications for when the reforms occurred and which cohorts were affected, however, we use the reforms as described in Begerow and Jürges (2022), as their research process for identifying the timing of reforms seems to be the most thorough one. See Table 1 for the reform years. The reform was introduced due to a shortage in labor market opportunities and apprenticeships for

school leavers, and to also increase the school leaving age (see Pischke and von Wachter 2008, for details).

Coinciding with these extensions of compulsory schooling was the introduction of two short school years (SSY), in 1966 and 1967 in some states. The start of the school year moved from Spring to Fall but it was already in Fall for Bavaria, see Pischke and von Wachter (2008) for details. However, it is commonly found that these only have little explanatory power for health outcomes (Kemptner, Jürges, and Reinhold 2011), which is why we only include them as a robustness check.

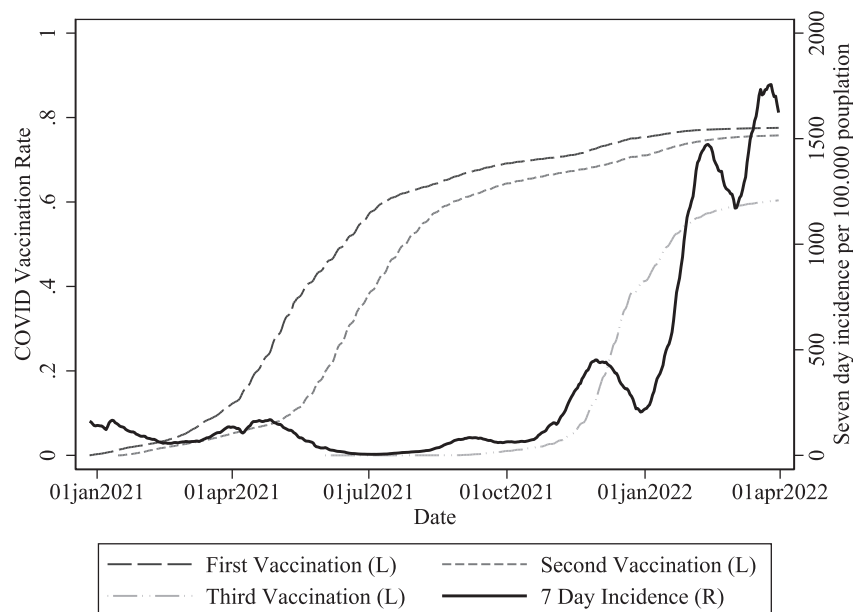
## 2.2 | COVID Vaccination

The first case of COVID in Germany occurred in January 2020 (Rothe et al. 2020). The first vaccine was approved by the European Medical Agency (EMA) at the end of December 2020,

**TABLE 1** | Reform years and corresponding first birth cohorts.

Federal state	Pivotal birth cohort	Reform year
Schleswig Holstein	1932	1947
Hamburg	1931	1946
Lower Saxony	1947	1962
Bremen	1944	1959
North Rhine-Westphalia	1951	1966
Hesse	1951	1966
Rhineland Palatinate	1952	1967
Baden-Wuerttemberg	1952	1967
Bavaria	1954	1969
Saarland	1943	1958

Source: Begerow and Jürges (2022). Pivotal cohort is the first birth cohort the reform applies to.



**FIGURE 1** | Vaccination status and 7 day incidence in Germany. L, Left-hand side axis; R, Right-hand side axis. This figure does not use the estimation sample used in the paper but shows official nationwide numbers. Source: Bundesministerium für Gesundheit (2022) and Robert Koch-Institut (2022a).

vaccinations started in Germany shortly after (Die Bundesregierung 2020). While at first there was a prioritization of the vaccination for those who were at risk of severe consequences of an infection (Vygen-Bonnet et al. 2021a), this was lifted in June 2021, when enough vaccines were available (Vygen-Bonnet et al. 2021b).<sup>1</sup> Figure 1 shows the share of the vaccinated population together with the 7 day incidence between January 2021 and April 2022. The share of individuals having received at least one vaccination (black long-dashed line) is increasing slowly at first due to a limited supply of vaccination doses. From April 2021 to July 2021 it increases sharply from about 12% to more than 60% and then converges to around 80%. The vaccination rate for the second COVID vaccination (gray short-dashed line) follows a similar pattern with a delay of 2 month. The third vaccination was available as of June 2021 (gray dash-dot) and the share of vaccinated converged to a lower number of around 60%. In this paper, we restrict the analysis to the first vaccination dose against COVID.

