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Experimental evidence from the ZuBAb study 0.5 years after high school graduation

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Abstract

Do intensive guidance programs reduce social inequality in the transition to higher education in Germany?
Experimental evidence from the ZuBAb study 0.5 years after high school graduation

by Melinda Erdmann, Irena Pietrzyk, Marcel Helbig, Marita Jacob, Stefan Stuth*

This paper examines the effect of an intensive counseling program to promote university access among students who are eligible for university. Using data from the experimental panel study ZuBAb, we examine the average effect on university enrollment directly after high school graduation and the effect heterogeneity by educational background. No positive effect of participation is found. We discuss these results in relation to the potential of reducing inequalities through individual counseling in Germany.

Keywords: university access, educational intervention, experiment, social origin

Im Beitrag wird die Wirkung eines intensiven Beratungsprogramms zur Förderung der Studienaufnahme von Hochschulzugangsberechtigten untersucht. Mittels Daten aus der experimentellen Panelstudie ZuBAb werden der durchschnittliche Effekt auf die Studienaufnahme direkt nach dem Abitur und die Effektheterogenität nach Bildungsherkunft überprüft. Es zeigt sich keine positive Wirkung der Teilnahme. Diese Ergebnisse werden in Bezug auf die ungleichheitsreduzierenden Potentiale individueller Beratung in Deutschland diskutiert.

Schlüsselwörter: Studienaufnahme, Bildungsintervention, Experiment, soziale Herkunft

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1 Introduction[1]

Participation in higher education is highly unequal in Germany. This is despite growing numbers of students who are eligible for higher education, rising university enrollments, and many educational reforms (Bundesamt für Statistik 2019; Autorengruppe Bildungsberichterstattung 2020, p. 190). Thus, to this day, the social background of students with a university entrance qualification has a critical influence on their decision to actually pursue higher education (Becker and Hecken 2008; Hillmert and Jacob 2010; Becker 2012; Lörz 2012; Reimer and Schindler 2013; Helbig et al. 2015). Researchers have discussed differences in access to information and social support during the transition to higher education as possible reasons driving these disparities (Hoxby and Turner 2013; Barone et al. 2017; Ehlert et al. 2017; Daniel et al. 2018). To counteract these inequalities, many individual programs were initiated by educational policy and civil society groups about a decade ago to foster educational success and higher education access among disadvantaged groups (BMBF 2010, pp. 51/56), including *Arbeiterkind.de*, *TalentRuhr,* and *Talentscouting NRW.*

Empirical evidence, mainly from the U.S. and Canada, supports the idea that such programs might be effective. The current state of research, especially in North American studies, suggests that individual counseling increases university enrollment among low-SES students who are eligible for higher education and that it tends to be more effective than short workshops providing information (for an overview: Herbaut and Geven 2020). In the European context, (quasi-)experimental studies examining the impact of shorter information workshops on enrollment are still scarce, but the existing ones offer a rather pessimistic assessment of their effectiveness (e.g., Barone et al. 2017; Ehlert et al. 2017; Daniel et al. 2018). So far, only very few studies have investigated individual counseling programs in European countries (Herbaut and Geven 2020). Transferring the results from North America to the German context considered in this article is problematic given the significant differences in school and university systems. For example, general schooling in the U.S. is less stratified, whereas higher education is considerably more differentiated. Both aspects may affect the extent and kind of guidance and support that adolescents need and could thus lead to different impacts of such programs in Germany and the U.S.

[1] The research for this article was funded by the Ministry of Culture and Science of the state of North Rhine-Westphalia.

In light of the illustrated research gap, this article aims to investigate whether an intensive individual counseling program promotes university enrollment among eligible high school graduates. For the first time in the German context, this research question is analyzed by applying a randomized controlled trial (RCT). Since reducing educational inequalities is of great socio-political importance, the study also investigates whether the program's impact on university enrollment depends on students' educational background. This is essential because the extent to which educational interventions reduce inequalities depends, among other things, on the degree of effect heterogeneity by educational origin (see Pietrzyk und Erdmann 2020). The analysis uses data from the experimental ZuBAb study (*Zukunfts– und Berufspläne vor dem Abitur*[2]) to examine the effect of the program on university enrollment six months after high school graduation. The data was collected from students attending academic high schools (Gymnasium) and comprehensive schools (Gesamtschule). Both school types award the German university entrance diploma (*Abitur*), which is a prerequisite for entry into tertiary education.[3] The findings on the program's impact six months after high school graduation, as presented here, differ largely from the program's effect one year later. More specifically, an impact of the program on educational choices cannot be confirmed immediately after graduation, whereas a strong effect on educational pathways unfolds one and a half years after graduation (Erdmann et al. 2022).

This article is structured as follows: The next section (2) presents programs designed to reduce educational disparities at the transition to higher education and the current state of research on their effectiveness. The following section (3) introduces the counseling program examined here (3.1), the survey data (3.2), and the experimental design (3.3). The fourth section offers a descriptive examination of the different educational pathways six months after high school graduation for both experimental groups depending on the random assignment (4.1), presents the results of the impact analysis on university enrollment (4.2), and discusses the robustness of the findings (4.3). The paper concludes with a discussion of the inequality-reducing potential of guidance

[2] *Future and Career Plans before High School Graduation* in English.

[3] Secondary schools are highly stratified in Germany. Whereas vocational tracks (i.e., *Realschule, Hauptschule*) end before upper secondary school and therefore do not lead to the *Abitur,* the academic or comprehensive tracks (i.e., *Gymnasium, Gesamtschule*) continue through upper secondary education, with most students enrolled in these tracks acquiring the *Abitur*. For reasons of simplicity, the term "high school graduates" is used in the following to refer to students who graduate from upper secondary school and, as a result, are eligible for university admission (*Hochschulzugangsberechtigte).*

counseling at upper secondary schools in Germany in international perspective (5).

