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ABSTRACT

Shot Across the Bow, Stigma or Selection? The Effect of Repeating a Class on Educational Attainment*

The German practice of compelling weak students to repeat a class has come under heavy criticism recently. Many observers fear that this practice is, at best, useless or even counterproductive. However, little is known so far on the consequences of having to repeat a class, as compared to be confronted with new course material in the next class. This paper, therefore, aims at generating empirical evidence on the effect of class repetition on individual educational attainment. Since an experimental study is precluded, we utilize an instrumental variable approach to control for unobserved heterogeneity between respondents. Our estimation results suggest that there exists a negative association between repeating a class and educational attainment. However, taking unobserved heterogeneity into account yields a statistically significant and quantitatively substantial positive effect of class repetition on educational outcomes.

JEL Classification: I21, J13

Keywords: schooling degree, instrumental variables estimation

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1. Introduction

Around 3% of German students repeat their class[1] each year. In virtually all cases, this is not a voluntary choice. Poor performance in the current year leads schools to the decision, not to allow enrollment in the next highest grade. This aspect is idiosyncratic for the German education system. In Canada, Japan and England, for instance, weak students do not have to repeat a class until 9th grade; in Scandinavia it is very uncommon. Instead, these countries focus on special tutoring courses for weak students during leisure time and school holidays. Recently, the German practice of compelling weak students to repeat a class has come under heavy criticism[2] and the Green parliamentary party even calls for its abolition.

Opponents of this practice argue that it is costly and that students who have to repeat a class are discouraged by experiencing a failure early in life and by having to cope with a completely new classroom environment without their peers and friends. Furthermore, one might argue that these students are stigmatized and loose one year, i.e. they are older than necessary when leaving school and entering the labor market. According to this line of arguments, compelling students to repeat a class is a waste of resources and exhibits a negative impact on further educational outcomes.

On the other hand, proponents of the current practice – interestingly, the majority of parents seems to be among them (see *Süddeutsche Zeitung*, No. 174, July 30, 2004) – argue that students who are forced to repeat a class receive a shot across the bow to the right time. That is, they receive an unambiguous signal that their knowledge and learning effort is too low and, therefore, the chance to catch up and to modify their behavior. Furthermore, class repetition can work as an instrument to transfer students whose maturity is too low compared to their peers into a learning environment which is more adequate for them. According to this line of arguments, class repetition exhibits positive effects on the educational attainment of students.

Finally, it is also possible that the observed association between educational outcomes and class repetition is not causal, but the effect of selection on unobservables. That is, those students who lack in cognitive ability, intrinsic motivation or attention potential might more frequently be forced to repeat a class. They might also receive a lower schooling degree irrespective of the necessity to repeat a class. In this case, the negative association between class repetition and educational success would only reflect ability bias. Any empirical study dealing with this issue in a convincing manner must, therefore, effectively eliminate unobserved heterogeneity.

This paper aims at investigating this nexus empirically. To this end, we analyze two educational attainment outcomes for a sample of German youngsters aged 18-29 at the time of the interview (birth cohorts 1961-1973). To control for unobserved heterogeneity, we employ an instrumental variable approach. Our results suggest a strong and statistically significant negative association between repeating a class and educational attainment. However, taking unobserved heterogeneity into account yields

[1] In the following the terms “grade” and “class” are used synonymously.
[2] See e.g. the articles in respected newspapers *Süddeutsche Zeitung*, No. 174, July 30, 2004 or *Die Zeit*, No. 30, July 15, 2004.

a statistically significant and quantitatively substantial positive effect of class repetition on educational outcomes.

This suggests that the negative association is mainly driven by ability bias and that the typical student who is forced to repeat a class benefits from this measure. Clearly, this does not mean that alternative measures like special tutoring courses are unable to generate the same outcome – perhaps at even lower cost. However, it is evidence that the practice of class repetition is an effective intervention; at least it was for the birth cohorts under investigation.