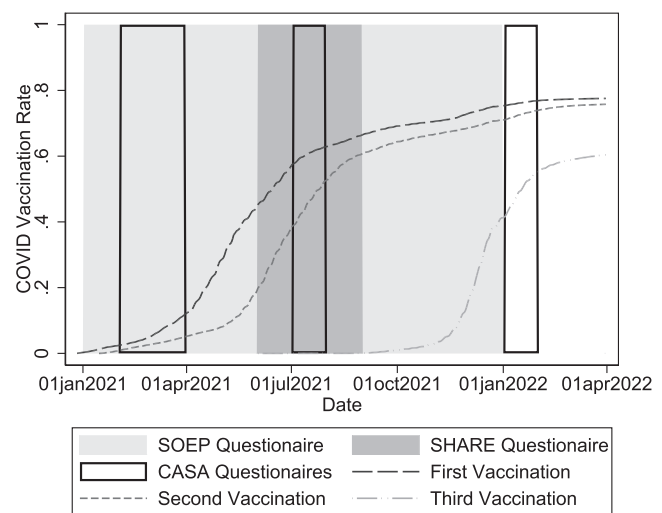
### 3 | Data and Empirical Approach

#### 3.1 | Sample Selection and Outcome Variables

We combine three data sources. The first is the CASA monitor data set (henceforth called *CASA data*), an online-survey put together by infas 360.<sup>2</sup> This representative survey consists of three cross sections with each around 10,000 respondents. The first wave was conducted in February and March 2021, the second wave in July 2021 and the third in January 2022.<sup>3</sup> We augment the CASA data with the German part of the Survey of Health, Aging and Retirement in Europe (SHARE)<sup>4</sup> which—for wave 9 (also called SHARE Corona survey 2)—was in the field from June to August 2021, thereby coinciding with the second CASA wave. Lastly, we include the German Socio-Economic Panel (SOEP)—the most prominent and long-running German representative household data set (Goebel et al. 2019)—in our analysis. We use data from the survey in 2021, which was collected throughout the whole year.

Figure 2 reports the time periods when the data sets were collected in combination with COVID vaccination rates in Germany. Since only a minority of individuals had the possibility to get a COVID vaccination in February 2021, we do not make use of wave 1 of the CASA data.<sup>5</sup> Similarly, we exclude individuals from the SOEP sample if they were surveyed before May 2021.

The outcome variable of interest is COVID vaccination status, defined as a binary indicator equal to one if an individual has received at least one COVID vaccination. Figure 3 reports vaccination rates by age and data source/wave in our sample. As can be seen, the share of individuals with at least one COVID vaccination was above 75% for all age groups in all data sets. This is slightly larger than the numbers in Figures 1 and 2. Note, however, that those report the numbers from the full population while only individuals older than 50—who have higher vaccination rates—enter Figure 3. For instance, the prioritization of older individuals and those at risk of severe infection was lifted in June 2021, when enough vaccines were available and 90% of individuals over the age of 70 had



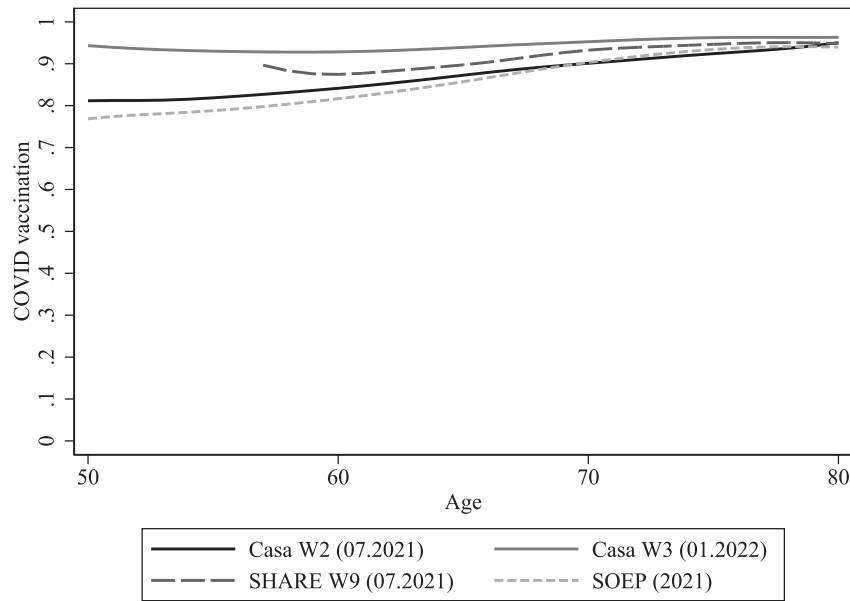
**FIGURE 2** | Timing of the surveys. *Source:* Bundesministerium für Gesundheit (2022).

received at least one vaccination. We observe a small age trend in the first COVID vaccination dose.

