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The effect of compulsory schooling on vaccination against COVID and Influenza

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Daniel Monsees
Hendrik Schmitz

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Daniel Monsees and Hendrik Schmitz[1]

The Effect of Compulsory Schooling on Vaccination against COVID and Influenza

Abstract

We study the effect of education on vaccination against COVID and influenza in Germany and Europe. Our identification strategy makes use of changes in compulsory schooling laws and allows to estimate local average treatment effects for individuals between 59 and 91 years of age. We find no significant effect of an additional year of schooling on vaccination status in Germany. Pooling data from Europe, we conclude that schooling increases the likelihood to vaccinate against COVID by an economically negligible effect of one percentage point (zero for influenza). However, we find indications that additional schooling might increase fear of side effects from COVID vaccination.

JEL-Code: I10

Keywords: COVID; influenza; vaccination; education; compulsory schooling

March 2023

1 Daniel Monsees , RWI and Leibniz Science Campus Ruhr; Hendrik Schmitz, Paderborn University, RWI and Leibniz Science Campus Ruhr. *– Financial support by the Deutsche Forschungsgemeinschaft (Project: "COVID-19 as amplifier of social inequality – SMall ARea Analyses with German neighborhood Data") is gratefully acknowledged. We thank the participants of the Health Economics seminar at RWI Essen for many helpful comments and suggestions.* *– All correspondence to: Daniel Monsees, RWI – Leibniz Institute for Economic Research, Hohenzollernstraße 1-3, 45128 Essen, Germany; Email: daniel.monsees@rwi-essen.de.*

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 developed world. 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 et al., 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 the developed world – 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. Understanding the determinants 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. Numerous studies report positive associations between COVID vaccination willingness and educational status around the world. See, e.g. Cascini et al. (2021) for a general overview and Borga et al. (2022), Graeber et al. (2020), Mondal et al. (2021), Bergmann et al. (2021), Walkowiak and Walkowiak (2021), Huebener and Wagner (2021), Humer et al. (2021), for a non-exhaustive list of studies. Some of theses 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 – even if sometimes economically small – 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 et al., 2004).

Another question is whether the relationship between education and vaccination is causal. 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 like vigorous activity (Brunello et al., 2016) or use of preventive services (like receiving flu shots, Fletcher and Frisvold, 2009). However, apart from that, 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 the first part of this paper, we study the effect of education on vaccination against COVID in West Germany using information from two 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 – around 4000 individuals. Our identification strategy makes use of changes in compulsory schooling laws and allows to estimate local average treatment effects for individuals between 59 and 91 years of age. The reforms increased compulsory schooling from eight to nine years in the 1940s to 1960s in Germany with some variation in timing across federal states. We augment the analysis using vaccination against influenza as a second outcome. Our results only hold for a specific subgroup of individuals but, arguably, a highly important one: older individuals – usually denoted the high risk group – at the lowest margin of education (those forced to increase their years of education from eight to nine years). For Germany, we do not find a significant effect of one additional year of compulsory schooling on vaccination, neither against COVID, nor against influenza.

In the second part of the paper, we extend the analysis to several European countries. We now find a statistically significant positive effect of education on COVID vaccination. However, the effect size is a very small one percentage point increase in COVID vaccination (compared to around 90 percent average probability to be vaccinated against COVID). Effects on influenza vaccination are precisely zero. Further analyses show that health status and labor force participation do not seem to be able to explain this result.

We additionally observe that education significantly increases the likelihood to report fear from side effects of COVID vaccination in Germany. Due to data limitations we cannot probe how robust this finding is but it is not completely new in the literature (that, again, only studied associations, however): Wu and Zhang (2022) investigate reasons given for vaccine hesitancy among not fully vaccinated individuals in the US. Within this subgroup, higher educated individuals tend to be more concerned about the safety of vaccines and do not see the vaccine as necessary. However, according to Wu and Zhang (2022) there appears to be no education gradient for trust in the vaccine.

We do not claim that our main result – no sizeable local average treatment effect of education on vaccination against COVID and influenza – 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 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 for Germany are reported in Section 4 while Section 5 shows results for Europe. We study the effects of schooling on fear of side effects in Germany in Section 6. We conclude in Section 7.

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

In Germany, children enter primary school at the age of six.[1] After four years in primary school they attend one of the three secondary school tracks. Secondary schools in Germany can, generally, 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. Before the German educational reform, which occurred from 1946 to 1969 in West Germany, basic track schools covered grades five to eight. The reform increased the number of compulsory schooling years from eight years to nine years. Decisions and policies regarding the educational system in Germany are made at the federal state level, hence the reform was implemented in different years by the various states. 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). See Table 1 for the reform years. The reform was introduced due to a shortage in labor market opportunities

[1] This first paragraph is taken word by word from Schmitz and Tawiah (2023).

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, which is why we do not include them in our analysis (Kemptner et al., 2011).