The remainder of this note is organized as follows. **Section 2** explains the utilized dataset and the employed empirical strategy. In **section 3** the estimation results are reported and **section 4** offers some conclusions.

2. Data and Empirical Strategy

In our empirical application we utilize data from the *Young Adult Longitudinal Survey 1991-1995/1996* ("Junge-Erwachsene-Längsschnitt") conducted among 18-29 year old individuals in East and West Germany in 1991, 1993 and 1995/1996. This survey contains a large set of retrospective questions with the explicit aim to reveal information about the respondents' transition from childhood to adolescence and further on to adulthood. In addition, the dataset provides standard socio-demographic characteristics on the respondent and some core characteristics for his/her parents. Information on the parent-child relationship is also included.

Table A.1 in the appendix gives detailed descriptions of the considered variables. The outcome measures of interest are (i) whether an individual has received a high schooling degree, and (ii) whether the individual has attained a low schooling degree. Treatment is modeled by an indicator variable taking the value of 1 if an individual reported to he/she repeated a class between the age of 6 and 19. To control for observed heterogeneity, we take a variety of socio-demographic characteristics like gender, number of siblings, religious denomination etc. into account. Furthermore, we control for parental education to model the intergenerational dependence of educational attainment and implement two indicator variables for the parent-child relationship during the early years of childhood. These variables aim at modeling whether parents exhibited positive attitudes towards their child and the extent to which parents took care of their child[3].

Finally, to pin down the *causal* effect of class repetition on educational attainment, we implement an instrumental variables approach. As delineated above, the relationship between these two variables might be contaminated by ability bias. Thus, we employ a two-step estimation procedure in which the probability to repeat a grade is instrumented by the indicator variable "Physical development" in a first stage regression. This indicator variable takes on the value of 1 if an individual reported that he/she was *physically* less mature than his/her peers in 5^{th} *grade* and 0 otherwise (see

[3] **Table A.2** in the appendix provides some descriptive statistics for our sample. From this table it becomes transparent that the dataset comprises the birth cohorts 1961-1973.

also **Table A.1** in the appendix). The predictions of this first-stage regression are then used as a regressor in the second stage.

However, such an approach is only valid if the instrument meets two criteria. Firstly, it has to be related to the regressor of interest, i.e. is has to be correlated with class repetition. The second criterion that has to be met for the instrument to be valid requires that it must not exert any direct impact on observed outcomes, i.e. it must not be correlated with students' unobserved ability.

The idea behind this instrument is that students being physically less mature than their peers in 5th grade feel less comfortable in their learning environment and are, therefore, more likely to having to repeat a class. In the German schooling system with its rather rigid three disjunctive branches (*Hauptschule, Realschule, Gymnasium*), 5th grade marks the transition from primary to secondary education and is, therefore, a central grade for all students. Hence, the risk of class repetition might be higher for students feeling uncomfortable in their class environment during this time.

Typically, German 5th graders are 10 or 11 years of age. **Table 1** reports the age distribution of students having repeated a grade. From this table it becomes transparent that the incidence of class repetition is the highest for the "complicated years" of puberty around the age of 13-16. However, it is also relatively high for the typical age group of 5th grade.

Table 1: Age Distribution of Students Repeated a Class

Age of Repetition	Absolute Number	Share in %
6	6	1.57
7	9	2.35
8	20	5.22
9	11	2.87
10	21	5.48
11	9	2.35
12	34	8.88
13	44	11.49
14	59	15.40
15	54	14.10
16	54	14.10
17	34	8.88
18	22	5.74
19	6	1.57
Total	**383**	**100.00**

While given the discussion above it is likely that the chosen instrument meets the first criterion (which is also confirmed by the first-stage IV results, see below), it is *a priori* not clear whether it also fulfills the second one. Naturally, it is not possible to test whether the employed instrument is uncorrelated with students' unobserved ability. In consequence, this choice is an identification assumption which has to be judged upon economic reasoning alone.