Since we exploit the West German compulsory schooling reforms, we restrict the analysis to West Germany, that is, all federal states listed in Table 1. Except for the SOEP, we do not have the information in which state individuals lived when attending school and therefore assume in Casa and SHARE that they attended school in the same state they currently reside in. While this seems to be a strong assumption, it is commonly used in the literature (see e.g. Kemptner, Jürges, and Reinhold 2011). In their online appendix, Begerow and Jürges (2022) show how in only about 5% of all cases in the SOEP the instrumental variable would be incorrectly assigned if it was based on the federal state individuals are currently living in instead of the federal state they went to school in. This suggests that moving and staying out of the federal state where education took place is a rare event in Germany in this age group. This should hold even more for the group of compliers at the lower education margin that does not move federal states to, for example, attend a university.

In order to focus on individuals born around the reform cohorts, we only include individuals born 7 years before to 7 years after the pivotal cohort. In robustness checks we make different sample selections, such as five or 10 years around the pivotal cohorts (Schneeweis, Skirbekk, and Winter-Ebmer 2014) or based on birth years, specifically the years 1945–1960 or 1940 to 1970. In our main specification, we have a sample size of 6010.

More descriptive statistics are presented in Table 2. The average immunization rate in our sample mirrors the one from Figure 3. The birth cohort restrictions imply that our estimation sample only includes individuals between 59 and 95 years of age. While the restriction to older individuals limits the generalizability of our results, we argue that this nevertheless is a very interesting subsample as, in particular, individuals older than 60 years are generally said to be the high risk group for COVID. We assign individuals their years of schooling based on their highest degree of schooling resulting in an average of 10.41 years of schooling.



**FIGURE 3** | Distribution of COVID vaccination by age and data source. CASA, SHARE and SOEP data. Data for West Germany only. No further sample restrictions (e.g. regarding birth cohorts). The figures present unconditional sample means by age in years which are smoothed by LOWESS.

### 3.2 | Baseline Empirical Approach

As a baseline model we start with the following linear regression model:

$$Y_{ics} = \beta_0 + \beta_1 S_{ics} + \beta_2 male_i + \gamma_c + \delta_s + \alpha(\eta_s \times c) + \theta_{survey} + \varepsilon_{ics} \quad (1)$$

where  $Y_{ics}$  is a binary indicator whether an individual  $i$  of birth cohort  $c$  in federal state  $s$  has received a COVID vaccination.  $S_{ics}$  are years of schooling.  $male_i$  is a dummy variable for being male.  $\gamma_c$ ,  $\delta_s$  and  $\theta_{survey}$  are birth cohort, federal state and survey/wave fixed effects respectively.  $\eta_s \times c$  accounts for state-specific linear birth cohort trends as typically done in this literature (e.g., Pischke and von Wachter 2008; Kamhöfer and Schmitz 2016; Cygan-Rehm 2022.). We vary the specification in robustness checks below.  $\varepsilon_{ics}$  denotes the individual error term. Standard errors are clustered by state  $\times$  birth cohorts, as this is the level of treatment assignment in the two stage least squares regressions (Pischke and von Wachter 2008).

In order to derive estimates for the effect of education on vaccination status, we use the changes in compulsory schooling as an instrumental variable for years of schooling. This approach was first used by Angrist and Krueger (1991) for the US and later by Pischke and von Wachter (2008) for Germany to estimate the effects of education on wages. We use a two stage least squares (2SLS) approach where the years of schooling are regressed in the first stage on the same variables as before, including  $Z_i$  which is an indicator variable equal to one if individual  $i$  was born into a state-year cohort, for which compulsory schooling years were 9 years instead of eight and zero else.

To derive a causal effect, the instrument  $Z$  needs to be valid and relevant. To be considered relevant, an instrument must be

**TABLE 2** | Descriptive statistics.

	Mean	St. dev	Min	Max
First covid vaccination	0.91		0	1
Years of schooling	10.41	1.81	8	13
Birth year	1952	4.79	1925	1961
Age	69.11	4.79	59	95
Male	0.53	0.5	0	1
Data source: Casa Wave 2	0.27		0	1
Data source: Casa Wave 3	0.26		0	1
Data source: SHARE Wave 9	0.14		0	1
Data source: SOEP 2021	0.33		0	1
Observations		6010		

Notes: CASA, SHARE and SOEP data after sample selection.

highly correlated with the explanatory variable of interest. We show this in Section 4.1. We argue that the instrument is indeed valid in our setting, meaning that the extension of compulsory schooling had no effect on vaccination status other than through individual years of schooling. It may be questionable to what extent the estimation results are driven by between state differences since the federal states are solely responsible for educational reforms, which—as also argued by (Pischke and von Wachter 2008)—also includes the content of the curricular in the additional year of educational attainment after the reform. We argue that this is a minor issue in our analysis as the variation used for deriving the causal estimate predominately consist of within state variation. A similar argument could be made concerning local infection dynamics, as all individuals within a region are exposed to the same infection dynamics, irrespective of their educational attainment. Nevertheless, we control for local infection dynamics in our robustness checks. The instrument of compulsory schooling in Germany has been used and discussed extensively in the literature for different



outcomes, such as wages (Cygan-Rehm 2022; Kamhöfer and Schmitz 2016; Pischke and von Wachter 2008), health (Kemptner, Jürges, and Reinhold 2011; Begerow and Jürges 2022) or fertility (Cygan-Rehm and Maeder 2013), where these authors argue that use of this instrument is a suitable way to deal with endogeneity of schooling.