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

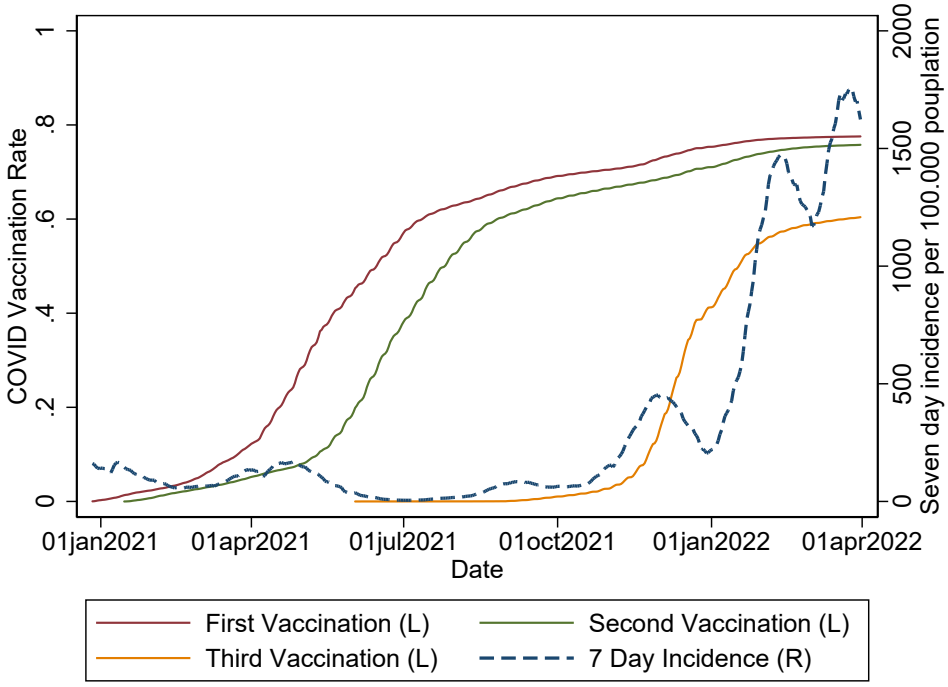
Source: Begerow and Jürges (2022). Pivotal cohort is the first birth cohort the reform applies to.

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, 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).[2] Figure 1 shows the share of the vaccinated population together with the seven day incidence between January 2021 and April 2022. The share of individuals having received at least one vaccination (red 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 (green line) follows a similar pattern with a delay of two month. The third vaccination was available as of June 2021 (yellow line) 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.

[2]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.

Figure 1: Vaccination status and seven day incidence in Germany



Notes: 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. Sources: Bundesministerium für Gesundheit (2022) and Robert Koch-Institut (2022a).

3 Data and empirical approach

3.1 Sample selection and outcome variables

We use combine two data sources. The first is the CASA monitor data set (henceforth called *CASA data*), an online-survey put together by infas 360.[3] 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. In the Supplementary Materials we provide the questionnaire as well as a comparison of descriptive statistics with the German Socio-Economic Panel (Goebel et al., 2019) – the most prominent and long-running German representative household data set – and show that they are very similar in terms of demographics and regional coverage (Table A1). We augment this data with the German part of the Survey of Health, Ageing and Retirement in Europe (SHARE)[4] 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.

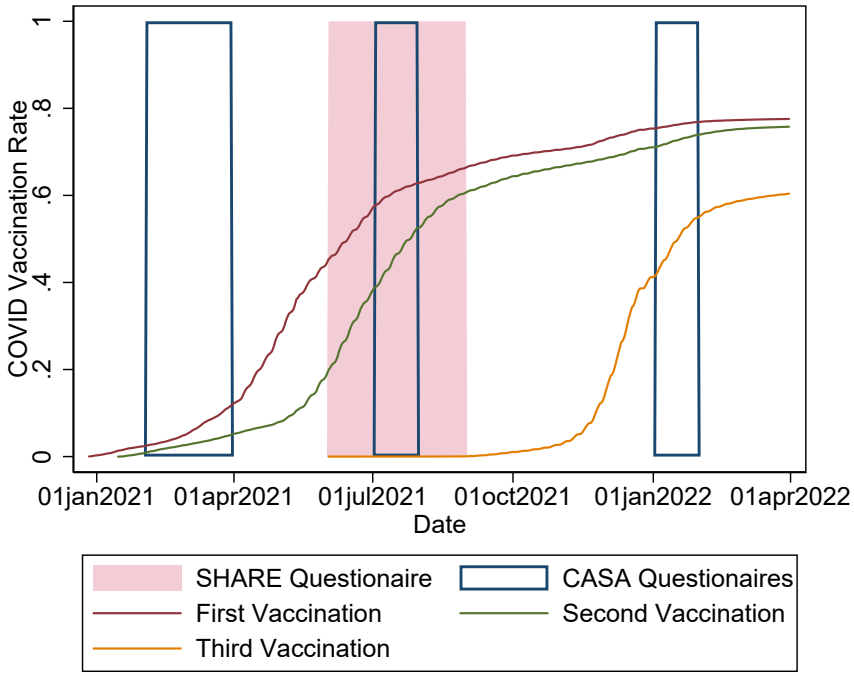
Figure 2 reports the time periods when the CASA data and the 9th SHARE wave were collected in combination with COVID vaccination rates in Germany. Since only a minority

[3]https://www.infas360.de/casa-monitor/

[4]See Börsch-Supan et al. (2013), Börsch-Supan (2022), and Scherpenzeel et al. (2020).

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.[5]

Figure 2: Timing of the surveys



Notes: Source: Bundesministerium für Gesundheit (2022).