2 State of research on programs promoting university enrollment

2.1 Programs to reduce social inequalities in university enrollment

Many programs seeking to reduce educational inequalities draw on sociological findings about the causes of social disparities at educational transitions. These interventions are based on decision-making theories, like the rational choice approach, or conflict theories from the sociology of education, which are also used frequently in empirical research. From a theoretical rational choice perspective, social inequality in educational decisions is the result of primary and secondary effects of social origin (Boudon 1974; Erikson and Jonsson 1996; Breen and Goldthorpe 1997; Esser 1999). Primary effects relate to varying levels of academic performance dependent on social origin due to class-specific differences in access to material and nonmaterial resources. Secondary effects, by contrast, refer to people's subjective assessment of costs and benefits, as well as probability of success of different educational alternatives (Boudon 1974). From the perspective of Bourdieu and Passeron's (1971) cultural reproduction theory, social inequality in educational choices is the result of existing class cultures that are inscribed into individuals as habitus. This class-specific habitus includes, among other things, language conventions, social attitudes, and a range of lifestyles, which shape educational behavior and aspirations (e.g., Ecarius and Wahl 2009). Inequality in educational choices in part arises because the habitus of higher classes strongly corresponds with the academic system (see Bourdieu and Passeron 1995). At the same time, the distance between the socio-cultural codes of the academic system and the habitus of individuals from lower social classes can evoke feelings of exclusion and of being "out of place" (e.g., Schmitt 2010).

Interventions to reduce educational inequality are based on the assumptions of both rational choice theory and cultural reproduction theory. For example, there are programs that concentrate on providing information about the costs and benefits of going to university, such as information portals of student services (*Informationsportale der Studierendenwerke*) or student guidance services (*Studienberatungen der Hochschulen*), both offered by higher education institutions and addressing prospective as well as enrolled students. The goal of these interventions is to change the subjective estimation of the above-

mentioned factors, which are assumed to strongly influence post-secondary educational decisions. Programs that provide financial aid or offer financial incentives also potentially influence the estimation of costs (e.g., *Deutschland Stipendium*, *mystipendium.de*). Other interventions, in turn, additionally aim to expand social networks in order to reduce the social distance to higher education and strengthen social support (e.g., *ArbeiterKind.de, Talentscouting NRW*, universities' mentoring programs for students).

2.2 Investigating the effectiveness of programs through experimental studies

In the German context, the effectiveness of such interventions and programs has not been sufficiently examined so far. Especially more robust procedures such as experimental evaluation methods have rarely been used in Germany – even though experimental designs are particularly suitable for evaluating (educational) programs, as they allow for internally valid causal conclusions about the programs' effects. Compared to other designs in empirical social research, they are relatively robust to biases due to confounding variables (e.g., Cook 2002; Zangger and Becker 2019). Since individuals are randomly assigned to different groups (e.g., intervention/no intervention), observed and unobserved characteristics are distributed randomly over the given conditions, therefore maximizing average similarity between the individuals in these groups and thereby avoiding selection bias. Especially in case of non-curricular educational intervention, self-selection based on, for example, strong motivation to pursue university studies among specific students or selection by others, for example by teachers, cannot be ruled out entirely. Such a selection bias can distort the results on the program's effectiveness through observed and unobserved confounding variables. Of course, it is possible to control for observed characteristics in non-experimental data in order to consider a potential selection bias (e.g., via commonly used regression analysis or via propensity-score matching). However, the risk of unobserved confounding variables biasing the results cannot be minimized to the same extent as it can be in experiments.

In line with the aforementioned advantages of experimental designs, randomized controlled trials (RCTs) are now frequently applied when evaluating interventions promoting university enrollment. Thus, the number of scientific works using RCTs to analyze such interventions has increased remarkably (for an overview, see Herbaut and Geven 2020). Some of these studies explicitly

concentrate on the impact for disadvantaged high school graduates and hence specifically address the interventions' potential to mitigate inequality. Overall, the results indicate that individual counseling programs can foster university enrollment more effectively than short information workshops. Out of 17 studies included in a systematic review on the effect of individual counseling and support programs, most papers showed significant effects on university enrollment, and three quarters found effect sizes of more than 5 percentage points, whereas information workshops hardly influence enrollment (Herbaut and Geven 2020).

Despite the growing interest in such research, RCTs evaluating educational programs in the European context remain scarce. For example, Herbaut and Geven (2020) identified only six studies from Europe – out of the 70 studies they considered in their review of interventions to promote university enrollment. In Germany, only two studies have examined educational interventions to encourage university enrollment among high school graduates using (quasi-)experimental methods. Both studies evaluate relatively short interventions. In their study on a one-time information workshop, Daniel and colleagues showed that participating students both felt better informed and assessed the returns of attending university shortly after attending the intervention differently than the control group. However, there was no effect on participants' intention to enroll in university (Daniel et al. 2018).

The "Best Up" study (*Berliner–Studienberechtigten–Panel*), a randomized-controlled trial providing panel data of upper secondary school students eligible for university, was used to investigate how a 20-minute information workshop in schools (Ehlert et al. 2017) on the one hand and the prospect of receiving a monthly stipend of 300 euros (Peter et al. 2017) on the other hand had an effect on whether participants with a high intention to pursue university studies eventually applied to university. Impact analysis of the information workshop showed positive effects for a very specific subsample of students with low educational background: Individuals who already had a strong intention to enroll in university displayed a higher probability of applying to university than individuals from the control group (Ehlert et al. 2017). In contrast, the prospect of financial support did not have any effect on enrollment, neither for the entire group nor for subgroups (Peter et al. 2017).