The central argument in favor of the instrumental variable "Physical development" is the fact that we have not found any evidence in the literature on child development that physical and mental development are related except the possibility that malnutrition during early childhood might exert a negative impact on mental development. Such malnutrition, however, is very unlikely to happen systematically in an industrialized country like Germany during the 1970s or 1980s. Furthermore, it is difficult to find a channel linking the physical development status of a child during the pre-puberty years to educational outcomes.

A channel one might think of is that physical development status in the years directly preceding the finals exams for a specific schooling degree might have an impact on this outcome variable because students being physically more developed display a higher ability to assert themselves or are more motivated to leave school and enter the labor market to earn money. If this is the case and physical development in later school years are strongly related to physical development in early school years (persistence of physical development status), then our instrumental variable might unfold an effect on educational outcomes other than via the age at school entry.

Our dataset allows us to investigate this hypothesis in more detail since we also have information on the (self-assessed) physical development status in 7^{th} and 9^{th} grade. The correlation between the self-assessed physical development level in 5^{th} (pre-puberty) and 7^{th} (early puberty) or 9^{th} (puberty) grade, respectively, indicates that the relationship between these variables is rather weak, and declining the older the individuals become. Specifically, the correlation between physical development relative to the peers in 5^{th} and 7^{th} grade is 0.68 and that between 5^{th} and 9^{th} grade is 0.43. Finally, estimation results of an ordered probit model containing self-assessed physical development relative to the peers in 7^{th} or 9^{th} grade indicate no statistically significant impact of these variables on educational outcomes[4].

Hence, we are confident that our instrument is valid and that the instrumental variables approach yields the causal effect of class repetition on educational attainment. The next section reports the estimation results utilizing the dataset described above.

3. Estimation Results

To investigate the relationship between class repetition and educational success, we firstly estimate a model in which the outcome measures are regressed on the treatment indicator and all control variables. In this endeavor, we employed several specifications with various sets of control variables all yielding qualitatively identical results. The results of the final specification are reported in **Tables 2** and **3**.

For both outcomes we estimated a (non-linear) probit model as well as a linear probability model. The latter model is necessary since in the two-step estimation procedure employed later, we do not receive consistent estimates if both steps contain a non-linear model (see WOOLDRIDGE (2002), chapter 15). Thus, we estimate a linear

[4] Estimation results are available form the author upon request.

probability model for the outcome equation of interest and report the estimation results together with those of the probit model for the purpose of comparison.

Estimation results indicate a statistically significant negative relationship between class repetition and educational attainment. The estimated marginal effects of the probit and the linear probability model are very close and suggest that students who have repeated a grade exhibit a lower chance of around 5-6% to receive a high schooling degree than their peers who had not repeated, everything else equal (see **Table 2**). Estimation results in **Table 3** indicate that the risk to attain a low schooling degree increases significantly by around 4-5% (c.p.) if a students had to repeat a class.

Table 2: Estimation Results for Probit and Linear Probability Model – High Schooling Degree

	Probit Model		**Linear Probability Model**	
	Marginal Effect	**t-value**	**Coefficient**	**t-value**
Repeated class	-0.0550	-2.06	-0.0470	-1.91
Female	0.0043	0.22	0.0029	0.18
Year of birth	-0.0108	-3.59	-0.0093	-3.59
Number of siblings	0.0354	1.65	0.0244	1.39
Number of siblings squared	-0.0118	-2.30	-0.0080	-2.05
Father low education	-0.1629	-6.48	-0.1413	-6.19
Mother low education	-0.0923	-3.73	-0.0822	-3.73
Father high education	0.2005	6.83	0.2111	8.38
Mother high education	0.1292	3.65	0.1277	4.34
Joint activities	0.0331	1.52	0.0279	1.47
Parental attitudes	0.0932	4.85	0.0795	4.65
West	0.1281	4.85	0.1148	4.54
Peers	-0.0150	-0.59	-0.0097	-0.45
Atheist	-0.0285	-1.03	-0.0229	-0.93
Note: Number of observations: 2,642.				