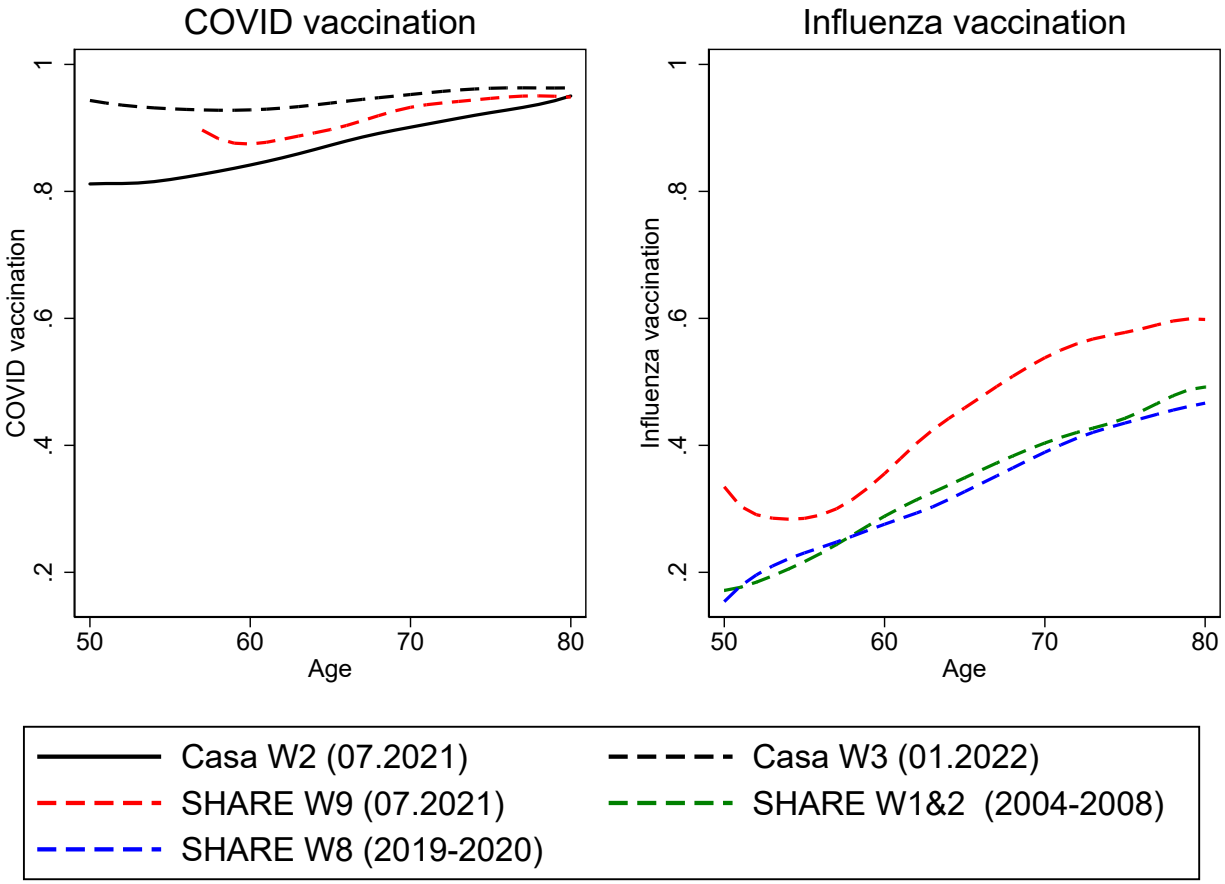
Our first outcome variable of interest is COVID vaccination status, defined as a binary indicator equal to one if an individual has received at least one COVID vaccination. The second outcome variable is an indicator of having received an influenza vaccination within the previous 12 months, included in multiple SHARE waves. Figure 3 reports vaccination rates by age and data source/wave for both outcome variables in our sample. As can be seen, the information on COVID vaccination status is included in both of the CASA waves used, as well as the 9th SHARE wave. Information on influenza vaccination status in only included in the SHARE data, more precisely in waves 1, 2, 8 and 9 (Börsch-Supan, 2019a,b, 2021, 2022).[6]

The share of individuals with at least one COVID vaccination was above 80% for all age groups in all data sets, as can be seen in the left panel of Figure 3. 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

[5]In wave 1, individuals are asked about the willingness to vaccinate once a vaccine is available. However, we focus on actual vaccination instead of reported willingness.

[6]Note some duplication in the naming of some waves in SHARE. By wave 8 we mean the regular wave 8, not the SHARE Corona survey 1, which is also called wave 8 by the SHARE group. By wave 9, we mean SHARE Corona survey 2, additionally called wave 9 by the SHARE group.

Figure 3: Distribution of vaccination by age and data source



Notes: CASA and SHARE 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.

of severe infection was lifted in June 2021, when enough vaccines were available and 90% of individuals over the age of 70 had received at least one vaccination. We observe a small age trend in the first vaccination dose.

Influenza vaccination status, defined as having received a flu vaccination in the last 12 months, was only recorded in the SHARE data. It is significantly lower than COVID vaccination status. Between waves 1 and 2, and 8, there was little change in the vaccination rate. However, the vaccination rate in wave 9 is substantially higher. Given that wave 8 was in the field shortly before COVID (starting October 2019), this jump could indicate that older individuals received an influenza vaccination to gain some protection against COVID or avoid influenza infections in a period of sparse medical resources. It could also be due to a stronger awareness of the importance to vaccinate against respiratory diseases.

We restrict the analysis to West Germany, that is, all federal states listed in Table 1. In order to focus on individuals born around the reform cohort, we only include individuals born seven years before to seven years after the pivotal cohort. In robustness checks we make different sample selections, such as five or ten years around the pivotal cohorts

(Schneeweis et al., 2014) or based on birth years, specifically the years 1945 to 1960 or 1940 to 1970. Variants of these selections have been made in the literature (Pischke and von Wachter, 2008; Kamhöfer and Schmitz, 2016; Begerow and Jürges, 2022). In our main specification, we have a sample size of 4,024 for the regressions for the COVID vaccination and 2,675 for influenza.

More descriptive statistics for the COVID vaccination sample and for the influenza vaccination sample are presented in Table 2. The average immunization rates in our sample mirror the ones from Figure 3. The birth cohort restrictions imply that our estimation sample only includes individuals between 59 and 91 years of age in the COVID sample. Given that SHARE wave 1 was in the field in 2004, the sample has a larger age range and is a bit younger for the influenza vaccination. 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, both for COVID and influenza. We assign individuals their years of schooling based on their highest degree of schooling.[7] Average years of schooling in the samples are 10.45 and 9.74.