For intensive counseling programs, which go beyond one-time information workshops, some studies from North America exist that establish an effect on university enrollment. Even though the examined programs prove to be more efficient than short information interventions, the effect sizes turn out

to be heterogeneous, with estimates ranging from 0 to 20 percentage points difference in university enrollment (for an overview, see Herbaut and Geven 2020). This large range probably results from the major differences between the interventions, which, among other things, might stem from varying goals and theoretical groundings. For example, some interventions set a special focus on financing (e.g., Bettinger et al. 2012), whereas other programs aim at promoting academic competences (e.g., Avery 2013). Likewise, the programs differ remarkably in terms of duration: Whereas some interventions start three years before high school graduation (e.g., Ford et al. 2012), others address the period between finishing school and university enrollment (e.g., Castleman et al. 2014). Furthermore, efficiency was investigated within different education systems. Despite the increased research interest, a standardized typology of one-on-one counseling programs promoting university access has not yet emerged. In combination with intensified research efforts, such a typology would enable a systematic comparison of the interventions and their impact in different education systems and, on this basis, a differentiated prediction of how effective a specific counseling program might be in a particular education context.

2.3 Hypotheses

In contrast to existing research on information workshops, individual counseling has not yet been studied in Germany using methodologically rigorous RCTs. This research gap is particularly remarkable given the number of such programs already established (e.g., *Arbeiterkind.de*, *Talentscouting NRW, TalentRuhr, Lebensbegleitende Berufsberatung (LBB) für Schülerinnen und Schüler der Sekundarstufe II der Bundesagentur für Arbeit*). Although German research on information workshops comes to a rather pessimistic assessment of the possibility to influence education decisions after the end of upper secondary school, individual counseling programs might still prove to be efficient in promoting university enrollment in Germany, given the findings of international research. This is because such programs, unlike information workshops, provide information potentially more tailored to students' individuals interests and connect them to a reliable contact person. In addition, the negative effects of experienced social distance to higher education could potentially be reduced in a more targeted manner than is the case with general information workshops.

Therefore, we hypothesize that an individual counseling program will promote university enrollment among high school graduates (Hypothesis 1).

Furthermore, it can be assumed that individual counseling programs will influence university enrollment differently depending on the participant's educational background. As mentioned above, social origin influences both the estimation of the costs and benefits of attending university and the estimation of the probability of successfully completing higher education. The experienced social distance to tertiary education is also influenced by social origin. Thus, the positive effect of the program could be greater for participants without a university-educated background than for participants from university-educated families.

Hence, we expect the individual counseling program to promote the university enrollment of high school graduates differently depending on their educational backgrounds, with the strongest benefits occurring for students without a university–educated background (Hypothesis 2).

3 Research design

3.1 The counseling program

The hypotheses presented above are investigated on the basis of an individual counseling and support program run by several universities in the German state of North Rhine-Westphalia (NRW). The counsellors are part of the universities advisory services but have been additionally qualified for the program. The core of the program is individual and intensive student counseling in upper secondary school. The counseling is carried out individually and, if necessary, several times. The entire program is designed to provide long-term support and continues beyond high school, if necessary. At the time of the evaluation, the primary goal of the program was to foster university enrollment among students without a university-educated background.

At the beginning of the program, participating students and professional counselors meet for an initial individual counseling session. In this first meeting, they discuss the students' future aspirations, interests, and problems regarding their choice of post-secondary education and, if necessary, address initial concrete concerns. The further progression of the program is tailored to students' individual needs, questions, and uncertainties. In subsequent

individual meetings, educational paths can be discussed in detail (e.g., with regard to the choice of university major or the specific occupation for which an apprenticeship qualifies), and questions about the concrete implementation of an aspiration can be clarified (e.g., financing, requirements for applying for university admission or an apprenticeship position). Depending on a student's aspirations, the counselors offer various additional support services. These include networking meetings with other students with similar aspirations and uncertainties regarding post-secondary education, networking with professionals in the occupations students are considering, visiting campuses, taking placement tests, and referrals to other counseling services.

In addition to the comprehensive and accurate information provided by the counselors, the program focuses on establishing a reliable relationship of trust between the counselors and the participating students. The advisors see themselves as contact persons for all questions concerning post-school education, which can also include personal uncertainties. To ensure low-threshold accessibility and regular exchange, communication channels outside of counseling are also used in everyday practice, such as exchanges via short messages. These forms of exchange, as well as counseling in one-on-one meetings, remain available to the young adults once they have started a post-school educational path. In this way, uncertainties arising during university study or vocational training can be addressed.