If our assumptions hold, the estimated coefficient of instrumented years of schooling in the IV regression can be interpreted as the effect of an additional year of schooling on vaccination status. Allowing for heterogenous treatment effects and additionally assuming monotonicity, we get an estimate of a local average treatment effect (Imbens and Angrist 1994), where the complier subpopulation is the group at the lowest margin of education: individuals who would have liked to take 8 years of schooling but are forced to take nine. In the setting at hand, monotonicity implies that individuals do not reduce their schooling years *because* of the increase of compulsory education. We argue that this is the case, also since compulsory education is the legal lower bound for educational attainment. Therefore we identify effects for the subpopulation of compliers.

## 4 | Results

### 4.1 | Baseline Specification

Table 3 reports the results from linear regressions with the specification described in Section 3.2. Column (1) show the OLS results, while column (2) shows results from instrumental variables regressions.

Starting with the OLS results, educational attainment seems to be positively related to vaccination. An additional year of schooling goes along with a 0.7% point higher likelihood to get COVID vaccination, which is statistically significant. This positive correlation is in line with the findings from the literature. It is difficult to compare the size of the coefficient to those in the previous literature as most studies estimate odds ratios for different levels of education. Among the few examples that use linear probability models are Huebener and Wagner (2021), showing for Germany that people with intermediate education are 6% points more likely to receive a vaccination than people with basic education. According to Lindholt et al. (2021), within Europe, people with university education are 2% points more willing to receive a vaccination. Borga et al. (2022) estimate that in an EU-wide sample, people with university education are 3.7% points less likely to be vaccination hesitant. Our OLS estimate of a 0.7% point increased vaccination rate therefore seems quite low in comparison, however, it needs to be kept in mind, that it is based on years of schooling. Given that German individuals who qualify to attend a university have four more years of schooling than individuals with basic track education, this would translate into 0.7 times 4 = 2.8 more percentage points to be vaccinated. This is at the lower end of the cited numbers but not completely out of range.

The reason why our estimate is—in our interpretation—relatively small is likely that our sample is restricted to older individuals,

**TABLE 3** | Regressions results of the effect of education on COVID vaccination.

	OLS (1)	2SLS (2)
First stage coefficient of the instrument		
Post reform		0.443*** (0.072)
Second stage		
Years of schooling	0.007*** (0.002)	0.000 (0.029)
Male	0.015** (0.007)	0.018 (0.012)
Casa W2	0.002 (0.010)	0.004 (0.014)
Casa W3	0.056*** (0.009)	0.058*** (0.013)
Birth cohort FE	Yes	Yes
Federal state FE	Yes	Yes
State-specific linear cohort trends	Yes	Yes
Observations	6010	

Note: Standard errors in parentheses, clustered on birth-cohort × state level. The first stage includes the same control variables as the second stage. Complete first stage results are reported in Table A2 in the Appendix. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

who are concentrated at the lower margin of educational attainment. Many were born before the German educational expansion took off in the 1950s and 60s and most of the individuals in our sample have basic track education only. Thus, the potential drivers of a positive correlation—particularly individuals with tertiary education are more likely to get vaccinated—probably play a minor role in our data set.<sup>6</sup>

Next, we turn to the instrumental variables estimation, presented in Column (2). The first row presents the estimate for the effect of the instrument on the years of schooling from the first stage regression (the complete first stage results are provided in Table A2 in the Appendix). We use a binary indicator that is equal to one if an individual is born in a state-year cohort, for which 9 years of schooling were compulsory. We find a strong positive effect of compulsory schooling on educational attainment. The increase in compulsory schooling thus increased educational attainment by 0.443 years. The estimated coefficients in the previous literature range from 0.19 (Pischke and von Wachter 2008) to more than 1 (Kamhöfer and Schmitz 2016). Our estimate is therefore on the lower end of first stage coefficients, more in line with Pischke and von Wachter (2008), Kemptner, Jürges, and Reinhold (2011), or Begerow and Jürges (2022) (with estimates ranging from 0.5 to 0.69).

The lower panel of Column (2) in Table 3 reports the estimates of the second stage. We find no significant effect of the years of schooling on vaccination status. While this is also a matter of increased standard errors, the point estimate is zero.

## 4.2 | Robustness Checks

In carrying out the regressions, the researcher has many degrees of freedom. This holds for the sample selection or types of control variables included. We run further regressions similar to the one presented in Section 4.1 to test the robustness of our estimate to these choices. The results are presented in Figure 4. In the graphs, the dots represent the estimate for the effect of years of schooling on vaccination status derived from the 2SLS estimation, while the lines represent the 95% confidence interval. We include the same covariates as in Section 4.1 unless otherwise specified.