Table 2: Descriptive statistics

	COVID sample				Influenza sample			
	Mean	St. dev	Min	Max	Mean	St. dev	Min	Max
Covid vaccination	0.92	0.27	0	1				
Influenza vaccination					0.37	0.48	0	1
Years of schooling	10.45	1.79	8	13	9.74	1.77	8	13
Birth year	1952.09	4.53	1930	1961	1950.09	5.74	1926	1961
Age	69.21	4.52	59	91	65.18	7.89	43	93
Male	0.54	0.50	0	1	0.47	0.50	0	1
Data source: CASA Wave 2	0.40	0.49	0	1				
Data source: CASA Wave 3	0.39	0.49	0	1				
Data source: SHARE Wave 1					0.22	0.42	0	1
Data source: SHARE Wave 2					0.06	0.24	0	1
Data source: SHARE Wave 8					0.40	0.49	0	1
Data source: SHARE Wave 9	0.21	0.41	0	1	0.31	0.46	0	1
Observations		4,024				2,675		

Notes: CASA and SHARE data after sample selection.

[7] Note that SHARE and CASA have exactly the same questions and answer categories for the education variables and the first COVID vaccination dose and, thus, can be pooled.

3.2 Empirical approach

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

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

where Y_{ics} is a binary indicator whether an individual i has received a vaccination (either against COVID or influenza). S_{ics} are years of schooling. $female$ is a dummy variable for being female. γ_c, δ_s and θ_{survey} are birth cohort, federal state and survey/wave fixed effects respectively. $\eta_s \times c$ accounts state-specific linear birth cohort trends. ε_{ics} denotes the individual error term.

In order to derive causal estimates for the effect of education on vaccination status, we use the changes in compulsory schooling as an instrument 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 nine years instead of eight and zero else.

To derive a causal effect, the instrument Z needs to be valid and relevant. 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. To be considered relevant, an instrument must be highly correlated with the explanatory variable of interest. We show this in section 4.1. The instrument of compulsory schooling in Germany has been used extensively in the literature for different outcomes, such as wages (Cygan-Rehm, 2022; Kamhöfer and Schmitz, 2016; Pischke and von Wachter, 2008), health (Kemptner et al., 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 causal 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. 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 – individuals who would have liked to take eight years of schooling but are forced to take nine.

4 Results

4.1 Baseline specification

Table 3 reports the results from linear regressions with the specification described in Section 3.2. The regressions are carried out independently for the COVID and influenza samples. Figure 3 and Table 2 in Section 3.1 make clear which data sources/waves enter the regressions. Columns (1) and (2) show OLS results, while columns (3) and (4) show results from instrumental variables regressions.

Table 3: Regressions results

	OLS		2SLS	
	COVID vaccination (1)	Influenza vaccination (2)	COVID vaccination (3)	Influenza vaccination (4)
First stage coefficient of the instrument:				
Post reform			0.368***	0.491***
			(0.086)	(0.151)
Second stage:				
Years of schooling	0.001	0.009	-0.033	0.019
	(0.003)	(0.007)	(0.051)	(0.058)
Male	0.023**	-0.050**	0.036*	-0.054*
	(0.009)	(0.021)	(0.019)	(0.030)
CASA W2	-0.020*		0.008	
	(0.012)		(0.043)	
CASA W3	0.034***		0.062	
	(0.011)		(0.043)	
SHARE W1		-0.367***		-0.371***
		(0.026)		(0.035)
SHARE W2		-0.300***		-0.302***
		(0.035)		(0.038)
SHARE W8		-0.154***		-0.154***
		(0.019)		(0.019)
Observations	4,021	2,668	4,021	2,668

Standard errors in parentheses, clustered on birth-cohort × state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. First stage includes the same control variables as the second stage.

Starting with the OLS results, educational attainment does not seem to be related to receiving either COVID or influenza vaccination. This result contradicts findings from the literature, where it is commonly found that schooling and vaccination status are positively correlated. However, our sample is restricted to older individuals, who are more concentrated at the lower margin of educational attainment. Many were born before the German educational expansion took off and most of the individuals in our sample have basic track education only. Thus, the potential drivers of a positive correlation like that particularly individuals with higher education are more likely to get vaccination

probably play a minor role in our data set which might explain this result. In Table A2 in the Appendix, we present results for the whole data set without birth-cohort restrictions. There, a positive correlation between schooling and vaccination status can be found. An additional year of schooling is associated with a 1.4 percentage points higher probability of being vaccinated against COVID and a 1 percentage point higher probability of being vaccinated against influenza. Yet, even this positive correlation is small. Another finding from Table 3 is that men are more likely to receive a COVID vaccination but less likely to receive a vaccination against influenza.

Next, we turn to the results of the instrumental variable estimations, presented in columns (3) and (4) of Table 3. The first row presents the estimate for the effect of the instrument on the years of schooling from the first stage regression. We use a binary indicator that is equal to one if an individual is born in a state-year cohort, for which nine years of schooling were compulsory. We find a strong positive effect of compulsory schooling on educational attainment. The increase of the compulsory schooling thus increased educational attainment by 0.368 years in the COVID sample and by 0.491 years in the influenza sample. 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 estimates are therefore on the lower end of first stage coefficients, more in line with Pischke and von Wachter (2008), Kemptner et al. (2011), or Begerow and Jürges (2022) (with estimates ranging from 0.5 to 0.69).