Table 3: Estimation Results for Probit and Linear Probability Model – Low Schooling Degree

	Probit Model		**Linear Probability Model**	
	Marginal Effect	**t-value**	**Coefficient**	**t-value**
Repeated class	0.0490	2.15	0.0415	1.87
Female	-0.0825	-5.47	-0.0774	-5.20
Year of birth	0.0002	0.07	-0.0008	-0.36
Number of siblings	-0.0387	-2.49	-0.0513	-3.23
Number of siblings squared	0.0120	3.59	0.0152	4.30
Father low education	0.1365	6.51	0.1563	7.61
Mother low education	0.0819	4.15	0.0837	4.22
Father high education	-0.0631	-2.64	-0.0439	-1.94
Mother high education	-0.0684	-2.41	-0.0276	-1.04
Joint activities	-0.0331	-1.95	-0.0302	-1.77
Parental attitudes	-0.0436	-2.77	-0.0398	-2.59
West	0.1698	8.81	0.1518	6.68
Peers	-0.0312	-1.46	-0.0304	-1.57
Atheist	-0.0395	-1.72	-0.0440	-1.99
Note: Number of observations: 2,642.				

Hence, we clearly observe a negative association between class repetition and educational attainment supporting the stigmatizing effect of class repetition. To investigate whether and to which extent this association is causal or the effect of unobserved heterogeneity, we implement the instrumental variable approach delineated above. The first-stage comprises a probit model in which the dependent variable is the treatment indicator for class repetition and the independent variables comprise all controls together with the indicator variable "Physical development". The results of this first stage are reported in **Table A.3** in the appendix.

Estimation results suggest that (c.p.) West Germans are around 20% more likely to repeat a class than their peers in the eastern part of the country, whereas females exhibit a lower risk than males. Furthermore, younger birth cohorts, individuals without a religious denomination and respondents with siblings display higher probabilities of class repetition, whereas high parental attitudes reduce this likelihood, again everything else equal. Finally, individuals who reported that they had been physically less mature than their peers in 5th grade also display a higher risk to repeat a grade. The estimated impact of physical development is shy of being significant at the 95% level.

In the second stage of the IV approach, we utilize the predictions from this first-stage estimation as an explanatory variable. Since these predictions are an estimated regressor, it is necessary to account for this when estimating the standard errors of the second-step coefficients. This is done by implementing the Murphy-Topel correction for two-step estimation procedures (MURPHY AND TOPEL (1985); see also GREENE (2000)). Estimation results of the second-stage linear probability model are reported in **Table 4** for both outcomes.

Table 4: Instrumental Variables Estimation Results (Linear Probability Model)

	High Schooling Degree		**Low Schooling Degree**	
	Coefficient	**t-value**	**Coefficient**	**t-value**
Repeated class (first stage prediction)	0.4808	4.26	-0.2639	-2.89
Female	0.0228	2.81	-0.0889	-13.50
Year of birth	-0.0117	-9.73	0.0005	0.56
Number of siblings	0.0089	1.09	-0.0423	-6.43
Number of siblings squared	-0.0058	-3.41	0.0139	10.02
Father low education	-0.1543	-15.47	0.1638	20.27
Mother low education	-0.0542	-4.92	0.0675	7.57
Father high education	0.2095	19.78	-0.0430	-5.01
Mother high education	0.1322	10.65	-0.0302	-3.00
Joint activities	0.0272	3.42	-0.0297	-4.61
Parental attitudes	0.1022	11.79	-0.0529	-7.54
West	-0.0024	-0.09	0.2197	10.02
Peers	-0.0116	-1.27	-0.0293	-3.98
Atheist	-0.0595	-4.60	-0.0228	-2.18
Note: Number of observations: 2,642.				

Ability-adjusted estimates suggest a statistically significant and quantitatively substantial *positive* impact of class repetition on educational attainment. All other

things equal, the typical student in our sample experienced a 48% higher chance to obtain a high schooling degree and a 26% lower risk to attain a low schooling degree than comparable peers who had not repeated. Hence, the observed negative association between class repetition and the educational success are – to a large extent – driven by ability bias. Once the impact of ability is removed, the intervention under investigation exerts a positive effect and is, therefore, effective.