We first test for robustness against different sample selections. We repeat our main specification using cohorts that are born up to 7 years before or after the pivotal cohort. This is then compared to changing the bandwidth to five and 10 years around the pivotal cohort. Next, we present results where the same birth cohorts are used for all states and include the birth cohorts from 1945 to 1960 and birth years 1940–1970. In another robustness check, we drop individuals who state to work in the health care sector. This information is provided in the Casa data, the SOEP and partly via the ISCO codes in SHARE. Given that most of the individuals in the sample are out of the labor force, this does not make a big difference.

We also try different specifications. Here, we use our main sample selection, using the birth cohorts 7 years around the pivotal cohort. In the first specification we include age fixed effects instead of birth cohort fixed effects. In the following row, we exclude the state-specific linear birth cohort trends. We cluster standard errors on the state-level instead of the birth-cohort  $\times$  state level in the third specification. Next, we include an indicator for cohorts that experienced a short school year. It might be a concern that local infections dynamics influence individuals' decision to receive the vaccination. For this purpose we include the aggregated number of new COVID infections per state and in

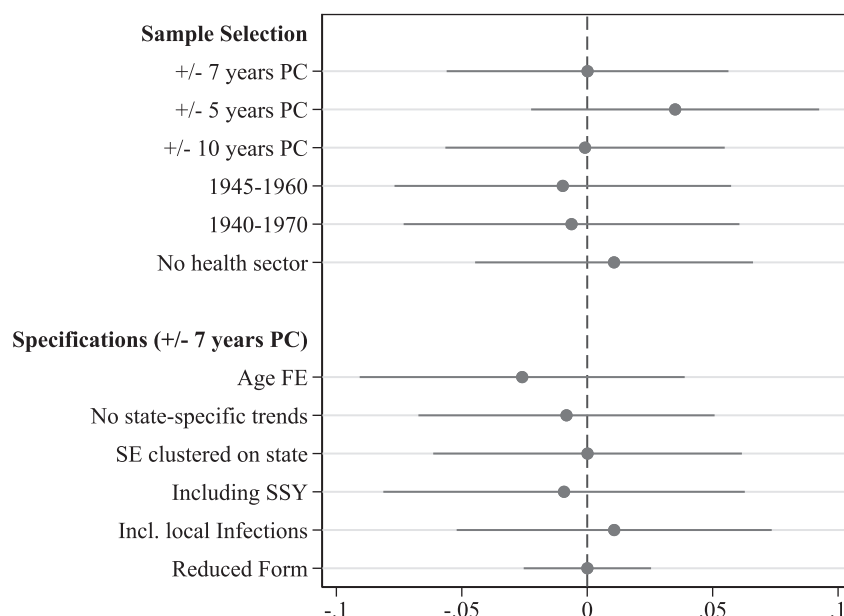
the month of the interview in the next specification. Lastly, we include the point estimate of the reduced form, that is, including a dummy whether an individual faced eight or 9 years of compulsory schooling instead of the years of schooling in an OLS estimation.

While the estimated coefficients fluctuate around zero for the different specifications, none of them is close to being statistically significant at the 5% level. Taken together, the additional regressions leave the impression of a robust finding of basically no sizable effect of an additional year of compulsory schooling on vaccination against COVID. Nevertheless, a small sample size and, thus, potential problems of statistical power are important drawbacks of our analysis. Thus, in the next section, we address the issue of statistical power of our analysis.

## 4.3 | An Ex-Post Power Analysis

The point estimate in our main specification is zero but the confidence intervals are fairly large. Given that we have a comparably small sample, this raises the question whether there might be a larger effect that we just fail to identify. To get an estimate of the minimum detectable effect size in our setting we apply a simulated ex-post power analysis developed by Black et al. (2022). The main idea is to assign a random treatment status with a known treatment effect to a sample of untreated individuals, run a regression and then see whether we can get a statistically significant estimate of this effect. The smallest imposed treatment effect detectable at the 5% significance level in 80% of the times is called the minimum detectable effect (MDE).