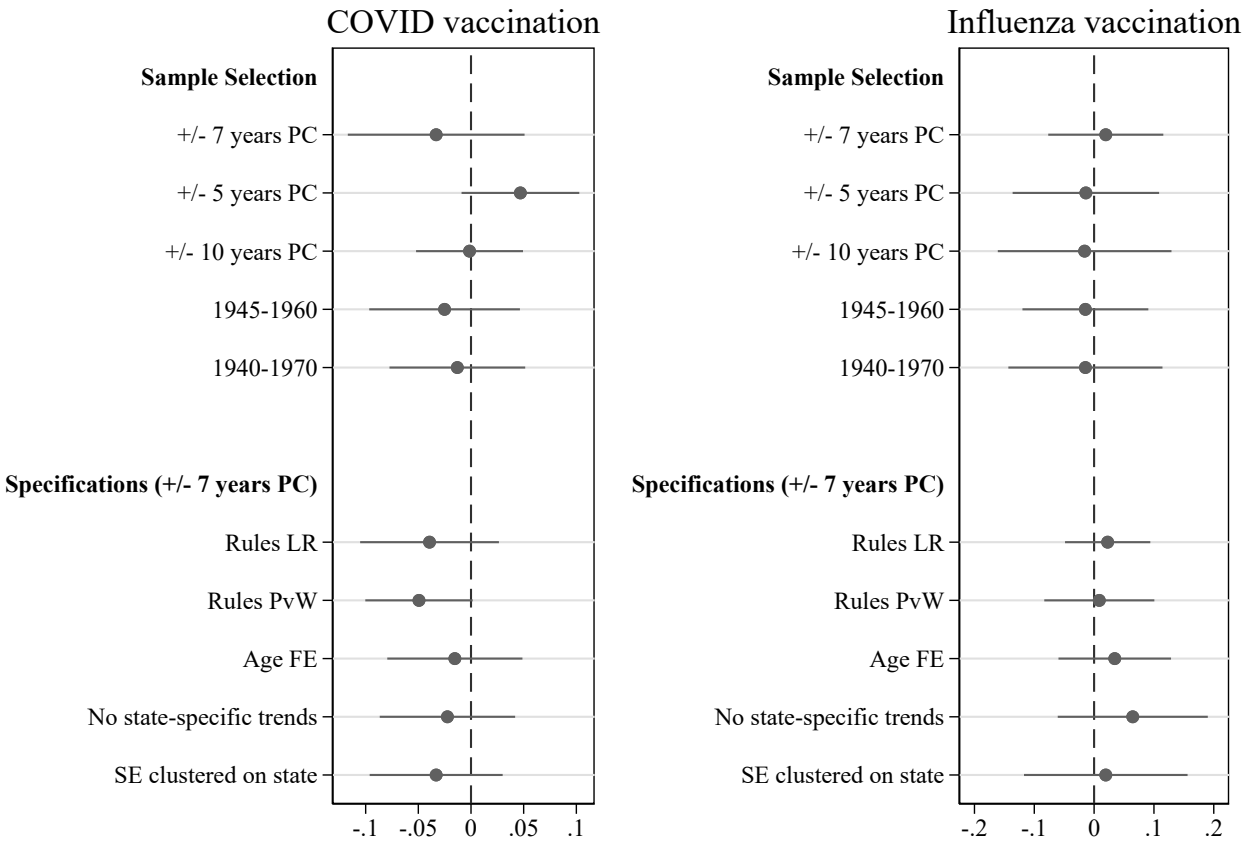
The lower panel of Table 3 reports the estimates of the second stage. We find no significant effect of the years of schooling on vaccination status. While this is also a matter of increased standard errors, the point estimates are close to zero, too. In the case of COVID vaccination, we even receive a negative sign and a reduced likelihood to get vaccination by 3 percentage points (not significant).

4.2 Robustness checks

In carrying out the regressions, the researcher has many degrees of freedom. This holds for the sample selection, types of control variables and even the "definition" of the reform years in Germany. We run further regressions similar to those presented in Section 4.1 to test the robustness of our estimates to these choices. The results are presented in Figure 4. The two panels represent the two outcomes, following the columns in Table 3. 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 90% 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 seven years before or after the pivotal cohort.

Figure 4: Robutness checks and effect heterogeneity



Notes: CASA and SHARE data. Note: 90% confidence intervals only. Each dot is a regression coefficient from 2SLS regressions as before with the same covariates as before.

This is then compared to changing the bandwidth to five and ten 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 to 1970.

We also try different specifications. We use our main sample selection, using the birth cohorts seven years around the pivotal cohort. First, we include the specification of compulsory schooling laws as they are described in Leschinsky and Roeder (1980) and used by Cygan-Rehm and Maeder (2013) that slightly differ from those used by Begerow and Jürges (2022). In the same way we use the compulsory schooling reforms are described in Pischke and von Wachter (2008) in the second row. In the third specification, we include age fixed effects instead of birth cohort fixed effects. In the following row, we exclude the state-specific birth cohort trends. We cluster standard errors on the state-level instead of the birth-cohort $\times$ state level in the fifth specification.

The results seem to be sensitive to these changes and the estimated coefficients fluctuate around zero. Yet, none of the coefficients is large in economic terms and taken together the additional regressions leave the impression of a robust finding of basically no sizeable

effect of an additional year of compulsory schooling, neither on vaccination against COVID nor against influenza. Nevertheless, a small sample size and, thus, potential problems of statistical power are important drawbacks of our analysis. Thus, in the next section, we pool information from several European countries to gain power.

5 Europe-wide analysis

We make use of the fact that the SHARE is collected for multiple countries and extend our analyses to several European countries. While it can be argued that education systems differ between countries, reducing the comparability, we argue that pooling significantly increases sample size and can, thus, provide further insights in addition to our country specific analysis. Many recent papers have studied effects of compulsory schooling reforms in Europe on different outcomes in pooled analyses (e.g. Schneeweis et al., 2014, Brunello et al., 2016, Schiele and Schmitz, 2023). For Germany we use the same educational reforms as before, following Begerow and Jürges (2022). For the additional countries we use the reform described in Schiele and Schmitz (2023). An overview over the reforms, together with the sample size per country, we use can be found in Table 4. We use the same sample restrictions as before, including individuals born seven years surrounding the pivotal cohort.