However, "traditional" instrumental variable approaches identify the effect of an intervention only if the treatment effect is constant for individuals with the same value of the covariates (see FLORENS ET AL. (2002) and IMBENS AND ANGRIST (1994)). In the case of heterogeneous treatment effects, i.e. the impact of the intervention, here the repetition of a class, varies over the population, the "traditional" instrumental variable approach identifies the mean effect of the intervention for the sub-population of the so-called compliers. That is, for those individuals whose value of the treatment indicator changes in reaction to an exogenous change in the instrument[5].

Thus, in the case at hand the quantitatively rather large estimated effect of class repetition identified by invoking the instrumental variable "Physical Development" might be the effect only for a sub-population of students. It might reflect the benefits of repeating the class for those students whose maturity was too low at some point during their school career. However, since the homogeneity or heterogeneity of treatment effects across the population is unobservable, it is impossible to resolve this problem ultimately.

With respect to other explanatory variables, the IV approach yields results comparable to that of the simple probit or linear probability model of **Tables 2** and **3**. Some explanatory variables exhibit a statistically significant impact in the IV approach but not in the original model, whereas the significant difference between East and West Germans for the high schooling degree in the original model vanishes. In general, however, the estimated second-stage coefficients resemble the results of the original model very closely.

4. Conclusions

This paper investigated the causal impact of class repetition on educational outcomes in Germany utilizing a sample of individuals born between 1961 and 1973. The practice of compelling weak students to repeat a grade has come under heavy criticism recently since opponents argue that it is either a useless waste of resources or even exerts a negative impact on the relevant students. To analyze whether and to which extent the observed relationship between having to repeat a class and educational success is contaminated by ability bias, we implemented an instrumental variable approach in which class repetition is instrumented by (self-reported) *physical* development status of respondents in 5th grade.

[5] This is also known as the concept of local average treatment effects (LATE; see IMBENS AND ANGRIST (1994)).

Our estimation results indicate a statistically significant *negative* association between repeating a class and educational attainment. However, taking unobserved heterogeneity into account yields a statistically significant and quantitatively substantial positive effect of class repetition on educational outcomes. Hence, ability-adjusted estimates suggest that the negative association is driven by ability bias and that a typical student who is forced to repeat a class benefits from this measure. In other words, the intervention turns out to be effective.

However, it is worthwhile to note some limitations of the above analysis. Since the dataset does not comprise information on the behavior of students in response to having to repeat a grade, it is not possible to investigate if the subsequent success of treated students is simply due to a change in behavior/learning effort or the result of further interventions like, for instance, private tutoring courses. For the same reasons, it is, furthermore, not feasible to discover how successful these students had been in their final exams. That is, since we do not have information on marks, it is possible that treated students might have just managed to pass the final exams. Along the same lines, we are not able to pin down the extent to which the school itself or specific features of the education system contributed to the positive impact of class repetition due to missing information on school characteristics and on the Federal State in which respondents received their schooling degree.

Finally, our results do obviously *not* imply that alternative measures like special tutoring courses are unable to generate the same outcome – perhaps at even lower cost. However, it is evidence that the practice of class repetition is an effective intervention, at least for the birth cohorts under investigation. Whether and to what extent alternative interventions are more effective and/or efficient is anything but guaranteed. The provision of an answer to this question requires that alternative interventions are implemented – while current practice is not completely abolished – and that both intervention regimes are evaluated carefully.