Here, we run the reduced form regression. That is, Equation (1) where years of education are replaced by the (randomized) instrument. We do this because, after simulating the instrument (being affected by the reform), we do not need to make further assumptions on how individuals react to the reform, that is,



**FIGURE 4** | Robustness checks and effect heterogeneity. CASA, SHARE and SOEP data. Each dot is a regression coefficient from 2SLS regressions as before with the same covariates as before. Lines indicate 95% confidence intervals.



whether they are compliers or always takers (Schmitz and Tawiah 2023). To achieve the MDE, we apply the following steps<sup>7</sup>

1. We base this analysis on our main sample ( $N = 6010$ ). From this, we only take the untreated individuals, that is, individuals born before the pivotal cohort in their respective state. We do this to prevent confusion of the imposed treatment effect with potential actual treatment effect. Given that 55% of individuals in our sample are born in or after the pivotal cohort, this leaves us with 2704 observations.
2. Next, we refill the sample to get 6010 observations. We do this by oversampling untreated individuals such that each federal state has the same number of observations as before.
3. We then randomly assign a treatment status—that is, being subject to the compulsory schooling reform—to 55% of individuals and define a constant treatment effect of 0.001.
4. We estimate the reduced form effect in our model as described above.
5. We then repeat the entire procedure over 40 rounds where we stepwise increase the treatment effect by 0.001 every round to a maximum of 0.04.
6. These steps are repeated 1000 times per round.

This leaves us with 40,000 estimations of which we only consider whether the point estimate of the treatment effect is significantly different from zero. The question for each iteration is if we are able to (correctly) reject the null-hypothesis that the estimated coefficient is zero, using the three common alpha values 10%, 5% and 1%. The results are depicted in Figure 5 where the share of rejected null-hypotheses for each true treatment effect is depicted. The MDE is—somewhat arbitrarily but following Black et al. (2022)—defined by the x-value of the point, where the  $\alpha = 5\%$  line passes the rejection rate of 80%. In our case this is 0.008. Thus, our approach should be able to detect reduced-form treatment effects of 0.8% points most of the time. Or, combining it with the first stage, IV effects of about 1.6% points. Thus, we conclude that our data would also allow to identify effects that are not very large.

#### 4.4 | Event-Study Specification to Account for Possible Treatment Effect Heterogeneity

The implementation of compulsory schooling reforms occurred at different points in time and in different states. This raises concerns spelled out in the recent literature on difference-in-differences with staggered treatment timing where treatment effect heterogeneity may cause biased estimates (e.g., Goodman-Bacon 2021; Sun and Abraham 2021). A possible problem in our application is that the federal states are responsible for education policies in Germany, especially including the contents of the curricula, which could be one of the main drivers of potential treatment effects. To get an idea whether these problems might be relevant in our case, we turn to an event study estimation of the reduced form (the effect of the compulsory schooling reform on vaccination) and use the estimator proposed by Sun and Abraham (2021) that takes potential problems into account.<sup>8</sup>

We use the relative birth year as the event time. It is defined as  $e_{ics} = \text{Birth year}_{ics} - \text{Pivotal cohort}_{ics}$ . The birth year before the pivotal cohort ( $e_{ics} = -1$ ) is used as the reference period. The resulting event study model takes the following form where the control variables in addition to the event time indicators are those also included in Equation (1):

$$Y_{ics} = \sum_{\substack{j \geq -4; \\ j \neq -1}} \delta_j \mathbb{1}(e_{ics} = j) + \delta_{-5} \mathbb{1}(e_{ics} \leq -5) + \beta_2 \text{male}_i + \gamma_c + \delta_s + \alpha(\eta_s \times c) + \theta_{survey} + \varepsilon_{ics} \quad (2)$$

Here,  $\delta_{-5}$  is the combined estimate for all event times smaller than  $-4$  in order to separate event time from calendar year effects. The remaining  $\delta_j$  are estimates of the effect of being subject to the compulsory schooling reform  $j$  for the birth cohort  $j$  years away from the pivotal cohort. Here, we do not account for years of schooling but only for the age relative to the pivotal cohort. We estimate the coefficients of Equation (2) using the procedure suggested by Sun and Abraham (2021).

The results are presented in Figure 6. No pre-trends are visible, and the post-reform effects do not show a convincing pattern