Figure 5 reports regression results in the same spirit as for the robustness checks before. We use the same specifications as in the previous section but replace federal state dummies with country dummies. In line with Table 3, no association between schooling and influenza vaccination status can be found, now however with smaller standard errors. Regarding COVID vaccination, we do observe a small positive correlation. One additional year of education goes along with a one percentage point higher likelihood to vaccinate against COVID.

Going through the IV results across separate specifications, similar coefficients as in OLS appear, by and large. One additional year of education increases COVID vaccination for the compliers of compulsory schooling reforms by around one percentage point and does not affect influenza vaccination at all. The confidence bands are considerably smaller than in the previous section (Figure 4) and all confidence bands in the Europe-wide analysis lie within most of the bands when using Germany alone and, thus, the results appear plausible. Regarding COVID vaccination, even though significant at the ten percent level (sometimes also at the five percent level), the effect sizes are very small only. Thus, we conclude that both approaches (a uniform educational system but small sample vs. pooled countries but larger sample) lead to the same conclusions of no sizeable effect of schooling on vaccinations.

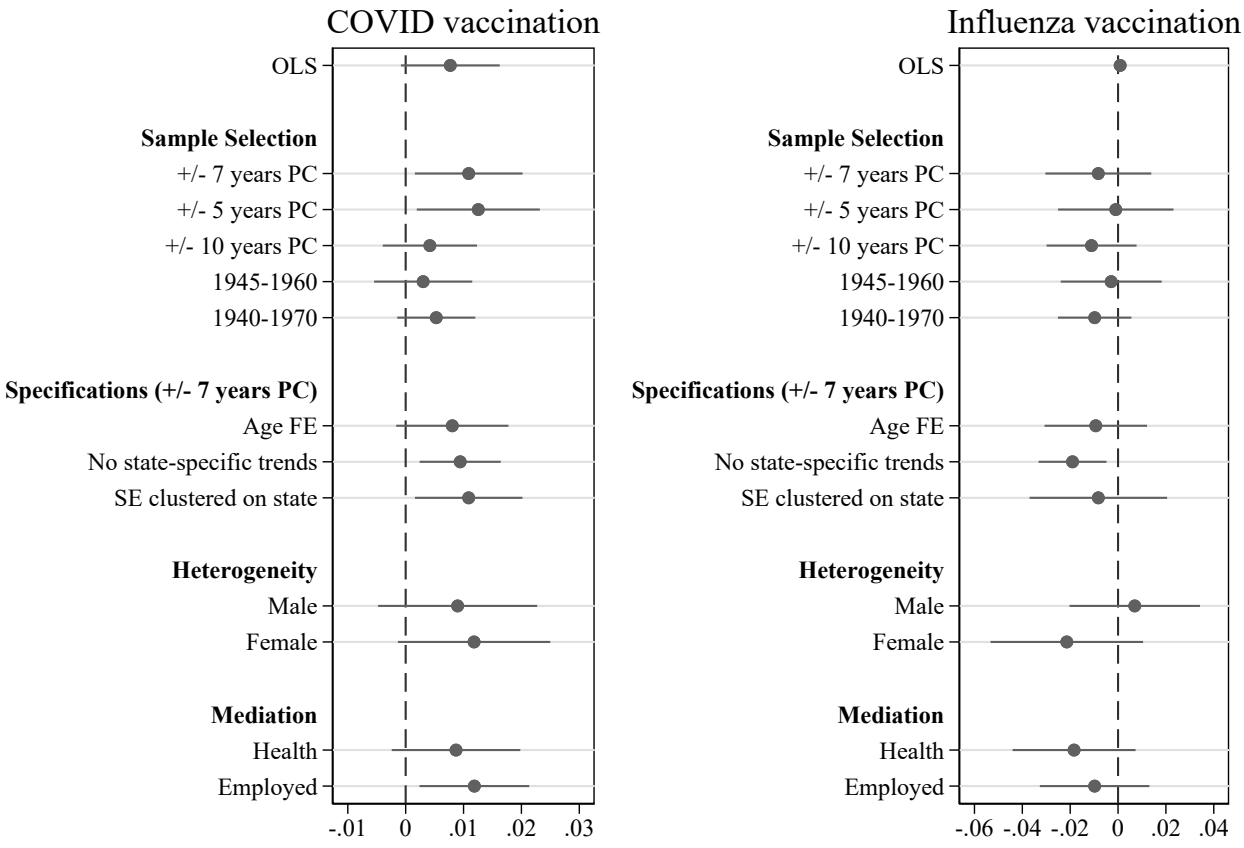
Table 4: Compulsory schooling reforms in Europe and number of observations

			Number of observations	
	Change in years	Pivotal cohort	Sample COVID	Sample influenza
Austria	8-9	1951	1,273	2,636
Belgium: Flanders	8-9	1939	580	1,776
Czech Republic	8-9	1947	1,371	4,012
Denmark	4-7	1947	815	2,645
France	8-10	1953	1,109	3,319
Greece	6-9	1963	971	1,965
Italy	5-8	1949	1,804	3,944
Netherlands	7-9	1936	137	1,620
Spain	6-8	1957	702	1,900
Germany			841	2,682
BW	8-9	1952		
BY	8-9	1954		
HB	8-9	1944		
HH	8-9	1931		
HE	8-9	1951		
NI	8-9	1947		
NRW	8-9	1951		
RLP	8-9	1952		
SL	8-9	1943		
SH	8-9	1933		
Total			9,603	26,499

Notes: The table shows compulsory schooling reforms for each country together with the change in years of compulsory schooling and the first cohort affected by the reform. Information on schooling reforms taken from Begerow and Jürges (2022) for Germany and Schiele and Schmitz (2023) for additional countries.

We also separate effects by gender. While there seems to be no difference regarding COVID vaccination, the effect of schooling on influenza vaccination is even slightly negative for women (but not significantly different from zero). Finally, we ask for potential reasons for the small effects of education on vaccination. One factor that comes to mind is health status. Especially at the beginning of the COVID vaccination campaigns, people with underlying health conditions were prioritized to receive a vaccination against COVID-19 (Vygen-Bonnet et al., 2021a; WHO, 2022), as they were most at risk for severe consequences of infection. In line with this it is commonly found that those at risk for severe consequences of a COVID infection are also more likely to get vaccinated (an overview can be found in Cascini et al., 2021). Now, if education improves health, but better health reduces the likelihood to vaccinate against COVID or influenza, it may well be that a direct effect of education on vaccination is offset by an indirect negative effect through the channel of health. It should be noted that the evidence on health effects of compulsory schooling is mixed and many studies find zero effects, see, e.g. the literature reviews in Hamad et al. (2018) and Schmitz and Tawiah (2023). We nevertheless test for this by controlling for health status. Specifically, we carry out the same regressions as before but, additionally,

Figure 5: Regression results Europe



Notes: SHARE data. Note: 90% confidence intervals only. Each dot (except for the first line) is a regression coefficient from 2SLS regressions as before with the same covariates as before.

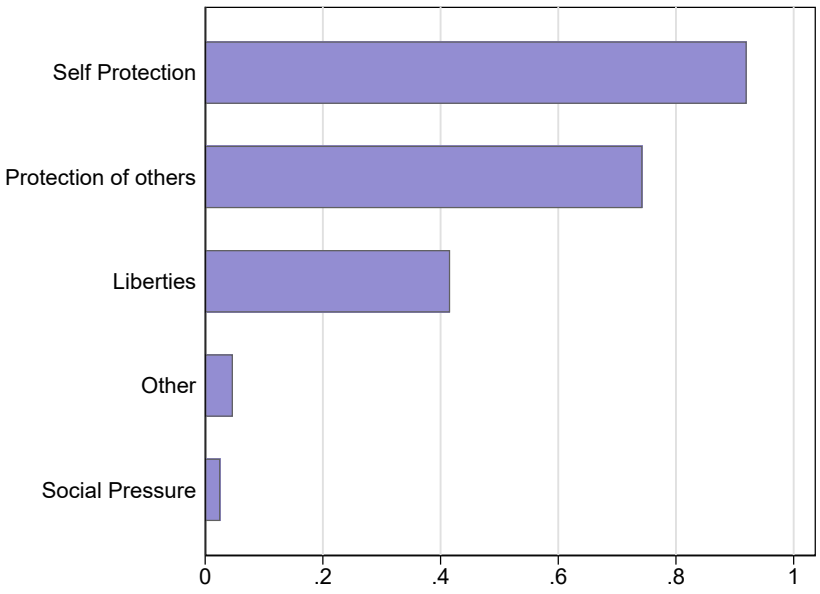
we control for binary indicators whether an individual has ever had chronic lung disease, high blood pressure, stroke, cancer or a heart attack or whether they have been sad or depressed in the last month. It is obvious that these variables are potentially endogenous and that a proper mediation analysis would also instrument those (as, for instance, done by Frölich and Huber, 2017). Nevertheless, this analysis can give suggestive evidence of the potential relevance of health as a potential channel. A similar analysis was performed by Eibich (2015) and Decker and Schmitz (2016) in another context. The results show, that the potential room for health being the reason for the small effect is negligible as the results are almost unaffected by the inclusion of health status.

The same holds for another potential mediator, namely labor force participation. While our sample mainly consists of older individuals outside the labor force, some are around retirement age. Again, education might affect labor force participation and, in turn, labor force participation might affect vaccination status. Again, taking an indicator of current employment into account does not affect the results.

6 Further mechanisms: fear of side effects

Turning back to the case of Germany, one advantage of the CASA data is that individuals were asked why they chose to get vaccinated against COVID or why not. In both cases it was possible for individuals to choose multiple options. The reasons for deciding for a COVID vaccination, given by vaccinated individuals, are presented in Figure 6. This question was only included in wave 3. Each bar represents the share of individuals who stated the given option as a reason for receiving a vaccination. The most important reason was *Self Protection*, being important for more than 90% of respondents, followed by *Protection of others*, stated by almost three quarters of participants. In 2021 in Germany, many parts of public life required either COVID vaccination or evidence of absence of COVID infection (that is, a current negative official test result). This was an important factor for more than 40% of the respondents. Other factors played only minor roles for individual decisions.

Figure 6: Reasons for receiving a vaccination

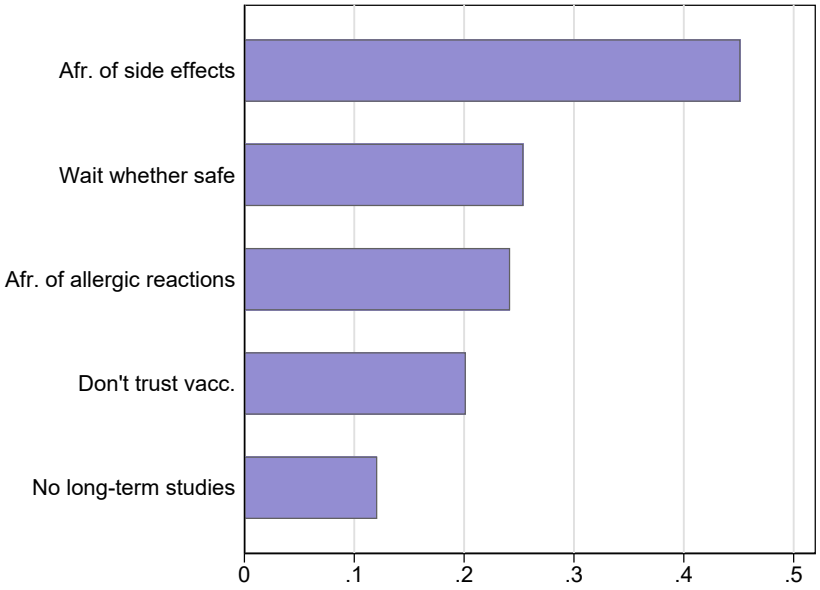


Notes: CASA data. Number of observations: 1,631. Question: "What were the motives for getting vaccinated?"; Answers: *Self Protection* = "To protect my own health.", *Protection of others* = "To protect others." or "Because I have at-risk patients in my environment.", *Liberties* = "Because vaccinated people have more liberties." or "Because the vaccination saves me tests." or "Due to 2G regulations, I can no longer participate in life without vaccination." (*2G* refers to *Geimpft oder genesen* (vaccinated or recovered). 2G regulations meant that only vaccinated or recovered individuals were allowed to enter certain establishments, such as restaurants), *Social Pressure* = "Because of social pressure.".