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Appendix

Table A.1: Description of Variables

Variable	Description
	Outcome Measure
High schooling degree	Indicator variable taking on the value 1 if respondent has upper secondary or technical schooling degree (*Abitur*); 0 otherwise.
Low schooling degree	Indicator variable taking on the value 1 if respondent has no schooling degree or completed secondary (*Hauptschule*); 0 otherwise.
	Treatment variables
Repeated class	Indicator variable taking on the value 1 if respondent repeated a class between 6 and 19 years of age; 0 otherwise.
	Control Variables
Female	Indicator variable taking on the value 1 if respondent is female; 0 otherwise.
Year of birth	Year of birth of the respondent.
Number of siblings	Number of siblings of respondent.
Atheist	Indicator variable taking on the value 1 if respondent reported no religious denomination; 0 otherwise
Peers	Indicator variable taking on the value 1 if respondent reported having had friends during childhood and adolescence; 0 otherwise.
Father low education	Indicator variable taking on the value 1 if respondent's father has no schooling degree or completed secondary schooling degree; 0 otherwise.
Mother low education	Indicator variable taking on the value 1 if respondent's mother has no schooling degree or completed secondary schooling degree; 0 otherwise.
Father high education	Indicator variable taking on the value 1 if respondent's father has upper secondary or technical schooling degree; 0 otherwise.
Mother high education	Indicator variable taking on the value 1 if respondent's mother has upper secondary or technical schooling degree; 0 otherwise.
Joint activities	Indicator variable taking on the value 1 if respondent reported having shared at least two of the following four joint activities with her parents during childhood: reading, sports, music and sharing other hobbies; 0 otherwise.
Parental attitudes	Indicator variable taking on the value 1 if respondent reported her parents having had at least two of the following four positive attitudes towards her during childhood: to put hope into the child, to believe that the child is highly able, to be ambitious with the child and to have plans with the child; 0 otherwise.
West	Indicator variable taking on the value 1 if respondent was raised in West Germany; 0 otherwise.
	Instrumental Variable
Physical development	Indicator variable taking on the value 1 if respondent reported having been physically less developed than his/her peers during 5th grade; 0 otherwise.

Table A.2: Summary Statistics

Variable	Mean	Standard-deviation	Minimum	Maximum
		Outcome Measures		
High schooling degree	0.3225	0.4675	0	1
Low schooling degree	0.2271	0.4190	0	1
		Treatment Indicator		
Repeated class	0.1370	0.3439	0	1
		Control variables		
Female	0.4951	0.5001	0	1
Year of birth	67.70	3.2121	61	73
Number of siblings	1.3471	1.1304	0	6
Number of siblings squared	3.0920	5.0797	0	36
Atheist	0.3285	0.4698	0	1
Peers	0.8210	0.3835	0	1
Father low education	0.4845	0.4999	0	1
Mother low education	0.5363	0.4988	0	1
Father high education	0.2385	0.4262	0	1
Mother high education	0.1249	0.3307	0	1
Joint activities	0.2721	0.4451	0	1
Parental attitudes	0.6143	0.4869	0	1
West	0.6779	0.4674	0	1
Physical development	0.1760	0.3809	0	1
Note: Number of observations: 2,642				

Table A.3: First-stage Instrumental Variables Estimation Results (Dependent Variable: Repeat Class)

	Coefficient	t-value	Marginal Effect	t-value
Physical development	0.1618	1.94	0.0310	1.82
Female	-0.1896	-2.88	-0.0340	-2.87
Year of birth	0.0262	2.52	0.0047	2.52
Number of siblings	0.1504	2.16	0.0270	2.16
Number of siblings squared	-0.0217	-1.43	-0.0039	-1.43
Father low education	0.1138	1.24	0.0205	1.24
Mother low education	-0.2467	-2.83	-0.0448	-2.79
Father high education	-0.0011	-0.01	-0.0002	-0.01
Mother high education	-0.0531	-0.45	-0.0093	-0.46
Joint activities	-0.0138	-0.18	-0.0025	-0.18
Parental attitudes	-0.2151	-3.23	-0.0398	-3.14
West	1.4262	11.30	0.1976	15.14
Peers	0.0260	0.29	0.0046	0.30
Atheist	0.3719	3.72	0.0723	3.45

Note: Number of observations: 2,642.