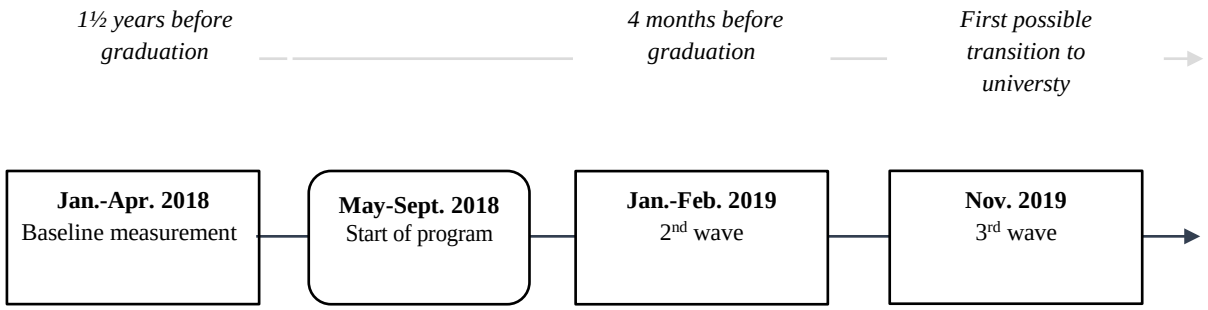
3.2 Data

Both for the experimental design and for the subsequent empirical analyses, panel data of high school students attending *Gymnasium schools* (academic schools) and *Gesamtschulen* (comprehensive schools) are used (for a detailed overview of the study, see Pietrzyk et al. 2019).[4] Initial data collection was conducted in the form of a 90-minute written survey given to the students in school at the beginning of 2018 (N = 1.766). The survey instrument included questions about career aspirations and future plans, attitudes towards different post-secondary educational pathways, as well as measures of various socio-emotional competencies, academic achievement, and social background. The questionnaire was augmented by a test of cognitive skills (Heller and Perleth

[4] Only schools attended by socioeconomically disadvantaged students were considered for the panel study ("Standorttypenkonzept," see Isaac, 2011). Out of 125 eligible schools, 42 schools were recruited for the study.

2000) (for a more detailed description of the survey instrument, see Pietrzyk et al. 2019). The data collected in this first wave serves as a baseline measure for the experimental evaluation of the program. Subsequently, participants were individually and randomly assigned to the different experimental conditions (see below). The counseling program began in May 2018. Since then, two additional surveys have been completed using an online questionnaire to obtain, among other things, information about the participants' further educational and career paths (see Fig. 1; for the second wave: N = 1.512; for the third wave: N = 1.374).[5]

Figure 1: Steps of the panel study



Source: Own diagram

3.3. Design of the randomized controlled trial (RCT)

Due to the advantages of experimental designs for evaluating interventions (as outlined above), the RCT is a central component of the impact analysis.[6] For the RCT, a total of 1,404 students at 31 schools (22 academic schools, 9 comprehensive schools) were considered.[7] In accordance with the goal of the program, students were included into the RCT stratified by educational background within each school, meaning: students without a university-educated parent were given priority access to the RCT, any remaining slots were

[5] A fourth study was conducted in winter of 2020/21. At the time of writing this article, the data were not yet available.

[6] The RCT of the ZuBAb-Study is registered on the *social science registry* platform with the identification number 2738: https://www.socialscienceregistry.org/trials/2738/

[7] Because capacity to offer the program was limited, the counseling program could not be offered at all study schools. Schools were randomly selected for the RCT, with a priority on comprehensive schools because they were slightly underrepresented in the pool of study schools.

filled with students with university-educated parents.[8] Among the participants of the RCT, students were assigned individually and randomly to the treatment group (program participation) or the control group (no program participation).[9] School and educational background served as blocking variables for the randomization, which was performed externally through an employee of GESIS (Leibniz Institute for the Social Sciences). Ultimately, *n* = 702 students were assigned to the program condition and *n* = 702 students to the control condition. The randomization led to an equal distribution of relevant observed predictors of university enrollment between experimental conditions, namely initial intention to enter university and initial performance level (see Table 1).

Table 1: Distribution of selected characteristics in control and treatment group

Characterstics in wave 1	Control group	Treatment group	Diff.
Initial performance level	8.89	9.10	-0.21
N	657	653	1310
Initial intention to enter university	3.62	3.65	-0.03
N	687	675	1362
No university-educated parents	0.52	0.52	0,00
At least one university-educated parent	0.48	0.48	0,00
N	681	679	1360

Operationalization: Initial performance level on a 15-point grading scale; initial intention to enter university on a 5-point Likert scale. Differences are reported on the original scale of the construct or as difference in percentage points.

Shortly after the randomized assignment, the responsible teachers were informed about the allocation result and asked to invite students assigned to the treatment group to participate in the program.

Further on, the effect of the program is centrally investigated by comparing the educational trajectories between the experimental conditions (treatment group/control group). Thus, we follow the experimental intention-to-treat strategy, in which an analysis includes the randomized *assignment* to the experimental conditions rather than *actual participation* in the program. This strategy allows to maintain the experimental assignment to the groups and, in

[8] Missing information on educational background was imputed using the number of books in the household, parents' occupational status, and parents' occupations.

[9] Because of the small number of study schools, we decided to randomize at the individual level instead of a random assignment of schools to experimental conditions.

addition, has the advantage of being highly relevant for education policies: the estimated results should be close to the actual average effect of the program under real-world conditions (e.g., Hollis and Campbell 1999).[10]

To examine the program's effect, we use data from the third wave, conducted only a few months after participants' high school graduation. After excluding one school, where the school leadership decided not to continue their participation in the study shortly after the first survey, the data includes n = 1,034 students, divided into treatment (n = 510) and control group (n = 524). Without taking the excluded school into account, panel mortality between first and third wave is 23 percent.[11]

4 Results

In the following, we present the effect of the investigated individual counseling program on university enrollment a few months after participants' graduation from high school. We start by describing the educational pathways of the two examined groups (treatment group / control group) after graduation (4.1.). Subsequently, we offer a multivariate examination of the average treatment effect (H1) and an investigation of effect heterogeneity by educational origin (H2) (4.2.). Finally, we address possible violations of statistical assumptions and check for the robustness of our results (4.3.).