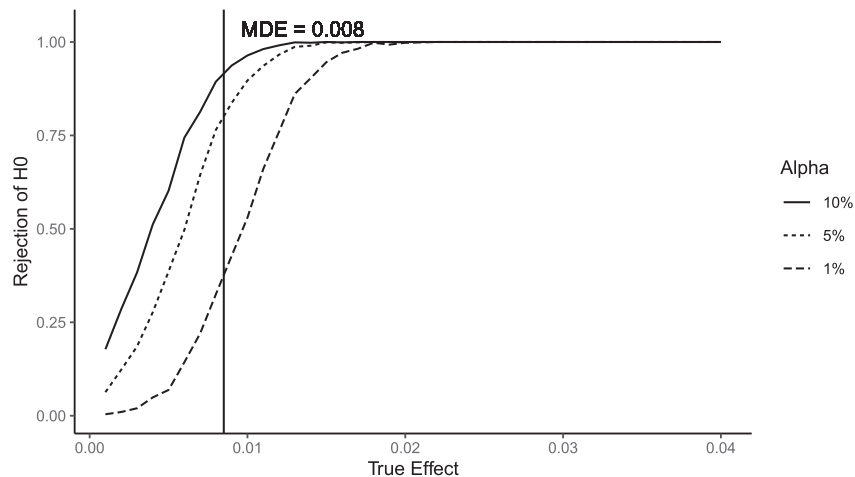
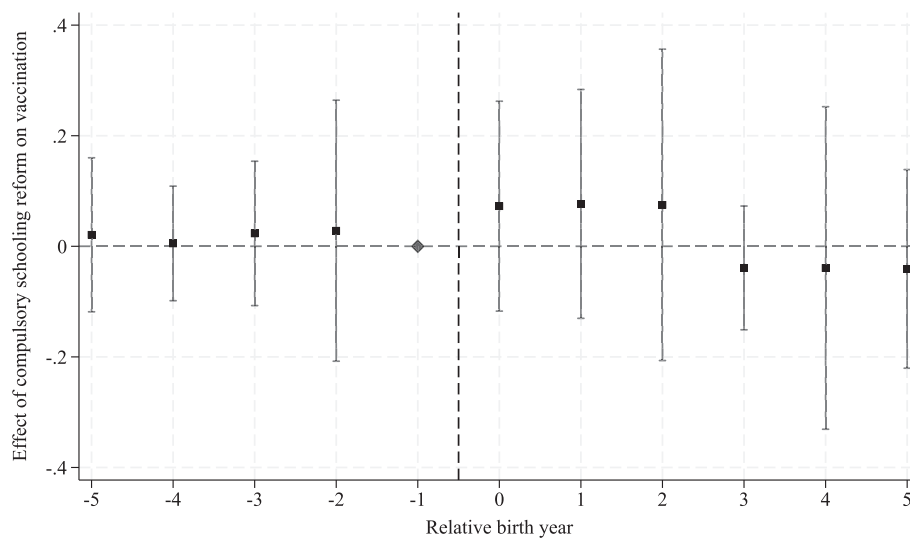


FIGURE 5 | Power analysis. Based on sample of untreated individuals.



**FIGURE 6** | Event-study effects of the reduced form.  $N = 6010$ . Points indicate point estimates, gray lines represent 95% confidence intervals. Standard errors are clustered on birth-cohort  $\times$  state level.

other than a fluctuation around zero. We take this as evidence in two directions: given that there are no pre-trends, this makes us more confident that our identifying assumptions can be defended. Second, given that the estimator that accounts for possible problems of treatment heterogeneity in staggered-entry settings comes to the same result as our baseline approach, we feel that the baseline approach does not suffer from this problem. Of course, power is an even more serious problem in this specification with more parameters to be identified. Thus, this should merely be seen as suggestive evidence and a robustness check but not as the main specification.

## 5 | Conclusion

The literature has widely studied the association between education and vaccination against COVID. Our main contribution is to present—to the best of our knowledge—first evidence on the effect of education on COVID vaccination and to add to the scarce literature on the effect of education on vaccination behavior. We use data on West German individuals from three different data sources to estimate the effect of education on COVID vaccination status. We exploit the widely adopted instrument of compulsory education reforms in an instrumental variables approach as exogenous variation in years of schooling. We do not find any evidence for a significant effect of schooling on vaccination status in Germany for the group of compliers. Thus, we do not find compelling evidence that this relationship between education and vaccination is causal.

We stress again that we do not claim that our results can be generalized to other age groups or other education margins (e.g. individuals with higher education) but, on the other hand, argue that our specific set of compliers is a very policy relevant one: individuals who due to their age belong to the high-risk group of a COVID infection. Moreover, individuals with low educational attainment make up a large share of this age group in Germany.

One potential mechanism to explain the small estimates could be barriers to vaccination. This would mean that individuals have a higher willingness to vaccinate due to their educational background, but institutional barriers prevent them from actually receiving vaccinations. However, we argue that this is unlikely to be the case. Older individuals—who make up our treatment group—were prioritized in the beginning of the vaccination campaign (Vygen-Bonnet et al. 2021a) and a key goal in the early pandemic response was to ensure the safety of older individuals. Moreover, there were no copayments for COVID vaccination. We therefore argue that for the individuals in our treatment group barriers to receiving a vaccination were low. Because these barriers were so low, we possibly face another problem: the vaccination rate in our sample is quite high with 91%, meaning that potential effects may be bounded to be small. However, the working paper version of this paper also included influenza vaccination as an outcome. The average vaccination rate was 37% in this sample and, thus, far lower. Still the point estimate was less than 2% points (Monsees and Schmitz 2023) implying that the high general vaccination rate against COVID may not be the driver of the result.