Figure 7 displays the opposite: reasons why unvaccinated participants did not get vaccinated. This information was collected in waves 2 and 3. We focus on answers that indicate fear of vaccination in any form rather than, e.g., religious beliefs or general distrust in the government. The most important reason against a vaccination was that 45% of the individuals were afraid of side effects. One fourth wanted to wait to see whether the

vaccines are safe or said they were afraid of allergic reactions. 20% said, they did not trust the vaccines in general, while 12% were waiting for results of long-term studies.

Figure 7: Reasons for not receiving a vaccination



Notes: CASA data. Number of observations: 248. Question: "Why do you not want to be vaccinated or why are you unsure whether you will be vaccinated?"; Answers: *Afr. of side effects* = "I am afraid of side effects.", *Wait whether safe* = "I will wait and see if the vaccine is safe and maybe get vaccinated afterwards.", *Afr. of allergic reactions* = "I am afraid of an allergic reaction to the vaccine.", *Don't trust vacc.* = "I don't trust the vaccination.", *No long-term studies* = "I wait for long-term studies.".

We next investigate whether years of schooling had an impact on reporting fear of side effects. We use OLS and a 2SLS regression in a similar fashion as before – including the same sample selection regarding birth cohorts. The results are presented in Table 5. The outcome variable is a binary indicator equal to one if individuals were afraid of the vaccination, indicated by stating at least one of the options depicted in Figure 7, and zero else. We argue that all indicators measure some sort of fear of side effects. Since only unvaccinated individuals were asked why they did not receive a vaccination, the outcome is not observed for vaccinated individuals. In columns (1) and (3), we include only those individuals who did not receive a vaccination, while in columns (2) and (4) we also include individuals, who received a vaccination. There, we argue that receiving a vaccination is revealed preference that fear of side effects are *not* a reason for not getting vaccinated and, thus, code the outcome variable as zero in columns (2) and (4).

The OLS results, presented in columns (1) and (2), indicate economically and statistically insignificant associations between education and fear of side effects. The results for the IV estimations are presented in columns (3) and (4). According to them, an additional year of schooling increases fear of side effects by 20 percentage points within the group of unvaccinated individuals. However, this estimate is based on a small sample size and should thus be interpreted with caution, especially since it is only significant at the 10%

level. However, even when including vaccinated individuals the estimate remains positive and large (almost 5 percentage points). Even if it is statistically insignificant, the analysis provides a hint that education may make individuals more afraid of side effects in our sample. Investigating explanations for this is beyond the scope of this paper. We note, however, that education has been shown to be correlated with prudence, for instance (Noussair et al., 2014).

Table 5: Regressions of fear of side effects

	OLS		IV	
Dep. var: Fear of side effects	Sample: No vaccination (1)	Sample: All (2)	Sample: No vaccination (3)	Sample: All (4)
Years of schooling	-0.022	-0.001	0.207*	0.047
	(0.020)	(0.002)	(0.116)	(0.033)
Male	-0.033	-0.023***	-0.013	-0.041***
	(0.067)	(0.008)	(0.079)	(0.014)
CASA W2	-0.210***	0.021***	-0.226**	0.022***
	(0.077)	(0.008)	(0.088)	(0.008)
Birth cohort FE	yes	yes	yes	yes
State FE	yes	yes	yes	yes
State-specific birth cohort trend	yes	yes	yes	yes
Observations	238	3,184	238	3,184

Standard errors in parentheses, clustered on birth-cohort × state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Only CASA data used here.

7 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 very scarce literature on the effect of education on influenza vaccination.

We use data on West German individuals from two different data sources to estimate the effect of eduction on COVID and influenza 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. By extending our analysis to several European countries, we find that years of schooling have a significantly positive but close to zero impact on vaccination status. We do, however, find indications that education seems to be related to the fear of side effects from COVID vaccination in Germany. The compliers in our sample report stronger fear of side effects from COVID vaccination as the reason not to be vaccinated due to an additional year of education.

We present a result that is in contrast to the well-established positive association between COVID vaccination and education. 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 or influenza infection. Moreover, individuals with low educational attainment make up a large share of this age group in Germany and Europe.

An important limitation of this study is its small sample size, which we counteract with extending our analysis to multiple European countries at the other drawback of pooling different school systems. Yet the results of both approaches are very consistent. Moreover, 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.

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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
$30 \leq$ Age < 50	0.29	0.30
$50 \leq$ Age < 70	0.33	0.36
$70 \leq$ 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

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

Table A2: OLS Regressions without sample selection

	(1) COVID vaccination	(2) Influenza vaccination
Years of schooling	0.014***	0.010**
	(0.002)	(0.005)
Male	0.013**	-0.020
	(0.005)	(0.015)
CASA W2	-0.078***	
	(0.009)	
CASA W3	0.058***	
	(0.009)	
SHARE W1		-0.340***
		(0.020)
SHARE W2		-0.264***
		(0.026)
SHARE W8		-0.145***
		(0.014)
Observations	17,570	5,499

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