4.1 Descriptive results: Educational pathways shortly after high school graduation

As presented in Figure 2, six months after graduating from high school, a large share of respondents had enrolled in university (around 45 percent). One third had not yet decided on their next educational step and was taking a gap year[12] (around 33 percent). A smaller share of respondents had started vocational

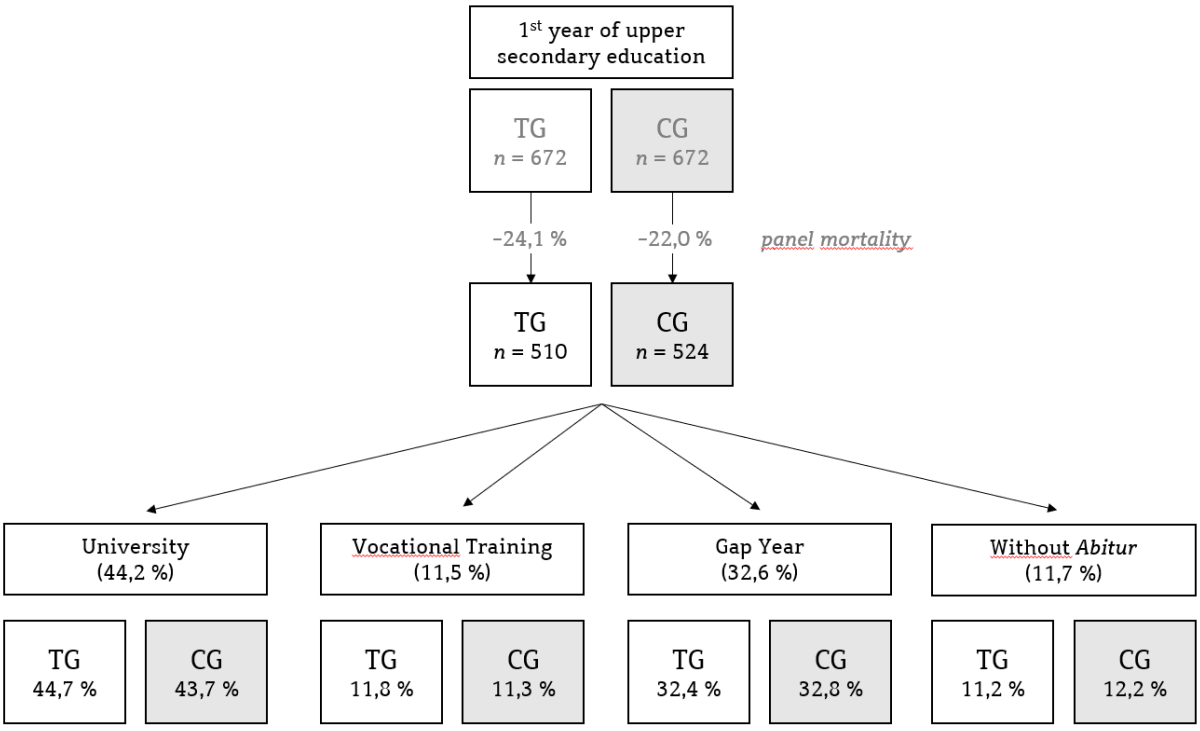
[10] This is the case because it is to be expected that even under real-world conditions, individual students will not take up a program offer – and they should strongly resemble those students who forgo program participation in the RCT.

[11] We address the issue of panel mortality for inferences about the program's efficiency below by examining whether there was a systematically unequal dropout between experimental groups on important predictors of university enrollment (see Section 4.3).

[12] Respondents were assigned to this category if they did not take up post-secondary education after their high school graduation. Table A1 in the appendix shows the specific activities of these individuals.

training[13] (12 percent) or had not (yet) obtained the *Abitur* at the time of the survey (around 12 percent).

Figure 2: Educational paths of treatment and control group 0.5 years after graduation



Note: TG: Assignment to Treatment Group; CG: Assignment to Control Group; University: includes dual study programs; without Abitur (high school diploma): includes individuals who dropped out of school or repeated one year. In parentheses: Distribution over the entire sample without taking into account the assignment to the experimental conditions' groups.

When looking at how university enrollment is distributed in both experimental conditions, hardly any differences between treatment group (TG, assigned to the counseling program) and control group (CG, not assigned to the counseling program) can be seen. In fact, the descriptive presentation strongly suggests that the counseling program had no average effect on university enrollment a few months after respondents obtained the *Abitur*.

[13] The German vocational education and training system consists of a dual apprenticeship system and a smaller number of fully school-based programs. Both types lead to occupation-specific certificates.

4.2 Results of the impact analysis

Both hypotheses—expecting the individual counseling program to foster university enrollment (H1) and expecting the treatment effect to depend on educational background (H2)—are tested using university enrollment as the main dependent variable. We operationalize this variable as the current educational path (Models H1a and H2b) and under consideration of the intention to enter university (Models H1b and H2b). In the first case, we consider respondents' situation at the time of the survey. Accordingly, those who have taken a gap year or have not (yet) attained their high school diploma have not (yet) enrolled at university. In the second case, we take into account that a program-related difference in the intention to enter university might have emerged, especially among respondents who have taken a gap year. Therefore, we additionally use participants' intention to study at higher education institutions as a proxy for university enrollment. This means respondents who report a high intention to attend university six months after graduation are considered as enrolled university students.[14]

We use linear probability models for hypothesis testing.[15] In these models, the random assignment to the experimental conditions is the main independent variable. We add participants' initial intention to enter university measured in the first survey wave to the models to increase power, as well as participants' school.

Table 2 shows the results of the linear probability models for university enrollment, calculated with robust standard errors.[16] In the models H1a and H1b, we test the first hypothesis about the treatment effect on university enrollment (Model H1a) and additionally under consideration of the intention to enter higher education institutions (Model H1b). In both models, the random

[14] This operationalization is a procedure frequently chosen in analyses of the German DZHW's panel on university enrollment of students eligible for higher education. Here, too, students and respondents with a high intention to enter university are considered in one group (e.g., Helbig et al. 2011; Lörz 2012). In the ZuBAb survey, respondents were asked about their intention to pursue higher education as follows: "What do you think: How likely is it that you will actually go to university?" For the answers a five-point Likert scale was used (1 = "very unlikely" to 5 = "very likely"). All respondents with a value of 4 or 5 are categorized as university students in this study. Even when lower cut-off-values are chosen (not shown), the results do not differ from the ones presented here.

[15] However, the results do not differ when we apply logistic regression models. These are shown in Table A2 in the Appendix.

[16] After exclusion of missing data, information of n = 999 respondents could be included in the analyses. Since the proportion of listwise missing values is very small at three percent, we do impute data.

assignment to the experimental conditions has (almost) no impact on university enrollment. This is clear when looking at the coefficients of group affiliation, which hardly differ from zero. Moreover, the coefficients are not significant. Thus, contrary to our expectations, the investigated individual counseling program cannot be assumed to positively influence university enrollment a few months after high school graduation.

In order to test the second hypothesis on effect heterogeneity by educational origin, we add an interaction term between educational origin and group affiliation. Models H2a and H2b in Table 2 show the results of this estimation. Regarding the hypothesis of interest, which assumes that individuals without university-educated family backgrounds profit more from program participation, the results are, again, not in line with our hypothesis.[17] For both of our operationalizations, focusing on actual educational pathways only (Model H2a) and considering intention to pursue higher education as well (Model H2b), no significant interaction effect between experimental condition and educational background can be found.[18]

[17] In light of research on strong social origin effects at educational transitions, the negative coefficient of the educational background in Model H2a seems counterintuitive at first. However, this can be explained by the fact that the models control for initial intention to attend university in order to increase power. In agreement with earlier research on social inequality in university access, a clear social selection can be found, disadvantaging individuals without a university-educated background, when this variable is not considered (see table A3 in the Appendix, Model H2b).

[18] A separate subgroup analysis of individuals without a university-educated background, as performed in Model H1a and H1b, shows no treatment effect either (not shown).

Table 2: Results of the linear probability models for the program's effect on university enrollment, *intention-to-treat* analysis

	Model			
	H1a	**H1b**	**H2a**	**H2b**
Assignment to the experimental conditions (Reference: Control group)				
Treatment group	-0.001	0.015	-0.029	0.028
	(0.029)	(0.023)	(0.040)	(0.033)
Educational background: (Reference: no university-educated background)				
At least one parent with university degree			-0.099**	0.047
			(0.043)	(0.033)
Interaction effect			0.062	-0.029
(Experimental conditions/educational background)			(0.060)	(0.046)
Constant	0.479***	0.755***	0.512***	0.740***
	(0.088)	(0.068)	(0.091)	(0.068)
Observations	999	999	999	999
Adj. R^2	0.135	0.295	0.138	0.296

Note: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$;
Calculated with robust standard errors, controlled for attended school (school fixed effects) und initial intention to enter university;
Model a: University enrollment operationalized as current educational path,
Model b: University enrollment operationalized as current educational path and intention to enter university

In summary, neither an average effect of the program on university enrollment nor a positive impact for students without university-educated parents can be found based on our data. Considering the importance of these results for the educational potential of individual counseling programs in Germany, and given the program's defined goal to reduce educational inequality, additional analyses checking for the robustness of the results are conducted in the following.

4.3 Robustness check

Experimental designs have numerous advantages over observational data. However, if the underlying statistical assumptions are not met, the internal validity of the estimation of causal effects may be compromised. One of the most important assumption is the *stable unit treatment value assumption* (SUTVA), which can be violated though spillover effects, among others. Another assumption forbids that unit nonresponses are distributed unequally between the experimental conditions in a systematic manner. Both of these assumptions

concern internal validity, or, more specifically, the estimation of the causal effects. In addition, specific patterns of noncompliance may raise questions about the external generalizability of the results. Below, we discuss these three issues for the RCT of the ZuBAb study.

Spillover effects

At its core, SUTVA consists of the assumption that one unit's potential outcome is independent of the treatment received by the other units (see Imbend and Rubin 2015). Among other things, so-called spillover effects can violate this assumption. Theoretically, this is the case if individual A influences another individual B with respect to the investigated outcome (e.g., university enrollment) due to individual A's program participation. In reality, this might happen via friendships between students. As a result of their interactions, a positive treatment effect might then also influence the control group, possibly making its members more likely to enroll in university as well. Thus, the estimation of the treatment effect presented above would be biased downwards.

Aiming to cautiously estimate the robustness of our results regarding spillover effects, we explicitly asked respondents about friendships with individuals taking part in the program. We use this information to recalculate the treatment effect, adjusted for spill-over effects. This procedure is not ideal, because a subpopulation is excluded from the analysis after randomization, potentially jeopardizing group comparability. However, this method could provide evidence of any spillover effects.

Table 3: Results of the linear probability models for the program's effect on university enrollment under consideration of spillover effects, *intention-to-treat* analysis

	Models			
	H1a-SP	**H1b-SP**	**H2a-SP**	**H2b-SP**
Assignment to the experimental conditions (Reference: Control group)				
Treatment group	-0.008	-0.001	-0.027	0.039
	(0.041)	(0.034)	(0.057)	(0.047)
Educational background: (Reference: no university-educated background)				
At least one university-educated parent			-0.105*	0.064
			(0.059)	(0.046)
Interaction effect (Experimental conditions/educational background)			0.054	-0.092
			(0.084)	(0.068)
Constant	0.467***	0.735***	0.497***	0.717***
	(0.100)	(0.076)	(0.103)	(0.077)
Observations	517	517	517	517
Adj. R^2	0.146	0.309	0.149	0.309

Note: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$;
Calculated with robust standard errors, controlled for attended school (school fixed effects) und initial intention to enter university;
Model a: University enrollment operationalized as current educational path,
Model b: University enrollment operationalized as current educational path and intention to enter university

Table 3 shows the results after excluding individuals who had reported having friends participating in the program. Accordingly, the sample is reduced by 482 persons (217 in the control group and 265 in the program group). The results presented in Table 3 do not differ substantially from the results shown in Table 2.[19] We therefore do not find empirical evidence of (strong) spillover effects that could have biased the estimation of the treatment effect.

Systematically unequal panel mortality

Another threat to the internal validity of the calculated estimates appears when panel mortality is distributed unequally between the experimental conditions in

[19] Likewise, a different sample, in which only individuals *in the control group* reporting to have friends who participated in the program are excluded from the analysis, leads to comparable results (not shown).

systematic ways and concerns characteristics associated with the investigated outcome (e.g., Shadish et al. 2002). Unequally distributed panel mortality would lead to a lack of comparability between the randomized groups regarding the outcome under examination.

To rule out systematically unequal panel mortality, we use the respondents' academic performance and their intention to attend university at the time of the first survey as well as their educational background and compare the groups' panel mortality regarding these variables. For this purpose, Table 4 additionally shows participation (or non-participation) in the third survey wave, comparing control and treatment groups with respect to the mentioned characteristics from the first wave. We do not find systematically different panel mortality regarding the investigated characteristics. Between individuals who did not take part in the third wave ("*no participation in W3*," left column) and individuals who did ("*participation in W3*," right column), about the same differences in the investigated characteristics are found for both experimental conditions. If panel mortality were unequally distributed between groups, unequal differences between the left and the right column could be found. Consequently, based on the analysis performed, no systematically unequal panel mortality can be assumed for the groups of the experimental conditions with regard to the investigated characteristics. However, there could still be unequally distributed drop-outs for unobserved characteristics.

Table 4: Differences between treatment and control Group regarding important predictors of university enrollment, differentiated by participation in the third survey wave

	Participation in the panel surveys					
	No participation in W3[1]			*Participation in W3*[2]		
Examined characteristics	*CG*	*TG*	*Diff.*	*CG*	*TG*	*Diff.*
Initial performance level W1	8.54	8.85	-0.32	9.00	9.16	-0.16
N	141	151		485	464	
Initial intention to enter university W1	3.60	3.57	0,04	3.41	3.44	-0.04
N	149	160		285	275	
No university-educated parents	0.53	0.51	0,02	0.54	0.53	0.01
At least one university-educated parent	0.47	0.49	-0.02	0.46	0.47	-0.01
N	140	154		510	489	

Note: TG: Assignment to Treatment Group; CG: Assignment to Control Group; *Diff.*: CG-TG; Operationalization: initial performance level on a 15-point grading scale; initial intention to enter university on a 5-point Likert scale. Differences are reported on the original scale of the construct or as difference in percentage points.

[1] Participation in Wave 1 only or participation in Waves 1 and 2

[2] Participation in Wave 1 and Wave 3 (without participation in Wave 2) or participation in all three waves

Noncompliance

Another challenge for the adequate estimation of the treatment effect is posed by so-called noncompliance, which occurs when study participants do not comply with their randomized assignment to the experimental conditions. Unlike the issues discussed above, the problem of noncompliance does not threaten the internal validity of the estimation of causal effects but raises the question concerning the groups to which the estimate of the program's effect can be generalized (for an overview of procedures for estimating the program effect when facing noncompliance, see Sagarin et al. 2014).

The *intention–to–treat* strategy we follow allows for estimating the treatment effect for the overall population, irrespective of received treatment. This is the case because some individual students are likely to refrain from participating in the program under real-world conditions as well. Besides investigating the effect for the whole population, researchers might also be interested only in the treatment effect for participating students who were motivated to participate in the program by their assignment (*local average*

treatment effect). For this purpose, an estimation of the effect using an instrumental variable is particularly suitable. Here, the randomized assignment is used as the instrumental variable, which affects the outcome indirectly via an endogenous variable (actual program participation) but has neither a direct effect nor an indirect effect via other variables (Howell et al. 2002; Sussman and Hayward 2010). Using this approach, both actual program participation and randomized assignment can be taken into account by weighting the results of the impact analysis with noncompliant participation behavior (Sagarin et al. 2014). The result provides information about the effect of actual participation by those students who were induced to participate by the randomized offer.

In our study, 80.9 percent of the students complied with their randomized assignment.[20] 9.7 percent did not attend counseling sessions even though they had been assigned to the treatment group, and 9.5 percent took part in the program although they had been assigned to the control group (this data refers to the sample examined above). To test the effect for participating students, we estimate the impact using the randomized assignment as an instrumental variable.

Table 5 shows the results of the estimation with instrumental variables (IV). As it turns out, even when considering students' actual behavior, we find no treatment effect on participants' university enrollment (Models H1a–IV and H1b–IV); there is no effect heterogeneity by educational background either (Models H2a–IV and H2b–IV).

[20] Such a proportion of noncompliance is common for RCTs, with estimates stemming primarily from medical studies (e.g., Zhang et al. 2014).

Table 5: Results of the linear probability models for the program's effect on university enrollment, estimation with instrumental variables

	Models			
	H1a–IV	**H1b–IV**	**H2a–IV**	**H2b–IV**
Treatment (Reference: without Treatment)				
Treatment	-0.001	0.023	-0.043	0.043
	(0.043)	(0.034)	(0.059)	(0.049)
Educational background: (Reference: no university-educated background)				
At least one university-educated parent			-0.113**	0.056
			(0.053)	(0.040)
Interaction effect (Experimental conditions/educational background)			0.094	-0.044
			(0.089)	(0.069)
Constant	0.479***	0.752***	0.526***	0.729***
	(0.088)	(0.071)	(0.093)	(0.070)
Observations	999	999	999	999
Adj. R^2	0.134	0.296	0.135	0.296

Note: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$;
Calculated with robust standard errors, controlled for attended school (school fixed effects) und initial intention to enter university;
Model a: University enrollment operationalized as current educational path,
Model b: University enrollment operationalized as current educational path and intention to enter university

5 Summary and discussion

In this article, we explored whether an individual, intensive counseling program fosters university enrollment among students who are eligible for higher education. Given the high level of social selectivity at the transition to tertiary education, we investigated potential differences in treatment effect by educational background; that is, we analyzed whether program participation might be particularly beneficial for children of non-university-educated parents. The program's effect was investigated using a randomized controlled trial. Based on our results, no average effect on university enrollment could be found. Thus, the program did not achieve the intended goal to increase the number of students who enroll in university. Likewise, testing for effect heterogeneity by educational background did not show the expected results. Given the persistent social inequalities in university enrollment resulting, for example, from a lack of information or an experienced social distance to the higher education system among students from non-academic backgrounds, we theorized that issues of this kind could be addressed in the intensive counselling program investigated here. However, contrary to our expectations, no such positive effect of the program could be found for this group. We checked our results for robustness with complementary analyses on possible spillover effects, panel mortality, and actual participation in the program, all confirming the absence of a treatment effect.

These robustness tests are limited methodologically: We could only check whether there is a systematic unequal panel mortality between the experimental conditions based on observed characteristics. Thus, we cannot rule out the possibility of unequal panel mortality on unobserved characteristics, which might account for the program's non-existent impact. Likewise, to assess whether our failure to find an effect might result from spill-over effects, we used a procedure in which the sample was redefined after randomization based on friendship networks. This procedure is much less valid than a randomized allocation to different groups would have been. Ideally, spill-over effects should be determined by identifying three different groups: a program group, a control group at schools where the program is offered, and another control group at schools where the program is not offered. Accordingly, we cannot completely rule out the possibility that spill-over effects within our study schools may have been responsible for the fact that we were unable to detect an effect.

The results of our analyses are at odds with international research literature, which finds individual counseling programs to have both an average

effect on university enrollment and a specific impact on disadvantaged students (see Herbaut and Geven 2020). Both effects could not be replicated in our work. This means, on the other hand, that our results are in line with German research on the impact of short information workshops. Studies in the German do not find an effect on students' intention to pursue higher education (Daniel et al. 2018). Even among students without a university-educated background, a positive effect could only be shown for a very specific subsample (Ehlert et al. 2017).

The strongest limitation of the analyses presented here is the fact that our observational period is comparatively short. The findings on the program's impact six months after high school graduation differ largely from the findings on the program's effect one year later. More specifically, a strong effect on respondents' educational pathways unfolds one and a half years after graduation (Erdmann et al. 2022).

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Appendix

Table A1: Percentage distribution of occupations among individuals who took a gap year (multiple answers were possible)

	Assignment		University-educated background		
Current occupation	Control group	Treatment group	without	with	all
Work & travel	16.3	17	13.1	19.6	16.6
Voluntary military service	4.7	1.8	4.1	2.1	3.3
Volunteer service	38.4	41.2	33.8	43.9	39.8
Internship/further education/training/qualification	18	13.9	15.9	16.4	16
Work	57	59.4	57	52.4	58.2
N	*172*	*165*	*145*	*189*	*337*

Table A2: Results of logistic models (AME) for the program's effect on university enrollment, *intention-to-treat* analysis

	Models			
	H1a	**H1b**	**H2a**	**H2b**
Assignment to the experimental conditions (Reference: Control group)				
Treatment group	-0.000	0.009	-0.027	0.015
	(0.029)	(0.024)	(0.039)	(0.031)
Educational background: (Reference: no university-educated background)				
At least one university-educated parent			-0.094**	0.041
			(0.042)	(0.034)
Interaction effect			0.055	-0.017
(Experimental conditions/educational background)			(0.058)	(0.047)
Observations	999	960	999	960
Pseudo R^2	0.130	0.287	0.134	0.285

Note: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$;
AME calculated with robust standard errors, controlled for attended school (school fixed effects) und initial intention to enter university;
Model a: University enrollment operationalized as current educational path,
Model b: University enrollment operationalized as current educational path and intention to enter university
Due to low variance on the dependent variable in models H1b and H2b for two schools, these are excluded from the estimation by the statistical software. This leads to a somewhat smaller number of cases in these models.

Table A3: Results of the linear probability models for the program's effect on university enrollment, estimation with instrumental variables, *intention-to-treat* analysis WITHOUT control for initial intention to enter university

	Models			
	H1a	**H1b**	**H2a**	**H2b**
Assignment to the experimental conditions (Reference: Control group)				
Treatment group	0.004	0.022	-0.011	0.051
	(0.031)	(0.027)	(0.043)	(0.039)
Educational background: (Reference: no university-educated background)				
At least one university-educated parent			-0.046	0.115***
			(0.046)	(0.038)
Interaction effect (Experimental conditions/educational background)			0.034	-0.065
			(0.063)	(0.053)
Constant	0.406***	0.658***	0.420***	0.622***
	(0.097)	(0.093)	(0.098)	(0.094)
Observations	999	999	999	999
Adj. R^2	0.025	0.043	0.024	0.051

Note: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$;
Calculated with robust standard errors, controlled for attended school (school fixed effects);
Model a: University enrollment operationalized as current educational path,
Model b: University enrollment operationalized as current educational path and intention to enter university

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