Our findings do not rule out the hypothesis that education, while not affecting the general willingness of COVID vaccination, could affect the timing of vaccination. Elinder, Erixson, and Öhman (2023), for example, show how individuals with higher cognitive abilities took up COVID vaccination faster than individuals with lower cognitive abilities. Unfortunately, we are not able to test this hypothesis, as we have no information on the timing of vaccination. However, given that compulsory schooling did not affect cognition in Germany (Kamhöfer and Schmitz 2016) this does not speak in favor of an effect of education on timing of vaccination through via mechanism.

An important limitation of this study is its small sample size. Nevertheless, the power analysis indicates that we should still be able to identify treatment effects that are not very large. Another concern is selective mortality, especially affecting older

individuals, who make up our treatment group. Ultimately, we cannot address this issue but note that Schmitz and Tawiah (2023) in an analysis with a similar setting and data (effect of compulsory schooling on health in Germany) find that selective attrition does not seem to be a sizable issue. Related to this are concerns about local infection dynamics that influence individual vaccination decisions and may interfere with federal state level differences in educational reforms. However, including these in the robustness checks had little impact on our estimates.

All in all, we argue that it is of scientific value to present evidence on a first part of the general picture of effects of education on vaccination. Our results suggest a more cautious use of the general expression that education is an important determinant of vaccination behavior.

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[Correction added on 07 January 2025, after first online publication: Projekt DEAL funding statement has been added.]

## Conflicts of Interest

The authors declare no conflicts of interest.

## Data Availability Statement

We use data from the Survey of Health Ageing, and Retirement (SHARE), the German Socio-Economic Panel (SOEP), and the Casa Monitor, that we are not allowed to publish online. Upon individual registration for the SHARE and SOEP (which is simple and not restricted), the data are available to each interested researcher. The Casa Monitor data we use are available to interested researchers for on-site use upon registering at the research data center at the RWI Essen, Germany.

## Endnotes

<sup>1</sup> Everybody in the age group relevant in our sample (age 60 and older) had the possibility to get their first COVID vaccination as of April 2021.

<sup>2</sup> See Hörnig and Schaffner (2023) for a data description and the questionnaire.

<sup>3</sup> In the Appendix we provide a comparison of descriptive statistics with the SOEP (Table A1), showing that they are very similar in terms of demographics and regional coverage

<sup>4</sup> See Börsch-Supan et al. (2013), Börsch-Supan (2022), and Scherpenzeel et al. (2020).

<sup>5</sup> In wave 1, individuals are asked about the willingness to vaccinate once a vaccine is available. However, we restrict the analysis to actual vaccination instead of reported willingness in this paper.

<sup>6</sup> When we do not make the birth-cohort restrictions, the correlation becomes stronger.

<sup>7</sup> The description follows Freise, Schmitz, and Westphal (2022).

<sup>8</sup> We use the Stata command *eventstudyinteract*, see Sun (2021).

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## Appendix

**TABLE A1** | Representativeness of CASA monitor compared to SOEP.

Variable	Mean CASA monitor	Mean SOEP
Age < 30	0.17	0.14
30 ≤ age < 50	0.29	0.30
50 ≤ age < 70	0.33	0.36
70 ≤ age	0.21	0.20
Male	0.49	0.49
Household size	2.39	2.36
Fulltime work	0.41	0.39
German	0.87	0.94
Married	0.50	0.50
Private health insurance	0.13	0.13
Basic track or less reference	0.35	0.32
Intermediate	0.31	0.32
University-entrance diploma 1	0.27	0.28
University-entrance diploma 2	0.07	0.08
Brandenburg	0.03	0.03
Berlin	0.04	0.04
Baden-Wuerttemberg	0.14	0.13
Bavaria	0.16	0.15
Bremen	0.01	0.01
Hesse	0.08	0.08
Hamburg	0.02	0.02
Mecklenburg-West Pomerania	0.02	0.02
Lower Saxony	0.10	0.10
North Rhine-Westphalia	0.21	0.21
Rhineland Palatinate	0.05	0.05
Schleswig Holstein	0.03	0.04
Saarland	0.01	0.01
Saxony	0.05	0.06
Saxony-Anhalt	0.03	0.03
Thuringia	0.03	0.03
Observations	10,251	22,101

Note: CASA monitor data, wave 3, from January 2022 and SOEP wave 37 from 2020. Means in both samples weighted by sampling weights.

**TABLE A2** | Additional estimation results.

Outcome	First stage (1) Years of schooling	Reduced form (2) COVID vaccination
Post reform	0.443*** (0.072)	0.000 (0.013)
Male	0.391*** (0.049)	0.018** (0.007)
Casa W2	0.298*** (0.068)	0.004 (0.010)
Casa W3	0.286*** (0.068)	0.058*** (0.009)
SHARE W9	−0.575*** (0.080)	0.022* (0.013)
Birth cohort FE	Yes	Yes
Federal state FE	Yes	Yes
State-specific linear cohort trends	Yes	Yes
Observations	6010	6010

Note: Standard errors in parentheses, clustered on birth-cohort × state level.  
\* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .